Does Intensive Job Search Assistance Help Job Seekers Find and Keep Jobs?*

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Abstract

We study the effects of intensive job search assistance (JSA) targeted to long-term unemployed in Geneva. In 2006, Geneva randomly assigned job seekers to the program, and we follow them from two years prior to five years after assignment to treatment. Treated job seekers leave unemployment faster, especially around six months after starting intensive JSA. Intensive JSA does not affect the total number of job seekers who ever find a job. However, treated job seekers are more likely to leave employment, especially after one year of employment – the period needed to qualify for unemployment benefits. Intensive JSA shortens both job search duration and employment duration. Neither differences in active labor market programs nor re-employment wages rationalize lower job stability. Intensive JSA may have led job seekers to accept jobs that were less well matched, triggering higher employment loss once unemployment benefit eligibility is re-established. Intensive JSA is expensive, and the short-term employment gains do not compensate for the extra cost.

JEL codes: J64, J68.

Keywords: job placement, long-term unemployment, active labor-market policy, private sector, public sector.

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1 Introduction

Long-term unemployment is a pervasive problem in today's rapidly evolving economies. As technology has radically changed the professional activity of most people in the last decades, individuals whose skills are not needed or up-to-date anymore are left behind and those who become unemployed see their skills depreciate even faster. The role of active labor market policies as a tool to correct market failures and improve workers' welfare thus becomes salient.

Job search assistance (JSA) is a cornerstone of active labor market policy. Job search assistance comes in various forms consisting of any combination of the following elements: career counseling and skills assessment, guidance on writing job application packages, help in locating appropriate job openings, and preparation for job interviews. JSA intervenes in all phases of the unemployment spell, its intensity often increasing over the unemployment spell. JSA shortens unemployment duration, and is one of the more successful elements of the mix of active labor market policies.

Does JSA also affect employment stability? JSA might increase the re-employment wage, for example by improving a job seeker's resume or self-esteem. JSA might also improve match quality, for example by matching a job seeker who likes soccer with a boss who likes soccer. If JSA improves job quality or match quality, job stability will increase. But JSA might also reduce job stability. JSA intervenes in the job search strategy of an individual, often quite strongly, by forcing her to apply to a job she might not have considered applying to. If the job offer arrives, the job seeker has to accept this job, or face benefit sanctions. Worse match quality thus reduces job stability.

We study the longer-term effects of offering intensive JSA to long-term job seekers. In 2006, the labor market authorities of Geneva, Switzerland, set-up a pilot study to assess the effectiveness of a novel and intensive JSA program designed by Hestia, a job placement firm, in helping job seekers find jobs. Over a period of one year, a total of 890 job seekers were randomly selected to potentially receive services of the new intensive JSA program. Among those selected, 50 percent were in line with eligibility criteria for the program. Even though not all job seekers received treatment, we will call job seekers randomly selected to potentially receive treatment as the "treated". Control job seekers were not offered to receive the services. We measure "the intention to treat" (ITT) effects by comparing treated to control job seekers, Treated and control job seekers had access to *all* services of the public employment service (PES).

Hestia received payments that depended on the duration a job seeker had been receiving its services. The payment was 1000 Swiss Francs (SFr)[1] per month during the first six months, decreasing to 500 SFr per month for the next twelve months, and decreasing again to 350 SFr per month after 18 months, per job seeker. Once a job seeker left the program, Hestia received a new job seeker, randomly assigned from the pool of long-term unemployed. Decreasing payments per job seeker likely encouraged Hestia to place

[1] 1 SFr $\approx$ 0.62 EUR $\approx$ 0.8 USD in 2006.

job seekers just around the date when benefits decrease, especially at six months, as not many job seekers remained with Hestia for 18 months. Hestia was not remunerated for the types of jobs people find, but labor market authorities signaled that they would set-up a long-term contract with Hestia if the program worked. The possibility to obtain the long-term contract could encourage Hestia to pay attention to quality, or job stability.

We combine data collected in the original pilot study with new data from social security, social assistance, and detailed labor market program records. The merged data base provides information on employment, earnings, income from social insurance programs, along with labor market program participation on the period two years before to five years after job seekers were allocated to treatment.

Like the initial short-term evaluation report Flückiger and Kempeneers (2008), we find that the program was very successful in placing job seekers into jobs during the first twelve months after the pilot started. About six months from the start, when the payment per job seeker drops by 50 percent, employment is four to five percentage points higher, and unemployment benefit receipt about six percentage points lower for treated job seekers.

Turning to the longer-term evaluation, we find that the employment gain from the first twelve months dissipates quickly in the second year, and even turns significantly negative in the third after assignment to program. Analyzing transitions, we find that transitions from unemployment to employment are about 25 percent higher during the first year, peaking strongly around months six when the payment drops. This pattern of effects on job finding is consistent with the decreasing payment structure in Hestia's contract, as we show in a stylized model of Hestia's effort allocation. We also find that about the same number of individuals ever found a job during our observation period. Intensive JSA changed the timing of job finding but did not succeed in finding jobs for people that would not otherwise have found jobs.

Among those who ever found a job, treated job seekers are more likely to leave the new job than control job seekers, especially 13 to 18 months after the new job started. Establishing causality for job finders is challenging since the treated job seekers, on average, have spent less time looking for a job. We address this important point by adding controls for job search duration. Adding this control does little to our findings.

How can intensive JSA hurt job stability? We explore several possible explanations. Hestia might have targeted a specific set of job seekers, those easy to place but also very likely to lose employment. We find no differences among job seekers who ever found a job in terms of the characteristics we observe. Offering intensive JSA might have displaced other services offered by the PES that are central to job stability, for example courses on how to establish a good relationship with your boss. Based on detailed records of ALMP program participation, we find no differences in ALMP participation between treated and control job seekers.[2] We explored changes in log earnings between the previous and new job, but find no differences

[2] The control group participated significantly less in JSA, due to Hestia, and had a higher share participating in nothing.

between treated and control job seekers, except perhaps, those who leave unemployment 18 months after being assigned to treatment.[3]

Hestia intervened actively in the job search process. Hestia contacted firms, and acquired new vacancies for job seekers. Hestia might have succeeded in placing job seekers into jobs that offer somewhat less match quality. By contacting the PES, Hestia has some leverage to interfere directly with job search, by threatening to trigger a benefit sanction process. Hestia's clients might have accepted jobs with lower quality. While we have no direct indicators for match quality, we offer an indirect approach to studying the lower match quality hypothesis. Qualifying for unemployment insurance requires a work history of twelve out of the previous 24 months. If Hestia places more job seekers in jobs that are not well suited to them, employment exits should be higher once job seekers have passed the one year qualifying threshold. We find that treated job seekers leave employment significantly more once they have completed the first twelve months of employment. This is not purely an effect of qualifying for unemployment benefits, as control job seekers do leave employment as well. It is the effect of qualifying, coupled with the willingness to leave the job.

A competing explanation is due to fixed term contracts. Perhaps Hestia specialized in finding positions with fixed-term contracts thereby placing job seekers faster but with lower job stability. The increase in unemployment entries could be triggered by temporary contracts of one year duration. We have no direct data on contract type so we can not rule out that Hestia clients were placed into fixed-term contracts. But we know that only six percent of all contracts were temporary, with a duration of less than three years. Also, about one third of all temporary contracts are shorter than one year, so the unemployment entry effect should also be visible before twelve months, which it is not. These two elements suggest that the fixed-term contract explanation is not the only explanation.

Several studies have documented the short-term effects of JSA.[4] van den Berg and van der Klaauw (2006) study two components of JSA, counselling and monitoring, for high skilled individuals. Neither counselling nor monitoring has an effect on unemployment duration, but monitoring shifts search effort from informal to formal channels. Arni (2015) presents a recent randomized evaluation of a job search assistance program for job seekers aged 45 years or older. The program increases the proportion of job seekers with a job, without shortening job search duration. Many other papers have studied the effects of job search assistance on unemployment duration. Instead of providing detailed summaries, we refer to the survey by Card *et al.* (2010) who show that job search assistance programs generally have fairly positive effects on employment in the short-run. We also assess the effects of intensive JSA in Geneva on how long job seekers take to find a job. We then follow job seekers also into their job and ask how long they keep the job. This has not been

[3]We have no information on occupation or industry, indicators that could also matter for match quality.

[4]Several programs in the US have job search assistance components. Job Corps offers support in education and job finding, see for example Schochet *et al.* (2008). The job training and partnership act offers on-the-job training, and job search assistance to disadvantaged young adults, see for example Bloom *et al.* (1997).

explored very much so far.

Several recent studies document the effects of outsourcing job placement, monitoring, or counselling from the public employment service to private providers. In a non-experimental setting, Cockx and Baert (2015) compare public, private for profit, and private not for profit providers in Belgium and find that both private providers shorten unemployment duration of the long-term unemployed, at the cost of employment stability. In a randomized experiment, Bennmarker *et al.* (2013) study young, disabled, and immigrant, job seekers in Sweden, and find no significant difference in the probability of employment between public and private providers, the privately placed job seekers have higher work income up to 12 months after the experiment. In a randomized experiment, Rehwald *et al.* (2015) study Danish job-seekers with a university degree, and find private providers deliver more intensive services, but public and private provision of employment services are equally effective and equally costly from a public spending perspective. In a randomized experiment, Behaghel *et al.* (2014) study intensive private, intensive public, and standard, less intensive, public provision of job placement services, for about 200,000 job seekers in France. They find that intensive counseling leads to faster transition to employment for both public and private providers, but that the public program outperforms the private one in all indicators considered. In a German randomized experiment, Krug and Stephan (2013) find that public in-house provision of services reduces accumulated days in unemployment by one to two months but that about two thirds of this effect is attributable to labor market withdrawals.

Our study is similar to the outsourcing literature in that we study intense JSA as offered by a private provider, Hestia. However, private provision of active labor market programs is the norm in Switzerland. A key difference lies in the interpretation of the treatment. An outsourcing setting normally replaces a mix of public services with a mix of privately provided services. In contrast, job seekers assigned to intensive JSA continued to have the same services as job seekers in the control group. Intensive JSA comes, as we show in the body of the paper, in addition to the existing services, rather than by replacing services. Treatment effects are therefore due to offering more intense JSA compared to nothing in our setting, whereas treatment effects are harder to interpret in an outsourcing setting.

Several earlier papers have found that active labor market programs can work through their threat effects, often more effective than training itself. Black *et al.* (2003) show that job seekers leave unemployment more quickly when they receive a letter to report for training. Rosholm and Svarer (2008) provide estimates of the threat effects of active labor market programs in a duration setting. Graversen and van Ours (2009) show that job seekers who live far from the treatment site are more likely to lave unemployment. Arni *et al.* (2013) show that unemployment benefit sanctions increase exits from unemployment but sanctioned individuals fare worse after leaving unemployment. Arni *et al.* (2015) confirm this threat effect of benefit sanctions and employment programs but find attraction effects from other labor market training. We complement this literature by finding negative medium-run effects of intensive JSA. Unlike threat effects, which occur ex ante, negative effects of job search assistance occur, presumably, because job search assistance forces job seekers to accept worse jobs.

Several recent studies study the job quality effects of unemployment benefit payments, for example the total number of benefit weeks, or potential benefit duration (PBD). Card *et al.* (2007) and Lalive (2007) find little evidence on wages and/or job stability in an Austrian context. van Ours and Vodopivec (2008) find that a reduction in the potential benefit duration has only small effects on wages, on the duration of subsequent employment and on the probability of securing a permanent rather than a temporary job. Schmieder *et al.* (2016) study the effects of PBD changes on re-employment wages in Germany finding sharp negative effects of PBD extensions for older workers, as we do. Two studies on the Austrian context find positive effects of benefit extensions. Degen (2014) and Nekoei and Weber (2016) study the effects of PBD for job quality in Austria, exploiting a sharp increase in PBD from 30 to 39 weeks for workers aged 40 years or older. Both papers find a positive effect of prolonged PBD on wages on the order of 0.5 percentage points. Nekoei and Weber (2016) rationalize this finding in a directed job search framework and discuss the implications of this finding for policy. Adopting similar identification strategies as Schmieder *et al.* (2016) and Nekoei and Weber (2016) we study the effects of intensive JSA on job stability and wages.

The rest of the article is organized as follows. Section 2 gives an overview of the context in which the experiment has been implemented, describes the experimental setup, and discuss what outcomes can be expected from such a scheme. Section 3 presents the data and provides descriptive statistics. Section 4 explains the empirical framework. Section 5 presents the results. Section 6 explores different mechanisms that could explain the results. Section 7 concludes the study.

2 Experiment

In this section, we present the environment in which the experiment has been conducted, explain the experimental setup and then discuss the incentives faced by the private job placement provider.

2.1 Background

In what follows, we explain how the unemployment insurance works in Switzerland and provide more specific details about the labor market in Geneva. This section draws extensively on our earlier work on Swiss active labor market policies (Lalive *et al.*, 2008b, 2005; Arni *et al.*, 2013).

Participants in the experiments were job seekers. The rules concerning benefit eligibility are the same all across Switzerland. Job seekers need to fulfil two requirements in order to be eligible for unemployment insurance benefits. First, they must have paid unemployment insurance taxes for at least twelve months in the two years prior to registering at the public employment service (PES). Job seekers entering the labor market are exempted from the contribution requirement if they have been in school, in prison, employed outside of Switzerland or have been taking care of children. Second, job seekers must possess the capability to fulfill the requirements of a regular job – they must be "employable". During the unemployment spell, job seekers have to fulfill certain job search requirements and participate in active labor market programs in order to remain eligible for benefits.[5] Job seekers who are ineligible for unemployment insurance can claim social assistance. Social assistance is means tested and replaces roughly 76 percent of unemployment benefits for a single job seeker with no other sources of earnings (OECD, 1999).

