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IZA DP No. 11766

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ABSTRACT

Does Job Search Assistance Really Raise Employment?*

We study how job search assistance (JSA) affects employment in a randomized pilot study with long run administrative data. JSA increases employment in the first year after assignment. In the second year, when most job seekers have left JSA, the employment gains evaporate, and even turn into losses in the third year. This sinusoidal pattern is consistent with job finding and employment loss transitions. Job seekers assigned to JSA find employment faster but, once employed, also lose employment faster, especially once eligible for new unemployment benefits. Job seekers assigned to JSA have similar types of contracts and re-employment earnings, but somewhat worse positions in the firm and are more likely to have a part time job.

JEL Classification: J64, J68

Keywords: job placement, long term unemployment, job loss, job search assistance, active labor market policy

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1 Introduction

Long-term unemployment is a key problem in many labor markets and officials often rely on job search assistance (JSA) to tackle it. JSA programs can help job seekers by offering career counseling and skills assessment, providing guidance on writing job application packages, preparing for job interviews, or locating appropriate job openings. Job search assistance is available during all phases of the unemployment spell, and its intensity often increases over the unemployment spell.

JSA shortens job search, and is considered as one of the more successful elements of the mix of active labor market policies (Card *et al.*, 2010, 2018). Two studies have also documented positive long-run effects of JSA on employment for newly unemployed (Maibom *et al.*, 2017; Manoli *et al.*, 2018). We provide new evidence on the effects of JSA for job seekers who have already been unemployed for one year when the program starts. Whether JSA is effective for the long-term unemployed is not clear. The long-term unemployed likely use job search strategies that are not successful, and JSA may improve these strategies thereby creating long-lasting employment. Yet the long-term unemployed are also hard to place, and JSA may push job seekers into jobs of lower quality thereby increasing employment in the short-run but not in the long-run.

Specifically, we study the effects of a JSA program designed by *Les Maisons Hestia* (Hestia), a job placement firm in Geneva, to help long-term unemployed. The JSA program offered both guidance on writing job application packages and help in finding job vacancies. JSA case-workers called prospective employers to inquire for vacant positions and referred candidates from their pool of clients. Over a period of one year, a total of 890 job seekers were randomly selected to potentially receive services of Hestia's intensive JSA program. Among those selected, 50 percent were in line with eligibility criteria for the program. In all our empirical analyses, we compare Hestia and control job seekers, measuring "the intention to treat" (ITT) effects of being assigned to Hestia's services.

Our main analyses use data collected in the original pilot linked with administrative information on employment and earnings over the period two years before to five years after job seekers were allocated to treatment. The period before the treatment allows us to assess whether labor market trajectories of Hestia and control job seekers were balanced. Information on up to five years after assignment to the program provides us with the opportunity so see how long job seekers remained employed. We use information on labor market program participation and find no differences in ALMP participation between Hestia and control job seekers. This information is useful in inter-

preting the treatment effect as the additional effect of being offered Hestia's services. A smaller survey provides information on the quality of the first job upon re-employment (e.g. position in new firm, part-time, and contract duration).

We find that about six months from the start, employment is four to five percentage points higher, and unemployment benefit receipt about six percentage points lower for job seekers assigned to Hestia compared to control job seekers. The program was very successful in placing job seekers into jobs during the first twelve months after the pilot started (see also Flückiger and Kempeneers, 2008). Yet, the employment gain from the first twelve months evaporates in the second year after assignment, and turns significantly negative in the third after assignment to program. Assignment to JSA produces a sinusoidal pattern in employment: immediate employment gains, followed by employment losses later on.

To understand this sinusoidal pattern in employment, we analyze transitions to employment, noting that establishing causality is challenging due to dynamic selection (Eberwein *et al.*, 1997). We find that transitions from unemployment to employment are about 50 percent higher among job seekers assigned to Hestia, especially in the period four to six months after assignment which is just before payments to Hestia decrease. Transitions to employment can rationalize the immediate employment gain of the sinusoidal pattern.

We then analyze exits from employment to non-employment for job seekers who found a job. Job seekers assigned to Hestia are more likely to leave their new job in the first three months after it started, when most job finders are in the probation period. Hestia job seekers are somewhat less likely to leave employment in months ten to twelve after their job started, but substantially more likely to leave their job 13 to 18 months after the new job started. This waiting-and-leaving pattern can be rationalized in at least two ways. A contract explanation holds that fixed-term contracts are typically running out after one year, and JSA might have placed more job seekers into fixed-term contracts. A job quality explanation holds that job seekers could be waiting to accumulate twelve months of employment, enough to renew eligibility for unemployment benefits (UB). This makes sense for workers who hold down marginally attractive jobs, those which are better than unemployment without benefits but worse than unemployment with new benefits. If JSA places more job seekers into the marginally attractive jobs, more job seekers will wait to qualify for benefits and leave once they do.

To disentangle these competing explanations, we look at the effects of Hestia on job quality. In a small survey on conditions upon re-entry, we find that job seekers served by Hestia are somewhat

more likely to have a permanent contract, albeit not significantly so. The contract view is unlikely to account for the waiting-and-leaving pattern in employment loss. With regards to job quality, we explore changes in log earnings between the former job and the new one but find no differences between Hestia and control job seekers. Average earnings are not affected by Hestia in the first two years after the program started despite the higher employment rate among Hestia's job seekers during that period. This suggests that there is a small but negative effect on average earnings for Hestia's job seekers. The small survey also contains information on job quality. While not statistically significant, Hestia job seekers are more likely to have a supporting role in the new firm, and are more likely to work part-time than control job seekers. This pattern of results suggests that Hestia placed job seekers into jobs that offer somewhat lower match quality. Hestia has leverage to interfere directly with job search by threatening to trigger a benefit sanction process through its contacts with the public employment service.

Our paper is related to the literature on the short-run effects of JSA.[1] van den Berg and van der Klaauw (2006) study two components of JSA, counseling and monitoring, for high skilled individuals. Neither counseling 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, see e.g. 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 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.

The literature on the long-term effects of job search assistance is most closely related to our study.[2] Maibom *et al.* (2017) analyze the long-term effects of randomly assigned meetings with the case-worker, individually or in group, for newly registered job seekers in 2009. Individual meetings raise cumulative employment by about 4.2 weeks, while group meetings do not have a

[1] Several programs in the U.S. 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).

[2] Decker *et al.* (2000) is an early government report that studies the effects of job search assistance up to two years after assignment in two sites, Washington DC and Florida. Compliance with the program is very low, about one percent in Washington DC, and about 10-20 percent in Florida. The report finds that JSA reduces unemployment benefits, and finds small positive or insignificantly negative effects on employment.

statistically significant effect. Manoli *et al.* (2018) study the long-term effects on newly registered job seekers of a program that offers an eligibility review and a personalized job search counseling in Nevada in 2009. This program raises employment by 8 percentage points right after it started, and employment gains remain positive throughout the next five years.[3]

We complement the body of literature that documents long-run effects of job search assistance. The existing literature documents persistent positive effects for newly unemployed job seekers, positive in the Maibom *et al.* (2017) and Manoli *et al.* (2018) settings. Whether effects are persistent in the long-run depends to a strong extent on a program's capacity to deliver substantial training gains to job seekers who would not have found a job without the program. Programs that lift more job seekers into employment, e.g. the Nevada program in Manoli *et al.* (2018), deliver persistently positive effects. Programs that deliver the same number of jobs faster will also improve employment in the short-run, but not permanently. We study a job search intervention for long-term unemployed. It is an open question to what extent job search assistance can deliver persistently positive effects for the long-term unemployed.

