Are job search assistance programmes always zero-sum games ?[*]

Philippe Zamora
Helene Naegele
Thomas Le Barbanchon
Roland Rathelot
Bruno Crépon[†]

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Abstract

This paper presents the results of a randomized controlled trial evaluating the impact of a job-search assistance programme designed for disadvantaged young people seeking to enter apprenticeship. We show that job-search assistance has substantial effects: the probability to sign an apprenticeship contract increases by around 20%. We also find that the programme had no visible effect on search effort but improves the returns to a given application. Most of the effect is driven by substituting applications through formal channels with applications through informal channels. Finally, we provide a new test of the hypothesis that the improvement of treated individuals' employment rate only came at the expense of the untreated. Preliminary results suggest that we can reject this hypothesis.

Keywords: apprenticeship, informal search channels, social networks, job-search assistance, high school dropouts, randomized controlled trial.
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[†]Zamora: French Ministry of Labor; Naegele: DIW Berlin; Le Barbanchon: Bocconi University and CEPR; Rathelot: University of Warwick and CEPR; Crépon: CREST.

1 Introduction

Job-search assistance programmes are abundantly used to help job seekers find a job. Many studies have documented that this kind of programmes had positive effects on labour-market outcomes and earnings in the short run (see Card et al. (2010); Rosholm (2014); Crépon and van den Berg (2016); Card et al. (2017) for recent reviews). The literature disagrees on whether these programmes generate effects in the long run[1] and how large the general-equilibrium they produce are. For this reason, it is important to open the black box of job-search assistance and understand how they can produce effects.

In this paper, we investigate the impact of a job-search assistance programme that aims at helping young people to find an employer for an apprenticeship contract. The programme is assigned randomly across eligible young people that expressed an interest in entering apprenticeship. First, we find that the programme increases the probability to sign a contract from 45% to 50% and in smaller cities from 53% to 62%. Second, using a survey that we specifically design for this experiment, we investigate how such results were obtained. We find that the programme does not increase the search effort but improves the returns to these efforts. The number of interviews by application increases sharply. We also find that the effect is driven by a substitution of search effort from formal to informal channels, rather than an effect of the traditional activation tools (help in drafting CV, preparing youth for interviews).

Whether using informal channels improves matching outcomes is an open question is the literature. While restricting the search in one's social network may limit the set of possibilities and harms both the job finding rate and the post-employment outcomes (Bentolila et al., 2010), social networks can be used to reduce uncertainty and to communicate more and better-quality information (Montgomery, 1991; Calvo-Armengol and Jackson, 2004, 2007).[2] Van den Berg and

[1]There is less research about the long-run effects. Maibom et al. (2017); Manoli et al. (2018) find positive effects 5 years after programme assignment, while Cottier et al. (2017) find no effect.

[2]A large literature highlights the important of informal channels(Pellizzari, 2010; Dustmann et al., 2011; Cingano and Rosolia, 2012; Kramarz and Skans, 2014; Hensvik and Skans, 2014) and their relative efficiency compared to formal ones (Addison and Portugal, 2002; Eppel et al., 2014; Brown et al., 2014).

Van der Klaauw (2006) present the results of a counseling and monitoring intervention that causes a shift from informal to formal job search. They find that this programme has no effect on the job finding rate. Bonoli et al. (2014) present a randomized experiment where Swiss job-seekers are told that informal job-search is very efficient. Receiving the information increases the employment rate of female job-seekers, which is in line with our results.

Understanding which mechanisms are at play is crucial for at least two reasons. First, while job-search assistance programmes are flourishing in most countries, there is a wide heterogeneity of implementation practices, which might relate to differences in effectiveness (Crépon and van den Berg, 2016).[3] Understanding why some strategies obtain positive effects is key to guide policy-makers and help providers fine-tune their programmes. Second, we argue that different mechanisms might lead, under some assumptions, to different general-equilibrium outcomes. Intuitively, programmes playing on search efficiency or search cost will generate displacement effects (Cahuc and Le Barbanchon, 2010; Crépon et al., 2013; Gautier et al., 2018), while programmes playing on the cost to post vacancies could be less sensitive to this issue.

We also provide a new test of the hypothesis that the programme only changed the allocation of available jobs from non-beneficiaries to beneficiaries. If the treatment only affects which individual gets which job (for instance, increase the success of treated ones at the expense of others), the treatment should not affect the number of jobs that are located close to a given individual. Because of random assignment, we should also find that the total number of jobs around people assigned to treatment and control should be identical. We find that individuals assigned to treatment have more jobs around them than individuals assigned to control (whoever occupies these jobs). Under some assumptions, we relate the difference in the density of jobs around treatment and control individuals to the number of jobs that are created because of the existence of the programme. Our preliminary results suggest that we can reject the hypothesis that the positive impact of the treatment is purely due to a redistribution of jobs across participants.

[3] Behaghel et al. (2014) study the heterogeneity of providers themselves (private vs. public providers). Maibom et al. (2017) examine the impact of collective vs. individual counseling. van den Berg et al. (2016) look at the impact of “search clubs”.

Section 2 details the institutional context and how the experiment is set up. Section 3 presents the data sources and summary statistics of the main variables. Section 4 shows the main empirical results. Section 5 presents the test of the rat-race hypothesis.

2 Institutional context

2.1 Apprenticeship in France

Apprenticeship is a form of vocational education, combining classroom education (both on general and occupation-specific skills) and on-the-job training, and targeting young people aged 16 to 25. This paper focuses on apprenticeship at the secondary level. Apprentices are trained to work in occupations such as waiters, hairdressers, bakers, craftsmen, construction workers. In a typical timetable, apprentices spend the first three weeks of the month at the workplace and the last week of the month in classrooms at an Apprentice Training Center (*Centre de Formation des Apprentis-CFA*), which looks very much like a high school.