Job seekers are eligible for 18.5 months of benefit payments during a two-year framework period. Job seekers aged 55 years or older who had contributed for at least 18 months prior to entering unemployment are eligible for two full years. The replacement ratio is 80 percent for low-income workers (earning less than 3,536 SFr before unemployment.[6] The replacement rate is 70 percent for high-income workers (earning more than 4,340 SFr) and is smoothly adjusted between so that there are no discontinuities in the replacement rate.[7] Job seekers pay all earnings and social insurance taxes except the unemployment insurance tax rate (which stands at about two percent) so the gross replacement rate is similar to the net replacement rate.

Active labor market policy might vary across Switzerland but the main features of the system are homogeneous. Job seekers are in regular contact with a caseworker at one of about 150 PES offices. When individuals register at the PES office, they are assigned to a caseworker on the basis of either previous industry, previous occupation, place of residence, alphabetically or the caseworker's availability. Job seekers meet at least once a month with the caseworker. Caseworkers monitor job search by checking that job seekers fill in the details of the jobs to which they have applied. Job seekers are typically required to apply to about ten jobs per month. Caseworkers have some discretion to adjust this target. They count the number of new applications in all cases and they may also check up on the applications claimed by job seekers. Participation in a labor market program is monitored by the caseworker because program suppliers only get paid for the actual number of days a job seeker attends the program.

The experiment is set in the canton of Geneva which consists mainly of the city of Geneva. With about 200,000 inhabitants, Geneva is home to a range of international organizations and features a large financial sector. The experimental context is hence typical of large cities, where the occupational dimension of the labor market plays a larger role than the geographical one. Geneva has higher unemployment than the rest of Switzerland. Around the time of the experiment, in April 2006, 7.1 percent of Geneva's workers were seeking employment, whereas only 3.5 percent of workers in all of Switzerland were seeking employment.

[5] See Gerfin and Lechner (2002) and Lalive *et al.* (2008a) for detailed background information on and an evaluation of the active labor market programs.

[6] 1 SFr $\approx$ 0.93 EUR $\approx$ 0.99 USD as of December 2016. Source: XE currency converter.

[7] Benefits insure monthly earnings up to a top cap. The cap is currently at 10,500 SFr per month. See Eugster (2013) discusses effects of the benefit replacement rate in the Swiss setting.

Altogether, the Geneva labor market poses a challenge for workers wanting to re-enter a job. Each canton is in charge of organizing its active labor market policy and Geneva is known to rely less on unemployment benefit sanctions than the nation and somewhat more on training programs.

2.2 Experimental design

The evaluation of the job search assistance scheme is based on a randomized controlled experiment which took place in the canton of Geneva, Switzerland, between 2006 and 2007. The goal of this experiment was to determine whether using additional JSA could help lower the comparatively high level of long-term unemployment in Geneva. Note that the word "additional" is used to emphasize that the JSA program was set up as a complement to the standard PES track, and not as a substitute.

On the one hand, the private but non-profit firm, called "Les Maisons Hestia" (henceforth Hestia) offered a tailor-made monitoring program with two phases. The catch-up phase, which lasted a few of months, aimed to improve some of the job seekers' skills. It provided workshops such as job interview training, networking, and resume writing. Once all their candidates had upgraded their skills and CVs, Hestia moved on to a search-and-place phase. Most of the effort was devoted to collect vacancies, call firms to ask about unadvertised positions, transmit them to their candidates, and follow up applications. Hestia's self-proclaimed goal was to find jobs that fit well with their candidates' needs and profiles so as to maximize their chances of success in their new jobs and make sure they reach stable positions.

On the other hand, the public placement service offered a more standard program aiming to help and encourage the unemployed in their job search and application process without playing a truly active role in it. As a means of comparison, Hestia had a team of three consultants plus two telemarketing operators for a pool of about 100 job seekers, while the public scheme has one or two employees per 100 job seekers on average (Flückiger and Kempeneers, 2008). All individuals enrolled in the experiment had to follow the PES track, while the treatment group had to follow Hestia's program additionally. Note that if an individual allocated to the treatment group found a job (as a result of the JSA program) and lost it before the end of the experiment, she would not return to Hestia's program but be taken care of by the PES.

The partnership with Hestia was a pilot project monitored by the PES in Geneva. It had a fixed duration of one year but could potentially be extended to a contract of indefinite duration in case of positive results, which was Hestia's goal. Note that the decision about a possible extension had not been transmitted to Hestia until the very end of the pilot. On top of the long-term incentives linked to the potential contract renewal, Hestia was provided with short-term incentives in the form of a payment scheme. Hestia did not receive money upon placement but instead was given a decreasing monthly flat rate per job seeker enrolled. It received 1000 SFr a month per job seeker enrolled in their program for six months or less; 500 SFr per job seeker already enrolled in their program for six to 18 months; and 350 SFr per job seeker enrolled for more than 18 months. Thus, the total amount received by Hestia for a job seeker that stays one full year in the program is 9000 SFr. The idea behind this payment scheme was to encourage Hestia to place job seekers rather fast but also to give money in the medium-run in order to avoid a potential "parking problem", that is, providing minimal effort to the harder-to-place job seekers.

The experiment targeted long-term unemployed job seekers. Due to Hestia's capacity constraint, it was based on a cohort system and saw ten cohorts of randomly allocated job seekers entering the experiment between October 2006 and July 2007. The randomization was as follows. In September 2006, a group of experts responsible for the pilot project selected a first cohort of reference population made of individuals who had accumulated twelve months of unemployment (but not more) as of September 2006. The stratification of the reference population was done by PES agency (each agency takes care of a specific category of job seekers) and respected the relative importance of each agency in the reference population (for example agency one represents 22 percent, agency two 13 percent, etc.). Specifically, the treatment allocation followed a random draw of 100 individuals from the reference population, taking 22 from agency one, 13 from agency two, etc. to be sent to the treatment group. The control group was then formed of the reference population minus those allocated to treatment. After this first stage, the lists were sent to each PES agency for an eligibility check (eligible to unemployment benefits and/or to labor market measures, not on medical leave or maternity leave, and not currently involved in any cantonal program for criminals) to ensure that job seekers were immediately available to participate in the experiment.

After one month of experiment Hestia announced how many job seekers it had placed and thus how many new ones it needed to stay at full capacity. The whole process was repeated with new job seekers who had accumulated twelve months of unemployment as of October 2006 to create the second cohort. The same was procedure was used each month until the end of pilot.

We observe that about half the individuals sent to the treatment group did not actually participate to the experiment. One might think that this rather low participation rate indicates a serious problem of compliance, which will bias our results. However, it is actually not due to compliance problems but rather to organizational problems when setting the experiment, screening the potential subjects and splitting them into groups. All individuals leaving the experiment were asked about their reason for not following the treatment and the main reasons were: 22 percent had subsidized employment; 17 percent had already found a job; 13 percent were following another labor-market measure. A myriad of other reasons were also given but refusal to follow the intensive placement program account for only 2 percent of the total. We show in Section 3.1 that the difference in characteristics between the control and the treated group are not statistically significant. We keep the job seekers assigned to treatment but not following it in all our analyses and report "intention to treat" (ITT) effects

The first cohort entering the experiment saw 355 job seekers being split between the private provider

(60 percent) and the public one (40 percent), while the following nine cohorts were designed to ensure that Hestia had at least 100 job seekers to take care of. The reason why the first cohort exceeds Hestia's capacity is that the organizers anticipated the potential problem of non-compliance, thus setting a larger first wave. Note also that the upper limit of 100 job seekers was self-imposed by Hestia to ensure they could provide high-quality services.

2.3 Theoretical predictions

In this section, we come back to the incentives facing Hestia, discuss how they could lead to specific behaviors, and what differences in outcomes can be expected between treatment and control groups.

Hestia faces two types of incentives. In the short- and medium-run, it receives money in exchange of providing services to job seekers. In the long-run, it hopes to be able to convert the "pilot project" into a contract of indefinite duration and secure its collaboration with the PES.

Consider first the short- and medium-run incentives. The monthly flat fee per job seeker enrolled is monotonically decreasing. Its highest value is in the first six months of the experiment. However, it never goes down to zero and its minimum value (from 18 months onward) remains high enough to cover administrative costs and minimal services to job seekers. Such a scheme raises two major concerns. First, a profit-maximizing firm could enroll as many job seekers as possible, and focus exclusively on the easier-to-place ones in order to pocket the high fee in the first months and receive new job seekers to place. Second, it could provide minimal services to the harder-to-place individuals, keeping them for the extra cash that they bring in the long run.

The former problem is similar to that of cream-skimming, where a provider chooses the individuals that it wishes to enroll based on their potential. The latter problem refers to parking, where the private firm offers minimal services to the harder-to-place job seekers. This financial scheme would thus not be very satisfying if it were not accompanied by a condition on the maximum number of job seekers that Hestia can have. Namely Hestia's capacity must not exceed 100 job seekers at a time. This upper limit has two effects. On the one hand, it ensures that Hestia's staff is large enough to provide optimal services to all their job seekers. It does not force Hestia to provide the same services across individuals with different placement prospects but it is a good step in the direction of limiting the parking problem. On the other hand, it prevents Hestia from focusing only on easy-to-place job seekers. Assuming that the proportions of easy-to-place job seekers in the population of unemployed is a third and constant (for the sake of this example), focusing heavily on these individuals is not sustainable in the long-run. Indeed, after placing all easy-to-place individuals of the first cohort, the second cohort will only replace a third of them with new easy-to-place job seekers, while the rest of the top-up will have lower placement prospects. Thus, the share of harder-to-place individuals enrolled will be growing over time, until the point where Hestia has only hard-to-place individuals. This example is of course extreme but it illustrates well the problem of focusing only on a certain type of individuals while having capacity constraints. This effect should both limit the problem of cream-skimming and parking. Additionally, note that Hestia was not able to fully cream-skim by choosing whom to enroll, since job seekers were randomly attributed to one of the two programs by the PES. Altogether, short-run incentives should then improve short-run return to employment (or alternatively exit from UB) and even out the differences in outcomes (e.g. UB received, employment rate, etc.) across job seekers' types.

Let us focus now on the long-run incentives. Hestia's goal is to secure its place in the market for services to job seekers in Geneva. To achieve it, it needs to show that it can fulfill its goals and outrun the PES when it comes to providing service to long-run job seekers. This brings us to Hestia's mission, which is to lower the long-term unemployment rate by improving job re-entry and providing job seekers with jobs that fit their needs and are stable over time. Its main force compared to the PES is a much higher staff to job seekers ratio, which allows pro-active services such as calling firms to find unadvertised job vacancies. Overall, long-run incentives should thus materialize in the form of a higher employment rate and more stable jobs over time.

3 Data

We use two different types of data. The first type are the data collected in the experiment described in Section 2.2, while the second type are administrative data about the individuals who took part in the experiment.

The data specific to the experiment contains various socio-demographic and job related variables, such as gender, marital status, education, age, residence permit, and placement prospects. The variable "Placement prospects" is an indicator created by a PES caseworker when a new job seeker enters the unemployment database. It groups the job seekers into four categories according to their personal and professional background: excellent placement prospects means that the job seekers does not need any help for finding a new job; good placement prospects indicates that the job seekers needs very little support; average placement prospects means that the job seeker's background is not as good as the first two categories and/or that the individual suffers from lower than average professional qualification; and poor placement prospects, meaning that on top of having a weaker background, the individual may lack professional qualifications and/or even base qualification (e.g. poor education). The creation of this variable relies on objective measures linked to the job seekers abilities and experience, but can also incorporate soft-information gathered by the consultant in charge of this person, such as past placement of similar job seekers, current labor market situation, observed motivation, etc. Note that this variables had been created before allocating job seekers into the control and treatment group, and is thus available for everyone in our sample.

We use administrative data from the Social Security Administration (SSA) to track labor market histories of job seekers in the experiment. The main purpose of this data is to keep track of labor market participation in order to assess old age or disability pension eligibility which depends on social security contributions. Both firms and unemployment insurance agencies inform the SSA every year about total earnings and start and end month of a spell of employment or unemployment because the SSA levies taxes on earnings from employment and unemployment. From this raw data, we construct a detailed monthly calendar that spans two years prior to the experiment and up to five years after it. In each month, we have information on whether the individual receives any earnings from employment (regardless of whether this is self-employment or salaried employment) or from unemployment benefits. We use this information below to characterize whether someone is employed without unemployment benefits, receiving unemployment benefits, or neither of the two. We center individuals' administrative record data around the time when they enter into the experiment. In all table and figures, $t = 0$ corresponds to the month when the individual was assigned to Hestia's placement service or to the control placement service.

In addition to SSA data, we have social assistance records for the same time period. This data is similar to the SSA, in the sense that it contains monthly payments to beneficiaries enrolled in the experiment. Not all individuals in the experiment necessarily have social assistance records but this allows us to check whether some leave the JSA program to claim social assistance. Finally, we also have the list of ALMPs followed by job seekers enrolled in the experiment.