Two strands of the literature provide context. The first argues that active labor market programs work because they are perceived as a threat. Black *et al.* (2003) show that job seekers leave unemployment more quickly when they receive a letter to report for training.[4] 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 leave 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. Our finding that JSA improves job finding, but lower employment stability, suggests that some elements of Hestia's JSA program could be perceived as a threat by job seekers. The second, discusses the role of job quality. A related literature studies the effects of changes to the generosity of unemployment insurance, notably potential benefit duration (PBD), on job quality. 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 sub-

[3]Petrongolo (2009) analyses the effects of the 1996 Jobseekers' Allowance (JSA) reform that tightens eligibility requirement and raises administrative hurdles for newly unemployed job seekers. The JSA reform increases outflow from unemployment but reduces employment in the year after leaving unemployment.

[4]Bolhaar *et al.* (2018) shows that mandatory job search periods for welfare applicants appears to detract them from applying for welfare benefits, and increases their earnings.

sequent 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. Two studies on the Austrian context find positive effects of benefit extensions. Degen (2014), and Nekoei and Weber (2017) 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 (2017) 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 (2017) we study the effects of intensive JSA on job stability and wages.[5]

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 presents the results. Section 5 concludes the study.

2 Experiment

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

2.1 Unemployment Insurance

Job seekers need to fulfill two requirements in order to be eligible for unemployment insurance benefits.[6] 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".

[5] A recent literature on the effects of outsourcing all services from the public employment service to private providers is related, but different. See e.g. Cockx and Baert (2015) on Belgium, Bennmarker *et al.* (2013) on Sweden, or Rehwald *et al.* (2015) on Denmark, or Krug and Stephan (2013) on Germany. 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.

[6] This subsection draws extensively on our earlier work on Swiss active labor market policies (Lalive *et al.*, 2005, 2008; Arni *et al.*, 2013).

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.[7] Job seekers who are ineligible for unemployment insurance 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).[8] The replacement rate is 70 percent for high-income workers (earning more than 4,340 SFr) and is smoothly adjusted in between so that there are no discontinuities in the replacement rate.[9] 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.

Job seekers contact their caseworker at least once a month in 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. Caseworkers monitor job search, check up on some of the applications claimed by job seekers. Caseworker monitor participation in Active Labor Market Programs because program suppliers only get paid for the actual number of days a job seeker attends the program. Caseworkers can sanction job seekers who refuse to apply to a suitable job, or do not participate in a program. Benefit sanctions often amount to one week of benefit payments, but can go up to two months of benefit payments.

2.2 Experimental Design

The study 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

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

[8] 1 SFr $\approx$ 0.93 EUR $\approx$ 0.99 USD as of December 2016. Source: XE currency converter.

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

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.

The evaluation of the job search assistance scheme is based on a randomized controlled experiment which took place between 2006 and 2007. The goal of this experiment was to determine whether JSA could help lower the comparatively high level of long-term unemployment in Geneva. The private but non-profit firm Hestia offered a tailor-made JSA program with two phases. The catch-up phase aimed to improve some of the job seekers' skills through job interview training, networking, and resume writing. In the search-and-place phase, Hestia devoted most of the effort to collect vacancies, call firms to ask about unadvertised positions, transmit them to their candidates, and follow up applications. Job seekers could refuse to apply to the vacancies, but refusal can lead to a benefit sanction if the case-worker at the PES learns about it. 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.

Hestia had a team of three consultants plus two tele-marketing 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 Hestia group had to follow Hestia's program additionally. Note that if an individual allocated to the Hestia 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.[10] Hestia received 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 seven 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.

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

[10]The pilot 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.

entering the experiment between October 2006 and July 2007. The randomization was as follows. In a first stage, 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 as of September 2006. These job seekers were randomly allocated to receive treatment, stratified by unemployment insurance agency. The control group was then formed of the reference population minus those allocated to treatment.

After the first stage, each PES agency checked whether individuals in the treated group were eligible for unemployment benefits, or 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. Hestia announced each month 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 until the end of pilot.

About half the individuals assigned to Hestia did not start the program, a low non-compliance rate compared to the literature that studies outsourcing of job placement.[11] The reasons why job seekers were excluded from the program is important for the interpretation of our results. Individuals who were excluded from the program were asked about their reason for not following the treatment. Of the answers given, 22 percent had subsidized employment; 17 percent had already found a job; 13 percent were following another labor-market measure. Several other reasons were also given but refusal to follow the intensive placement program accounts for only two percent of the total. This evidence suggests that the prime motivation for excluding job seekers is tightly linked to the program admission criteria. Job seekers self-selection into the program, based on expected outcomes, is perhaps less likely to be present. Our results are therefore likely to reflect the effects of assigning the intense JSA program to job seekers that satisfy its admission criteria, because self-selection played a minor role. In all our analyses, we keep the job seekers assigned to treatment but not following it in all our analyses and report "intention to treat" (ITT) effects.

The program was targeted to the long-term unemployed. How this particular sample affects the interpretation of our findings is not clear. Assisting long-term unemployed in finding a job is likely to yield positive results as these individuals may already have exhausted their own search possibilities, or be discouraged. However, there might be good reasons for these individuals not to

[11] Compliance is substantially less than 50% in recent experimental studies by Bennmarker *et al.* (2013), Rehwald *et al.* (2015), or Behaghel *et al.* (2014).

have found a job before, which is likely to complicate their placement for the caseworkers as well. Individuals who have spent a long time out of the labor force might face more difficulties to adapt to their new job once placed, increasing job loss rates.

2.3 Incentive Effects

Hestia's receives money in exchange of providing services to job seekers. The monthly flat fee per job seeker enrolled is monotone decreasing. Its highest value is in the first six months of the experiment, before a first drop in the payments. 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, cream-skimming, in order to pocket the high fee in the first months and receive new job seekers to place. In particular, the payment scheme greatly incentivizes Hestia to target efforts around six months into the spell, just before monetary rewards drop.

Second, Hestia could provide minimal services to the harder-to-place individuals, keeping them for the extra cash that they bring in the long run. 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.[12]

Whether JSA affects the stability of employment depends on how it affects the quality of accepted jobs. Hestia's payments do not depend on job quality, but Hestia could have an interest in persuading job seekers to accept lower quality jobs since that helps Hestia to get job seekers off its rolls faster. Job seekers served by Hestia might also consider their jobs to be of lower value even if, objectively, they are not. Hestia interferes with job search strategies, and can induce job seekers to accept jobs that they would not normally have accepted if left on their own.

3 Data

We use data from the pilot that contain 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 that reflects how likely a job seeker will find a new job based on objective and subjective information.[13]

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. For each month, we have information on whether the individual receives any

[12]Note that parking could still be present if taking care of the long-term unemployed, which earns Hestia 350 SFr, costs more than a new unemployed brings home, i.e. 1000 SFr. This issue should nonetheless be reduced by Hestia's willingness to perform well to secure a long-run contract. 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.

[13]Specifically, placement prospects 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 variable had been created before allocating job seekers into the control and Hestia group, and is therefore available for everyone in our sample.

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 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 total, the experiment had 378 individuals in the control group and 512 individuals in the Hestia group. After linking these individuals with their social security records, this yields data on 372 job seekers, and 502 job seekers in the Hestia group.

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 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 ten 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 Hestia groups, and the seventh column, where we report t-tests on the differences.

Job seekers assigned to Hestia and receive JSA differ from job seekers assigned to Hestia but do not receive JSA (Table A1). We observe some important differences with respect to work experience and age. Hestia 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. Hestia individuals were also older than the non-treated individuals (17 percent treated individuals aged 55-64 years vs seven 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.

