

Conditional Cash Transfers on the Labor Market: Evidence from Young French Jobseekers*

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Youth participation in programs designed to enhance their employability is usually low. This paper reports the results from the first randomized conditional cash transfer program in a labor market context: Young, unskilled jobseekers in France receive a monthly cash transfer for a two-year period totaling up to €4800, conditional on their participation in the French national career guidance program. Cash transfers lead to a significant increase in program participation (which mainly entails meetings with counselors), and sharply reduced drop-out rates. As a result, there is a large increase in the job offers, vocational training and career building workshops proposed to the young jobseekers. However, the jobseekers' response to these increased opportunities for employability investment is precisely estimated to be zero. Moreover, we observe a significant reduction in employment over the first six months. The results point to a strong impact of financial incentives, but also to the need to condition incentives directly on outcomes of interest, rather than on intermediate targets.

Keywords: Conditional cash transfer, youth unemployment, randomized experiment

JEL: J68, J64, C93.

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1 Introduction

Many youth enter the job market with few qualifications and little to no knowledge of the job market. Across OECD countries, 13.4% of youths aged between 15 and 29 years are not in employment, education, or training (NEET) (OECD, 2021). In France, 14% of youths are NEET, a rate that rises to 15.8% for youths who do not possess a high school diploma (Eurostat, 2021). Helping these young people achieve personal and professional development, and to increase their overall human capital and employability, has been a policy priority across countries (Quintini et al., 2007). This paper evaluates the first randomized conditional cash transfer program in a labor market context. It assesses whether a monthly cash transfer conditional on participation in a large national employment program creates effective incentives to participate and engage with the program, increase employability investments, and improve employment outcomes.

Designing programs that are effective and attractive is not an easy task. Existing programs across major industrialized countries have focused on skill assessments, career planning, general or specialized training, job search assistance and employment experience through internships and subsidized job contracts. Whatever lever is used, such programs have a positive short-term effect at best, but do not build human capital to the degree necessary to improve long-term employment outcomes: In a recent meta-analysis of 113 impact evaluations, only one third of youth employment programs show positive effects on employment or earnings – and most of these are in low-income countries (Kluve et al., 2019).¹ Programs that help jobseekers find work tend to yield better results, but benefits are often temporary and may displace workers who are not supported by the programs (Crépon et al., 2013). The measures comprise counseling, mentoring, provision of information, as well as short-term training and application coaching (Caliendo and Schmidl, 2016). The rationale behind these employment services is to decrease people’s time searching for a job and increase their fit to the job, thus decreasing job rotation. These types of intervention are widespread in developed countries. Caliendo and Schmidl (2016) collected evidence from 16 studies about job search assistance and monitoring in 8 European countries which on average found significantly positive long-run effects on employment. However, the risk of displacement of non-treated youth might distort the results, and thus needs to be considered. The treatment shows decreasing marginal effects. Moreover, monitoring and sanctioning provided only weak results, always bearing the risk that treated individuals leave the labor force. Important to mention is that the effect diminishes under bad economic conditions. Kluve et al. (2017)

¹A notable exception is the high-intensity Job Corps program in the U.S. (Schochet et al., 2008). See the meta-analyses by Card et al. (2018, 2010) for active labor market programs across all age groups, and reviews by LaLonde (2003) of U.S. programs; see Heckman et al. (1999) for a more general review.

support these findings with a meta-analysis of employment service programs in high-income countries, mentioning positive employment and earnings effects, although just of transitory nature. No robust evidence for causality between the services and increased labor-market participation, employment stability, job quality or earnings could be found.

Available empirical evidence also points to a lack of attractiveness of these programs, resulting in low participation levels and high dropout rates. Heckman et al. (2000) show that participation rates in assistance programs are low, and Behaghel et al. (2014) find that less than 50% of those assigned to assistance programs in France actually attend. Black et al. (2003) even show that assigning jobseekers to this type of program makes them rush to find employment to avoid attending. LaLonde (2003) and Ivry and Doolittle (2003) also report that many youths who would benefit most from assistance programs do not enroll, and that the majority of those youths who do enroll invest little energy in the programs offered, as they judge them to be ill-suited to their needs or do not see the point of a medium-term commitment to training or skills-building.

Creating programs for the unemployed youth is particularly difficult as it risks incentivising taking up low quality jobs that do not last in the long-term as well as resorting to alternative forms of financial support such as relying on parents (Caliendo and Schmidl, 2016). In a study reviewing the current approaches to active labour market policies (ALMPs) globally, Romero and Kuddo (2019) clearly highlight the importance of demand-driven training models in coordination with the private sector, which should provide industry accreditation and skills training standards. An evaluation of 8 studies in Europe by Caliendo and Schmidl (2016) analyzes the effect of wage with and without supplementary training. Without training, wage subsidies had an employment effect of zero, whereas with training, the outcome was among the best of all evaluated forms of ALMP. The deadweight effect (meaning that employers hire youth with wage subsidy), as well as worker substitution, replacing ineligible workers with eligible ones, are also an issue for the interpretation of the results. This program is a valuable tool to improve the employability of students by smoothing the transition from education to work (O'Connor and Bodicoat, 2017). This is especially of use in the context of an increasing share of highly educated young people, a declining labor market for graduates, and neoliberal labor market structures. Youth is often subjected to a catch-22, which means that job experience is required to be hired, but cannot be collected without being hired (McKenzie et al., 2016).

Benefits of internships that go beyond skills development were observed in a holistic approach to foster personal transformation for unskilled young people in South Africa, for which the internship was part of a "continuum of care" after the initial treatment (Meyer and Chetty, 2018). The program improved confidence and self-esteem, which hints at potential benefits

of internships in combination with other interventions. Using content analysis for messages by pupils in online peer groups at the German Federal Employment Agency, Felgenhauer et al. (2021) found that online peer groups for career counselling can provide types of social support which traditional one-on-one counselling is unable to offer. Likewise, Klier et al. (2019) discover that such peer groups offer significant added value compared to traditional career counselling.

While the World Bank argues that the number of youth reached by counselling tends to be small and highly conditional on the overall job growth environment in which the intervention is taking place (Independent Evaluation Group, 2012), Caliendo and Schmidl (2016) do not contradict but instead agree with the latter, arguing that counseling and monitoring has overall been shown to have positive effects including an increased likelihood of beneficiaries finding stable employment. Importantly and agreeing with the World Bank study, they also find that those studies showing a zero or even a negative effect tend to have been carried out under difficult economic conditions such as in Denmark during the economic crisis in 2008 (Caliendo and Schmidl, 2016). They argue that if counselling becomes too intense, beneficiaries tend to escape such schemes, e.g. by relying on parental support or by becoming entirely demotivated, essentially creating zero or negative effects and making it difficult for policy-makers to find an appropriate intensity (Caliendo and Schmidl, 2016). Caliendo and Schmidl (2016) find that most programs tend to increase employment levels or have a zero effect, again with the latter being most likely linked to bad labor market conditions (Caliendo and Schmidl, 2016). Three issues are to be taken into account here: 1) deadweight loss, meaning whether or not those who found employment after completing the program would have done so without the program as well, with especially Caliendo and Schmidl (2016) criticising relevant impact evaluations for insufficiently researching deadweight loss substitution, meaning whether or not unpaid interns were essentially used to replace otherwise paid staff, and 3) displacement, meaning whether or not the use of intern gave a competitive advantage to certain companies (Arlow, 2019).