3.1 Descriptive statistics

Table 1 shows descriptive statistics on the variables specific to the experiment. The table also indicates the number of individuals allocated to the control group and to the treatment group (Hestia).

We have roughly 50 percent of men and women in both groups and about half of the job seekers are married. 25 percent of individuals have between one and three year of work experience, while more than 50 percent benefit from more than three years of experience. Around 10 percent of the sample is below 25 years old, 15 percent is above 55, and the rest is evenly spread in between. The highest education achieved is compulsory schooling for 40 percent of job seekers, secondary education (e.g. high-school) for 36 percent, and tertiary education (e.g. university level) for 20 percent. Half the sample has a Swiss citizenship and a third has a permanent residence permit. Finally, around 60 percent of job seekers are reported to have good or excellent placement prospects, 20 percent average placement prospects, and 20 percent poor placement prospects. All characteristics are equally balanced in both groups, which reflects the random allocation of individuals. This is confirmed by the sixth column, which indicates the difference between the control and treatment groups, and the seventh column, where we report t-tests on the differences.

Table 2 describes the same data as the one used for Table 1 but we now split up the treatment group (Hestia Group) into individuals who received the treatment and those who did not. We observe some im-

Table 1: Summary statistics

	Control Group		Hestia Group			
Variable	Mean (%)	s.e.	Mean (%)	s.e.	Difference (%)	t-stat
Women	50.5	0.03	48.8	0.02	1.7	0.50
Marital status						
Single	34.9	0.02	33.2	0.02	1.7	0.53
Married	50.8	0.03	53.5	0.02	-2.7	-0.80
Widower	0.5	0.00	0.4	0.00	0.1	0.30
Divorced	13.8	0.02	12.9	0.01	0.9	0.37
Experience						
None	1.3	0.01	2.5	0.01	-1.2	-1.33
Less than 1 year	7.1	0.01	8.6	0.01	-1.5	-0.80
1-3 years	25.4	0.02	26.0	0.02	-0.6	-0.20
More than 3 years	55.8	0.03	50.8	0.02	5.0	1.49
Age						
17-24	8.2	0.01	10.4	0.01	-2.2	-1.10
25-34	27.5	0.02	31.2	0.02	-3.7	-1.21
35-44	27.5	0.02	27.0	0.02	0.6	0.19
45-54	21.4	0.02	18.9	0.02	2.5	0.91
55-64	15.3	0.02	12.5	0.01	2.8	1.20
Schooling						
Compulsory	40.2	0.03	42.6	0.02	-2.4	-0.71
High-school level	36.0	0.02	35.9	0.02	0.0	0.01
University level	19.8	0.02	18.4	0.02	1.5	0.55
Workers						
Swiss	52.1	0.03	49.8	0.02	2.3	0.68
C permit	30.2	0.02	29.3	0.02	0.9	0.28
Other	17.7	0.02	20.9	0.02	-3.2	-1.19
Placement prospects						
Excellent	4.5	0.01	6.2	0.01	-1.8	-1.16
Good	55.0	0.03	52.9	0.02	2.1	0.62
Average	23.0	0.02	20.9	0.02	2.1	0.75
Poor	17.5	0.02	19.9	0.02	-2.5	-0.93
Number of observations	378	-	512	-	-	-

Notes: Table 1 presents summary statistics on the variables specific to the experiment. Some categories may not add up to 100 percent due to missing observations. The sixth column calculates the difference between control and treatment groups, defined as control minus treatment. The seventh column reports two-sided t-statistics on the differences.

portant differences with respect to work experience and age. Treated individuals had more work experience (56 percent with three years or more compared to 46 percent in the non-treated group) than non-treated individuals. Treated individuals were also older than the non-treated individuals (17 percent treated individuals aged 55-64 years vs 7 percent in the non-treated group). This is not surprising as the pilot program carefully screened job seekers to focus only on those that were immediately ready to start a job.

In what follows, we will keep anyone allocated to the treatment in that group and report intention-to-treat (ITT) effects. ITT analysis includes all individuals previously assigned to the treatment. It ignores withdrawals, protocol non-compliance, or more generally everything that happens after the random allocation. ITT results are usually more conservative because of the dilution of the treatment effects due to withdrawals and non-compliance.

4 Econometric approaches

In this section, we briefly discuss the different econometric methods used in the paper, namely linear regressions and transition analysis. We also give details on the variables used and how we construct our different analyses.

4.1 Linear regressions on labor market states

We start our analyses by looking at the monthly employment patterns of individuals. We can infer from our administrative dataset whether an individual works and/or receives UB each month during the whole observation window. We classify individuals in three mutually exclusive labor market states in order to isolate the different effects of the treatment. The states are: (i) employed, defined as having a positive income from work, and receiving no unemployment benefits; (ii) receiving unemployment benefits (with and without work income); and (iii) unemployed but without unemployment benefits (i.e. dependent on social assistance or another insurance scheme). Theses states represent any situation in which an individual can be at a given point in time. There are meant to describe the standard cases of a regular employee who works for her living, that of an individual receiving UB because she is unemployed or in a subsidized job, and that of an individual who has exited the labor force and cannot (or does not) claim UB. They can be seen as a snapshot of the employment situation of all the individuals in our sample for a given month. Figure A1 in the appendix shows the evolution of the labor market states for the whole sample over time.

We study the evolution of these labor market states using graphical evidence and OLS regressions. The randomized nature of our experiment makes it possible to simply plot labor market states in order to highlight the effects of the treatment. However, we also rely on linear regressions and control for individual characteristics to account for potential differences due to the small size of the sample. The model that we

Table 2: Summary statistics

			Hestia Group									
	Control Group (1)		Treated Group (2)		Non-treated Group (3)		Difference between groups					
Variable	Mean (%)	s.e.	Mean (%)	s.e.	Mean (%)	s.e.	(1)-(2)	t-stat	(1)-(3)	t-stat	(2)-(3)	t-stat
Women	50.5	0.03	49.6	0.03	48.0	0.03	0.9	0.23	2.5	0.62	1.6	0.36
Marital status												
Single	34.9	0.02	30.8	0.03	35.7	0.03	4.2	1.10	-0.8	-0.20	-4.9	-1.19
Married	50.8	0.03	55.0	0.03	52.0	0.03	-4.2	-1.05	-1.2	-0.29	3.0	0.68
Widower	0.5	0.00	0.4	0.00	0.4	0.00	0.1	0.27	0.1	0.24	-0.0	-0.02
Divorced	13.8	0.02	13.8	0.02	11.9	0.02	-0.1	-0.03	1.9	0.68	1.9	0.66
Experience												
None	1.3	0.01	1.2	0.01	4.0	0.01	0.2	0.19	-2.6	-1.94	-2.8	-2.01
Less than 1 year	7.1	0.01	6.9	0.02	10.3	0.02	0.2	0.11	-3.2	-1.36	-3.4	-1.37
1-3 years	25.4	0.02	23.8	0.03	28.2	0.03	1.6	0.45	-2.8	-0.77	-4.3	-1.11
More than 3 years	55.8	0.03	55.8	0.03	45.6	0.03	0.1	0.01	10.2	2.51	10.1	2.30
Age												
17-24	8.2	0.01	10.0	0.02	10.7	0.02	-1.8	-0.77	-2.5	-1.04	-0.7	-0.26
25-34	27.5	0.02	31.5	0.03	31.0	0.03	-4.0	-1.09	-3.4	-0.93	0.6	0.14
35-44	27.5	0.02	21.9	0.03	32.1	0.03	5.6	1.62	-4.6	-1.24	-10.2	-2.61
45-54	21.4	0.02	19.6	0.02	18.3	0.02	1.8	0.56	3.2	0.98	1.4	0.39
55-64	15.3	0.02	16.9	0.02	7.9	0.02	-1.6	-0.53	7.4	2.94	9.0	3.11
Schooling												
Compulsory	40.2	0.03	44.6	0.03	40.5	0.03	-4.4	-1.10	-0.3	-0.07	4.1	0.95
High-school level	36.0	0.02	33.1	0.03	38.9	0.03	2.9	0.76	-2.9	-0.74	-5.8	-1.37
University level	19.8	0.02	18.5	0.02	18.3	0.02	1.4	0.44	1.6	0.50	0.2	0.06
Workers												
Swiss	52.1	0.03	50.8	0.03	48.8	0.03	1.3	0.33	3.3	0.81	2.0	0.44
C permit	30.2	0.02	31.2	0.03	27.4	0.03	-1.0	-0.27	2.8	0.76	3.8	0.94
Other	17.7	0.02	18.1	0.02	23.8	0.03	-0.4	-0.11	-6.1	-1.83	-5.7	-1.59
Placement prospects												
Excellent	4.5	0.01	8.1	0.02	4.4	0.01	-3.6	-1.79	0.1	0.08	3.7	1.74
Good	55.0	0.03	53.5	0.03	52.4	0.03	1.6	0.39	2.6	0.65	1.1	0.24
Average	23.0	0.02	19.6	0.02	22.2	0.03	3.4	1.04	0.8	0.23	-2.6	-0.72
Poor	17.5	0.02	18.8	0.02	21.0	0.03	-1.4	-0.44	-3.6	-1.11	-2.2	-0.62
Number of observations	378	-	260	-	252	-	-	-	-	-	-	-

Notes: Table 2 presents a breakdown of the summary statistics on the variables specific to the experiment. Some categories may not add up to 100 percent due to missing observations. Columns seven to twelve calculate the difference between control and treatment groups, defined as control minus treatment and report two-sided t-statistics on the differences.

estimate is given by:

$$Y_{it} = \alpha + X_i'\beta + \mathcal{T}_t'\gamma + D_i * \mathcal{T}_t'\delta + u_{it} \quad (1)$$

where Y_{it} is the dependent variable for individual i at process time t, X_i is a vector of individual-specific, time-constant controls, D_i is a dummy taking the value one if a job seeker is assigned to the treatment (Hestia), and zero otherwise. $\mathcal{T}_t$ is a vector of time period dummies. $D_i * \mathcal{T}_t'$ is a vector of interaction terms between the treatment dummy and the time dummies, and u_{it} is the error term.

The parameter γ measures the detailed outcome dynamics for each outcome. The parameter δ measures the intention-to-treat effect (ITT). We report the components of δ in two separate groups. The first group contains the effects during before assignment to treatment has taken place so we can assess whether outcomes are balanced at baseline. To correctly identify the ITT, we need to make sure that the randomization worked. In other words, both groups should have similar outcomes had the treatment not been given. We can test balance of outcomes before the treatment with this set parameters. We call these effects "Randomization" parameters. The second group contains the effects after assignment to treatment has taken place. These parameters provide evidence on the effects of assignment to treatment. We call them "Treatment effects".

4.2 Transition analysis

The nature of our data suggests that we could also evaluate the treatment effects on transitions from one state to another. A natural point to start from is to look at how much time job seekers in each program need to find a new job, and how long they keep it.[8] Following Kaplan and Meier (1958), the nonparametric estimate of the survivor function is given by:

$$\hat{S}(t) = \prod_{j|t_j \leq t} \frac{n_j - d_j}{n_j} \quad (2)$$

where n_j is the number of individuals at risk of failure before time t_j, t_j represents the time at which failure occurs, and d_j is the number of failures at time t_j. This function estimates how long individuals "survive" in a given state. In what follows, we look at survivor functions for unemployment (i.e. how long it takes for job seekers to find a new job) and for employment (i.e. how long those who have found a new job stay in their new position).

We can push the analysis further by estimating the rates at which the transitions take place using Cox regressions (Cox, 1972). This will allow us to get an estimate of the hazard rate while controlling for other

[8]Note that when relying on this approach, we do not consider for the analysis all individuals who started the experiment being already employed.

factors. The hazard rate a time t is given by:

$$\lambda(t|X) = \lambda_0(t)\exp(X\beta') \tag{3}$$

where $\lambda_0(t)$ is the (unspecified) baseline hazard function, and X a vector of covariates. Note that two assumptions are required for the model to be valid. First, censoring must be non-informative.[9] In other words, cases of censoring (e.g. failure unobserved for some individuals) must not be related to the probability of an event occurring. This assumption is satisfied by design in our study as we follow individuals using administrative data and sample attrition is very low. The second assumption is that of proportional hazards. In our context, this means that the survival curves for the control and the treatment groups must have hazard functions that are proportional over time after controlling for other factors. Following Grambsch and Therneau (1994), this assumption can be formally tested using scaled Schoenfeld residuals. In our case, we cannot reject the proportional-hazard assumption both for the estimations on unemployment (chi-square p-value of 0.37 for the whole model) and on employment (chi-square p-value of 0.40 for the whole model).

5 Results

This section presents the main results. It is divided into three parts. The first subsection discusses short-term effects observed in the first twelve months following the start of the experiment. The second subsection considers long-term effects taking place between one and five years after the experiment. Finally, the third section presents a cost-benefit analysis.

5.1 Short-term effects

This subsection analyzes the effects of the treatment in the year following the start of the experiment. We first look at monthly employment patterns and, second, at transition from unemployment to paid job.