Table 1: A balanced sample

	Control Group		Hestia Group			
Variable	Mean	s.e.	Mean	s.e.	Difference	t-stat
Women	0.51	0.03	0.49	0.02	0.02	0.50
Marital status						
Single	0.35	0.02	0.33	0.02	0.02	0.53
Married	0.51	0.03	0.54	0.02	-0.03	-0.80
Widower	0.01	0.00	0.00	0.00	0.00	0.30
Divorced	0.14	0.02	0.13	0.01	0.01	0.37
Experience						
None	0.01	0.01	0.03	0.01	-0.01	-1.33
Less than 1 year	0.07	0.01	0.09	0.01	-0.01	-0.80
1-3 years	0.25	0.02	0.26	0.02	-0.01	-0.20
More than 3 years	0.56	0.03	0.51	0.02	0.05	1.49
Age						
17-24	0.08	0.01	0.10	0.01	-0.02	-1.10
25-34	0.28	0.02	0.31	0.02	-0.04	-1.21
35-44	0.28	0.02	0.27	0.02	0.01	0.19
45-54	0.21	0.02	0.19	0.02	0.02	0.91
55-64	0.15	0.02	0.12	0.01	0.03	1.20
Schooling						
Compulsory	0.40	0.03	0.43	0.02	-0.02	-0.71
High-school level	0.36	0.02	0.36	0.02	0.00	0.01
University level	0.20	0.02	0.18	0.02	0.01	0.55
Workers						
Swiss	0.52	0.03	0.50	0.02	0.02	0.68
C permit	0.30	0.02	0.29	0.02	0.01	0.28
Other	0.18	0.02	0.21	0.02	-0.03	-1.19
Placement prospects						
Excellent	0.04	0.01	0.06	0.01	-0.02	-1.16
Good	0.55	0.03	0.53	0.02	0.02	0.62
Average	0.23	0.02	0.21	0.02	0.02	0.75
Poor	0.17	0.02	0.20	0.02	-0.02	-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 one due to missing observations. The sixth column calculates the difference between control and Hestia groups, defined as control minus treatment. The seventh column reports two-sided t-statistics on the differences.
Source: Authors' own calculations.

Of the 512 job seekers randomly assigned to receive treatment, 260 job seekers actually did. In what follows, we keep all 512 job seekers allocated to the treatment in that group and report intention-to-treat (ITT) effects.[14]

3.2 ALMP Participation

Table 2 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 Hestia 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 eight percent, and advanced IT courses with three to five percent. Also, between 16-19 percent of job seekers have followed 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 Hestia group participation, 23 percent vs 62 percent respectively. The reason why the participation in the base program is not 100 percent for the Hestia 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 eight percent of the Hestia group is in the same situation. Job seekers assigned to Hestia used Hestia's JSA program in addition to other ALMP programs.

[14]ITT results are usually more conservative because of the dilution of the treatment effects due to non-participation and non-compliance. The ITT can be rescaled to the local average treatment effect (LATE), the average effect of the JSA program on job seekers receiving treatment. The LATE is the ITT divided by the participation rate in treatment. In our context, LATE is about twice as high as the ITT, since about one half of all job seekers (260 out of 512 job seekers or 50.8 percent) participated in the program.

Table 2: Identical ALMP participation

	Control Group		Hestia Group			
Variable	Mean	s.e.	Mean	s.e.	Difference	t-stat
Individual ALMPs						
Base program	0.03	0.01	0.04	0.01	-0.00	-0.37
Personality development	0.02	0.01	0.01	0.00	0.01	0.79
Basic skills acquisition	0.02	0.01	0.02	0.01	0.00	0.03
Language course	0.12	0.02	0.11	0.01	0.01	0.50
Basic IT skills	0.09	0.01	0.07	0.01	0.02	1.15
Advanced IT skills	0.03	0.01	0.04	0.01	-0.02	-1.26
Others	0.16	0.02	0.13	0.01	0.03	1.33
None	0.62	0.02	0.65	0.02	-0.03	-0.94
Group ALMPs						
Base program	0.23	0.02	0.62	0.02	-0.39	-12.89
Personality development	0.04	0.01	0.04	0.01	0.00	0.15
Basic skills acquisition	0.01	0.00	0.00	0.00	0.01	1.20
Language course	0.01	0.00	0.01	0.00	-0.00	-0.29
Basic IT skills	0.01	0.00	0.01	0.00	0.00	0.37
Advanced IT skills	0.00	0.00	0.00	0.00	-0.00	-1.00
Others	0.16	0.02	0.13	0.01	0.03	1.43
None	0.56	0.03	0.28	0.02	0.27	8.48
Number of observations	378	-	512	-	-	-

Notes: Table 2 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 Hestia groups, defined as control minus treatment. The seventh column reports two-sided t-statistics on the differences.
Source: Authors' own calculations.

4 Results

This section first documents the sinusoidal effect of JSA on employment, then shows job finding and employment loss transitions. The second part discusses what explains the sinusoidal pattern, presenting results for job quality, and simulations of the employment effects based on transition patterns.

4.1 Employment

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 unemployment benefits 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). These states represent any situation in which an individual can be at a given point in time. 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 estimate is given by:

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

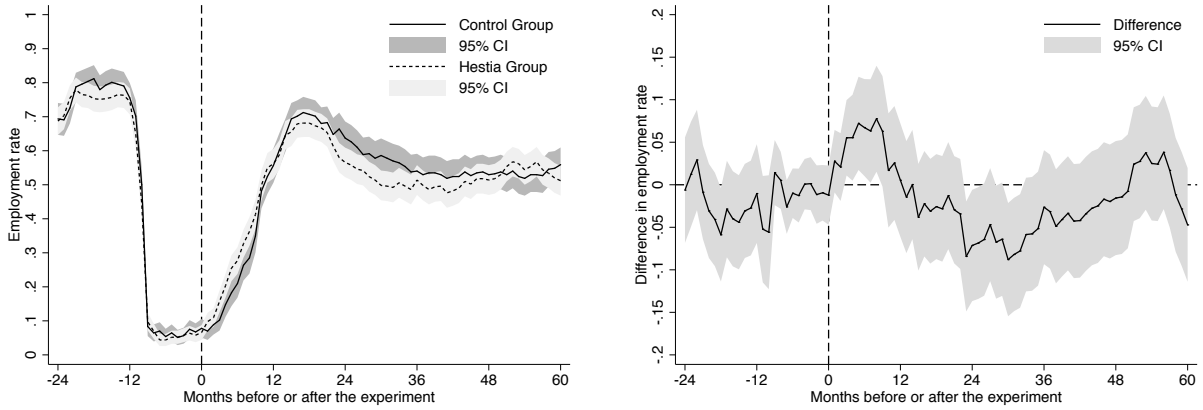
where Y_{it} is the dependent variable for individual i at process time t, X_i is a vector of individual-specific, time-constant controls, D_i is a dummy taking the value one if a job seeker is assigned to the treatment (Hestia), and zero otherwise. $\mathcal{T}_t$ is a vector of time period dummies.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

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".

Figure 1 plots the fraction of individuals who are employed (left) and the difference between the two groups (right), defined as Hestia minus control. Note that the timeline on the X-axis is normalized for each individual. Month zero represents the allocation into the Hestia and control groups. 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: JSA 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 Hestia minus control.
Source: Authors' own calculations.

Consider first graph (a). Two years before the experiment around 80 percent of the individuals were employed. Most of them lose their job twelve months before the experiment. The dynamics for the Hestia 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 loses their job again. The Hestia 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 Hestia 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 twelve 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 1bb shows the sinusoidal effects of JSA on employment: initially higher employment, then lower employment, followed by no impact.

Table 3 reports the results of OLS regressions on the three mutually exclusive labor market states, reporting estimates the parameter vector 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 into two categories. Panel (A) highlights treatment effects on the Hestia group, and panel (B) tests whether treatment and control groups differed before the experiment (B).

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 control group 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). We observe the opposite pattern to the one for employed individuals. Hestia's JSA program decreases the fraction of individuals receiving UB by six percentage points in the first twelve 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

Table 3: Effects on employment and UB receipt

	Employed, no UB		UB recipients		Unemployed, no UB	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Treatment Effects						
Hestia × 1-12 m. after	0.044**	0.038*	-0.059***	-0.052**	0.015	0.014
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Hestia × 13-24 m. after	-0.026	-0.031	0.002	0.007	0.024	0.023
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
Hestia × 24-36 m. after	-0.067**	-0.079***	0.033	0.044*	0.033	0.034
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Hestia × 37+ m. after	-0.011	-0.011	-0.002	0.000	0.014	0.011
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
B. Randomization						
Hestia × 24-11 m. before	-0.023	-0.032	0.006	0.013	0.016	0.018
	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Hestia × 12-1 m. before	-0.014	-0.017	-0.001	0.003	0.015	0.014
	(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	0.174	0.254	0.269	0.033	0.081
Individuals	874	844	874	844	874	844

Notes: Table 3 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. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

Source: Authors' own calculations.

statistical significance.