On a theoretical level, models of human capital investments and of behavioral economics have identified several potential factors likely to affect demand for employability investment. Heckman et al. (1999) propose a model which encompasses all services designed to improve job-market readiness, highlighting the role of opportunity costs and expected returns in terms of the perceived productivity of job searches and of the expected remuneration and stability of jobs. They also underscore the central role of financial constraints in the decision to invest, including when these constraints come from parents' incomes. In this case, paying a transfer to a young person with financial difficulties relieves him or her of those constraints. Behavioral economics models identify several reasons why the intrinsic motivation of young people

can be low. Babcock et al. (2012) summarize the lessons to be learned from these findings when designing labor market policy. The first obstacle, proven empirically by Spinnewijn (2015), is that the perceived benefit of this investment plays a central role in the decision to enroll but is very difficult to gauge correctly. The second obstacle is that choosing an orientation strategy adds an extra degree of complexity to the task of assessing the benefits of a training program. Lastly, intertemporal preferences for the here and now can also be a determining factor in demand: young people may systematically favor low-paying, insecure jobs, which are easy to find, and delay investments in human capital to a later time. In this context, paying a young person a conditional transfer re-shifts the priority to choosing investments in employability.

Providing a transfer is one way of alleviating active financial constraints. Many countries have opted for the payment of a minimum benefit, which gives young people more leeway in their choice of a career track.² Nevertheless, such systems can undermine incentives to get into and stay in the job market. For this reason, some transfer systems have evolved to ensure that incentives for paid work remain central.³ Unconditional transfers run the risk of being inefficient if low participation rates among young people are due to weak intrinsic motivation, which is linked, for example, to an undervaluation of program returns or a biased preference for the here and now. In this paper we look at the payment of a minimum transfer which attempts to support human capital investment incentives for young people by making payment conditional on participation in a national career guidance program. We compare two cohorts selected randomly from a sample of young adults enrolled in the national program. The two samples differ only in that one is given this conditional cash transfer, while the other is not.

The young adults targeted by the service have poor job prospects. Typically, they have had serious issues at school and have dropped out or failed multiple times. Most live in social environments and areas which condemn their chances of integration, such as the isolated housing estates found in major cities. When they are *Not in Employment, Education or Training* (NEET), these young people are usually offered enrollment in a national career program, the “Contrat d’Insertion dans la Vie Sociale (CIVIS)”, called hereinafter the *standard program*. The CIVIS program is characterized by low attendance and a high dropout rate. The experiment consisted of offering 3,000 of these young adults a place in a new program:

²Various approaches exist: Austria, Germany, Portugal, Sweden and others have systems based on parental income when the young person is still dependent upon them, and on the young person’s own income when they no longer depend on their parents. In Denmark, Finland and Netherlands, a minimum benefit is paid based solely on the young person’s income regardless of whether they depend on their parents or not.

³This is true of the Earned Income Tax Credit (EITC), the Working Income Tax Benefit (WITB) in Canada, and the Working Tax Credit (WTC) in the U.K.

the “Revenu Contractualisé d’Autonomie”, called hereinafter the “experimental program”, identical to the standard program except for a monthly benefit payment. A €250 transfer is paid monthly the first year. The amount decreases gradually the second year (€240 in the first quarter down to €60 in the last quarter). Furthermore, if a participant finds employment, the monthly amount transferred decreases at a rate of 24 cents per additional Euro earned in job-related income. Therefore, a participant who earns €0 in job-related income over the course of the two-year program can theoretically receive up to €4,800 in cash transfers. The benefit is paid as long as the youth complies with the guidance program. If the youth fails to attend meetings or comply with the tasks stipulated by the program, his or her counselor may decide to suspend payment of the transfer in coordination with the Job Youth Center (JYC) director. Due to the diverse and partially non-contractable nature of tasks in a personalized guidance program, the key contractable behavior, in practice, is attending the meetings.

Results show a significant increase in program participation. Because of the benefit, the program’s drop-out rate diminishes drastically. Young adults remain in the program for a longer period of time and have more meetings with their counselors: the average number of months spent in the program went from 12.1 (in the standard program, without the transfer) to 21.7 months (in the experimental program, with the transfer) and the total number of interviews with a counselor increased from 8.1 to 14.6 per participant. Transfers received increased steeply by €1,868 (\$ 2,577) to a total of €2,132 (approximately \$ 2,942 in 2011).⁴ This additional individual expense, though diminished, nevertheless applies to a potentially large group – 170,000 young adults in 2011 – thus representing a non-negligible direct financial commitment of about €318 million, or \$ 438 million.

Study results show that this noted improvement in participation is not followed by enhanced commitment to the program. Recipients do not invest more in their employability. Despite being offered a significantly broader range of services (combined services of all types increased from 8.12 to 12.6 in the first semester), we observe null effects on a wide range of outcome behaviors, from enrollment in the trainings proposed by the caseworkers to sending job applications and searching for jobs online.

Moreover, in the first six months, there is a three percentage-point decrease in full-time employment. This effect is consistent with the disincentives traditionally associated with transfer payments and income-based taper rules. Relatedly, we find that income increased by less than half of the theoretical transfer amounts. Benefits received as part of the program are initially diminished through the taper rule. We additionally observed substitution with

⁴This amount falls quite short of the €4,800 announced. We show that this is primarily due to the income taper rule.

other income sources, primarily employment income and transfers from friends or family. Lastly, variables collected to measure social integration show no notable improvement except in the confidence young people had in the JYC.

We employ a principal-agent model with a two-step effort task (meetings and training) to examine the possible mechanisms that produced these results. According to this theoretical framework, agents may underinvest in effort relative to the principal's preferences due to risk aversion, impatience, financial constraints, and perceived returns to effort (through either self-efficacy or perceived program quality). Conditioning transfers on the first effort step (meetings) will be effective in the case of financial constraints, partially effective in the case of impatience or low perceived returns to effort, and ineffective in the case of risk aversion. Empirically, we find no evidence for financial constraints, perceived low returns to effort, caseworker quality, or labour market conditions as a mediating factor for our treatment effects. Present bias and impatience constitute a possible explanation if the effort costs for human capital investments are disproportionately higher than the effort costs of interacting with the caseworker. Our findings emphasize the importance of conditioning incentives directly on outcomes of interest, rather than on intermediary steps.

This paper proceeds as follows. Section 2 provides a detailed description of the program and category of young adults concerned. Section 3 presents study design and collected data. Section 4 discusses the principal results regarding program participation, employability investment, employment, income and social integration. Section 5 assesses results found in each sub-sample of participants to determine whether there is heterogeneity in program outcomes. Section 6 analyses the robustness of the results and section 7 concludes.