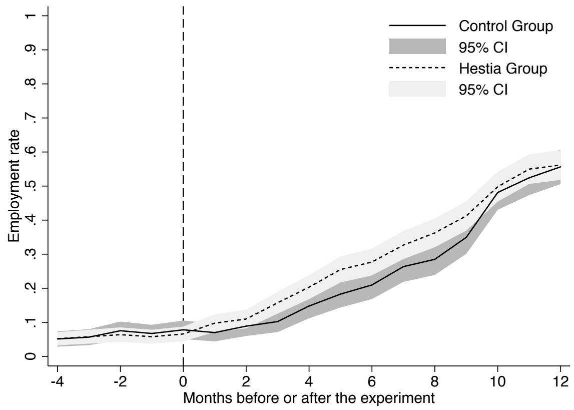
5.1.1 Labor market states

We start by looking at the share of individuals who are employed from a descriptive perspective, and then using OLS regressions. Figure 1 plots the fraction of individuals who are employed and the difference between the two groups. We consider an individual as employed if her work income for a given month is greater than zero and she does not receive any unemployment benefits. Note that the timeline on the X-axis is normalized for each individual. Month zero represents the allocation into the treatment and control groups. For the first cohort, it corresponds to October 2006. For the second cohort, it corresponds to November 2006,

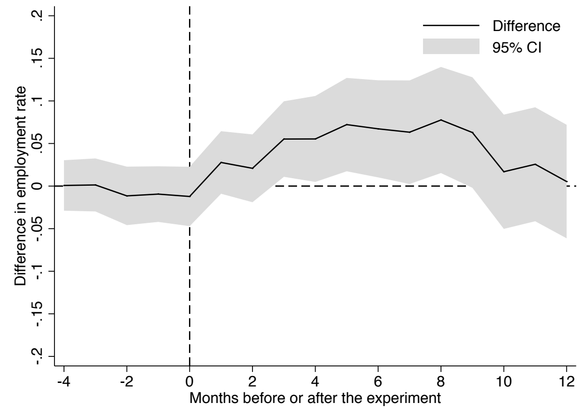
[9] Note that this assumption is also needed for the Kaplan-Meier estimates to be valid.

etc. Consequently, graph (a) can be read as the average employment rate in each group at a given point in time, while graph (b) takes the differences between these two rates, defined as treatment minus control.

Figure 1: Short-term effects on employment



(a) Employed



(b) Employed (difference)

Notes: Figure 1 plots the fraction of individuals who are employed (left) and the difference between the two groups (right), defined as treatment minus control.

Consider first graph (a). We observe that the two groups have an employment rate close to zero in the four months before the start of the experiment. This is mechanical as participants were only selected among long-term unemployed job seekers. Shortly after the start, the average employment rate of both groups increases steadily to reach 55 percent after one year. The treatment group seems to find jobs at a higher pace until the tenth month, where the control group catches up.

Consider now graph (b), which highlights the difference between the employment rates of the two groups. No pre-treatment differences arise in the four month before the experiment, suggesting that that the randomization worked well and that the observed differences are causal. A significant difference in favor of the treatment group arises quickly. Job seekers following Hestia's JSA program enjoy a six percentage point higher employment rate between the third and the ninth month. This differences vanishes after the tenth month, when the control group catches up. Figure A2 in the appendix provides the same graphs for the other two labor market states.

While descriptive evidence already suggests positive short-term effects of Hestia's JSA, we formally test for differences in all three labor market states. Table 3 presents the results of OLS regressions on the three mutually exclusive labor market states in which job seekers can be: (i) employed, defined as having a positive income from work, and receiving no unemployment benefits; (ii) receiving UB; and (iii) unemployed but without unemployment benefits (i.e. dependent on social assistance, on another insurance scheme, or in full-time education). All three states are continuous variables ranging between zero and one. We only

Table 3: Short-term effects on labor market states

	Employed, no UB		UB recipients		Unemployed, no UB	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Treatment Effects						
Hestia × 1-3 m. after	0.012	-0.001	-0.022	-0.014	0.010	0.015
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Hestia × 4-6 m. after	0.061***	0.046**	-0.078***	-0.069***	0.017	0.023
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
Hestia × 7-9 m. after	0.069**	0.050*	-0.095***	-0.075**	0.025	0.025
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Hestia × 10-12 m. after	0.035	0.023	-0.041	-0.036	0.006	0.013
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
B. Randomization						
Hestia × 4-1 m. before	-0.005	-0.021	-0.012	-0.001	0.017	0.022
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Control variables	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.141	0.188	0.172	0.251	0.017	0.088
Individuals	874	844	874	844	874	844

Notes: Table 3 reports point estimates of OLS regressions on the three labor market states from four months before to twelve months after the start of the experiment. All three states are continuous variables ranging between zero and one. The constant is included in the regressions but not reported here. Control variables include: gender, age, marital status, schooling, nationality, mother tongue, residence permit, professional qualifications, placement prospects, OCE job code, and cohort number. Standard errors clustered at an individual level in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

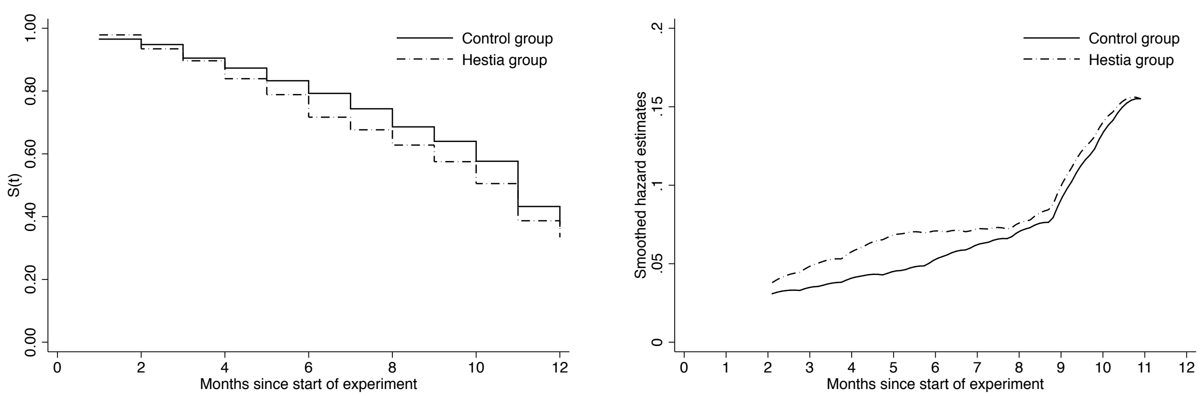
report estimates the parameter δ from equation 1. Coefficients can thus be interpreted as percentage point changes with respect to the control group for the given time period. We estimate the same model twice for each dependent variable. First without control variables (baseline model), and then adding control variables (main model). Regressions without controls are run on 874 individuals as the SSA data is missing for 16 of them, while regressions with controls lose another 30 individuals for whom we do not have information on education. Coefficients are grouped in two categories. The first part highlights treatment effects on the treatment group (Hestia) on a twelve-month period after the experiment (A). The second part tests whether treatment and control groups differed before the experiment (B).

First, consider models (1) and (2) which correspond to Figure 1 discussed previously. The two models show the treatment group does indeed have higher employment rate between four and nine months after the start of the experiment. The magnitude of this positive effect ranges between four and a half and seven percentage points. Second, consider models (3) and (4), which look at the share of individuals claiming UB. The effects mirror closely that of the employment rate, which is due to the fact that most of the sample falls in one of these two categories. Treated individuals claim much less UB between four and nice month after the start of the experiment. Lastly, consider models (5) and (6). A fair concern about this incentivized program is that Hestia may be tempted to push “hard-to-place” job seekers into social assistance or disability insurance so as to avoid costly efforts to place them. However, the absence of differences suggest that this is not an issue. Finally, the estimates under part B confirm that the randomization worked well. No pre-experiment differences are observed for any variable.

5.1.2 Job finding transitions

Previous analyses are useful to get the big picture but they remain averages of the different labor market states and do not tell much about individual transitions from one state to another. This is why we now turn to duration analysis. Figure 2 plots the Kaplan-Meier survivor function in unemployment (left) and one-period smoothed unemployment to job hazard estimates of (right). The origin is defined as the start of the experiment, while the failure is the entry into a new job. Note that here we only consider individuals who started the experiment unemployed.[10]

Figure 2: Short-term survival rate and hazard estimates



(a) Survivor function in unemployment

(b) Unemployment to job hazard

Notes: Figure 2 plots the Kaplan-Meier survivor function in unemployment (left) and one-period smoothed unemployment to job hazard estimates of (right). The origin is defined as the start of the experiment, while the failure is the entry into a new job.

Consider first graph (a), which depicts the transition from unemployment, the initial state for job seekers participating in the experiment, to employment. When the experiment starts, all individuals are unemployed

[10]While only long-term unemployment job seekers were meant to enroll in the experiment, it appear that few of them actually found a job right before the start. They were excluded of the experiment but kept in our previous analyses where we report ITT effects.

Table 4: Short-term unemployment to job transitions

	(1)	(2)
Hestia × 1-3 months	0.087	0.025
	(0.22)	(0.23)
Hestia × 4-6 months	0.506***	0.451**
	(0.19)	(0.19)
Hestia × 7-9 months	0.019	-0.036
	(0.18)	(0.18)
Hestia × 10-12 months	0.003	0.004
	(0.13)	(0.13)
Control variables	No	Yes
Subjects	820	792
Failures	534	520

Notes: Table 4 reports point estimates of Cox regressions on transitions from unemployment to job. The origin is defined as the start of the experiment, while failure is the entry into a new job. Control variables include: gender, age, marital status, schooling, nationality, mother tongue, residence permit, professional qualifications, placement prospects, OCE job code, and cohort number. Robust standard errors in parentheses. $^{*}p < 0.10, ^{**} p < 0.05, ^{***} p < 0.01$.

and thus the survivor function $\hat{S}(t)$ is equal to one. As time passes, some individuals find a new job and leave the state of unemployment, making the survivor function drop. In this situation, a faster drop of the survivor function is a positive result as it means that fewer individuals are (still) in the initial state of unemployment. We observe that the two groups are very close in the first three months. After this, the treatment group leaves unemployment significantly faster and Hestia's survivor function remain under that of the PES for the rest of the observation window.

Second, consider graph (b), which plots job entry hazard estimates. We observe that Hestia's hazards are greater that the control group ones for the whole period. Job seekers with intensive JSA are thus faster at leaving unemployment for a new job. The difference is biggest between months three and eight. We also observe a sharp increase in the hazards of the two groups after the ninth month. By design of the experiment, this correspond to the period when most job seekers run out of unemployment benefits.

Table 4 reports point estimates of Cox regressions on transitions from unemployment to job in the first twelve months following the experiment. The origin is defined as the start of the experiment, while failure is the entry into a new job. We estimate the same model twice. First without control variables (baseline model), and then adding control variables (main model). The change in the number of subjects is again due to the fact that we are missing the education level for some individuals.

We observe that the inclusion of covariates does not qualitatively change the findings. Both models suggest that Hestia's hazards in the first three months are not different from that of the control group. However, there is a significant positive effect between four and six months. Hestia manages to place job seekers much faster than the PES. The difference vanishes from seven months on.

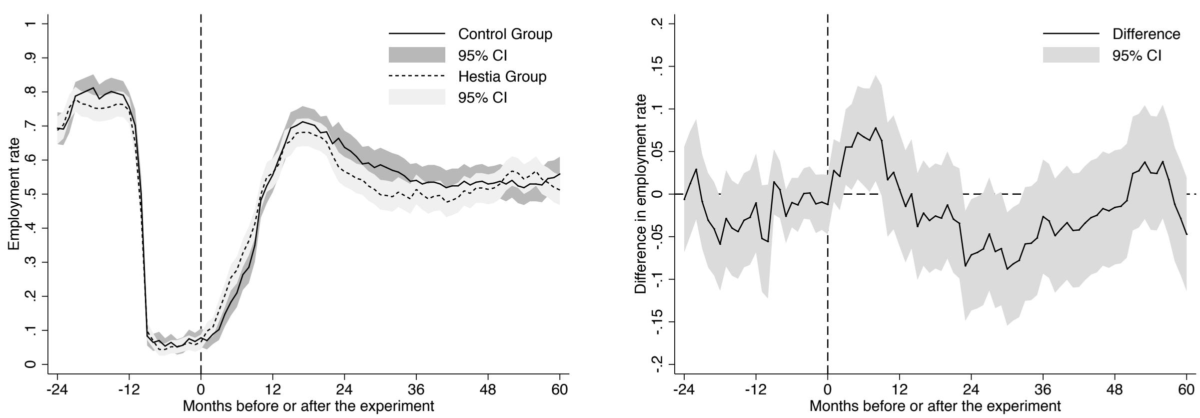
5.2 Long-term effects

This subsection analyzes the effects of the treatment in the five years following the experiment. We first look at monthly employment patterns. However, we now look both at job finding transitions and at job loss transitions.

5.2.1 Labor market states

As before, we start the analysis by providing the big picture. Figure 3 plots the fraction of individuals who are employed (left) and the difference between the two groups (right), defined as treatment minus control.

Figure 3: Long-term effects on employment



(a) Employed

(b) Employed (difference)

Notes: Figure 3 plots the fraction of individuals who are employed (left) and the difference between the two groups (right), defined as treatment minus control.

Consider first graph (a). Looking at two years before the experiment show that around 80 percent of the individuals were employed. Most of them lose their job twelve month before the experiment. The dynamics for the treatment and control groups are very similar. After the start, the average employment rate of both groups increases steadily to peak at 70 percent after 18 months. After the peak, a non-negligible fraction of individuals lose their job again. The treatment group seems to be significantly below the control group. The drop stabilizes at a level of 55 percent and the two groups converge after three years.