Apprentices and training firms sign fixed-term employment contracts with specific conditions.[4] The contract has an extended trial period of two months, during which parties can leave the contract without penalties. After the first two months, standard labor law applies. The law defines specific thresholds for the minimum wages of apprentices, which depend on their age and degree advancement. Almost all apprentices, at the secondary level, earn these minimal levels. For example, 16/17 years-old apprentices earn 25% of the minimum wage during their first year of training, yielding a monthly net wage of 264 euros in 2010.[5] Firms must let apprentices attend classes. Apart from these specific conditions, apprentices are considered as standard employees of the firm. Firms employing apprentices benefit from generous tax credits and receive a bonus of 1,000 euros

[4] Candidates must be at least 15 years old on the 31st of December to be eligible to this specific contract.

[5] This fraction increases to 37% during their second year.

per year when they hire an apprentice.[6] No social contributions are due.
Thus the total monthly cost of labor for the firm of a 16/17 year-old apprentice is close to 264 euros.

Two years are necessary to obtain a secondary apprenticeship degree.[7] The degree is awarded by the training center. On top of attendance and positive evaluation from the firm, apprentices must take final exams (at the end of each training year), assessing both technical and general skills.

2.2 The programme: Counseling aspiring apprentices

Getting into apprenticeship is not an easy task as it involves both finding a training firm and enrolling in a training center that matches the occupation of the firm. In their vast majority, aspiring apprentices are finishing junior high school and have little prospects to enter a general-education senior high school, so that they can be considered as dropouts of the general-education system. The alternative for them lies between: starting an apprenticeship, entering a (classroom-only) technical or vocational high school, or entering the labor market. When they first signal their interest for apprenticeship, most of them have no relevant work experience and no definite career plan.

The programme under study aims to provide aspiring apprentices with counseling to help them find a training firm. The programme has been designed and is implemented by local structures, specialized in youth counseling (*Mission locales*). A first pilot of the programme has been implemented, since 2008, in Corrèze (one of the 100 French *départements*). In 2010, the national board of *Mission locales* decided to scale up the programme in and to test its impact using a large randomized control trial.

[6]Since January 2014, this bonus is restricted to small firms-less than 10 employees. But, at the time of the RCT, there was no restriction.

[7]These degrees are referred to as *Certificat d'Aptitude Professionnel-CAP* and *Brevet d'Etudes Professionnelles-BEP*. After these first two years, apprentices may further train two extra years to obtain technical degrees at the upper secondary level such as *Brevet Professionnel* or *Bac Pro*.

The programme mostly consists in meetings with a caseworker at the *Mission locale*. Caseworkers discuss the career plan with the young aspiring apprentices (and potentially with their parents). They deliver information about the different local training centers and job vacancies in training firms. If needed, young people can further enroll in job search programmes with CV and job interviews workshops. *Mission locales* can also provide financial help to cover commuting costs between the training firms and the parents' home or relocation costs. In case of strong hurdles to job search (very weak financial position, depression...), caseworkers can propose more intensive programmes (*CIVIS*), involving more frequent meetings and a monthly benefit. While these services are delivered by *Mission locales* irrespective of the program, the innovation is in targeting a new audience: the young people aspiring to apprenticeship. Caseworkers in *Mission locales* need to look for vacancies in training firms, which was not the standard practice and to coordinate their services towards this final objective.

Once young people start their apprenticeship, they can remain in contact with their caseworker, the services of whom remain available. This may prove useful if financial or relational difficulties arise during the contract. However because of budgetary reasons, the national board of *Mission locales* decided that this on-the-job counseling aspect should not be emphasized and caseworkers are not proactive once the participants have signed their first contract.

2.3 The experiment

The experiment takes place in 2010 and 2011. The programme is tested on two cohorts of young people who wish to enroll in a training programme starting respectively in September 2010 and in September 2011. The programme is implemented in seven large French cities and their direct surroundings agglomerations[8] (out of 100). The head social agency, usually located in the main city of each département, coordinates the implementation of the programme in the

[8] Le Mans (Sarthe), Orléans (Loiret), Blois (Loir-et-cher), Tours (Indre-et-Loire), Limoges (Haute-Vienne), Bourg-en-Bresse (Ain), Roanne (Loire)

surrounding areas.[9]

The design of the experiment is as follows:

1. **Identifying potential participants**: Starting in April of year N, all the different institutions in contact with aspiring apprentices (general secondary schools, *chambre de commerce et d'artisanat*...) direct the young people to the training center, where they are registered as aspiring apprentices on an extranet. They also receive broad informed on how to become an apprentice. This first step ends in the summer of year N, as the training starts in September of each year.

2. **Randomization and assignment to treatment**: Immediately after the extranet registration, young people are randomized into test or control groups. The probability to be assigned to treatment is 1/2, except in one city where it amounts to 0.7. Contact details of the young people in the test group are then sent to the local *Missions Locale* in charge of administering the treatment.

3. **Test group invited to participate**: In the weeks following the extranet registration, caseworkers of *Missions Locales* invite young people to meet at the local social agency. As detailed above, they discuss career plans and job search strategies. They propose to enroll in the social agency to benefit from further services. These meetings take place between May and August, soon after web registration and assignment to treatment, and constitute the starting point of the programme.