2 Programs and Participants

2.1 Background on the Study Population

A considerable number of young people in France exit the education system early. According to a 2010 survey of French youths who left the education system in 2007 – four years before the conditional cash transfer program commenced – 18 % leave school without any diploma and 17 % only complete the equivalent of junior high school. The survey further reveals substantial difficulties in entering the labor market: 21.8 % of respondents have been either primarily unemployed (9.3 %) or inactive (12.5 %) in the three-year period after leaving school. These problems are linked: 58.6 % of those struggling with long-term unemployment or inactivity do not have a high school diploma.

Assistance to youth between the ages of 16-25 who encounter problems finding work is provided by 450 Job Youth Centers (JYCs) located throughout France. 20.6 % of young

people who finished school in 2007 went to a JYC at least twice by 2010. Among those who sought help at the JYC, youths without a high-school diploma were disproportionately represented (63.7 %). Compared to the population as a whole, youths who visited the JYC at least twice by 2010 were 9.6% more likely to have repeated a year in primary school, and 19.3% more likely to have left school before their 18th birthday. They were 8.2% more likely to have immigrant parents, and 4.2% more likely to live in deprived suburbs.

Notably, not all young people who have trouble finding a job go to JYCs. According to the survey, overall attendance ranges from 20.6 % overall to 41.9 % for those struggling with unemployment or inactivity. It increases to 52.3 % for those who additionally lack a high school diploma.

2.2 The Guidance Program (G)

The JYCs offer a guidance program to facilitate labor market integration: the *Contrat d'Insertion dans la Vie Sociale*, which we will refer to as the 'guidance program (G).' Approximately 170,000 young adults enrolled in this program in 2011. It is a one-year program which may be extended for a second year, aimed at helping participants to establish a career plan (in the first three months), and then implement it.⁵ Participation is formalized by the signature of a contract. There is no financial assistance, except for the reimbursement of selected job search costs. Meetings with the counselor are offered at least once a month, in addition to the possibility to call or email as required. The program acts as a platform to identify and steer participants towards employability investments that are best suited to their individual skills and situation: training courses (typically offered by partner companies), career workshops, subsidized job contracts, or job shadowing at companies. If participants enroll in a course or find short-term work, they remain in the program, and are expected to remain in touch with their counselor. They leave the program when they secure an employment contract of at least six months, when they reach the end of the program, or when the program contract is revoked by the counselor, typically following lack of participation.

A known concern with the guidance program is that participants invest little effort, and dropout rates are high: in 2011, only 27 % of participants exited the program into long-term employment; 15 % left after being enrolled for the maximum of two years, and 58 % dropped out either during the program, or because their counselor did not extend their contract after one year (usually because the participant had stopped contact with the JYC) (Dares, 2014).

⁵Very hard-to-place jobseekers are allowed to extend more than once. They are offered an enhanced version of the program which includes more frequent meetings with their JYC counsellor.

2.3 Guidance + Conditional Cash Transfer (G+CCT)

In late 2008, the French Ministry of Youth launched an initiative for innovative policies to address key difficulties faced by young adults. The French government’s 2009 Green Paper on Youth (Livre Vert de la Jeunesse, 2009) identified a lack of financial independence as an important concern: In France, adults below 25 years are not eligible for welfare payments. Unless they have previously paid into the unemployment insurance, young adults may find themselves in precarious situations without a guaranteed minimum income. The resulting financial constraints may hinder human capital investment, and with it, labor market integration. The Green Paper recommended that new forms of youth cash transfers be tested to address this problem.

Policymakers decided on a monthly cash transfer, conditional on participation in the national guidance program. Though formally considered a novel program, the new *Revenu Contractualisé d’Autonomie* was identical to the existing *Contrat d’Insertion dans la Vie Sociale*, except for the provision of financial assistance. We thus refer to it as ‘guidance + conditional cash transfer (G+CCT).’ Participants received €250 per month in the first year, equal to 23% of the French monthly minimum wage (SMIC) and 54% of the guaranteed minimum income scheme (*Revenu de Solidarité Active*, or *RSA*). In the second year, the amount transferred decreased progressively: €240 monthly the first quarter; €180 monthly the second quarter; €120 in the third quarter; and €60 in the fourth quarter. Thus, the maximum amount a participant could receive over two years was €4,800. Transfers were subject to participation, formalized in a program contract between the JYC and the jobseeker. Contracts clearly stated the conditions for termination: *“the contract shall be terminated if: the beneficiary fails to meet his or her commitments; if he or she does not come to appointments set by the counselor without just cause, or refuses, without just cause, training or employment opportunities suggested by the counselor which comply with the career plan defined in the contract. Should this occur, and after the beneficiary has been given a chance to explain, the counselor shall terminate the contract on legitimate grounds and notify the beneficiary by registered mail”*. While contracts specified a broad definition of program participation, the key enforceable criterion in practice was the attendance of the monthly meetings with the counselor.

Importantly, the amount of the transfer was tapered off in relation to employment income, and designed to hit zero once a participant made €1,050 – the minimum monthly salary as of April 2011. The tapering implied a linear tax on employment income of $250/1050=24\%$ (see Figure 1b). Employment revenue includes wages, unemployment insurance and training compensation. The tapering of the program thus directly interacts with the employability

investments it seeks to promote: A participant who starts a certified training course would earn €325 per month, but see their conditional cash transfer reduced by €78. Similarly, an apprentice would earn €470 per month in the first year, but see their transfer reduced by €113.

3 Experimental Study Design and Data

3.1 Experimental Study Design

A nationwide randomized study was implemented to evaluate the effect of conditional cash transfers on participation in the guidance program, dropout rates, employability investments, and employment. A call for applications to take part in the study was issued to the 427 JYCs in France. Of these, 82 JYCs agreed to participate. The randomization design posed a challenge: An individual-level randomization was not feasible for ethical and political reasons – control group participants would have learned about the transfers, and complained about preferential treatment by the JYC. Randomization at the level of the JYC would have yielded limited statistical power, and created endogenous selection in who registers for the guidance program (this invalidates the design because we observe only those who register). We solve these selection and power issues by randomizing *within* each JYC, based on whether individuals signed up for the standard guidance program in February or in March 2011. This was done as follows: First, registrations for the standard guidance program were observed in February and March 2011, yielding 5498 new enrollees in the participating 82 JYCs. At this time, there was no public information about the experimental cash transfers, and it is unlikely that participants had any knowledge of this possibility when they signed up. Once registration lists for February and March were closed, the JYCs were paired according to existing characteristics, including the number of youths per counselor and the proportion of youths with a high school degree. Members of each pair were then randomly assigned to either group F or M. Group F JYCs contacted all subjects who had registered in February, and offered them to switch to a new contract including cash transfers (G+CCT). Group M JYCs did the same for subjects who registered in March. For both groups, cash transfers started in April. Compliance is high but imperfect: 82 percent of those offered the cash transfer contract accepted it. Across JYC groups, 2661 subjects were assigned to the treatment group (G+CCT), and 2837 to the control (G). Figure 2 illustrates the locations of the JYCs on a map of France, and Table 1 breaks down the sample by observable characteristics.