Second, consider graph (b). No significant pre-treatment differences arise in the year before the experiment, despite a light difference in favor of the control group between one and two years before the start. As seen previously, a significant difference in favor of the treatment group arise quickly once the experiment starts. Individuals following Hestia's JSA program enjoy a much higher employment rate in the first year. However, this difference vanishes after 18 months and reverts after that. The employment rate of Hestia's clients are well below that of their PES counterparts until the 36th month, when both groups converge again. Figure A3 in the appendix provides the same graphs for the other two labor market states.

As before, we now turn to formal econometric analyses. Table 5 reports the results of OLS regressions on the three labor market states. The only difference compare to Table 3 lies in the time dummies. Each of them now spans a period of one year. The last one (37+ m. after). concerns months 37 to 60.

Consider first the treatment effects on the fraction of employed individuals with no UB (part A, columns 1-2). We observe a positive effect of Hestia in the first twelve months after the experiment. The fraction of employed individuals is around four percentage points higher than the public service and is statistically significant. However, this positive impact disappears after twelve months and even reverts after 24 months. The effect of Hestia on the fraction of employed individuals between two and three years after the experiment is significantly negative. This fraction is around seven percentage points lower than for individuals without JSA. The effect again vanishes over time.

Second, consider the treatment effects on the fraction of individuals receiving UB (part A, columns 3-4). Not surprisingly, we observe the opposite pattern to the one for employed individuals. Hestia's JSA program decreases the fraction of individuals receiving UB by 6 percentage points in the first 12 months following the start of the experiment. The effect is significant at a one percent level. In the second year after it, the effect vanishes and reverses in the third year, yet with a weak statistical significance.

Finally, consider the treatment effects on the fraction of unemployed individuals without UB (part A, columns 5-6). All coefficients are positive, which suggests that a higher share of Hestia's job seekers tend to fully exit the labor force compared to their PES counterparts. However, the effect is not statistically significant for any time period.

The randomization (part B) has worked well. We do not see any significant differences between the control and the treatment group before the experiment for any of the dependent variables, which supports our empirical approach.

5.2.2 Job finding transitions

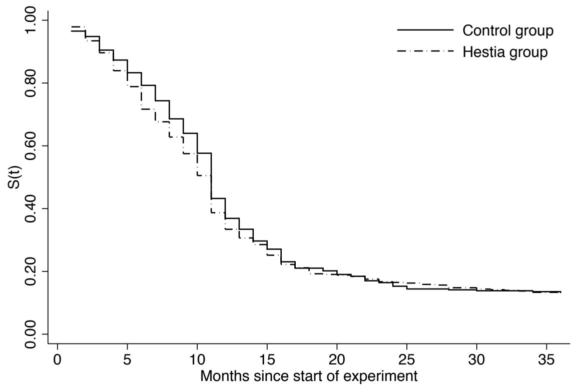
Figure 4 plots Kaplan-Meier survivor function in unemployment (left) and one-period smoothed unemployment to job hazard estimates of (right). The origin is defined as the start of the experiment, while the failure is the entry into a new job.

Table 5: Long-term effects on labor market states

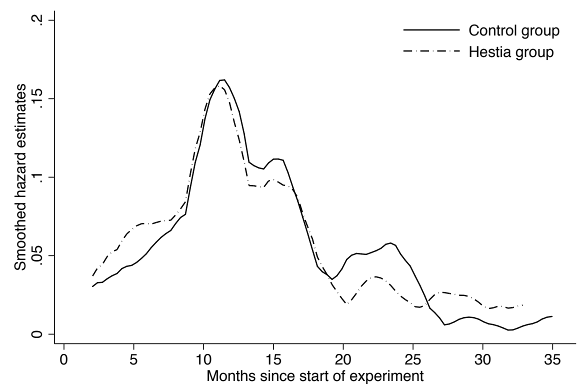
	Employed, no UB		UB recipients		Unemployed, no UB	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Treatment Effects						
Hestia*1-12 m. after	0.044**	0.035*	-0.059***	-0.054**	0.015	0.019
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Hestia*13-24 m. after	-0.026	-0.034	0.002	0.005	0.024	0.028
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
Hestia*24-36 m. after	-0.067**	-0.082***	0.033	0.042*	0.033	0.039
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Hestia*37+ m. after	-0.011	-0.015	-0.002	-0.001	0.014	0.016
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
B. Randomization						
Hestia*24-11 m. before	-0.023	-0.035	0.006	0.012	0.016	0.023
	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Hestia*12-1 m. before	-0.014	-0.020	-0.001	0.001	0.015	0.019
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Control variables	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.138	0.172	0.254	0.268	0.033	0.077
Individuals	874	844	874	844	874	844

Notes: Table 5 reports point estimates of OLS regressions on the three labor market states from 24 months before to 60 months after the start of the experiment. All three states are continuous variables ranging between zero and one. The constant is included in the regressions but not reported here. Control variables include: gender, age, marital status, schooling, nationality, mother tongue, residence permit, professional qualifications, placement prospects, OCE job code, and cohort number. Standard errors clustered at an individual level in parentheses. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Figure 4: Long-term job entries



(a) Survivor function in unemployment



(b) Unemployment to job hazard

Notes: Figure 4 plots Kaplan-Meier survivor function in unemployment (left) and one-period smoothed unemployment to job hazard estimates of (right). The origin is defined as the start of the experiment, while failure is the entry into a new job.

Consider first graph (a). As we can see, the survivor curve of the treatment group is almost always below that of the control group. This suggests that the private provider improved the transition from unemployment to employment. Similar to the previous results, the difference between the two groups is greatest around six months after the start of the experiment, and slowly vanishes afterwards.

Second, consider graph (b). We clearly see the positive impact of the treatment on the job entry hazards in the first nine months after the experiment. After that, the two groups converge and the hazards peak around twelve months, when most job seekers run out of unemployment benefits. After about a year, the hazards of the control group become higher than that of the treatment.

Columns (1) and (2) in Table 6 reports the results of Cox regressions on long-term transitions to job. The origin is defined as the start of the experiment, while failure is the entry into a new job. Recall that positive coefficients in column (1) are positive results since it means that the transition from unemployment to employment increases. Columns (3) and (4) will be discussed in section 5.2.3.

We observe again Hestia's positive short-term effect. Between four and six months, it significantly increases transition to employment. However, this positive effect vanishes and becomes negative after a one year. We observe a strong negative effect between 19 and 24 months after the experiment. In total, 820 individuals started the experiment unemployed and 739 found a job during the observation period, which represents 90 percent of the subjects.

Table 6: Long-term transitions

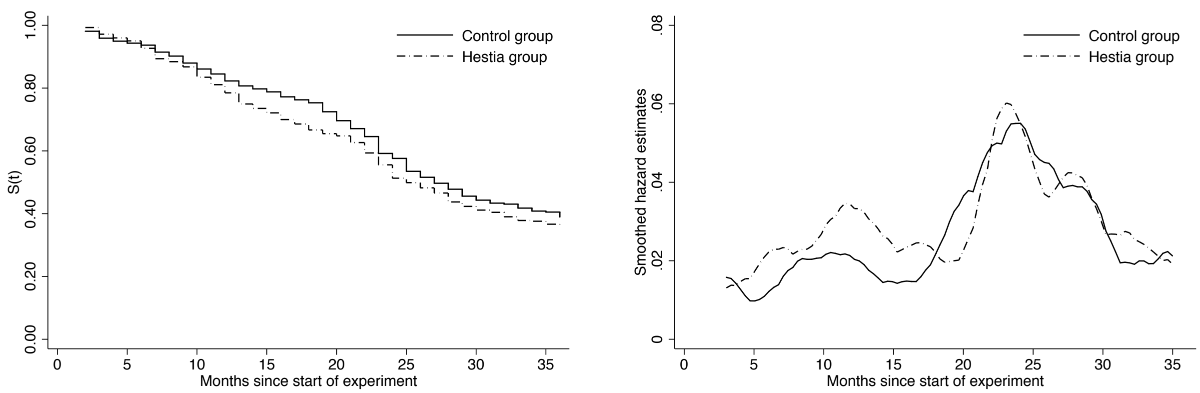
	To Job		To Unemployment	
	(1)	(2)	(3)	(4)
Hestia × 1-3 months	0.087	0.034	-0.379	-0.406
	(0.22)	(0.23)	(0.40)	(0.40)
Hestia × 4-6 months	0.506***	0.454**	0.697	0.660
	(0.19)	(0.19)	(0.44)	(0.44)
Hestia × 7-9 months	0.019	-0.047	0.053	0.020
	(0.18)	(0.18)	(0.31)	(0.31)
Hestia × 10-12 months	0.003	-0.007	0.401	0.302
	(0.13)	(0.13)	(0.29)	(0.29)
Hestia × 13-18 months	-0.047	-0.057	0.608**	0.613**
	(0.17)	(0.18)	(0.25)	(0.26)
Hestia × 19-24 months	-0.727**	-0.793**	-0.061	-0.030
	(0.35)	(0.36)	(0.18)	(0.19)
Hestia × 25+ months	-0.132	-0.202	-0.093	-0.138
	(0.28)	(0.28)	(0.13)	(0.13)
Control variables	No	Yes	No	Yes
Subjects	820	792	739	713
Failures	739	713	586	565

Notes: Table 6 reports point estimates of Cox regressions on transitions to job and to unemployment. For the transition from unemployment to job, the origin is defined as the start of the experiment, while failure is the entry into a new job. For the transition from job back into unemployment, the origin is the beginning of a new employment spell after the start of the experiment, and failure is the loss of the job. Control variables include: gender, age, marital status, schooling, nationality, mother tongue, residence permit, professional qualifications, placement prospects, OCE job code, and cohort number. Robust standard errors in parentheses. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

5.2.3 Job loss transitions

Consider now the transition from employment back to unemployment. In other words, how long newly employed individuals keep their job. There is no prior as to which group should enjoy a better situation. However, previous results suggest that privately placed individuals might reach less stable positions. Two points must be noted before analyzing the results. First, only individuals who have found a job after the start of the experiment are considered here. It is therefore a subset of the individuals considered in Figure 4. Second, the interpretation of graph (a) is reversed this time. A high survivor function is a positive outcome as it means that more people are still employed. Figure 4 plots Kaplan-Meier survivor function in unemployment (left) and one-period smoothed unemployment to job hazard estimates of (right). The origin is defined as the start of the experiment, while the failure is the entry into a new job.

Figure 5: Long-term job exits



(a) Survivor function in employment

(b) Job to unemployment hazard

Notes: Figure 5 plots Kaplan-Meier survivor functions in employment (left) and one-period smoothed job to unemployment hazard estimates of (right). The origin is the beginning of a new employment spell after the start of the experiment, while failure is the loss of the job. Only individuals who have found a job after the start of the experiment are considered here.

Consider first graph (a). We observe that the survivor curve of the treatment group is again below that of the control group, suggesting that treated individuals lose their job faster than the control ones. It is a negative outcome of the experiment which contrasts with the positive result discussed previously, namely faster job re-entry. The difference is greater between one and two years, before converging again.

Second, consider graph (b). The hazards show that Hestia's clients lose their job at a higher rate that their PES counterparts but specifically one year after the start of the experiment. One year fixed duration work contracts could explain this phenomenon. Alternatively, individuals become eligible again for UB (although not the full entitlement) after working for a whole year. This could also convince some individuals to leave a job that they do not like. We observe another peak in the hazards of both groups after 24 months. This is the time when all working individuals have re-gained full UB eligibility.

We can now discuss the results of Cox regressions on the transition from job back to employment, which are shown in columns (3) and (4) in Table 6. The origin is the beginning of a new employment spell after the start of the experiment, and failure is the loss of the job. This time, positive coefficients in are a negative outcome, since it means that people leave the state of employment at a higher rate. As before, only individuals who have found a job after the start of the experiment are considered here, which explains why the number of subjects in column (3) and (4) are equal to the number of failures in column (1) and (2) respectively.

The first thing to note is that none of the coefficients is statistically significant in the first year. Large standard errors may be the result of our small sample size. However, the signs of the coefficients are very interesting. Individuals without JSA seem to lose their job significantly faster in the first three months after the start of the experiment. This could suggest that these individuals were not satisfied with the positions that the reached and left when they first got the chance. In Swiss labor law, many contracts come with a three-month probation period, after which both sides (employer and employee) can break the contract unilaterally. After the first three-month period, all coefficients are positive, suggesting a negative treatment effect on job stability. Hestia's clients seem to lose their job at a faster (and increasing) rate. The negative difference is biggest and statistically significant between 13 and 18 months. After that, the two groups converge. Note that Hestia was not responsible for placing its job seekers after more than twelve months because most of them ran out of UB. This negative effect can therefore be seen as an indirect consequence of the intensive JSA program.

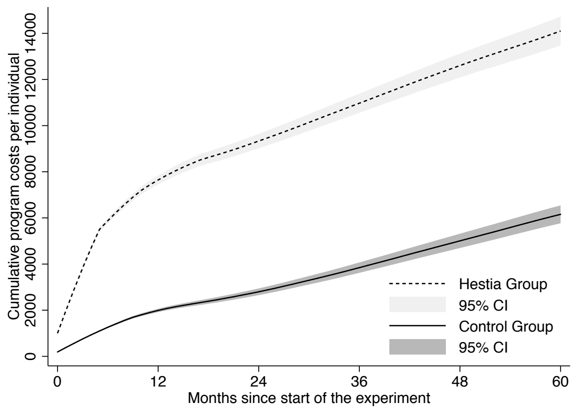
5.3 Cost-benefit analysis

Our evaluation of the placement scheme would not be complete without a cost-benefit analysis. From the State's perspective, there are three types of costs, and one type of benefit. The program costs represent the amount paid to Hestia and PES for their job placement programs. On top of it, there are indirect costs in the form of UB paid to job seekers enrolled and social assistance (SA) benefits paid to individuals who withdraw from the labor market. Finally, the benefits represent the amount saved on unemployment benefits thanks to shorter unemployment duration.