The experiment follows an encouragement design. Young people who are randomized in the test group receive mails and phone calls from the Mission Locale to register and benefit from the programme. However, both cases of noncompliance occur. First, not all individuals of the test group show up to the first meeting and we can reasonably consider those as non-treated. Second, some of the control find their way to the Mission Locale and benefit from services that are comparable to the treatment.

[9] In Roanne, the programme was implemented coordinated by *Chambre des métiers et de l'artisanat*.

Through the first step of the experiment, 2,720 young people are identified as aspiring apprentices (1,416 in 2010 and 1,304 in 2011). Over both cohorts, 1,485 young people are randomized in the test group and 1,235 young people are randomized out and constitutes the control group.[10]

3 Data

3.1 Data sources

The data that we use in this study come from several sources.

Baseline data: The baseline data is collected when young people register to enter the experiment. In order to enter the experimental sample, they have to be in contact with a caseworker working for one of the implementing partners. This caseworker fills in an online questionnaire with the individual's contacts, gender, birth date, education, parents' socioeconomic background, occupation targeted for apprenticeship, and whether he has already contacted potential employers or even found an employer. Once the questionnaire is filled in, the data are sent to the researchers' extranet server, randomization into treated or control group is immediately performed and the caseworker informed about the candidate's status in the experiment.

Survey: the first source for end-line data is collected through a survey performed in two waves. The first wave takes place in April of the year following assignment to treatment, roughly 11 months after the intervention. The second wave takes place in November two years after assignment, 30 months after the intervention and, in typical cases, a few months after graduation. Only the results of the first wave are used in the present paper.

These surveys are conducted by phone. The questionnaire is voluntarily short in order to maximize the response rate to the survey. Most importantly, individuals are asked about their current professional and educational status, with an emphasis on apprenticeship, as well as about how they sought and found a contract (if

[10]The empirical probability to be assigned to treatment is thus 54.6%.

they did). These variables are going to be our main outcomes.

Contacts with *missions locales*: In order to get a better idea of the job-counseling process, we have had access to an administrative dataset of the implementing partners. Each time an individual has a contact with a caseworker, the caseworker has to keep track of this contact in the information system of the organization. Aside from the time stamp, the nature of the contact (face-to-face meeting, phone, workshop...) and whether the caseworker or the beneficiary initiated the contact are available in this dataset. The interesting feature of this dataset is that it can be matched for both treated- and control-group individuals. As our experiment design is an encouragement design and control-group individuals were by no means forbidden to be in touch with caseworkers, we have to check whether treatment rates are actually higher in the group that was assigned to treatment. This dataset will also allow us to qualify the nature of the interactions between beneficiaries and their caseworkers.

Administrative file of all apprenticeship contracts: Each year, between 270 000 and 300 000 contracts are signed and registered by consular chambers (existing in each department for three different groups of firms :Agriculture, Trade and manufacturing sectors, Crafts). Files are then sent to the statistical service of the Labor Ministry (DARES) and used for statistics and research purposes. Characteristics of the apprentice, of the firms, of the training and the aimed certifications are precisely known. The files of 2011 and 2012 have been matched to the other datasets.

Surveys and administrative data on contracts are complementary. We mainly use the latter, as they are not subject to attrition and response bias, especially to compute the main outcomes of the programme. But the first wave of the survey are much useful to analyze the search and matching process, at stake in this paper.

Table 1: Treatment take-up and intensity

	Control	Treatment	Equality (p-value)	Number of observations
Any meeting with caseworker	28.8	80.1	0.000	2720
Any individual meeting	27.4	77.9	0.000	2720
(unconditional)				
Number of meetings	5.4	15	0.000	2720
Number of meetings on ML's initiative	1.5	4.5	0.000	2720
Number of individual meetings	2.3	5.4	0.000	2720
(conditional on at least one meeting)				
Number of meeting	18.8	18.7	0.9013	1545
Number of meetings on ML's initiative	5	5.6	0.1387	1545
(conditional on at least one individual meeting)				
Number of individual meetings	8.1	6.7	0.000	1495

3.2 Descriptive statistics

The experimental programme randomly selected participants into the treatment group which benefited from an intense and pro-active follow-up by the Mission locales. However, all apprenticeship candidates (including the control group) were free to contact the social services at their own initiative, just like before the beginning of the experiment. The data in table 1 shows that the treatment intensities nevertheless differ greatly between the treatment and control group. Overall, 29% of the control group and 80% of the treatment group had at least one contact with the Mission locale in the relevant time period. This difference varies across sites, but is significant for all of them. The identification of the social services' impact as well as the statistical power of our results crucially depend on this difference.

The first survey after about 6 months into the programme had a response rate of 67% and the second survey after two years had a response rate of 58%. Response rates to both surveys were virtually identical among both groups (table 2). The data from these surveys shows that the randomization worked well: none of the measured variables shows significant differences between treatment and control

Table 2: Response rate for both surveys

	Total	Control	Treatment	Equality (p-value)
Response rate survey 1	67.2%	67.4%	67.1%	0.90
Response rate survey 2	58.6%	58.8%	58.4%	0.83

Note: Out of 2,720 young people participating to the experiment, 1,829 and 1,593 answered resp. to the first and second survey.

groups (table 3).

The targeted sample is between 15 and 25 years old. Table 3 shows that about two thirds of the participants are at the lower end of this range, aged between 15 and 17 years. The large majority of the candidates aims for lower vocational certificates, i.e. certificates for which no previous high school degree is needed (after junior high school). Only 10% prepare for certificates of secondary (vocational senior high school) and 0.8% for post-secondary education level. The most common disciplines chosen are in the food processing and building sector.