3.2 Data

The empirical analysis uses administrative data from the JYCs, as well as surveys carried out 12 and 24 months after randomization.

The JYCs collect a range of administrative data about the youths when they first register at the center. These include demographic data, as well as information regarding the subject's housing situation, resources, and past experience in the labor market. In addition, administrative records trace all exchanges between registered youths and their counselors (meetings, phone calls, emails), as well as the details of these exchanges (dates, content keywords). This allows us to observe effects on participation and engagement with the program. The records also contain rich information on the service provided by the counselors: the content of the program and all the offers made to participants while in the program, including job offers, opportunities for training or career building services, proposals and matching. This is key to assessing whether more meetings with the counselor led to more opportunities for the participants. Finally, counselors recorded details on participants' current situation during the meetings - specifically, whether they were employed, unemployed, or in training at that time. In contrast to the regular public employment service, JYCs maintain records of registered youths (and keep them paired with a counselor) even after participants return to employment. A major drawback of the administrative records is that they stop when participants lose contact with the JYC. We thus rely on the administrative data mainly to measure effects on program participation and opportunities offered by the JYC.

In addition to the administrative records, two individual phone surveys were carried out: a midline survey after 12 months (April 2012) and an endline survey after 24 months (April 2013). Each survey lasted 25 minutes, and elicited detailed labor market outcomes including employment, training, career building, and job search. Employment outcomes included all employment events (full and part time) each month over the previous twelve months. The survey also asked about income, sources of income, expenses, social integration and personality traits (locus of control, patience, life satisfaction).

While the surveys provide more comprehensive data on outcomes than the administrative records, the response rates were both low and differential across treatment groups: Response rates to the midline and endline survey are 60 % and 40 %, respectively, in the control group. Response rates are 5-6 % higher in the treatment group, likely because of participants' increased willingness to take part due to the cash transfers (Table 6). In Section 6 and Appendix B, we demonstrate the robustness of our estimates to differential survey response rates. First, we show that observable characteristics from administrative records are balanced across treatment groups in all relevant samples: the administrative sample, the midline

survey sample, and the endline survey sample. Second, we compare the treatment effect on administrative outcomes across all three samples. Third, we use various alternative estimation methods for our treatment effects, including additional control variables, the non-response bias correction proposed in Behaghel et al. (2015), and the implementation of Lee bounds (Lee, 2009).

3.3 Balance Check and Sample Description

Table 1 shows the balance of administrative variables across randomized treatment and control groups. We verify the randomization is balanced in all three samples that are relevant for the analysis: The first set of columns uses administrative records for the entire sample, the second set of columns restricts to midline survey respondents, and the last set restricts to endline survey respondents.

Observable characteristics are well balanced across groups. In the full sample, we reject the equality of means between the treatment and control group in only two of 44 variables (having children, and having started a training before registering for the program). In the midline sample, three variables have different means at the 10 % significance level; one at the 5 % level and another at the 1 % level. In the endline sample, only one variable is not balanced at the 10 % significance level. The joint hypothesis of equal means in all variables is not rejected in any of the samples. This is particularly reassuring in light of the differential attrition discussed in the previous section.

On average, young adults who enrolled in the standard program in February and March 2011 are 19.7 years old, meaning that they fall on the younger end of the age range of people eligible to enroll in the experimental program (18-22 years old). Participants in the study have few qualifications and most dropped out of school at the high school or basic vocational level. Only 30 % of them have a driver’s license, which is an important but expensive asset for social inclusion in France (see footnote 9). Despite their young age, only 62 % still live with their parents. Roughly one in thirty has no stable housing or is homeless. At the time of JYC registration, personal income levels are very low at €77 per month, on average. Consistent with the low income levels, we observe that participants are highly disconnected from the job market. The number of days spent in employment (training) in the three months preceding randomization (the first quarter of 2011) is 6.7 (6.4). Only 14.5 % of participants declare having worked during that quarter, and 13.5 % were in training.

3.4 Estimation

We estimate Intent-To-Treat (ITT) effects by applying ordinary least squares to the model:

$$y_{m,i} = \alpha + \beta_{ITT}Z_i + \gamma X_i + \lambda_m + \epsilon_{m,i} \quad (1)$$

where y is the outcome of interest for individual i , Z is an indicator for assignment to the cash transfer group (G+CCT), X_i is a vector of observable characteristics from administrative records (those in Table 1), and λ_m is a JYC fixed effect. The error term $\epsilon_{m,i}$ allows for clusters at the JYC level. We include X_i to improve the precision of estimates, and to account for residual differences between treatment and control. The coefficient β_{ITT} estimates the ITT effect of being offered the cash transfer program (G+CCT), relative to the control group which received the standard guidance program (G) without cash transfers.

Given imperfect compliance (82 percent accepted to switch to a contract with cash transfers), we additionally estimate Treatment-on-the-Treated (TOT) effects. The TOT estimates the effect of participating in the cash transfer program (G+CCT), rather than just receiving the offer. We thus estimate

$$y_{m,i} = \alpha + \beta_{TOT}T_i + \gamma X_i + \lambda_m + \epsilon_{m,i} \quad (2)$$

where T_i is a dummy variable for participants who accepted the cash transfer program. The endogeneity of the participation decision is addressed by instrumenting T_i with the random assignment to treatment, Z_i . The identifying assumption is that the offer of the cash transfer program did not in itself change jobseekers' behaviour, other than through encouraging them to participate.

Finally, to look at heterogeneity in our results in relation to a subsample identified by a dummy variable I (such as patience vs. impatience, low vs. high financial constraints, or internal vs. external locus of control – see Section 5), we estimate an equation in which the treatment group variable interacts with the I dummy and the $(1 - I)$ dummy:

$$y_{m,i} = \alpha + \beta_{ITT,I}Z_i \times I_l + \beta_{ITT,1-I}Z_i \times (1 - I_l) + \delta I_l + \gamma X_i + \lambda_m + \epsilon_{m,i} \quad (3)$$

Regressions use the I variable as an additional control variable (if it is not in X_i already). The $\beta_{ITT,I}$ and $\beta_{ITT,1-I}$ coefficients represent the impact of being assigned to the program on the $I = 1$ subsample and on the $I = 0$ subsample.

4 Results

4.1 Increased Program Attendance

Figure 3 shows the estimated ITT effects on enrollment and participation in the program. As outlined in Section 2.2, participants left during the program when they found stable employment, or when the counselor terminated their contract due to lack of participation. Figure 3a shows enrollment rates, estimated using equation 1. The dotted line is the mean enrollment in the control (group G) for a given month. It also shows a 95 % confidence interval for the treatment effect (coefficient β_{ITT} in equation 1). We show the evolution of the treatment group (G+CCT) by adding the estimated ITT effect to the control mean, represented by a solid line.⁶ Months are numbered starting January 2011. Thus, program registrations occur in months 2 and 3, and cash transfers start in month 4.