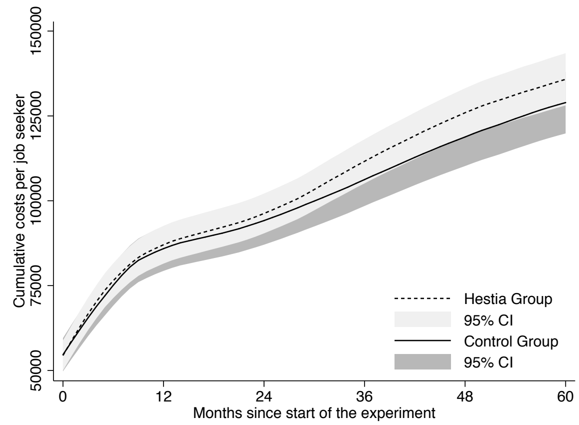
Flückiger and Kempeneers (2008) have estimated the average monthly program costs for the PES to be equal to SFr 573. It has been done by summing up two budgets: the PES budget; and a budget for additional ALMP in Geneva; and then dividing the amount by the total number of job seekers that benefited from it in the previous year (2006). Program costs for the Hestia group are more straightforward. In addition to the base PES cost, we know from Section 2.2 that a job seeker enrolled with Hestia costs an extra 1000 SFr a month in the first six months; 500 SFr a month for six to 18 months; and 350 SFr a month after it. As soon as this person is placed, the cost drops to zero. If this individual falls back into unemployment during the observation period, then her costs will be the same as for the public track, namely 573 SFr per month.

Figure 6 plots the average cumulative cost per individual enrolled in one of the two programs, first without accounting for UB and SA benefits, and then accounting for variations in UB and SA benefits. Note that we do not account here for effects on taxes (e.g. more or less taxes paid depending on changes in earnings, which would be a benefit from the State's perspective) and for non-monetary benefits for job seekers (e.g. improved well-being due to shorter unemployment period).

Figure 6: Costs per job seeker



(a) Program costs only



(b) Overall costs

Notes: Figure 6a plots the average cumulative cost per individual enrolled in one of the two programs without accounting for unemployment benefits, while Figure 6b does the same but also accounts for unemployment benefits.

Results are not surprising. By design, the intensive JSA track is much more costly than the PES, which is reflected in graph (a). Without accounting for UB, faster job entry in the short run does not compensate for a much higher monitoring cost.

A more interesting result can be seen in graph (b). Once the endogenous effect on UB and social assistance payments is accounted for, the intensive JSA scheme is not different from the standard track in the short-term. The amount saved on UB compensates for the higher program costs. However, the difference increases in the long-run, when Hestia's clients lose their job and get back to the benefit rolls. Overall, the intensive JSA program is significantly more costly for the State. Figure A4 in the appendix shows the costs for UB and social assistance payments separately.

6 Mechanisms

This section explores different mechanisms which could explain the positive short-term effects and the negative long-term ones. First, we look at the characteristics of the individuals who found a job. Second, we check whether the experiment has altered the ALMP mix used by job seekers. Third, we build a theoretical model to predict the effects of the financial incentive scheme and compare its prediction to the empirical results. Fourth, we look at (new) job quality.

6.1 Targeting

A question that arises naturally after seeing the results is: who found a job? It could be that Hestia focused on certain types of job seekers, while the PES took care of everyone in a standardized fashion. It is not clear how focusing on specific individuals would affect the placement rate dynamics over time but it could certainly create differences.

Table 7 presents the socio-demographic characteristics of the individuals who found a job within twelve months after the start of the experiment. It is constructed in the same way as Table 1 but it only considers the 534 individuals who found a job in the year following the experiment. We focus on the first twelve months post-experiment because most job seekers lost UB eligibility after this period and thus were not able to continue Hestia's JSA program. Categories containing very few individuals have been grouped. The last line shows the fraction of individuals who found a job among each group, while all other lines provide information on these individuals.

Without even adjusting the t-statistics for multiple hypothesis testing, we can see that there are no differences between treated and non-treated individuals. The characteristics of the individuals placed by the PES and Hestia are almost identical. They follow closely the socio-demographic characteristics of the whole sample, suggesting that Hestia did not target different individuals than the PES.

6.2 ALMP mix

We check whether the Hestia experiment altered the ALMP mix of the treatment group (compared to the controls). It could be the case that the intensive JSA program substituted for other training classes or prevented individuals to follow the same measures as the control group.

Table 8 presents summary statistics of the ALMP mix followed by job seekers from their entry into the experiment until July 2016. We focus on six types of training courses which are the most related to job search assistance, namely a base program, personality development, basic skill acquisition, language courses, basic IT skills, and advanced IT skills. For all these measures, we consider individual sessions and group courses separately.

Consider first individual ALMPs. There are no significant differences between control and treatment groups for any of the training classes. By order of importance, language courses have been followed by about eleven to twelve percent of individuals, followed by basic IT courses with seven to nine percent, and advanced IT courses with three to five percent. Also, between 16-19 percent of job seekers have followed

Table 7: Socio-demographic characteristics of individuals who found a job within twelve months

	Control Group		Hestia Group			
Variable	Mean (%)	s.e.	Mean (%)	s.e.	Difference (%)	t-stat
Women	45.2	0.03	47.9	0.03	-2.7	-0.62
Marital status						
Single	40.2	0.03	38.1	0.03	2.1	0.48
Married	46.6	0.03	50.2	0.03	-3.6	-0.81
Divored or widower	13.2	0.02	11.7	0.02	1.5	0.51
Experience						
3 years and less	39.3	0.03	37.5	0.03	1.8	0.42
More than 3 years	52.1	0.03	49.2	0.03	2.8	0.65
Age						
17-24	9.1	0.02	11.1	0.02	-2.0	-0.75
25-34	32.0	0.03	37.1	0.03	-5.2	-1.24
35-44	30.6	0.03	25.1	0.02	5.5	1.39
45-54	21.5	0.03	19.0	0.02	2.4	0.68
55-64	6.8	0.02	7.6	0.01	-0.8	-0.34
Schooling						
Compulsory	43.4	0.03	43.2	0.03	0.2	0.05
High-school level	34.2	0.03	37.8	0.03	-3.5	-0.84
University level	18.7	0.03	17.1	0.02	1.6	0.47
Workers						
Swiss	51.1	0.03	47.6	0.03	3.5	0.80
C permit	32.0	0.03	33.0	0.03	-1.1	-0.26
Other	16.9	0.03	19.4	0.02	-2.5	-0.73
Placement prospects						
Good or excellent	62.6	0.03	60.0	0.03	2.6	0.60
Average	23.7	0.03	22.5	0.02	1.2	0.32
Poor	13.7	0.02	17.5	0.02	-3.8	-1.19
Number of observations	219	-	315	-	-	-
Fraction of group with job	58.9	0.03	62.7	0.02	-3.9	-1.16

Notes: Table 7 presents the socio-demographic characteristics of the individuals who found a job within twelve months after the start of the experiment. Some categories may not add up to 100 percent due to missing observations. The sixth column calculates the difference between control and treatment groups, defined as control minus treatment. The seventh column reports two-sided t-statistics on the differences.

Table 8: ALMPs

	Control Group		Hestia Group			
Variable	Mean (%)	s.e.	Mean (%)	s.e.	Difference (%)	t-stat
Individual ALMPs						
Base program	3.4	0.01	3.9	0.01	-0.5	-0.37
Personality development	1.6	0.01	1.0	0.00	0.6	0.79
Basic skills acquisition	1.9	0.01	1.8	0.01	0.1	0.10
Language course	12.4	0.02	11.3	0.01	1.1	0.50
Basic IT skills	9.0	0.01	7.0	0.01	2.0	1.06
Advanced IT skills	3.2	0.01	4.5	0.01	-1.3	-1.02
Others	18.8	0.02	16.2	0.02	2.6	0.99
None	60.8	0.03	63.7	0.02	-2.8	-0.86
Group ALMPs						
Base program	23.0	0.02	62.3	0.02	-39.3	-12.89
Personality development	3.7	0.01	3.5	0.01	0.2	0.15
Basic skills acquisition	0.8	0.00	0.2	0.00	0.6	1.20
Language course	0.8	0.00	1.0	0.00	-0.2	-0.29
Basic IT skills	0.8	0.00	0.6	0.00	0.2	0.37
Advanced IT skills	0.0	0.00	0.2	0.00	-0.2	-1.00
Others	16.1	0.02	12.7	0.01	3.4	1.43
None	18.0	0.02	8.4	0.01	9.6	4.12
Number of observations	378	-	512	-	-	-

Notes: Table 8 presents summary statistics of the ALMP mix followed by job seekers from the entry into the experiment until July 2016. The sixth column calculates the difference between control and treatment groups, defined as control minus treatment. The seventh column reports two-sided t-statistics on the differences.

other types of individual ALMPs, while about two third of job seekers have not followed any individual training program.

Second, consider group ALMPs. In this category, the most followed training course is the base program. The Hestia experiment was officially recorded as a base program group ALMP, which explains the large difference across control and treatment group participation, 23 percent vs 62 percent respectively. The reason why the participation in the base program is not 100 percent for the treatment group is the non-participation in the experiment. The participation rate for the 260 compliers is 98 percent while that of the 252 non-compliers is 26 percent, which is not statistically different from the participation of the control group. The only other significant difference observed is in the share of job seekers who did not follow any group ALMPs. 18 percent of the control group did not follow anything, while only 8 percent of the treatment group is in the same situation.

We conclude that the Hestia experiment did not crowd out other training programs but that it did act as a complement for the treatment group, which was exactly what it was designed for. The results should therefore not be driven by a change in the ALMP mix.

6.3 Financial incentives

The idea behind this section is to provide a simple theoretical model to better understand the specific implications of the financial scheme in place for our experiment. Recall the two key aspects of the financial scheme: (i) decreasing monthly flat rate paid to Hestia for each individual enrolled in its programme; (ii) Hestia has a capacity constraint and each month receives a new inflow corresponding to the number of individuals that it placed in the month before.

Define ω as the "wage" paid to Hestia each period for each job-seeker enrolled, P as the number of job-seekers enrolled in the JSA programme at any given time (i.e. the capacity constraint), $\delta \in (0, 1)$ as a discount factor applied to the wage so as to mimic the decreasing payment scheme, and $e_t \in (0, 1)$ as the effort level put in by Hestia in order to place its clients, which yields a quadratic cost equal to $\frac{\omega P}{2} e_t^2$. Note that it is assumed that the effort level e_t put in by Hestia corresponds to the placement rate that it achieves for this period. In other words, if it exerts $e_t = 0.3$, it places 30 percent of its job seekers and it will receive the equivalent number of "fresh" job seekers in the next period. The incentives for Hestia come from the fact that it receives more money per new comer than per job seeker that was already enrolled, due to the decreasing payment. In the first period of the experiment ($t = 0$), Hestia's profit is thus:

$$\pi_0 = \omega P - \frac{\omega P}{2} e_0^2 = \omega P[1 - \frac{1}{2} e_0^2] \tag{4}$$

while in the second period of the experiment, it is given by:

$$\pi_1 \quad = \quad \omega P[e_0 + \delta(1 - e_0) - \frac{1}{2}e_1^2] \tag{5}$$

where the fraction of new comers e_0 brings in more money that the fraction of individuals that were not placed $(1 - e_0)$ since δ is smaller than one. In the third time period, the profit becomes:

$$\pi_2 \quad = \quad \omega P[e_1 + \delta(1 - e_1)e_0 + \delta^2(1 - e_1)(1 - e_0) - \frac{1}{2}e_2^2] \tag{6}$$

This logic remains the same for all future time periods, knowing that today's effort level influences tomorrow's profit. To see how the effort levels evolve over time, let's consider a simple version of this game. Assume that it has three time periods, starting with $t = 0$, and ending with $t = 2$. This version can be seen as a representation of the three-stage financial scheme used in the experiment. Hestia's maximization problem is then given by:

$$\begin{aligned} \mathcal{L} = \max_{e_0,e_1,e_2} \sum_{t=0}^{2} \beta^t \pi_t \quad &= \quad \omega P[1 - \frac{1}{2}e_0^2] + \beta\omega P[e_0 + \delta(1 - e_0) - \frac{1}{2}e_1^2] \\ &+ \quad \beta^2\omega P[e_1 + \delta(1 - e_1)e_0 + \delta^2(1 - e_1)(1 - e_0) - \frac{1}{2}e_2^2] \end{aligned} \tag{7}$$

where $\beta \in (0, 1)$ is the standard discount factor for future time periods. The first order conditions are:

$$\frac{\partial \mathcal{L}}{\partial e_0} : \quad e_0 \quad = \quad \beta[1 - \delta] + \beta^2 \left[\delta(1 - e_1) - \delta^2(1 - e_1)\right] \tag{8}$$

$$\frac{\partial \mathcal{L}}{\partial e_1} : \quad e_1 \quad = \quad \beta \left[1 - \delta e_0 - \delta^2(1 - e_0)\right] \tag{9}$$

$$\frac{\partial \mathcal{L}}{\partial e_2} : \quad e_2 \quad = \quad 0 \tag{10}$$