The social background of the participants is rather modest: about a quarter of their parents have no secondary school certificate and another quarter finished their education with a vocational secondary school degree. Note however that about a third of the participants do not know their parents" education level.

Interestingly, we also have information on the participant's school performance in Mathematics and French during the last year of schooling preceding their apprenticeship application, i.e. their entry into our experimental programme. This information is self-assessed by the participants, so that one might worry about a desirability bias. However, grades appear reasonably well distributed across the whole range of possible grades: about a third of the respondents consider their grades below average and a bit more than a third above average.

The survey data shows that sectors are not equally accessible for candidates. While the large majority of participants (90%) contact at least one company to apply for an apprenticeship contract, their success rate depends strongly on the

Table 3: Descriptive statistics and balancing tests: caseworker online questionnaire (unit: percent)

	Control	Treatment	Equality (p-value)
Men	65.3	66.5	0.515
Age on Dec 31			0.136
15 and under	10.6	9.4	
16	36.9	35.6	
17	16	18.5	
18	14	11.4	
19 and over	22.4	25.1	
Targeted diploma level			0.162
Lower secondary	89.3	87.3	
Upper secondary	9.9	11.2	
Tertiary	0.8	1.4	
Sector			0.600
Building sector	27.2	27.1	
Food Industry	25.0	22.6	
Hairdressing/beautician	10.0	10.1	
Mechanics	11.1	11.5	
Hospitality	13.9	13.7	
Sales and accounting	8.9	10.2	
Other	3.9	4.9	

N=2720 (Source: Caseworker online questionnaire)

Table 4: Descriptive statistics and balancing tests: Participant survey (unit: percent)

	Control	Treatment	Equality (p-value)
Mathematics level			0.728
Above average	35.0	35.2	
Average	23.6	22.2	
Below average	39.2	39.6	
N/A	2.3	3.0	
French level			0.558
Above average	38	37	
Average	28	26	
Below average	31	34	
N/A	3	3	
Father's highest degree			0.169
No diploma or lower secondary only	20	16	
Technical secondary degree	29	30	
General secondary degree	7	7	
College degree	4	4	
N/A	40	43	
Mother's highest degree			0.740
No diploma or lower secondary only	26	24	
Technical secondary degree	25	25	
General secondary degree	12	12	
College degree	5	7	
N/A	31	32	

N=1829 (Source: Participant survey)

discipline chosen. Two groups of sectors appear: on the one side, the “difficult” sectors building, auto mechanics, hairdressing/beautician, trade, accounting and administration (sector group 1) and on the other side the “easy” sectors cooking, butchery, bakery/pastry, hospitality and restaurant/bar service (sector group 2). In sectors of the more difficult group 1, about 80% of the candidates who contacted a company have an interview and between 50 and 60% (depending on the sector, see fig [etapes]) finally sign an apprenticeship contract. In the sectors of the more easy group 2, fig. [etapes] shows that each step seems more easy: 90% of the candidates who contacted a company have an interview and over 70% of them sign a contract. However, the apprentices in sectors of the “easy” group 2 have a slightly higher dropout rate (22% vs. 17% in sectors of group 1).

Not surprisingly, (self-assessed) school performance has a significant positive impact on the probability to entering into an apprenticeship contract.

The field experiment took place on seven different sites across France: Blois, Bourg-en-Bresse, Le Mans, Limoges, Orléans, Roanne and Tours. These sites differed both in how they conducted the experimental programme and their environment. The treatment intensity varied considerably across sites: while some sites ensured that *all* participants of the treatment group had at least one contact with the Mission locale (Orléans), some reached only about two thirds (Le Mans). More importantly, the difference in treatment intensity of the treatment and control group varies between 20 (Blois) and 70 (Orléans) percentage points. It is statistically significant for all sites nevertheless.

The likelihood to find an apprenticeship contract also depends strongly on the site: in some cities, only a third of the candidates find an apprenticeship (Blois, Limoges), while two thirds are successful in others (Roanne). This is not surprising, as macroeconomic conditions vary across different regions.

4 The impact of the programme on labour-market outcomes and search channels

4.1 The programme has a strong impact in smaller cities

Overall, the programme has a significant impact on the main outcomes: the probability to sign a contract and to still be enrolled in apprenticeship one year after treatment assignment (Table 5). These effects are confirmed by administrative as well as by survey data.

Table 5: Impact of treatment assignment

Source	Survey		Administrative data	
Outcome	Control mean	Treatment impact (1)	Control mean	Treatment impact (2)
Signed a contract	0.522	0.048**	0.451	0.050***
		(0.023)		(0.018)
Apprentice in March N+1	0.481	0.047**	0.374	0.038**
		(0.023)		(0.018)
Number of observations	1827		2718	
Among those who signed				
Drop out	0.111	0.005	0.172	0.006
		(0.021)	(0.038)	(0.021)
Number of observations	982		1256	

Source: Administrative data and follow-up survey

Assignment increases the probability to sign a contract by 5 percentage points (by a quite similar 4.8 percentage points according to survey data). The impact on the probability to be in an apprenticeship contract is slightly lower (+3.8 p.p) for administrative data, because of a rather high drop-out rate during the first months (17,2%). Survey data give a higher impact (+4.7 p.p), however not significantly different, probably because of a small response bias (current apprentices tend to answer more the survey).

Even if the two sources give quite different exit rates estimates (because of the same response bias), they converge on the impact estimation. The programme has no significant impact on the exit rates during the 6-9 first months period.