Third, 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.

Fourth, the tests for randomization in panel B show no significant differences between the control and the Hestia group before the experiment for any of the dependent variables, which supports our empirical approach.

These analyses indicate that JSA had a sinusoidal effect on employment. JSA initially helped long-term unemployed job seekers find jobs but later on appears to have decreased employment. Existing studies for the newly unemployment find positive effects of JSA on employment during several years after the program started (Manoli *et al.*, 2018; Maibom *et al.*, 2017). Petrongolo (2009), studying the effects of tightening benefit eligibility which lower the flow value of remaining unemployed, finds increased outflow from unemployment but lower employment in the long-run, similar to our finding of a sinusoidal employment effect pattern.[15] JSA for the long-term unemployment may lower the value of remaining unemployed, increasing exit to jobs by lowering job quality.

We now turn to better understanding what leads to this sinusoidal pattern. Two competing explanations are conceivable: catch-up and employment loss. The catch-up explanation says that the sinusoidal pattern merely reflects that the control group is catching up with the Hestia group. JSA helps job seekers find a job more quickly, but eventually also job seekers in the control group end up employed. The employment loss explanation holds that lower employment stability among job seekers assigned to JSA is essential to understanding the sinusoidal pattern. We turn to discussing these two views using transition data.

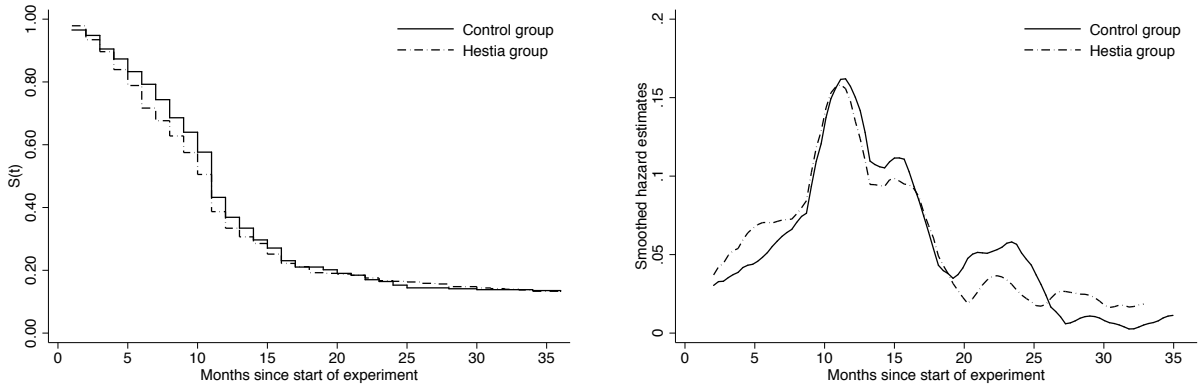
4.2 Finding Employment

We first want to understand how JSA affected job finding. Interpreting these analyses as causal is challenging because randomization ensures a causal interpretation of the survival curves but the hazard rates may be biased by dynamic selection in the presence of unobserved heterogeneity (Eberwein *et al.*, 1997). We therefore use transitions analysis as a way to provide descriptive infor-

[15] Petrongolo (2009) also finds that tightening benefit eligibility increases the probability to start a spell on incapacity benefits, i.e. that benefit substitution may take place. We do not find evidence for higher inflow into social assistance.

mation on how treated and control group transitions evolved. The sample only includes individuals who started the experiment unemployed.[16] 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.

Figure 2: Survival rates in unemployment and hazards to job



(a) Survivor function in unemployment

(b) Unemployment to job hazard

Notes: Figure 2 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.

Source: Authors' own calculations.

Consider first graph (a). As we can see, the survivor curve of the Hestia group is almost always below that of the control group. This suggests that Hestia 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 Hestia 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.

We also estimate 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 factors. The

[16] While only long-term unemployment job seekers were meant to enroll in the experiment, it appeared 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.

hazard rate a time t is given by:

$$\lambda(t|X) = \lambda_0(t)\exp(X\beta') \tag{2}$$

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.[17] 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 Hestia 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).

Table 4 reports the results of Cox regressions on transitions to job. Recall that positive coefficients in column (1) are positive results since it means that the transition from unemployment to employment increases. In total, 820 individuals started the experiment unemployed and 739 found a job during the observation period, which represents 90 percent of the subjects. 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 from 19 months after the experiment. Note that Hestia was not necessarily 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.

4.3 Leaving Employment

Consider now the transition from employment back to unemployment. Dynamic selection poses a considerable challenge as we only observe employment duration, or other outcomes related to the job, for individuals who have found a job, which may be a selected sub-sample of the original population. Lee (2009) discusses a similar context to ours, the Job Corps demonstration, a large JSA

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

Table 4: Cox regressions on job entry

	(1)	(2)
Hestia × 1-3 months	0.087	0.038
	(0.22)	(0.23)
Hestia × 4-6 months	0.506***	0.460**
	(0.19)	(0.19)
Hestia × 7-12 months	0.009	-0.012
	(0.10)	(0.11)
Hestia × 13-18 months	-0.047	-0.049
	(0.17)	(0.18)
Hestia × 19+ months	-0.367*	-0.447**
	(0.22)	(0.22)
Control variables	No	Yes
Subjects	820	792
Failures	739	713

Notes: Table 4 reports point estimates of Cox regressions on transitions 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. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.
Source: Authors' own calculations.

program in the US, where assignment to treatment is random, and treatment might affect whether job seekers are ever employed or not. Assuming that treatment affects job finding monotonically, Lee (2009) shows that (i) the treatment effect on the proportions of job seekers who ever find a job informs on the amount of selectivity, and (ii) a trimming procedure can recover the treatment effect.

Adopting Lee (2009)'s framework, we first look into how many job seekers ever find a job, and compare their observable characteristics. Table 5 shows the fraction of people who find a job (panel A), and the socio-demographic characteristics of the individuals who found a job (panel B). The proportion of job finders is the same in treated and control groups. In the Lee (2009) setting, this means that there is no selection problem, and job finders should be similar in the treated and control group. Indeed, Table 5B shows the characteristics of the individuals in the Hestia and control groups are almost identical, as though the treatment had been randomly assigned among job finders. Our result only refers to observed characteristics of job seekers, and unobserved characteristics might not be balanced. In the Lee (2009) setting, our finding that the share of job finders in treated and control groups is the same implies that all characteristics, both observed and unobserved, are balanced, because randomized assignment of JSA among job seekers is preserved among job finders.

Figure 3 plots Kaplan-Meier survivor function in employment (left) and one-period smoothed job to unemployment hazard estimates of (right). The origin is defined as the beginning of a new employment spell after the start of the experiment, while the failure is the loss of the job. Note that a high survivor function is a positive outcome as it means that more people are still employed.