Figure 3a shows a steady decline in enrollment rates throughout the first year of the standard guidance program (G), culminating in a sharp drop in months 11 and 12. Enrollment then stabilizes at 20% of the cohort of participants in the second year. The sharp drop at the end of year one is related to the program design: Counselors have to actively re-enroll participants for the second year of the program; otherwise the contract terminates. In contrast, terminating a contract at other times requires an active decision from the counselor. It is thus common that participants who miss several appointments in a row are not immediately excluded from the program, but simply not re-enrolled. In stark contrast, there is only a small decline in enrollment for the cash transfer group (G+CCT) at the end of the first year, and enrollment rates remain around 70 percent throughout the second year. This may reflect higher participation of the jobseekers, but it may also represent confounding factors introduced due to the counselors' discretion: altruistic counselors may be reluctant to terminate contracts because they do not want to deprive jobseekers of much-needed cash transfers. We thus look to more objective measures of participation: registered meetings and other exchanges.

Figure 3 shows the average number of counselor meetings by month per jobseeker. Unlike program enrollment, average monthly meetings decrease at a more steady rate over the two years in the standard program (G). If each enrolled participant went to one scheduled meeting per month, and dropped-out participants did not, this unconditional average should trace enrollment exactly. While the decline in monthly meetings is equally visible in the cash transfer group (G+CCT), the number of meetings for this cohort is consistently higher until the end of the program. This suggests that the cash transfers had real impacts on

⁶Relative to the presentation of unconditional means, this improves precision through the inclusion of JYC fixed effects and control variables.

program participation. The rate of overall exchanges with the JYC counselor (including emails and calls) closely mirrors the rate of meetings, and also shows consistent treatment effects throughout the program. Similarly, the proportion of participants who are no longer in contact with the JYC after a given month is considerably lower in the treatment group. Table 2 summarizes the estimated Intent-to-Treat (ITT) effects on program participation. The first row confirms that the control group did not have access to the cash transfers, while 82% of the treatment group accepted them. The offer of the cash transfer program increased the average months of program enrollment by 7.9 from an average of 11.4. The number of meetings over the two-year period increased by 5.1 from an average of 8.2.⁷ The table further shows the total payments received from the JYC during the program: While only €237 were paid without the cash transfers (mostly reimbursements of job search costs), cash transfers increased the program cost by €1528 per jobseeker over two years.

To summarize, Table 2 as well as Figures 3a and 3b suggest that young jobseekers are extremely responsive to financial incentives for program participation. Jobseekers offered conditional cash transfers remain enrolled in the career guidance program for longer periods of time. They maintain a more active relationship with the JYC, have more meetings and other exchanges, and are less likely to lose contact over time. This increased participation comes with a high price tag: Abstracting from the program’s stated objective to ease financial constraints, each additional meeting cost the government €287 ($= 1865/6.5$) in financial incentives in the form of CCTs.

4.2 Participants Are Offered More Opportunities But Do Not Seize Them

A central question is whether longer program participation and more meetings translate into more information and more opportunities for the jobseekers. The remaining panels of Table 2 present treatment effects on administrative records from the JYC.

Administrative records register and encode the content of meetings and exchanges with each participant. For the sake of simplicity, we use simple indicators on whether information was provided on job opportunities, training courses, or career planning services in a given meeting, and then sum these indicators across all meetings the participant attended. The JYC data further record instances in which youths were matched with providers of these job

⁷This result from JYC records can be cross-validated with information from the midline survey in April 2012: Respondents were asked how many meetings were held in the last three months (months 10 to 12 of the cash transfer program). Treatment significantly increased JYC meetings in three months by 0.77, from a control mean of 1.58. Interestingly, there is no drop in the number of meetings with other service providers: 0.33 meetings were held with the Public Employment Service (ITT effect 0.00), and 1.27 meetings with temporary employment agencies (ITT effect -0.02).

offers, training courses, and career services.

Finally, the JYC counselors record new information on employment periods, training, internships and apprenticeships during every exchange, or retroactively to update a participant's file. This includes jobs and trainings obtained via a channel other than the JYC. However, impacts on employment and training estimated using JYC records are biased upwards given the differential participation by treatment status: Participants in the cash transfer group were observed for longer periods. The middle panel of Table 2 presents ITT effects on the information a counselor gives to a participant and service matching, while the bottom panel shows actions actually taken by the jobseeker. Effects are reported for the first three months, the first six months, the first year, and the total after two years. To keep track of any differential reporting, the table also shows enrollment rates and numbers of meetings for each time horizon.

The table shows a clear link between program participation and increased exposure to information on available services and opportunities (middle panel). Counselors report an average of 5.2 events where they provided any kind of information on services – including health and housing services – in the control group in the first quarter following randomization. Assignment to the cash transfer program leads to a significant increase of 2.8 events per participant. This increase of 48% is roughly in line with the increase in meetings by 69% (1.25/1.82) observed in the first quarter. Disaggregating information by type (employment, training, career planning), or changing the time horizon to six months or one year, yields very similar results: Assignment to cash transfers increases the exposure to information about services by roughly 50 percent, with all effects significant at the 1% level.

The middle panel of Table 2 shows that the additional information is followed by an increase in service matching. In the first three months of the program, participants are matched with career planning services twice as often in the treatment group (0.28) than in the control (0.14). Rates of matching with training and job offers also increase significantly. All effect sizes are 30 percent and larger, and persist after six months and one year. Our results suggest that greater participation in the program entails increased exposure to opportunities and actual offers made.

The bottom panel of Table 2 shows employment and training outcomes, as recorded by the counselor during meetings. The observed rates of re-employment and training are the same for the treatment and control groups. The same applies to human capital investments, which include training courses, company internships and apprenticeships. As noted previously, these outcomes are skewed by the fact that treated participants come to more meetings, meaning that counselors observe them for a longer period (this likely explains the positive estimates after 1 year). However, counselors do monitor whether participants signed up for

the services the counselor matched them to. We would thus be able to observe if the increase in matched services translates to an increase in services taken up. Therefore, the absence of effects cannot be merely due to reporting bias, but suggests that participants do not seize the increased opportunities provided.

4.3 No Impact on Employability Investments

We obtain further information on human capital investments from two surveys: a midline survey after 12 months (April 2012) and an endline survey after 24 months (April 2013). Table 3 presents the results, differentiating between longer-term human capital investments (top panel), and short-term job search activities (bottom panel). We follow Kling et al. (2007) in establishing an index for each outcome category.⁸

We look at a wide range of outcomes to assess employability investments, including formal investments like apprenticeship programs, internships, number of courses completed, certified training and obtaining a driver’s license. Other outcomes capture subjective aspects, such as having an established career plan, or self-assessed prospects of finding suitable employment. This wide range of outcome variables reflects the targeted nature of the program: Counselors suggest investments suitable for the jobseekers’ individual situation, rather than promoting individual measures. Section 4.2 showed that treated participants attend more meetings and receive more recommendations and services. If participants follow their counselors’ recommendations, we thus expect responses to treatment to be spread over a variety of investment types, rather than concentrated among specific types.

The results largely confirm the administrative records from Table 2: We detect no effect on any type of employability investment, with the exception of driver’s licenses: the number of participants who start a course to obtain their driver’s license is 3 percentage points higher in the treatment group, with a mean of 41.9 % in the control group.⁹ We estimate a precise zero effect on the overall index of investment (see footnote 8 on minimum detectable effects), and conclude with high statistical power that the treatment has no impact on human capital investment.