Results for impact are also reported for 3 subgroups worthy of attention : younger applicants (15 and 16 y. old), applicants from smaller cities and lastly those in "tight" sectors (among all potential occupations, accommodation, Food Industry, Building). Indeed, greater impacts for the programme may be expected for these subgroups. In particular, younger applicants might be favored in apprenticeship hiring process, because minor youth are less paid in their apprenticeship contract (in the first year of the contract, they earn 23% of the minimum wage vs 41% for a youth between 18 and 21 y.old) and are so less costly for the employer. Moreover, as they are less experimented, they might more take advantage from the programme.

Table 6: Impact of treatment assignment

Source	Survey		Administrative data	
	Control mean	Treatment impact	Control mean	Treatment impact
All	0.522	0.048**	0.451	0.050***
		(0.023)		(0.018)
N. obs	1827		2718	
Cities ¡ 10000 inh	0.605	0.033	0.529	0.088***
		(0.029)		(0.023)
N. obs.	1 090		1 506	
Youth younger than 17	0.661	0.068**	0.602	0.057**
		(0.032)		(0.027)
N. obs.	901		1 245	
Tight sectors	0.601	0.075***	0.537	0.052**
		(0.028)		(0.023)
N. obs.	1 159		1 746	

Source: Administrative data and follow-up survey

One interesting result emerging from the data is that, according to administrative data[11], the programme impact is much greater in small cities than in large ones. Hence, it reaches between 8.8 p.p and 10.6 p.p in cities with less than 4 000 inhabitants and is non significant in cities with more than 10 000 inhabitants (see Table **??** - Annex).This phenomenon is difficult to interpret at this stage but will be enlightened later in the paper.

4.2 The programme activates the use of informal search channels

How does the programme change the search and matching process on this market? Table 7 shows how the numbers of applications and interviews vary across groups (according to the youth themselves in the survey). The probability to send at least one application is very large, indicating that the population targeted by the experiment was the one of interest for the implementing partners: young people motivated to enter in apprenticeship and that have not yet found a contract.

Less than 9% of them declare having sent no application at all. The programme has no significant impact on this proportion. The average number of applications in the control group is around 12, and the programme has no impact on this number either. In a second step, in order to be recruited, applicants have to be granted an interview. The large majority (75%) of the control group has at least one interview. It does not evolve significantly either.

The only step of the process that significantly changes is the average number of interviews by application. It increases from .397 to .44 for the treatment-group. This finding suggests that the impact of the counseling programme is intensive rather than extensive : it does not increase the number of research actions but improves their unitary efficiency.

Table 8 shows for all youth applicant (and for selected subgroups) through which channel the applicant found her contract. Each interviewee (currently in apprenticeship) is asked by which channel she/he found her/him contract. 4 channels are hence described. In the control group, the most important channels are friends and relatives (between 18% and 24%) and spontaneous applications (be-

[11]Surprisingly, except for smaller cities (see Annex) survey data do not confirm these results

Table 7: Impact of treatment assignment on search outcomes

Outcome	Control mean	Treatment impact
Number of applications	11.667	-0.240
		(0.858)
At least one application	0.913	0.025*
		(0.013)
Number of job interviews	2.341	0.242
		(0.208)
At least one interview	0.752	0.020
		(0.020)
Number of interviews by application	0.397	0.042**
		(0.021)
Number of observations	832	1,827

Source: Follow-up survey

Table 8: Impact of treatment assignment on search channels

		Institutional help	Via friends and relatives	Via ads	Direct contact	In apprenticeship	*N*
All	Control mean	0.114	0.181	0.017	0.175	0.480	
	Impact est.	0.003	0.036**	-0.004	0.007	0.046**	1822
		(0.006)	(0.018)	(0.015)	(0.018)	(0.023)	
Tight sectors	Control mean	0.140	0.147	0.007	0.201	0.489	
	Impact est.	-0.008	0.068***	0.009	-0.003	0.062*	865
		(0.024)	(0.026)	(0.007)	(0.028)	(0.033)	
Youth younger than 17	Control mean	0.101	0.236	0.022	0.228	0.586	
	Impact est.	0.019	0.068**	-0.002	-0.014	0.060*	901
		(0.022)	(0.030)	(0.010)	(0.028)	(0.033)	

Source: Follow-up survey
Notes: 48% of youth applicants in the control group are in apprenticeship in March N+1. 11.4% of them found a contract thanks to the help of an official organization. The global impact of 4.6 p.p decomposes in a non significant impact of 0.3 p.p on the share of youth who found via an institutional channel

tween 17.5% and 23%) while institutional channels (jobs provided by job centers, training centers, or the implementing partners) are less important.

The treatment impact (on the probability to be apprentice in March N+1) is decomposed (Table 8) in each channel. Interestingly, the global treatment effect is mainly comes mainly from one channel : friends and relatives. Being assigned to the treatment hence increases the probability to find a contract through friends and relating by 3.6 percentage points, that explains almost all the global impact (+4.6 p.p).

In the table 8 are also reported the same statistics for the three subgroups previously chosen. The results for 2 of these 3 subgroups confirm the previous observations: the programme impact on being apprentice is 6.2 p.p (resp 6.0 p.p) and is almost only explained by the impact on informal search channel via friends and relatives (+6.8 p.p). The impacts on other channels including institutional one are negligible.

These results give an insight of the way the programme influences the search behavior of treated youth. The programme does note make increase the number of applications but improves their returns. It plays on the intensive margin of the search behavior (and less on the extensive margin). In theory, there are many mechanisms through which returns to effort can be improved, that we first categorize into two large families. First, caseworkers may improve the effectiveness of applications (advice about drafting a CV and about how to talk and behave during interviews). They may manage - as in Arni (2015) - to improve the self-esteem of unemployed or encourage them to claim to lower wages. Second, they may help the candidate with which channel to use to search the jobs.