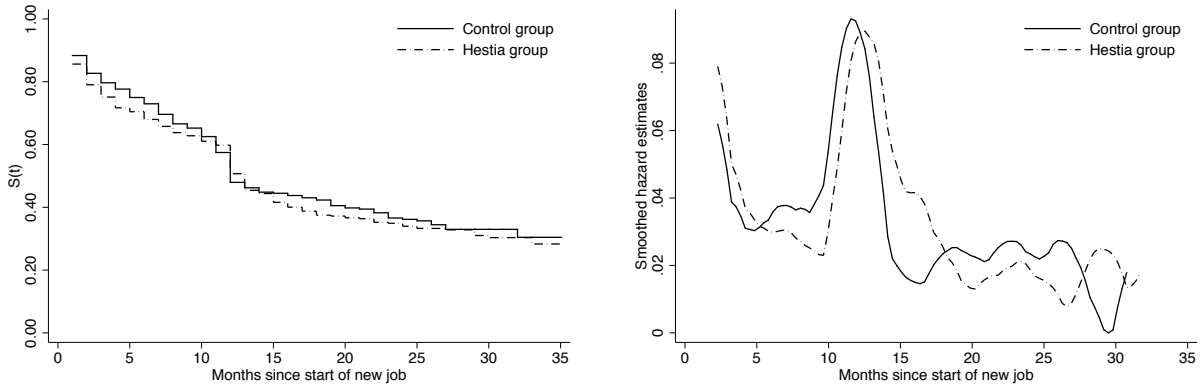
Table 5: Socio-demographic characteristics of individuals who found a job

	Control Group		Hestia Group			
Variable	Mean	s.e.	Mean	s.e.	Difference	t-stat
A. All job seekers						
Fraction of group with job	0.85	0.02	0.84	0.02	0.01	0.28
Number of observations	372	-	502	-	-	-
B. Job finders						
Women	0.50	0.03	0.50	0.02	-0.00	-0.10
Marital status						
Single	0.37	0.03	0.34	0.02	0.03	0.90
Married	0.49	0.03	0.52	0.02	-0.03	-0.92
Divored or widower	0.14	0.02	0.14	0.02	0.00	0.08
Experience						
3 years and less	0.36	0.03	0.37	0.02	-0.01	-0.38
More than 3 years	0.54	0.03	0.51	0.02	0.02	0.67
Age						
17-24	0.09	0.02	0.11	0.02	-0.02	-0.91
25-34	0.28	0.03	0.32	0.02	-0.03	-1.01
35-44	0.29	0.03	0.27	0.02	0.01	0.41
45-54	0.21	0.02	0.18	0.02	0.03	1.17
55-64	0.13	0.02	0.12	0.02	0.01	0.25
Schooling						
Compulsory	0.41	0.03	0.43	0.02	-0.02	-0.56
High-school level	0.36	0.03	0.37	0.02	-0.01	-0.36
University level	0.19	0.02	0.17	0.02	0.02	0.60
Workers						
Swiss	0.53	0.03	0.50	0.02	0.03	0.82
C permit	0.31	0.03	0.31	0.02	0.01	0.17
Other	0.16	0.02	0.19	0.02	-0.04	-1.30
Placement prospects						
Good or excellent	0.61	0.03	0.60	0.02	0.01	0.22
Average	0.22	0.02	0.22	0.02	0.01	0.21
Poor	0.17	0.02	0.18	0.02	-0.01	-0.51
Number of observations	316	-	423	-	-	-

Notes: Table 5 presents the socio-demographic characteristics of the individuals who found a job after the start of the experiment. Some categories may not add up to one due to missing observations. The sixth column calculates the difference between control and Hestia groups, defined as control minus treatment. The seventh column reports two-sided t-statistics on the differences.

Source: Authors' own calculations.

Figure 3: Survival in employment and hazards back to unemployment



(a) Survivor function in employment

(b) Job to unemployment hazard

Notes: Figure 3 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.
Source: Authors' own calculations.

Consider first graph (a). We observe that the survivor curve of the Hestia group is again below that of the control group, suggesting that Hestia's clients 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. There is also a sharp drop in the survivor curves of both groups after twelve months, when most individuals re-gain eligibility to UB.

Second, consider graph (b). The hazards show that Hestia's clients lose their job at a higher rate that their PES counterparts in the first six months. Then, the hazards of both groups peak one year after the start of the experiment. Fixed duration work contracts (e.g. one year) could explain this phenomenon. Alternatively, individuals become eligible again for UB (although not the full entitlement) after working for a whole year, allowing individuals to leave a job that they do not like. We observe higher levels in the hazards of Hestia's clients between twelve and 18 months, before converging to those of the control group.[18]

[18] An interesting question is whether job loss patterns are linked to local labor market conditions. Figure A3 in the appendix shows that unemployment rate in canton Geneva decreased in the two years that followed the start of the experiment. From 7.3 percent in January 2006, it went down to 5.6 percent at the end of 2008. Partly due to the financial crisis, it then bounced back to 7.2 percent at the end of 2009, before decreasing again. We have seen that about 75 percent of job seekers from both groups found a job in the twelve months that followed the start of the experiment, and that job losses happened mostly in the first twelve months of the employment spell. For both groups, this implies that most transitions happened while the overall unemployment rate was still decreasing. Differences in

Table 6: Cox regressions on job exits

	(1)	(2)	(3)
Hestia × 1-3 months	0.253*	0.252	0.335**
	(0.15)	(0.16)	(0.16)
Hestia × 4-6 months	-0.036	0.009	0.073
	(0.27)	(0.28)	(0.27)
Hestia × 7-12 months	-0.343**	-0.374**	-0.344**
	(0.16)	(0.16)	(0.17)
Hestia × 13-18 months	0.657***	0.713***	0.739***
	(0.25)	(0.27)	(0.27)
Hestia × 19+ months	-0.158	-0.208	-0.171
	(0.17)	(0.17)	(0.17)
Control variables	No	Yes	Yes
Control for job search duration	No	No	Yes
Subjects	739	713	713
Failures	586	565	565

Notes: Table 6 reports point estimates of Cox regressions on transitions back to unemployment. The origin is defined as the beginning of a new employment spell after the start of the experiment, while 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. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$.
Source: Authors' own calculations.

Table 6 shows Cox regressions on the transition from job back to employment. 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.[19]

Hestia job seekers 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, most contracts come with

job loss transitions are therefore unlikely to be explained by changing labor market conditions.

[19]The number of subjects in column (1)-(3) of Table 6 are equal to the number of failures in columns (1) and (2) of Table 6 respectively.

a three-month probation period, after which both sides (employer and employee) can break the contract unilaterally. Treated job seekers are less likely to leave their job in 10-12 months after starting it, but much more likely to do so between 13-18 months after the start of their employment. This waiting and leaving pattern can rationalized in at least two ways. A contract explanation holds that fixed-term contracts are running out typically after one year, and JSA might have placed more job seekers into fixed-term contracts. A job quality explanation holds that job seekers could be waiting to accumulate 12 months of employment, enough to renew eligibility for unemployment benefits (UB). This makes sense for workers who hold down marginally attractive jobs, those which are better than unemployment without benefits, but worse than unemployment with new benefits. If JSA places more job seekers into the marginally attractive jobs, more job seekers will wait to qualify for benefits, and leave once they do, in the group assigned to JSA.

4.4 Job Quality and Earnings

JSA reduces job stability if it leads to lower quality of employment. We start with how JSA affects earnings while employed before and after unemployment. Better paid jobs could make Hestia's participants accept jobs faster and keep them longer, while lower paid job should have the opposite effect. It is therefore not clear how differences in wages could explain the patterns observed, but it certainly has an effect on individuals' behavior.

We now turn to a more formal analysis of the impact of Hestia's JSA program on work income. Table 7 reports point estimates of OLS regressions on the change in log work income. The change is calculated by calculating the log of the new work income and subtracting the log of the former work income from it. We estimate two 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.

We observe from models (1) and (2) that Hestia's program did not affect work income growth significantly. The job entry and job loss dynamics should therefore not be explained by differences in work income. However, the issue of dynamic selection might bias the results. This could be the case if Hestia focused on placing individuals with high (or low) earnings potential first while the control group did not target anyone in particular. We therefore provide bounds on the probability of finding a better paid job. This variable is a dummy variable equal to one if the new work income is higher than the former one. Table 8 reports three types of bounds, namely worst-case bounds (no

Table 7: Effects on work income growth

	(1)	(2)
Hestia	0.006	-0.021
	(0.10)	(0.10)
Control variables	Yes	Yes
Control for job entry timing	No	Yes
Adjusted R^2	0.010	0.017
Individuals	674	674

Notes: Table 7 reports point estimates of OLS regressions on the change in log work income. Control variables include: gender, age, marital status, schooling, nationality, mother tongue, residence permit, professional qualifications, placement prospects, OCE job code, and cohort number. Model (2) adds controls for the timing of the job entry, which 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. The base category in model (2) is being placed in the first three months of the experiment. Robust standard errors in parentheses. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.
Source: Authors' own calculations.

assumption on the selection process), negative selection bounds (individuals in the Hestia group are more likely to experience a decrease in work income conditional on treatment assignment), and positive selection bounds (the opposite). The negative and positive selection bounds follow the monotone treatment response assumption of Manski and Pepper (2000). Imbens and Manski (2004) confidence intervals are provided.