⁸ Following Kling et al. (2007), we standardize variables by subtracting the control group mean and dividing by the control group standard deviation before summing them. A standardized variable demonstrates a minimum detectable effect 2.8 times the standard deviation estimated by the variable with a statistical power of 80 % and p-value of 5 % (Bloom, 1995). In essence, this means that our protocol is able to detect a minimum effect of between 9 and 10 % of a standard deviation: a weak minimum detectable effect compared to the literature (9.3 % = 3.5×2.83 for the job seeking effort index and 9.9 % = 3.3×2.82 for human capital investment index).

⁹ Obtaining a driver’s licence in France is a lengthy and expensive process. Learners must pass a demanding theory exam and complete a minimum of 20 driving lessons (average: 32), then register for the driving exam and wait for a spot to open. Average costs are around €1800. Due to the distance of underprivileged neighbourhoods from town centres, a driver’s licence is seen as a key asset in the search for employment.

In addition to human capital investments with a longer time horizon, the surveys also ask about job search behavior: actively seeking work, usage of different search channels, the distance participants are willing to travel for a job, and their willingness to move to take an indefinite term job contract. The bottom panel of Table 3 shows that treatment does not change job search behavior at all. Both with respect to individual job search outcomes and the overall job search index, we observe a treatment effect of zero with high precision.

4.4 Short-Term Negative Impact on Employment

Employment was a key targeted outcome of our study. The ultimate long-term goal of the program was to give participants improved access to high-quality jobs. This was to be achieved by increasing employability investments and job search. In the short-term, several effects were possible and expected: First, increased investment in human capital (especially trainings and apprenticeships) may initially and temporarily reduce employment rates (a “locking-in” effect). Second, transfers weaken the incentive to work (a classic income effect). Third, the tapering of cash transfers imposed an implicit tax rate of 24 % on employment income during the program (Section 2.3). This is likely to reinforce disincentives to work and encourage part-time work over full-time work.

The surveys contain comprehensive information on employment outcomes. For each month of the study, we observe whether participants worked, if the job(s) lasted the whole month or not, and if employment contracts were full- or part-time. We combine both surveys to establish a two-year timeline of employment: results for the first 12 months are obtained from the April 2012 survey while results for months 13-24 are obtained from the April 2013 survey. Figure 3 shows employment access rates, estimated using equation 1. Estimated using equation 1, Figure 3c presents the rate of employment on full-time contracts, while Figure 3d presents the rate of employment on part-time contracts for a given month. The top panel of Table 4 shows the overall employment results. Results clearly show that the experimental program has a negative impact in the first six months on full-time employment. However, negative effects on employment are both small in magnitude and short-lived: In the first six months, the employment rate (full- or part-time) declines by 8.3 % from a control mean of 2.42 months. By the second semester, the effect has disappeared.

Finally, the surveys provide valuable insights into the type and quality of the employment contracts obtained. The middle panel of Table 4 presents treatment effects on the type of contract (short-term, permanent, contracts through temp agencies, apprenticeship or internship), on formal or informal employment, on subsidized jobs, and on the type of employer (public or private). We find that the cash transfer program has no impact on the type of job found, with the exception of a slightly higher rate of informal employment, as well as

public-sector employment (significant at the 5 and 10% level, respectively).

In addition to providing financial incentives for participation in the guidance program, a key motivation of the cash transfer scheme was to relieve financial constraints. We hypothesized that financial constraints may keep youths in low-skill, insecure employment, and prevent them from investing in human capital to obtain more long-term, secure employment. One reason why we may fail to see such investments is that the program was not successful in relieving financial constraints. We investigate this possibility by studying the effect of the program on income from different sources. The bottom panel of Table 4 shows treatment effects of the cash transfer program on participants' income in March 2012 (March 2013), obtained during the midline (endline) survey in April 2012 (2013).

Overall, we find that treatment group income in March 2012 is €39 higher than that of the control group (€602 on average). The program only marginally increases the resources of participants, despite the fact that, in month 12 of the program, subjects were still entitled to the maximum theoretical transfer amount of €250. This is not a concern in itself, if the transfers allowed participants to move away from short-term, low-skill work, and invest more into their future. However, the table further shows that income from the JYC increased by only €88 on average. Substitution of income between sources is present but moderate: The increase in JYC income is associated with a decrease of €47 in other income, consisting of employment income (€21, not significant), family and friends (€9), and other non-work sources (€10). What, then, explains the gap between the observed income from the JYC and the theoretical transfer of €250? A more detailed look at the data reveals a combination of income tapering and imperfect compliance: The 82% who accepted the cash transfer program received €125 from the JYC, and earned €435 (465 - 30) from work. Income tapering reduces their transfer amount by $0.24 * 435 = €104$. This accounts for €229 (125 + 104) of the transfer. The remaining difference is explained by the fact that 6.4% of cash transfer participants dropped out of the program before month 12.

Our results suggest that participants largely chose not to substitute away from existing sources of employment income, and thus received reduced cash transfers. Two explanations are possible: First, financial constraints are not a significant barrier to human capital investment. Rather, young people stay in low-skill occupations out of choice. Second, financial constraints do prevent youths from investing in human capital. However, the cash transfers are simply not large enough to overcome them, thus forcing youth to remain in low-skill occupations for subsistence.

We find suggestive evidence for the second explanation: Young people seem to face significant financial constraints, and the cash transfer program does not measurably alleviate them (Tables 5). In the midline survey after one year, 27.7% of participants reported having

had trouble to pay bills in the past 12 months, 24.4% forwent medical care for financial reasons (despite a heavily subsidized public health care system), 13.7% forwent training, 45% overdrafted their bank account, and 19.4% went a day without a meal due to lack of money (Table A8) . None of these outcomes are significantly affected by the cash transfer program. While the existence of financial constraints does not prove that they are binding for human capital investment, our results do suggest that the cash transfer program may have been too small to significantly impact participants’ economic situation.

So where did the extra income go, and what about other measures of wellbeing? The midline survey additionally elicited key expenditures, including ‘temptation goods’ like nights out, restaurants, tobacco, and phones (not included in the endline survey). We find precisely estimated zero impacts across this spending category, as well as on the size of the largest expenditure in the last 12 months. In contrast, treated participants were 5 percentage points more likely (control mean: 45%) to report putting savings aside since the start of the program. In the three months leading up to the midline survey, moreover, treated subjects saved €37 more, on average, than the control group, a difference of 18%. Remembering the €39 increase in overall income (Table 4), it appears that participants used the cash transfers to increase neither their consumption nor their investment, but simply saved the surplus. This may seem puzzling, but it is consistent with the possibility of financial constraints to human capital investments: the cash transfers are too small to allow youths to abandon low-skill jobs and start training courses, but they are sufficient to allow saving for such investments. This explanation requires either income-generating activities or human capital investments to be indivisible. We explore this and other mechanisms theoretically and empirically in Section 5.