Solving for e_0, e_1 and e_2 yields the optimal effort levels:

$$e_0 \quad = \quad \frac{1 - \delta^2}{1 + \delta(1 - \delta)} \tag{11}$$

$$e_1 \quad = \quad \frac{1 - \delta^2}{1 + \delta(1 - \delta)} \tag{12}$$

$$e_2 \quad = \quad 0 \tag{13}$$

A few points are worth noting. First, neither the wage paid to Hestia nor the number of job seekers enrolled play any role in this setting. What matters is the speed at which the payment scheme decreases. Second, the effort levels e_0 and e_1 are decreasing in δ:

$$\frac{\partial e_0}{\partial \delta} = \frac{\partial e_1}{\partial \delta} = -\frac{\delta^2 + 1}{(1 + \delta(1 - \delta))^2} < 0 \tag{14}$$

This makes sense as an increase in δ means a lower decrease in the payment scheme. In financial terms, unplaced job seekers are worth more in the future than they used to be, thus reducing the need for a significant effort to place them in the present. If $\delta = 0$, i.e. if the payment scheme is just a flat rate until the end of times, effort levels fall to zero. This corresponds to a situation where the placement provider always receives a fixed amount of money to take care of job seekers but without any incentives to place them. In the Swiss system, this situation corresponds to the Public Employment Services. Third, there is no reason for Hestia to put in any effort in the last period of the experiment (t=T) since it is costly and it will not reap the future benefits of it. Let us now consider a more general version of this game. After normalizing ωP to one, the general form of Hestia's profit function is:

$$\pi_t = \begin{cases} 1 - \frac{1}{2}e_0^2 & \text{for } t = 0 \\ \sum_{H=1}^{t} \left(\delta^{H-1} \frac{e_{t-H}}{1-e_{t-H}} \prod_{h=1}^{H} (1 - e_{t-h}) \right) + \delta^t \prod_{h=1}^{t} (1 - e_{t-h}) - \frac{1}{2}e_t^2 & \text{for } t > 0 \end{cases} \tag{15}$$

Hestia's maximization problem can then be written as:

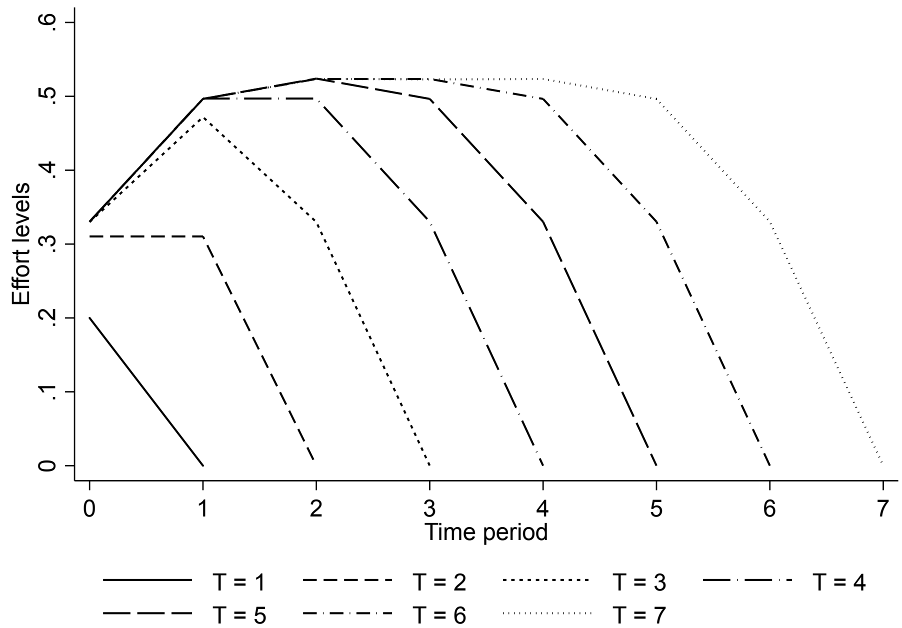
$$\mathcal{L} = \max_{e_t,\ldots,e_T} \sum_{t=0}^{T} \beta^t \pi_t \tag{16}$$

where $\beta \in (0, 1)$ is the standard discount factor for future time periods. The maximization problem yields the following first order conditions:

$$e_t = \begin{cases} \beta(1-\delta) \left[1 + \sum_{H=1}^{T-t-1} \left(\beta^H \delta^H \prod_{h=1}^{H} (1 - e_{t+h}) \right) \right] & \text{for } t = 0 \\ \beta \left[1 + \sum_{H=1}^{T-t-1} \left(\beta^H \delta^H \prod_{h=1}^{H} (1 - e_{t+h}) \right) \right] \times & \\ \left[1 - \sum_{H=1}^{t} \left(\delta^H \frac{e_{t-H}}{1-e_{t-H}} \prod_{h=1}^{H} (1 - e_{t-h}) \right) - \delta^{t+1} \prod_{h=1}^{t} (1 - e_{t-h}) \right] & \text{for } t = 1, \ldots, T-2 \\ \beta \left[1 - \sum_{H=1}^{t} \left(\delta^H \frac{e_{t-H}}{1-e_{t-H}} \prod_{h=1}^{H} (1 - e_{t-h}) \right) - \delta^{t+1} \prod_{h=1}^{t} (1 - e_{t-h}) \right] & \text{for } t = T-1 \\ 0 & \text{for } t = T \end{cases} \tag{17}$$

In other words, Hestia's effort level is a function of the two discounts rates, as well as past and future effort levels. In the initial period ($t = 0$), it is a function of future efforts only, while in the second last period ($T - 2$), it is a function of past efforts. The closed form solutions for optimal effort levels becomes exponentially difficult to compute as the number of time periods increase and does not add much to the mechanisms already observed in the three period model. However, we provide numerical solutions calculated for a given δ and various T in Figure 7.

Figure 7: Simulation of optimal effort levels, $\delta = 0.8$



Notes: Figure 7 shows the values of the optimal effort levels for a fixed δ and a different number of time periods. $T = 1$ means that Hestia's contract is not renewed after time period 1, and thus that the game only lasts two period ($t = 0$ and $t = 1$).

For each duration of the game, we observe that the optimal effort levels are increasing until the median period and then decreasing in a symmetrical fashion. As already mentioned above, Hestia does not exert any effort in the last period of the game as it would just reduce its immediate profit without improving future benefits. This implies that cohorts which entered the program in the second half of it are worse off compared to those which were enrolled at the start or in the middle.

We now link the theoretical predictions of our model with the empirical estimates from the experiment. This is complicated as the effort level exerted by Hestia is not directly observable. What we do observe is the placement rate of the two groups. If the model predictions are true, the PES effort level is zero and thus the placement rate of the control group can be seen as the natural job re-entry rate. Consequently, the difference in placement rate between Hestia and the PES should reflect the effort exerted by Hestia. The model tells us that this effort should be concave, starting close to zero at the start of the experiment, peaking after six months (half the duration of the pilot) and returning to zero at twelve months. This seems to fit well with the differences in employment rate presented in Figure 1 (b) and the results of the job entry transitions shown in Table 4 (assuming that each time dummy represents a three-month period in the model).

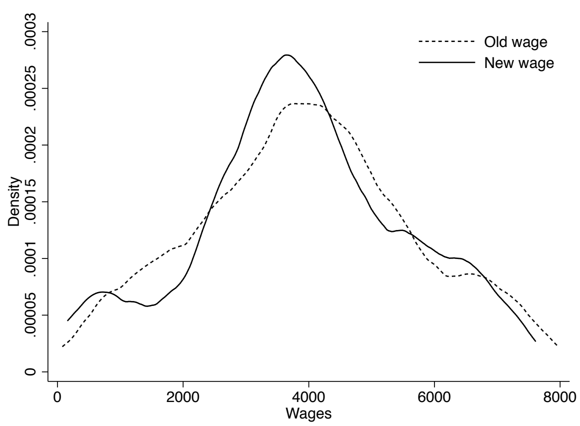
We conclude that the financial incentives to Hestia may explain the patterns observe in the short-term. The payment scheme greatly incentivized placement in the first six months, which is exactly what is found in the data. The model does not yield precise predictions on the long-term effects. The lack of financial reward to Hestia for the duration of their clients' new job may create incentives to place fast without considering job stability. However, Hestia's goal was to continue collaborating with the PES in the long-term, which should decrease the incentives to focus purely on short-term placement. Overall, predicted long-term effects are ambiguous.

6.4 Job quality

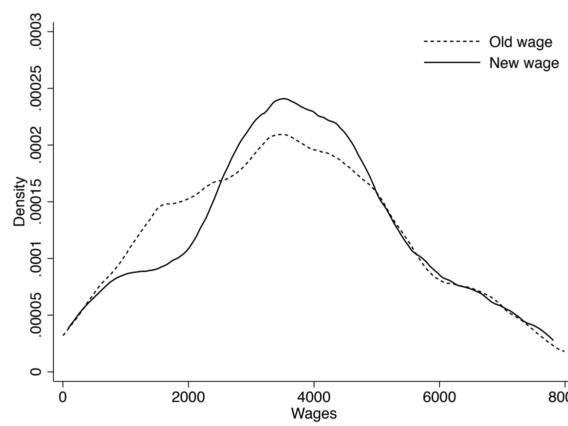
Our data does not allow us to know what types of jobs (e.g. sector, occupation, activity rate) individuals get. The only observable characteristic is the wage. This section thus explores differences in pre- and post-experiment wage distributions as well as how placement timing affects wages.

Figure 8 presents kernel density estimations of the pre- and post-experiment wage distributions of individuals who found a job within twelve months after the start of the experiment.

Figure 8: Wage distributions



(a) Control group



(b) Hestia group

Notes: Figure 8 presents kernel density estimations of the pre- and post-experiment wage distributions of individuals who found a job within twelve months after the start of the experiment. Both graphs use the Epanechnikov kernel with a bandwidth of 400.

We observe that the pre-experiment wage distribution is very similar both groups. The highest density is just around 4000 SFr but tails on both sides are pretty fat. Post-experiment wages have very similar patterns. The only difference is a discontinuity in the left tail, suggesting that less newly placed individuals have (very) low wages in their new job. Note that we are not able to know whether a job a part-time of full-time. Part-time jobs could explain the bumb in the density observed for wages around 500-1000 SFr. The control group exhibits higher densities on the left of its pre-experiment peak (i.e. at lower wages) while its remains at the same wage level for the treatment group. Overall, wage distributions do not show obvious differences in jobs before and after the experiment for either of the two groups.

Table 9: Placement timing and wages

	(1)	(2)	(3)
Hestia	0.030	0.003	0.380
	(0.10)	(0.10)	(0.32)
Hestia × Placed 4-6 months			-0.483
			(0.41)
Hestia × Placed 7-12 months			-0.281
			(0.35)
Hestia × Placed 13-18 months			-0.458
			(0.39)
Hestia × Placed 19-24 months			-0.960*
			(0.58)
Hestia × Placed 25+ months			-0.905
			(0.66)
Control variables	Yes	Yes	Yes
Control for job entry timing	No	Yes	Yes
Adjusted R^2	0.006	0.012	0.013
Individuals	674	674	674

Notes: Table 9 reports point estimates of OLS regressions on the change in log earnings. Control variables include: gender, age, marital status, schooling, nationality, mother tongue, residence permit, professional qualifications, placement prospects, OCE job code, and cohort number. Robust standard errors in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

We now turn to a more formal analysis of the impact of Hestia's JSA program on wages. Table 9 reports point estimates of OLS regressions on the change in log earnings. The change is calculated by calculating the log of the new wage and subtracting the log of the old wage from it. We estimate three different models. Model (1) just includes the usual control variable and a dummy variable for being treated (Hestia). Model (2) adds controls for the timing of the job entry. These are dummy variables equal to one if the individual has been placed in the first three months, in months four to six, in months seven to twelve, etc. Finally, model (3) adds interaction terms between the placement timing dummies and the treatment indicator. Table A1 in the appendix shows the results of quantile regressions on the same specifications.

We observe from models (1) and (2) that Hestia's program did not affect wage growth significantly. However, significant heterogeneity can be discovered when looking at the breakdown by placement timing. Despite very weak statistical significance, model (3) suggests that treat individuals placed within three months were actually better off than that control group, while individuals placed later were much worse off. The negative difference is greatest for treat individuals who found a job between 19 and 24 months after the experiment.

7 Conclusion

We are interested the effects of intensive job search assistance for long-term unemployed. We study the work trajectories of about 890 individuals gathered in a randomized controlled experiment conducted in 2006-2007, in conjunction with social-security data that cover two years before and five after the experiment. Focusing on the monthly employment patterns of individuals, we find that results change dramatically depending on the time horizon considered.

In the short-run, intensive JSA significantly improves job seekers re-entry into the labor force, with a difference of around four percentage points compared to the standard track. This also lowers by around 17 percent the average amount of unemployment benefits received by the same group. In the medium-run though, these positive impacts vanish and both groups have a similar performance until approximately two years after the experiment. Then, the patterns revert. Treated job seekers lose their job again, earn less when they are employed and claim more unemployment benefits than their control group counterparts. The difference is significant up to three years after the experiment and finally disappears when we looking at a longer horizon. These results suggest that the JSA provider focused on placing job seekers as fast as possible at the expense of their suitability to the position. Indeed, finding a job faster does not mean it is better.