The worst-case bound is almost perfectly centered around zero, which is in line with the results of the OLS regressions showing no effect of the treatment. The negative selection bounds suggest a positive impact of JSA on the probability of finding a better paid job. However these bounds correspond to the perhaps extreme situation where Hestia's assistance is assumed to focus on hard-to-place job seekers while the control group focused on job seekers with above average prospects. The negative selection bounds include 0, and also refer to another extreme scenario. With limited resources and a cap on the number of job seekers enrolled, Hestia had incentives to focus on individuals for whom its assistance would increase placement the most. As hard-to-place job seekers require a lot of efforts and easy-to-place ones can probably take care of themselves, individuals in between would be a good target. This probably suggests that the true effect of JSA on work income is somewhat close to the worst-case bounds, namely zero.

JSA does not appear to affect earnings. We turn to a small survey on job conditions, conducted among a subset of job seekers who had found a job about one year after the pilot started to com-

Table 8: Bounds on the probability of finding a better paid job

Type of bounds	Lower bound	Upper bound
Worst-case selection	-0.489	0.511
Negative selection	0.038	0.511
Positive selection	-0.489	0.038

Notes: Table 8 reports bounds on the probability of earning more in the new job than in the last one. Worst-case bounds make no assumption on the selection process, negative selection bounds assume that individuals in the Hestia group are more likely to experience a decrease in work income conditional on treatment assignment, while positive selection bounds assume the opposite.
Source: Authors' own calculations.

plement.[20] The survey provides information on whether job seeker have a fixed term, or interim contract, or a permanent contract. About 72 percent of job seekers in the control group hold a permanent contract, while 78 percent of job seekers in the treated group do so, the difference is not statistically significant (p-value chi2 = 0.37). This evidence suggests that differences in contract duration unlikely explain the waiting and leaving pattern in job loss.

The survey also provides information on the level in the new firm hierarchy. The most common positions for job seekers in our sample are those of a trained person, or of a person that is hired to support other trained personnel, other levels include apprentice, or manager. About 63 percent of job seekers in the control group work as trained specialist, and somewhat less, 56 percent, work as specialists in the group assigned to JSA (p-value chi2 = 0.39). The survey then considers full vs part-time employment. About 88 percent of the job seekers in the control group hold a full time job, while only 77 percent of the job seekers in the treated group do, and this differences is just marginally not significant (p-Value Chi2 = 0.104). These two pieces of evidence, while not statistically conclusive, suggest that JSA might have lowered job quality and reduced employment stability.

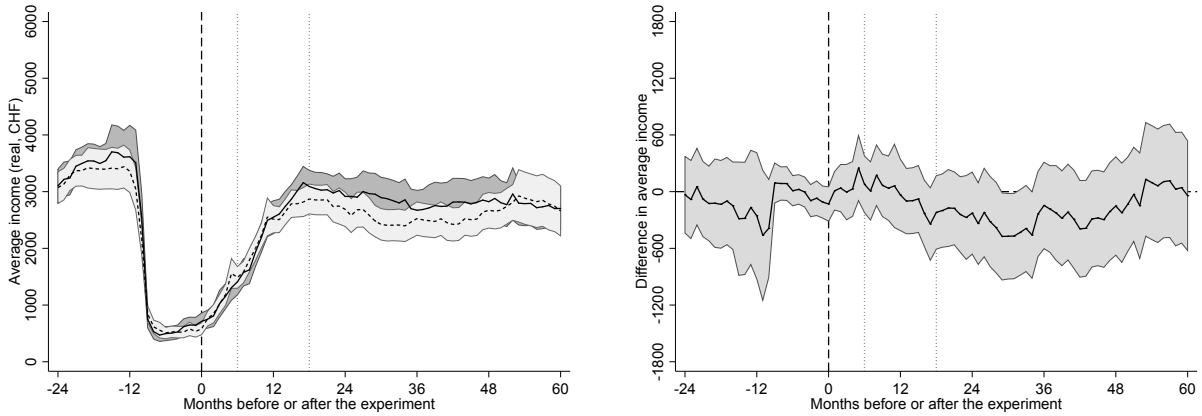
We present a last piece of evidence on job quality by documenting effects on monthly earnings. Earnings are observed for all individuals, regardless of whether they hold a job or not, so problems with dynamic selection are less important.[21] Figure 4 shows average earnings. Earnings fall due to job losses one year before the experiment. As the job placement programs start, the income level

[20] A total of about 160 job seekers of the 890 job seekers participated in the survey, with response rates roughly similar across the Hestia and control groups.

[21] We set earnings to zero for individuals who do not hold a job.

start increasing again but never reach its ex-ante level. It remains slightly lower even five years after the experiment. Looking at differences in income from work, we see that Hestia's job seekers have somewhat, but not significantly, higher earnings around 6 months after Hestia started, when the employment effects of Hestia are strongest (Figure 1). Thereafter, Hesta job seekers' earnings decline and are even significantly lower than the control group's earnings between two and three years after the experiment. The negative earnings effects are sizeable, around 10 percent of a monthly, significantly different from zero, and in the same order of magnitude as the employment effects of Hestia. The key reason for this decrease is that Hestia placed job seekers into less stable jobs (Table 3).

Figure 4: Effects on Earnings



(a) Earnings

(b) Earnings (difference)

Notes: Figure 4 plots average earnings, and the difference between the two groups, defined as treatment minus control. The two vertical dotted lines represent the changes in the payment scheme to Hestia after 6 and 18 months.

This pattern of employment effects suggests that Hestia placed job seekers into jobs faster, but those jobs did not offer the same earnings opportunities than the jobs held by the control group.

4.5 What is Required for Sinusoidal Employment Effects?

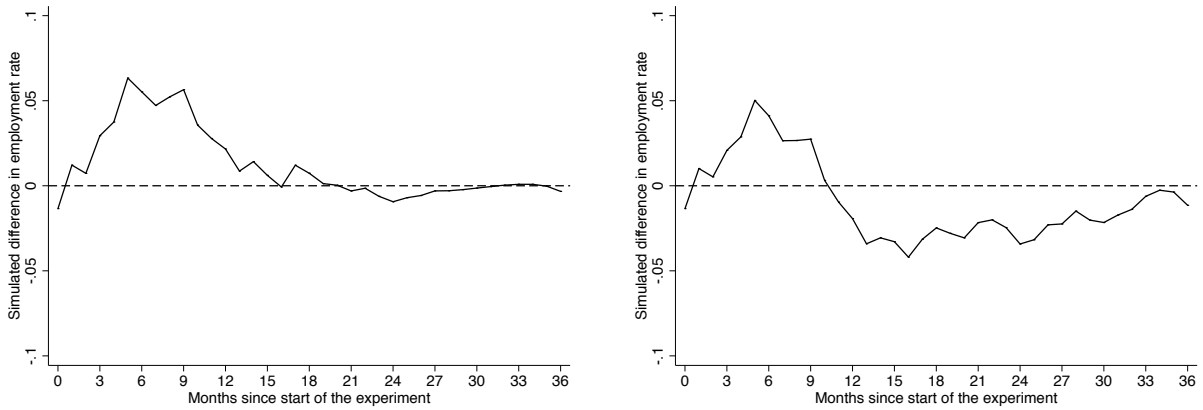
We now take our employment loss estimates to see whether they can account for the sinusoidal pattern in the employment effect of JSA. It is not obvious if the control group simply is catching up with the Hestia group in the long run, or if it is differences in job exits that create this pattern. Moreover, we have presented evidence that employment stability was affected but perhaps the effects are fairly small.