5 Evidence for Mechanisms

The following section seeks to understand the mechanisms behind the observed treatment effects (or the lack thereof). We start by introducing a theoretical framework using a modified principal-agent model. We discuss various reasons why the agent may underinvest in effort (human capital investment) relative to the principal, even when both care equally about the outcome (employment). We derive empirical predictions for each, and test these using our data.

5.1 Theoretical Framework: Benchmark Case

Consider a task – finding employment – which requires two successive levels of effort. First, the agent needs to meet with the caseworker, $e_1 \in \{0, 1\}$, incurring a cost $\psi_1 \equiv \psi(e_1)$.

During the meeting, she learns about the required second step to find a job, $e_2 \in \{0, 1\}$, which costs an additional $\psi_2 \equiv \psi(e_1 + e_2) - \psi(e_1)$. Effort e_2 can be thought of as the specific training, apprenticeship, or direct job search that is most suitable for the jobseeker.¹⁰ Jointly exerting e_1 and e_2 results in a probability of employment $P(\bar{Y}|e_1 = e_2 = 1) \equiv \pi_2$, while the baseline probability of finding a job without effort is $P(\bar{Y}|e_1 = e_2 = 0) \equiv \pi_0$. We denote by $\Delta\pi = \pi_2 - \pi_0$ the difference between the two. Going to the meeting by itself does not increase the probability of employment: $P(\bar{Y}|e_1 = 1, e_2 = 0) \equiv \pi_1 = \pi_0$. Thus, exerting e_1 without e_2 is strictly dominated (this will change with cash transfers). Finally, we denote by \bar{Y} the value to the agent of finding a job, and by \underline{Y} the outside option of unemployment. In a static, risk-neutral benchmark case, the agent invests in effort e_1 and e_2 if

$$\pi_2 \bar{Y} + (1 - \pi_2) \underline{Y} - \psi_1 - \psi_2 \geq \pi_0 \bar{Y} + (1 - \pi_0) \underline{Y} \quad (4)$$

and thus if

$$\Delta\pi(\bar{Y} - \underline{Y}) \geq \psi_1 + \psi_2 \quad (5)$$

Inequality 5 represents the optimality condition for a risk-neutral social planner, who shares the agent's valuation of employment as well as effort disutility. In the following subsections, inequality 5 will serve as a benchmark for evaluating underinvestment in effort. While distinct from a principal-agent model in that the agent directly cares about Y , and the social planner cares about effort disutility, there are parallels in that the social planner and the agent may disagree about the optimal effort level.¹¹ The social planner is able to contract on some types of effort (attending meetings), but not on others (sending job applications). Specifically, the social planner will be able to offer a transfer t conditional on exerting e_1 , but cannot contract on e_2 .

5.2 Risk and Time Preferences

An immediate channel for underinvestment relative to the preferences of a patient and risk-neutral social planner, is diverging risk and time preferences. Effort costs are certain, finding

¹⁰We keep effort cost ψ_2 deterministic and constant here. It is plausible to model ψ_2 as a stochastic draw from a distribution $F(\psi_2)$: By meeting the caseworker (exerting e_1), the agent learns how much effort will be required to find a job. Whether effort e_1 is exerted is a function of the prior $E(\psi_2)$. Effort e_2 is then exerted for ψ_2 realizations below a threshold value $\bar{\psi}_2$. A cash transfer conditional on e_1 makes more people learn ψ_2 , and thus exert e_2 iff it is cheap enough. Because the meeting cost ψ_1 is sunk at the point of exerting e_2 , the cash transfer does not affect the threshold value $\bar{\psi}_2$. Summing up, allowing for stochastic draws from the effort cost distribution would mostly predict heterogeneity in treatment effects by effort type: "Cheap effort" like web search and job applications should increase, while "expensive effort" like long-term training and apprenticeships should not respond. Because we do not see any heterogeneity by effort type, we abstract from this possibility, and assume for simplicity that ψ_2 is deterministic.

¹¹If the social planner did not care about effort disutility, underinvestment in effort would follow trivially.

a job is risky. Also, the benefits of employment are likely to accrue with some delay. It is straightforward to add concave utility $u(c)$ to inequality 4. Utility is assumed to be additively separable between consumption and effort, $u(c) - \psi(e)$. We assume that a human capital investment yields returns (if any) in τ periods, which are discounted by a factor D_τ . While searching, the agent consumes \underline{Y} . The agent exerts effort iff

$$D_\tau[\Delta\pi(u(\bar{Y}) - u(\underline{Y}))] \geq \psi_1 + \psi_2. \quad (6)$$

Comparing inequality 6 to a risk-neutral and patient social planner or principal, the agent underinvests in effort. Underinvestment increases in discounting and risk aversion (formally, effort increases in D_τ and $u''(c) < 0$).

5.2.1 Adding Conditional Cash Transfers

The social planner can offer a transfer t conditional on exerting effort e_1 (meeting attendance), but cannot contract on e_2 (human capital investment or job search). The transfer is immediate, certain, and large enough to make e_1 dominant: $u(\underline{Y} + t) - \psi_1 > u(\underline{Y})$. The agent is willing to additionally exert e_2 iff

$$D_\tau[\Delta\pi(u(\bar{Y}) - u(\underline{Y}))] \geq \psi_2. \quad (7)$$

The transfer simply acts to eliminate the effort cost of the meeting ψ_1 from the incentive constraint – the exact size of the transfer is irrelevant. By covering part of the total effort cost of human capital investment, $\psi_1 + \psi_2$, the transfer moves the agent closer to the margin. To the extent that less risk averse and more patient agents are closer to the margin (as they are closer to the social optimum), they are more likely to respond to the transfer.¹²

5.2.2 Theoretical Predictions: Risk and Time Preferences

The predictions of the model can be summarized as follows:

1. Impatience and risk aversion negatively predict human capital investment and job search.
2. Transfers conditional on meeting attendance will increase human capital investment, job search, and employment.¹³

¹²The model abstracts from the role of transfers as unemployment insurance: Transfer are paid during an initial search period, but not during the subsequent period of employment realizations. If agents received $\underline{Y} + t$ as an outside option to \bar{Y} , transfers would mechanically crowd out e_2 , and thus employment. Such crowd-out effects are both plausible and observed in our data, but they are temporary – transfers are limited in time. This modelling choice thus represents a long-term view of human capital investment and job search.

¹³This holds for the indivisible investments (binary e_2) considered in the model, like apprenticeships. It

3. Investments will respond more to transfers if agents are more patient or less risk-averse. This follows from their higher valuation of improved employment prospects.

5.2.3 Empirical Evidence: Time Preferences

Appendix D shows heterogeneous treatment effects across available measures of candidate mechanisms, estimated using equation 3. Unfortunately, we do not have a measure of risk preferences. However, the predictions in Subsection 5.2.2 are closely aligned for risk and time preferences. The predictions for time preferences can be studied using a simple proxy for patience from the baseline survey: Participants were asked whether they were willing to wait a given amount of time (between 6 months and one day) to receive a €250 prize, when the alternative is to receive €200 today. The left panel of Table A11 shows estimated treatment effects of the cash transfer on relatively ‘impatient’ participants – those with a below-median willingness to wait (60 days or less). The right panel shows treatment effects for relatively ‘patient’ participants (more than 60 days). The last column of Table A11 tests for equality of treatment effects across the two subgroups.