The previous empirical evidence argues in favor of the second mechanism. If the main mechanism was the first one, it would be more likely to observe an increase in all channels through which young people find apprenticeship contracts. Quite remarkably, only the "friends and relatives" channel is affected by the treatment.

The positive impact of the programme would be due to caseworkers pushing the beneficiaries to use their informal networks as well as formal ones. One important point worth underlining is that our result is quite opposite to those of Cheung and al (2017). The efficiency of the counseling programme they study results from the increase of job vacations propositions form caseworkers, i.e from an activation of formal channels.

5 Testing the impact on job creation

We turn to the question of the general-equilibrium impact of the programme. How does the programme change the job finding rate of applicants (whether they are assigned to treatment or control) on the apprenticeship market? In this section, we derive a theoretical framework and develop an empirical strategy to estimate the programme effect on the overall number of filled jobs.

5.1 Testing strategy: the theoretical framework

We denote as V_0 the set of firms that matched with applicants participating to the experiment in the counterfactual world where the programme was not implemented. V_1 is the set of firms with filled jobs in the world with the programme. If the programme has no effect at all, or just reallocates jobs across participants, we have $V_0 = V_1$, and there is no net job creation *stricto sensu*. This also implies that there are as many jobs filled in each world: $\#\{V_0\} = \#\{V_1\}$, where $\#\{A\}$ denotes the number of elements of set A. As V_0 is not observable, it is not possible to directly compare it with V_1. To design our test, we assume that, when $V_1 \neq V_0$, we have either (i) $V_0 \subset V_1$ or (ii) $V_1 \subset V_0$. This is a restrictive but reasonable assumption. In case (i), the programme indeed creates extra jobs. Case (ii) is an extreme case where the programme destroys jobs.

Because of randomisation, the average distance between the residence of *control* group individuals and **all** filled jobs in V_0 should be equal to the distance of *treatment* group individuals to V_0. While V_0 is not observed, we can compute similar distances of control and treatment groups to **all** filled jobs in V_1. Intuitively, if $V_1 = V_0$, the distance of control group individuals to V_1 is equal to the distance of treatment group individuals to V_1. Conversely, if average distances are not equal, we can conclude that $V_1 \neq V_0$.

The innovation of our test is to compute distances to **all** jobs, and not only to the job filled by each applicant. Then our test is robust to reallocation of jobs across individuals in different treatment arms. Consider the case where the programme has no equilibrium effects but gives to each treatment individual the closest job to their residence. In this case, the average distance of treatment individuals to their jobs is lower than the similar average distance of control individuals, while the average distances to all jobs would be equal across treatment arms.

The power of our test relies on the fact that jobs created by the programme are not equally distributed across space. If on the contrary new jobs are equally distributed, then our global distance test would not be rejected, even if $V_0 \subset V_1$. While this must be acknowledged, it is also reasonable to expect new jobs to be unevenly distributed. For example, if treated individuals find jobs through informal networks, it may be expected that jobs are close to their residence. If caseworkers have a better knowledge about some local economic area, new jobs could be concentrated in this locality.

Formally, let's define ζ_i^λ, the number of V_1-jobs within a distance $\lambda \in \mathbb{R}_+^*$ of individual i :

$$\zeta_i^\lambda = \#\{j \in V_1 | d(R_i, L_j) \leq \lambda\}$$

where R_i is the residence location of the applicant i, L_j is the workplace for match j, $d(.,.)$ is the geographic distance between two points, and # denotes the number of elements of a set. ζ_i^λ is a measure of V_1-job proximity to individual i. We define $\zeta_T^\lambda = E[\zeta_i^\lambda | T_i = 1]$ as the average of ζ_i^λ on the subsample of individuals assigned to treatment, ζ_C^λ as the average over control individuals, and ζ^λ as the average over the population. We denote r_i^λ the counterfactual of ζ_i^λ, the number of V_0-jobs within a distance λ around individual i:

$$r_i^\lambda = \#\{j \in V_0 | d(R_i, L_j) \leq \lambda\}$$

r^λ is the average of r_i^λ. Note that the r_i^λ and r^λ are unobservable, because they are relating to the counterfactual distribution of jobs V_0.

Over a total of N individuals, N_C are assigned to the control group and N_T are assigned to the treatment group. Denote t as the treatment share, N_T/N. The employment rate in the control (resp. treatment) group is δ_C (resp. δ_T). The employment rate in the absence of the programme is δ_0. In the formal test below, we also take into account the M_E employed youth not taking part to the experiment.

Lemma 1. *Assume that: (i) $V_1 \subset V_0$, (ii) individuals assigned to treatment are better off* *in the presence of treatment than in its absence (i.e. $\delta_T > \delta_0$). Then:*

$$|\zeta_T^\lambda - \zeta_C^\lambda| \leq r^\lambda \frac{(\delta_T - \delta_C)N_T}{(N_T + N_C)\delta_C + M_E}$$

The proof is in the Appendix. If $V_0 = V_1$, the lemma is trivial as the left-hand side of the inequality is equal to zero. In this case, the counseling programme is a zero-sum game and no new jobs are created. The lemma shows that if there is no creation of new jobs, the quantity $|\zeta_T^\lambda - \zeta_C^\lambda|$ is bounded above.

r^λ is only defined in the counterfactual world. The next lemma provides a bound using observable (V_1) quantities. Denote $\gamma = \frac{(\delta_T - \delta_C)N_T}{(N_T + N_C)\delta_C + M_E}$ and ϕ_1^λ as the fraction of V_1-jobs such that there is at least one individual located within a distance of λ.

$$\phi_1^\lambda = \frac{\#\{j \in V_1 / \exists i \; d(R_i, L_j) \leq \lambda\}}{\# V_1}$$

.