To disentangle the two effects, we provide a simulation based on the survivor functions provided in subsections 4.2 and 4.3. The goal is to simulate the employment rate of the two groups using transition dynamics. In the catch-up scenario, individuals from each group find jobs at a rate specific to their own group but lose them at the rate of the control group. In the employment-loss scenario, individuals from both groups find jobs and lose them at rates specific to their own group. The catch-up scenario allows us to compute employment patterns while only taking into account Hestia's effects on job finding rate, while the employment-loss scenario takes both Hestia's effect on employment exit and job finding into account, and shows whether the employment loss effects of JSA are large enough to drive the employment rate.

Specifically, we first estimate the survivor functions in unemployment and employment for each group. From the survivor function in unemployment, we calculate $JobEntryRate_i = 1 - SurvivorUnempl_i$ for $i \in \{Control, Hestia\}$. In the first scenario, the employment rates are then given by $Empl_i = JobEntryRate_i \times SurvivorEmpl_{Control}$ (i.e. both groups find jobs are their respective rate but keep them at the rate of the control group). In the second scenario, the employment rates are simply given by $Empl_i = JobEntryRate_i \times SurvivorEmpl_i$.[22] Finally, we compute the difference between the employment rates of the two groups, defined as $\Delta_{empl} = Empl_{Hestia} - Empl_{Control}$. Figure 5 plots the difference under each scenario.

[22] See Figure A4 in the appendix for the simulated employment rates under each scenario.

Figure 5: Simulated employment levels



(a) Catch-up scenario

(b) Employment-loss scenario

Notes: Figure 5 plots the difference in simulated employment rates between the group assigned to Hestia, and the control group. On the left, individuals from each group find jobs at a rate specific to their own group but lose them at the rate of the control group. On the right, individuals from both groups find jobs and lose them at rates specific to their own group.
Source: Authors' own calculations.

We observe a clear difference between the two figures. In the catch-up scenario (a), where only differences in job finding rate are allowed, the simulated employment rates show a positive difference in the short run, which disappears after one years, and remains around zero afterwards. This does not fit with the actual data. In the employment-loss scenario (b), which allows for JSA to affect both job finding and employment loss, difference in employment exit rates and job finding rates, our simulations suggest positive effects in the short run, followed by negative effects after one year, and finally convergence around three years. The employment-loss scenario reproduces the sinusoidal pattern observed previously. Hestia's JSA program affected employment both through job entry and employment exit, and the employment exit effects are sizeable.

5 Conclusion

We study the long-term effects of a job search assistance program designed by Hestia, a job placement firm in Geneva, on employment. 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 covers 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, JSA significantly improves treated job seekers re-entry into the labor force, with a difference of around four percentage points compared to the control group. In the medium-run though, these positive effects vanish and both groups have a similar performance until approximately two years after the experiment. Then, the patterns revert. Treated job seekers are less likely to be employed than their control group counterparts. The difference is significant up to three years after the experiment and finally disappears when we look at a longer horizon. This sinusoidal pattern is due to JSA placing job seekers into employment faster but, once employed, job seekers placed through JSA also lose employment faster.

JSA reduced employment stability probably by lowering job quality. Job seekers assigned to JSA wait until the end of their first year of employment, and leave it exactly once they qualify again for unemployment benefits. This waiting-and-leaving pattern is not due to contract length, but due to JSA placing job seekers in marginally attractive jobs which are better than unemployment without benefits, but worse than unemployment with benefits. Indeed, the incidence of part-time jobs is higher among job seekers assigned to JSA, and fails the 10% significance level only by a hair's breadth. Hestia appears to have assigned lower quality vacancies to job seekers than job seekers would have looked for by themselves.

JSA programs can place job seekers fast but at the expense of employment stability. Evaluations of JSA programs should assess whether job quality is affected and adopt a long time horizon in order to see whether employment stability is affected. Information on job quality alone may not be powerful enough to detect deterioration of quality. Long-run information on employment shows whether JSA affects employment stability.

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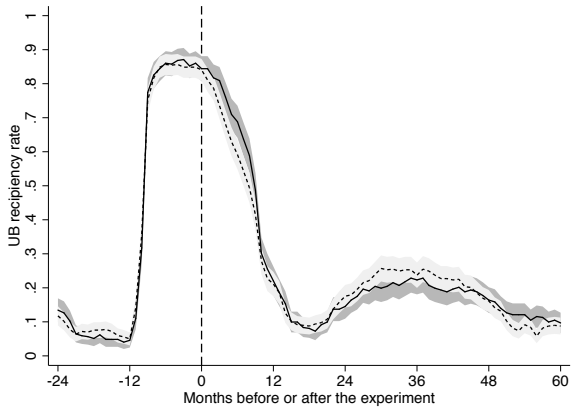
A Appendix

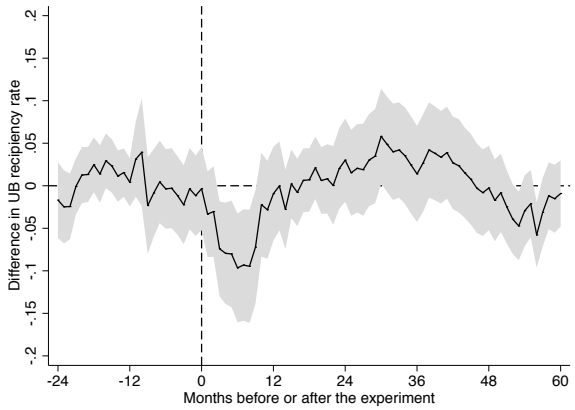
Figure A1: Labor market states over time

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 the sample for a given month.
Source: Authors' own calculations.

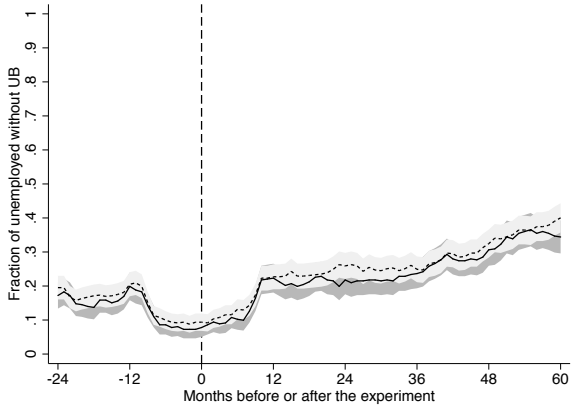
Figure A2: Long-term effects on unemployment



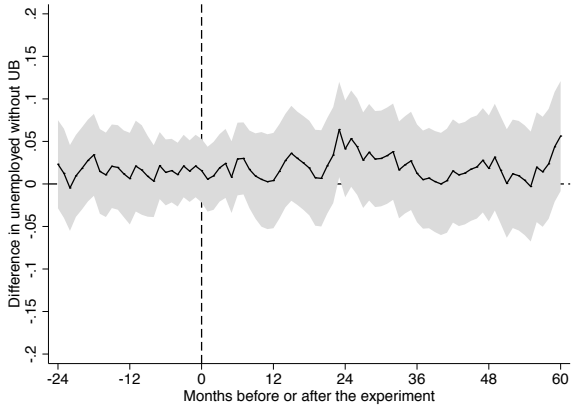
(a) UB recipients



(b) UB recipients (difference)