A cost-benefit analysis shows that intensive job search assistance of the form we study is expensive. The additional program cost can be recovered somewhat in the first year after the program runs. In the longer-run, the program is more costly than the alternative because of its detrimental effects on job stability. The effects on the economy are, perhaps, better captured by cumulative earnings of participants. Cumulative earnings of treated job seekers improve somewhat initially, but grow at a slower rate during the period of reduced job stability. Intensive JSA significantly decreases cumulative earnings.

Our study suggests that offering intensive JSA did not work, the way it was implemented. What could be done? The pattern of findings is consistent with the short-term incentive structure facing the provider. Incentives that attach some weight to job stability might have improved job stability, albeit, reducing the speed of job finding. How JSA intervenes in the job search process is a second important topic. Do job seekers search optimally? How can we improve on their actions if job search is not optimal? We believe that answering these important question is an excellent direction for future research.

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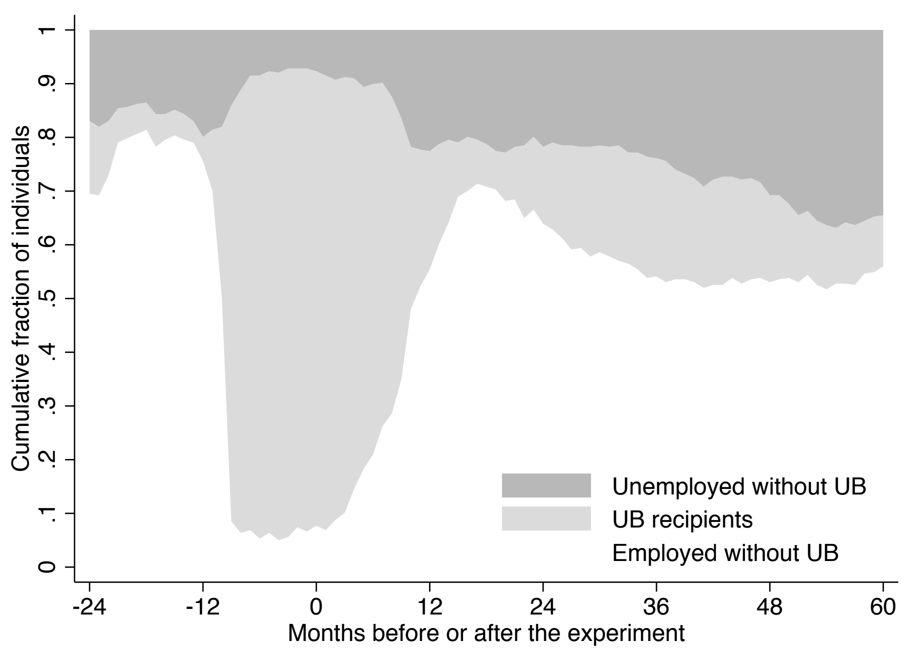
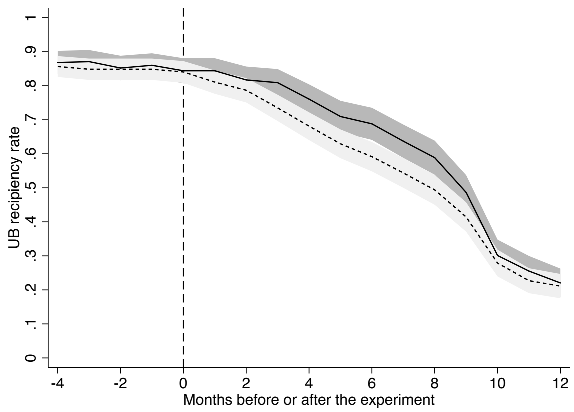
A Appendix

Figure A1: Labor market states over time

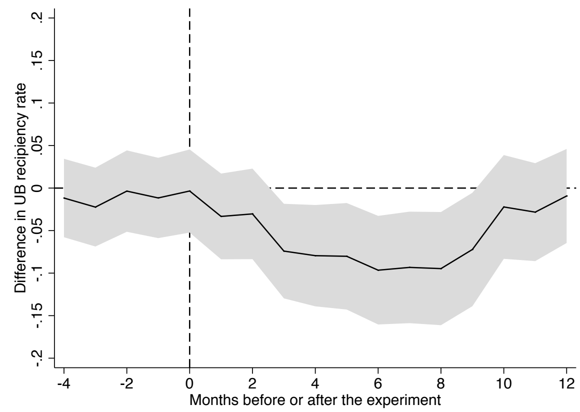


Notes: Figure A1 shows the labor market states in which individuals can be at a given point in time. These three states are mutually exclusive. The figure can be seen as a snapshot of the employment situation of all the individuals in our sample for a given month.

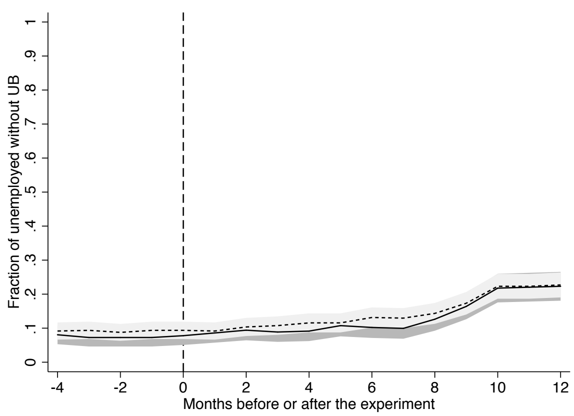
Figure A2: Short-term effects on unemployment



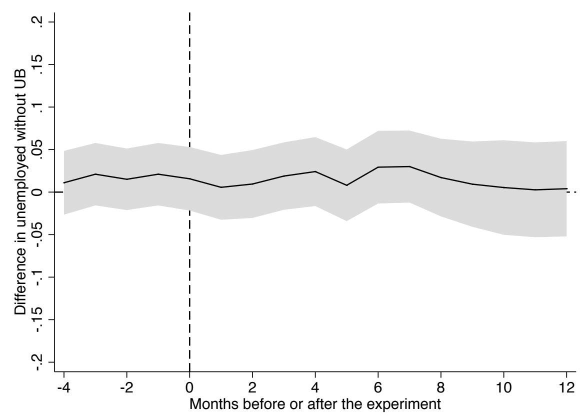
(a) UB recipients



(b) UB recipients (difference)



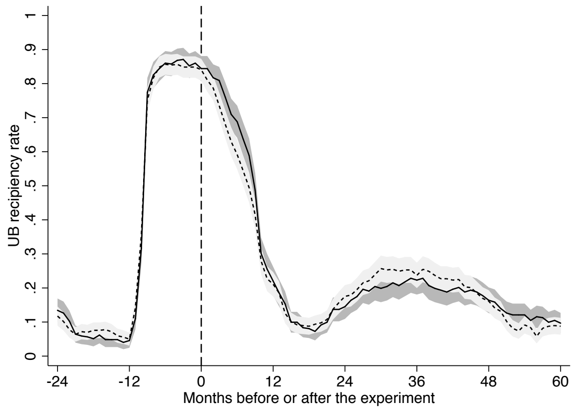
(c) Unemployed without UB



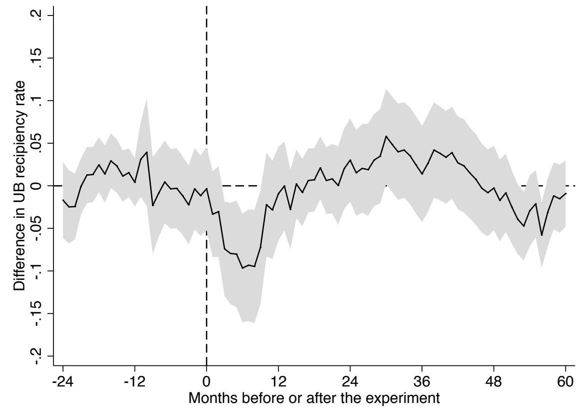
(d) Unemployed without UB (difference)

Notes: Figure A2 plots the fraction of individuals who receive UB (top), and the fraction of unemployed individuals who do not receive UB (bottom). For the three categories, we also report the difference between the two groups, defined as treatment minus control.

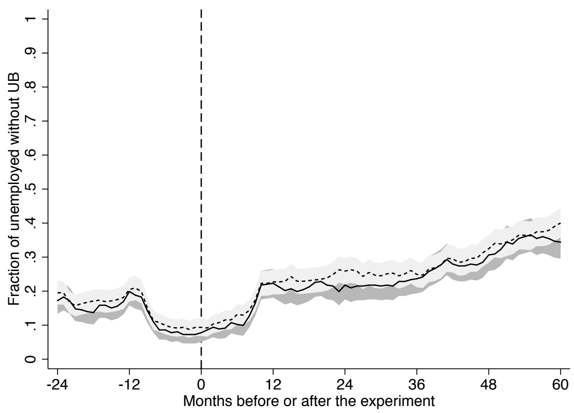
Figure A3: Long-term effects on unemployment



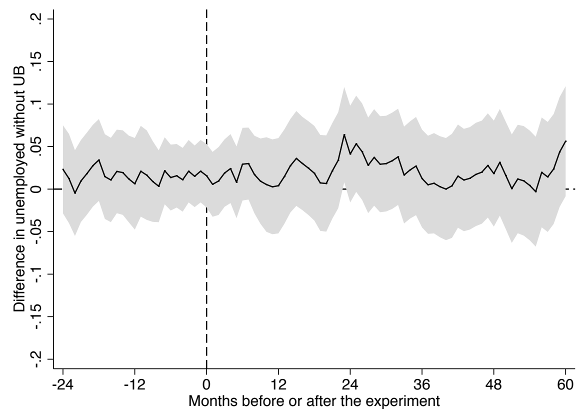
(a) UB recipients



(b) UB recipients (difference)



(c) Unemployed without UB



(d) Unemployed without UB (difference)

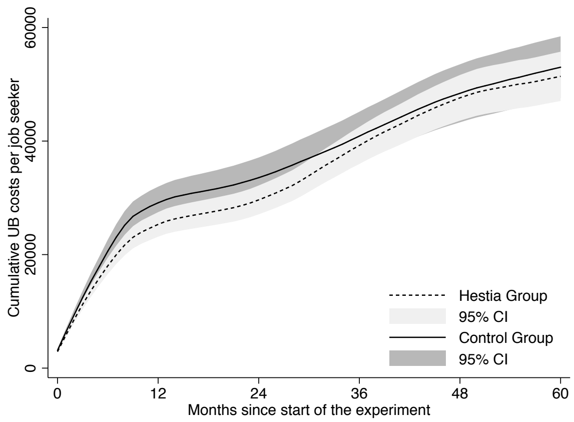
Notes: Figure A3 plots the fraction of individuals who receive UB (top), and the fraction of unemployed individuals who do not receive UB (bottom). For the three categories, we also report the difference between the two groups, defined as treatment minus control.

Table A1: Placement timing and wages, quantile regressions

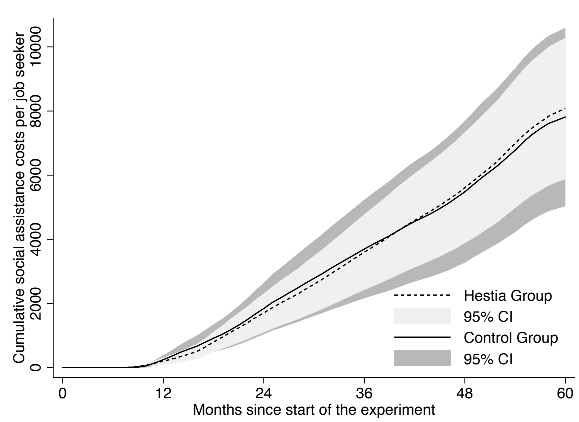
	(1) Median	(2) Median	(3) 1st quartile	(4) 3rd quartile
Hestia	0.029	0.035	0.001	0.384
	(0.06)	(0.14)	(0.22)	(0.27)
Hestia × Placed 4-6 months		0.011	-0.164	-0.713
		(0.17)	(0.28)	(0.44)
Hestia × Placed 7-12 months		0.067	-0.003	-0.362
		(0.16)	(0.25)	(0.29)
Hestia × Placed 13-18 months		-0.078	0.017	-0.650
		(0.21)	(0.29)	(0.45)
Hestia × Placed 19-24 months		-0.271	-0.960	-0.795*
		(0.39)	(0.64)	(0.45)
Hestia × Placed 25+ months		-0.246	-0.332	-1.399*
		(0.50)	(1.07)	(0.82)
Control variables	Yes	Yes	Yes	Yes
Control for job entry timing	No	Yes	Yes	Yes
Adjusted R^2				
Individuals	674	674	674	674

Notes: Table A1 reports point estimates of quantile regressions on the change in log earnings. Control variables include: gender, age, marital status, schooling, nationality, mother tongue, residence permit, professional qualifications, placement prospects, OCE job code, and cohort number. Robust standard errors in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

Figure A4: Costs per job seeker (continued)



(a) Unemployment benefits costs



(b) Social assistance costs

Notes: Graph (a) plots the cumulative UB costs per individual enrolled in one of the two programs, while graph (b) does the same for social assistance payments.