Lemma 2. *Under the same assumptions as above, we have:*

$$|\zeta_T^\lambda - \zeta_C^\lambda| \leq \Psi_\lambda \doteq \frac{\zeta^\lambda \gamma}{\phi_1^\lambda - \gamma(\phi_1^\lambda + 1)}$$

This lemma provide an implementable test for $V_1 \subset V_0$. The bound defined in Lemma 2 is conservative. If we further assume that the probability for a match realised in the counterfactual world to be destroyed because of the programme under the assumptions of the Lemma 1 is spatially uniform, we get (see Annex) $\Psi_\lambda = \frac{\zeta^\lambda \gamma}{(1-\gamma)}$. This bound is preliminary and assumptions will be relaxed in a next draft of this paper.

5.2 Empirical results

As we observe the universe of apprenticeship contracts signed in France (around 300,000 per year), we can compute ζ. We show that for λ small enough, ζ^λ is significantly different between treatment and control groups.

Table 9 shows the results of Poisson regressions of ζ^λ on the assignment variable to the program, controlling for program sites (which are our randomisation strata) and occupation fixed effects. The first row restricts the count to jobs very close to the youth residence, as $\lambda = 0.5km$. In the next rows, we gradually increase λ by increments of $0.5km$ until a max radius of $3km$. In columns, we consider different estimation sample based on the village/city size of the youth residence. In Column (1), we restrict the sample to youth living in small villages with less than 1,000 inhabitants. In the next columns, we gradually increase the maximum size of cities included in our estimation sample, until we consider the full sample in the last column.

Overall, we find that jobs are closer to assigned youth than to control youth. For example, in municipalities with less than 2000 inhabitants (Column 2), there are 0.22 more hiring firms in the 1km-neighborhood of assigned youth, 0.178 more in the 2.5km-neighborhood. The effect in larger municipalities is of the same sign and statistically significant, but with lower magnitude. In municipalities with less than 10 000 inhabitants, 0.055 more hiring firms are located in the 1km-neighborhood of assigned youth (0.039 in the 2.5km-neighborhood). These estimates suggest that the set of firms which hire apprentices under the program is different from that in the world without the program: $V_0 \neq V_1$.

Finally, we test whether the inequalities of Lemma 1 are verified. In Table 10, we report estimates of the upper bound: Ψ_λ. As in Table 9, we consider different radius λ in rows and different estimation samples based on village/city size in columns. Comparing Table 9 and Table 10, it appears that the inequality of Lemma (2) is violated for small cities under 10,000 inhabitants (Columns 2 to 4) and low radius – λ lower than 2 kilometers. This result shows that in smaller cities, the program created new jobs. It is consistent with the previous results on informal job networks. Informal networks might have been activated by the program in smaller cities and helped to make new jobs appear.

6 Conclusion

This paper presents the results of a randomized control trial about a programme helping high-school dropouts to enter apprenticeship. The intervention we con-

Table 9: Number of apprenticeship hiring firms depending on the treatment status

λ	$\leq$2000h	$\leq$5000h	$\leq$10000h	$\leq$15000h	All cities
0.5km	0.088	-0.081**	-0.027	-0.012	-0.018
	(0.083)	(0.041)	(0.032)	(0.030)	(0.014)
1km	0.219***	0.100***	0.064***	0.055***	-0.012*
	(0.056)	(0.027)	(0.020)	(0.018)	(0.007)
1.5km	0.232***	0.165***	0.084***	0.070***	-0.019***
	(0.043)	(0.022)	(0.015)	(0.014)	(0.005)
2km	0.211***	0.135***	0.052***	0.071***	-0.023***
	(0.036)	(0.018)	(0.013)	(0.011)	(0.004)
2.5km	0.178***	0.100***	0.013	0.039***	-0.020
	(0.031)	(0.016)	(0.010)	(0.009)	(0.003)
3km	0.135***	0.082***	0.020**	0.030***	-0.021***
	(0.026)	(0.013)	(0.009)	(0.008)	(0.003)
N	711	1204	1501	1617	2712

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$, *fixed effects for each program site and each profession*

Table 10: Upper bound of inequalities. Value of Ψ_λ for different λ

λ	$\leq$ 1000h	$\leq$2000h	$\leq$5000h	$\leq$10000h	$\leq$15000h
0.5km	0.002	0.004	0.008	0.011	0.012
1km	0.004	0.008	0.020	0.028	0.032
1.5km	0.007	0.014	0.032	0.049	0.058
2km	0.011	0.020	0.045	0.073	0.090
2.5km	0.017	0.027	0.061	0.104	0.130
3km	0.026	0.038	0.082	0.141	0.175

sider consists in providing these young people with assistance in their search for an apprentice position. The programme was developed by Youth Job Centers: caseworkers receive candidates and provide them with assistance in their search.

There are several interesting results. The first one is that there is a sizable impact of the programme on the number of participants starting their apprenticeship. Our design is an encouragement design: the participants are only offered the programme and contacting the caseworkers is by no means compulsory. The ITT estimate show an impact of 12 pp on a basis of a 58% access to apprenticeship in the control group. Given the 50% take-up rate, an approximated LATE would be around 24 percentage points.