(c) Unemployed without UB

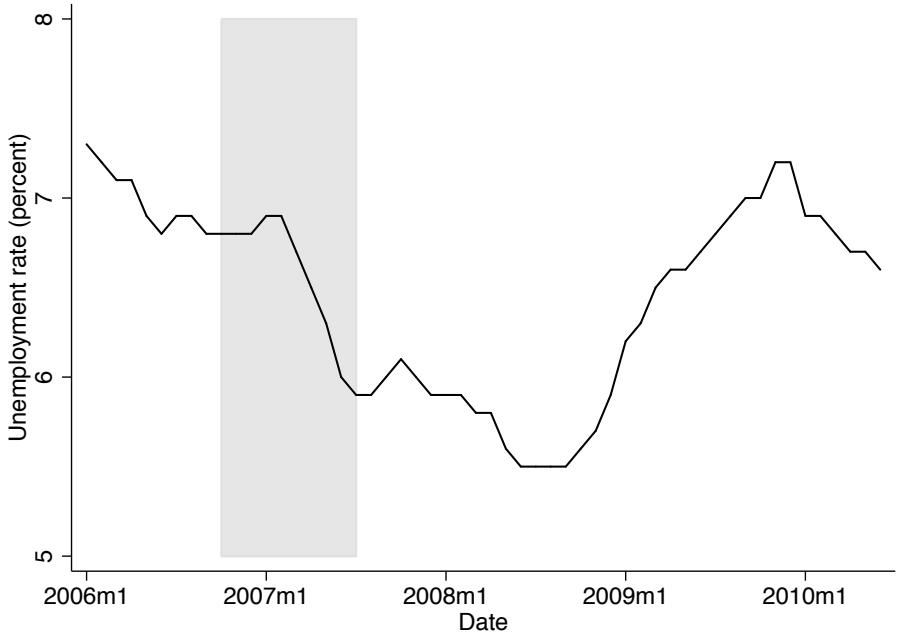


(d) Unemployed without UB (difference)

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

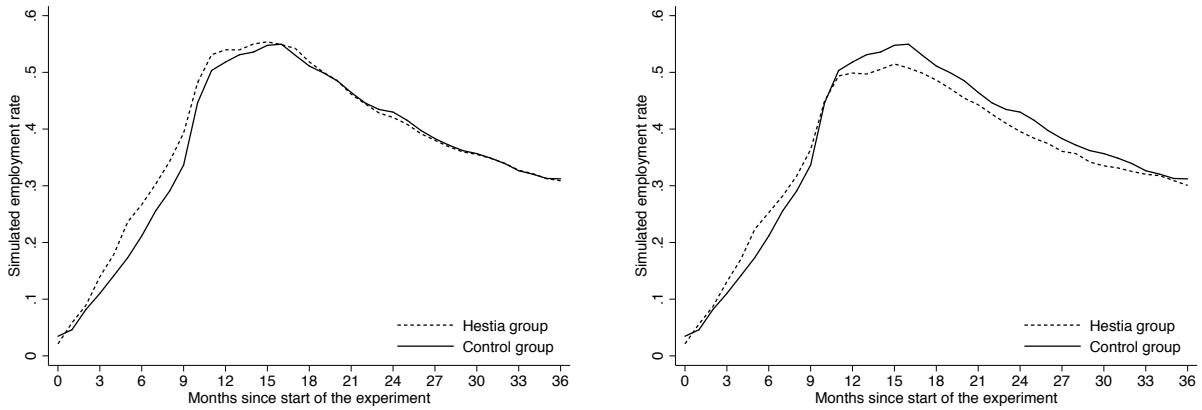
Source: Authors' own calculations.

Figure A3: Unemployment in the Canton of Geneva



Notes: Figure A3 shows the unemployment rate in the Canton of Geneva between January 2006 and December 2010. The shared area indicates the period during which the experiment took place – first cohort in October 2006, last cohort in July 2007.
Source: Authors' own calculations with data from the Swiss Federal Statistical Office.

Figure A4: Simulated employment levels



(a) Catch-up scenario

(b) Employment-loss scenario

Notes: Figure A4 plots the simulated employment rates between the group assigned to Hestia, and the control group. On the left, individuals from each group find jobs at a rate specific to their own group but lose them at the rate of the control group. On the right, individuals from both groups find jobs and lose them at rates specific to their own group.
Source: Authors' own calculations.

Table A1: Small randomization differences after accounting for non-participation

	Hestia Group						Difference between groups					
	Control Group (1)		Treated Group (2)		Non-treated Group (3)							
Variable	Mean	s.e.	Mean	s.e.	Mean	s.e.	(1)-(2)	t-stat	(1)-(3)	t-stat	(2)-(3)	t-stat
Women	0.51	0.03	0.50	0.03	0.48	0.03	0.01	0.23	0.03	0.62	0.02	0.36
Marital status												
Single	0.35	0.02	0.31	0.03	0.36	0.03	0.04	1.10	-0.01	-0.20	-0.05	-1.19
Married	0.51	0.03	0.55	0.03	0.52	0.03	-0.04	-1.05	-0.01	-0.29	0.03	0.68
Widower	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.27	0.00	0.24	-0.00	-0.02
Divorced	0.14	0.02	0.14	0.02	0.12	0.02	-0.00	-0.03	0.02	0.68	0.02	0.66
Experience												
None	0.01	0.01	0.01	0.01	0.04	0.01	0.00	0.19	-0.03	-1.94	-0.03	-2.01
Less than 1 year	0.07	0.01	0.07	0.02	0.10	0.02	0.00	0.11	-0.03	-1.36	-0.03	-1.37
1-3 years	0.25	0.02	0.24	0.03	0.28	0.03	0.02	0.45	-0.03	-0.77	-0.04	-1.11
More than 3 years	0.56	0.03	0.56	0.03	0.46	0.03	0.00	0.01	0.10	2.51	0.10	2.30
Age												
17-24	0.08	0.01	0.10	0.02	0.11	0.02	-0.02	-0.77	-0.03	-1.04	-0.01	-0.26
25-34	0.28	0.02	0.32	0.03	0.31	0.03	-0.04	-1.09	-0.03	-0.93	0.01	0.14
35-44	0.28	0.02	0.22	0.03	0.32	0.03	0.06	1.62	-0.05	-1.24	-0.10	-2.61
45-54	0.21	0.02	0.20	0.02	0.18	0.02	0.02	0.56	0.03	0.98	0.01	0.39
55-64	0.15	0.02	0.17	0.02	0.08	0.02	-0.02	-0.53	0.07	2.94	0.09	3.11
Schooling												
Compulsory	0.40	0.03	0.45	0.03	0.40	0.03	-0.04	-1.10	-0.00	-0.07	0.04	0.95
High-school level	0.36	0.02	0.33	0.03	0.39	0.03	0.03	0.76	-0.03	-0.74	-0.06	-1.37
University level	0.20	0.02	0.18	0.02	0.18	0.02	0.01	0.44	0.02	0.50	0.00	0.06
Workers												
Swiss	0.52	0.03	0.51	0.03	0.49	0.03	0.01	0.33	0.03	0.81	0.02	0.44
C permit	0.30	0.02	0.31	0.03	0.27	0.03	-0.01	-0.27	0.03	0.76	0.04	0.94
Other	0.18	0.02	0.18	0.02	0.24	0.03	-0.00	-0.11	-0.06	-1.83	-0.06	-1.59
Placement prospects												
Excellent	0.04	0.01	0.08	0.02	0.04	0.01	-0.04	-1.79	0.00	0.08	0.04	1.74
Good	0.55	0.03	0.53	0.03	0.52	0.03	0.02	0.39	0.03	0.65	0.01	0.24
Average	0.23	0.02	0.20	0.02	0.22	0.03	0.03	1.04	0.01	0.23	-0.03	-0.72
Poor	0.17	0.02	0.19	0.02	0.21	0.03	-0.01	-0.44	-0.04	-1.11	-0.02	-0.62
Number of observations	378	-	260	-	252	-	-	-	-	-	-	-

Notes: Table A1 presents a breakdown of the summary statistics on the variables specific to the experiment. Some categories may not add up to one due to missing observations. Columns seven to twelve calculate the difference between control and Hestia groups, defined as control minus Hestia and report two-sided t-statistics on the differences.

Source: Authors' own calculations.