The second interesting result is due to the fact that it was possible for us to identify the search channel of hires. Hires are mostly due to the use of the youth' social network, and not because caseworkers bring new vacancies. This suggests that there is little substitution effect among channels. The network appear a key asset in the search process and the programme just teach who to improve the use if this asset. A potential drawback of this programme is that if the network access in unevenly distributed then the programme might have the potential to increase inequality.

[To be completed]

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Annex 1: Proof of the Lemmas

Let's recall the notations.

- N_C and N_T the number of respectively control and assigned youth, N_E is the number of other applicants that seek also for an apprenticeship in the same sites but were not in the experiment (neither as assigned nor as control.

- δ^* the fraction of applicants who find a job (in the counterfactual world where there is no program). It is of course unobservable

- δ_E^* the fraction of non experimental applicants who find a job (in the counterfactual world where there is no program). It is also unobservable

- δ_T the fraction of assigned youth who find a job

- δ_C the fraction of control youth who find a job

- δ_E the fraction of non experimental youth who find a job

...and the assumptions

- $V_1 \subset V_0$: there are less matches than in the counterfactual world. The programme cancels some of them. As assigned group find more jobs, control youth and non experimental youth - that receive an usual help - are worse-off: $\delta_C \leq \delta^*$ and $\delta_E \leq \delta_E^*$

- It is assumed that assigned youth are better-off in the world with the programme than in the counterfactual one: $\delta^* \leq \delta_T$

Under these assumptions, the programme has no result but canceling some matches. Let's note ρ the rate of matches that the programme suppresses. ρ can be calculated by

$$
\begin{aligned}
(1-\rho)\left[(N_C+N_T)\delta^* + N_E\delta_E^*\right] &= N_C\delta_C + N_T\delta_T + N_E\delta_E \\
\rho\left[(N_C+N_T)\delta^* + N_E\delta_E^*\right] &= N_C(\delta^*-\delta_C) + N_T(\delta^*-\delta_T) + N_E(\delta_E^*-\delta_E)
\end{aligned}
$$

Note that the second and third terms of the right part of the equation are negative. As for $N_E\delta_E^*$, it is equal to the number of matches realized by non-experimental youth in the counterfactual youth. Given the first previous assumption, it is greater to the number of matches M_E observed in the world with the programme. $N_E\delta_E^* \geq M_E$

$$
\begin{aligned}
\rho[(N_C+N_T)\delta_C + M_E] &\leq N_T(\delta_T-\delta_C) \\
\rho &\leq \frac{N_T(\delta_T-\delta_C)}{(N_T+N_C)\delta_C + M_E} \qquad (1)
\end{aligned}
$$

We consider now the balls of radius λ around applicants (around their residence locations). In the counterfactual world, ζ_A^λ (i.e ζ^λ restricted the assigned group) has the same distribution than ζ_C^λ (ζ^λ restricted to the control group). It is no more true in the world with the programme. We prove now that

$$
-\rho_\lambda r_\lambda \leq \bar{\zeta_A^\lambda} - \bar{\zeta_C^\lambda} \leq \rho_\lambda r_\lambda
$$

with
r_C^λ the mean number of matched firms within the ball of radius λ around control applicants (in the counterfactual world, i.e without program)
ϕ_λ the fraction of the total number of matches included in these balls
and ρ_C^λ the fraction of matches that the programme suppressed within the balls centered around control applicants and of radius λ

Proof

$$
\begin{aligned}
\bar{\zeta_A^\lambda} - \bar{\zeta_C^\lambda} &= \frac{1}{N_A}\sum_{i=\text{assigned}} \zeta_{i1}^\lambda - \frac{1}{N_C}\sum_{j=\text{control}} \zeta_{j1}^\lambda \\
&\leq \frac{1}{N_A}\sum_{i=\text{assigned}} \zeta_{i0}^\lambda - (1-\rho_C^\lambda)\frac{1}{N_C}\sum_{j=\text{control}} \zeta_{j0}^\lambda \\
&\leq \rho_C^\lambda \frac{1}{N_C}\sum_{j=\text{control}} \zeta_{j0}^\lambda \\
&\leq \rho_C^\lambda r_C^\lambda
\end{aligned}
$$

We suppose here that $\rho_C^\lambda = \rho$ previously bounded. r_C^λ is not directly observable as it characterize the counterfactual world. Let's note now $\tilde{r}_C^\lambda$ the similar quantity as r_C^λ defined in the world with the programme and so directly measurable.

$$\tilde{r}_C^\lambda = r_C^\lambda(1-\rho_C^\lambda) = r_C^\lambda(1-\rho_C^\lambda)$$

So it implies

$$\bar{\zeta_T^\lambda} - \bar{\zeta_C^\lambda} \leq \tilde{r}_C^\lambda \frac{\rho_C^\lambda}{1-\rho_C^\lambda} \tag{2}$$

We get the other side of the inequality in the same way, but with r_T^λ and $\tilde{r}_T^\lambda$ instead of r_C^λ and $\tilde{r}_C^\lambda$

$$-\tilde{r}_T^\lambda \frac{\rho_T^\lambda}{1-\rho_T^\lambda} \leq \bar{\zeta_T^\lambda} - \bar{\zeta_C^\lambda} \tag{3}$$

We assume now a simplifying hypothesis to make the previous inequality tractable. Hence, we assume that $\rho_C^\lambda = \rho_T^\lambda = \rho$. This assumption is compatible with a uniform spatial distribution of matches suppressions. The inequality (2) and (3) become then:

$$|\bar{\zeta_T^\lambda} - \bar{\zeta_C^\lambda}| \leq \tilde{r}^\lambda \frac{\rho}{1-\rho}$$