Prediction 1 requires a comparison of means between the two subgroups in Table A11: As expected, more patient jobseekers invest significantly more in human capital (through apprenticeships or trainings) than their impatient counterparts. In contrast, they invest significantly less into job search. This stands in contrast with our highly stylized model, but it is intuitive when allowing differential delays of returns to effort e_2 : Human capital investments have more delayed returns than immediate job search, and will thus be relatively preferred by more patient agents.

We find no support for Predictions 2-4. In particular, key outcomes (human capital, search and employment) do not increase with the transfers, and there is no heterogeneity across subgroups. We find support for a crowd-out of employment among patient participants, but the difference is not significant. Overall, the available evidence does not support time preferences as a binding constraint to human capital investment.

5.3 Financial Constraints

Suppose instead that the agent is prevented from investing in effort simply because she cannot afford to. Specifically, suppose there is a minimum subsistence constraint c_L with $u(c) = -\infty$ for $c < c_L$. The agent needs to work in informal or low-skilled labour to earn c_L , with a time cost of ψ_L . An easy way to incorporate this subsistence constraint is to microfound the utility from unemployment as $\underline{Y} \equiv u(c_L) - \psi_L$. The effort cost of human

also extends to divisible investments (continuous e_2) – like the number of job applications – if returns and costs are linear in effort.

capital investment, $\psi_1 + \psi_2$, also represents a time cost (e.g., of participating in vocational training). The agent faces a time budget T which makes it impossible to invest in human capital and low-skill labour at the same time:

$$\psi_1 + \psi_2 \leq \psi_1 + \psi_L \leq T < \psi_1 + \psi_2 + \psi_L \quad (8)$$

Human capital investment continues to be socially optimal if the expected return in the employment period is larger than the foregone consumption in the search period:

$$\Delta\pi(\bar{Y} - \underline{Y}) \geq \underline{Y} + \psi_1 + \psi_2 \quad (9)$$

However, the subsistence constraint means this is not an option: Human capital investment is risky given $\pi_2 < 1$, and any chance to incur $U(c) = -\infty$ is unacceptable. Thus, the agent exerts ψ_L and obtains c_L , despite lower returns. As in the benchmark case, exerting e_1 without e_2 is strictly dominated.

5.3.1 Adding Conditional Cash Transfers

The social planner offers a transfer $t \geq \psi_1$ conditional on exerting effort e_1 . If $t \geq c_L$, the subsistence condition is satisfied with meeting attendance. Since human capital investment has higher returns than low-skilled labour (inequality 9), the agent exerts e_1 and e_2 .

More plausibly, the conditional cash transfer covers only part of the subsistence constraint, $t < c_L$. With the current assumption of an indivisible c_L costing ψ_L (such as low-skill or seasonal work projects with a minimum time commitment), the cash transfer has no effect on human capital investment: As long as the agent cannot afford to give up c_L , she cannot invest in e_2 . Meeting attendance e_1 will respond given $\psi_1 + \psi_L \leq T$. Note that divisibility of e_2 is irrelevant: The amount of job search that the agent can fit into the time constraint is not affected by the transfer. It is worth considering an extension where low-skill work ψ_L is divisible, akin to an hourly wage: $\lambda\psi_L$ yields λc_L for $\lambda \leq 1$. Keeping e_2 indivisible, the transfer needs to be sufficiently large to reduce subsistence work to a level that leaves enough time for human capital investment: e_2 will respond iff

$$t > c_L \left(1 - \frac{(T - \psi_1 - \psi_2)}{\psi_L}\right) \quad (10)$$

Finally, suppose that both low-skill work and human capital investment are divisible.¹⁴ Given higher proportional returns for human capital investment e_2 is now guaranteed to respond. The agent chooses γ to make the time constraint $\frac{c_L - t}{c_L}\psi_L + \psi_1 + \gamma\psi_2 = T$ hold with equality.

¹⁴Formally, an effort cost of $\psi_1 + \gamma\psi_2$ increases the probability of finding employment by $\gamma\Delta\pi$ for $\gamma \leq 1$.

5.3.2 Theoretical Predictions: Financial Constraints

The predictions of the model can be summarized as follows:

1. Financial constraints negatively predict human capital investment and job search, with a stronger impact on more time-consuming activities.
2. If income-generating activities during unemployment are indivisible, human capital investment will not react to a transfer that less than perfectly covers subsistence consumption.
3. If income-generating activities during unemployment are divisible, then divisible employability investments (like job search) will respond to the conditional cash transfer. Indivisible investments (like apprenticeships) will respond if the transfer is sufficiently large.

5.3.3 Empirical Evidence: Financial Constraints

To identify the subsample of participants most likely to experience financial constraints, we use the financial constraints index collected in the midline survey (Table A8). To address endogeneity to treatment status, we predict this index for control group subjects, using only administrative variables that existed prior to the study. We then extrapolate these predictions to the treatment group, and split the sample into two subgroups using the median control value.

Table A12 shows treatment effect heterogeneity by probability to face financial constraints. Effects are broadly similar across groups. In line with Prediction 1 for financial constraints, mean human capital investment is lower for financially challenged youths. This difference reverses for job search. However, the difference in means is explained with control variables and JYC fixed effects, and is thus not significant. In line with Prediction 2, but not Prediction 3, human capital and search effort do not respond to cash transfers, with no differential effect. We observe a significant 7-8 percent reduction in short-term employment in both groups. While there is no treatment effect heterogeneity on key outcomes, the probability of facing financial constraints itself is strongly reduced (39 percent) by the cash transfer, and only in the group most likely to face them. Overall, our results are consistent with either (a) financial constraints not being a barrier to employability investments, or (b) financial constraints being a barrier, but low-skill work being indivisible, and transfers being too small to cover subsistence needs.

5.4 Returns to Effort

A large class of possible frictions is contained in the mapping from e_2 to \bar{Y} . So far, we assumed that the agent learns during the caseworker meetings (e_1) which human capital or employability investments (e_2) are most suitable to help her find employment (\bar{Y}). The return to these investments is captured in $\Delta\pi = \pi_2 - \pi_0$, the increase in the probability to find a job. The agent and the social planner may disagree about $\Delta\pi$ for various reasons:

1. **Program quality:** The model captures program quality in the information which the caseworkers give to the jobseekers. If the caseworkers recommend human capital investments which are not suitable for the jobseeker, and will not lead to higher chances of employment, then $\Delta\pi$ may be small or zero. It is conceivable that the jobseeker, but not the central planner, realizes that the proposed investments are not profitable.
2. **Perceived program quality and trust:** Independent of the true quality of the program, the jobseeker may *perceive* the quality to be low. For instance, the jobseeker may not trust the system overall, and the caseworker in particular, to have the jobseeker's interests in mind. As a result, she may believe that the human capital investments suggested by the caseworker are not privately profitable. Importantly, the agent's investment decision depends on her belief $\Delta\tilde{\pi}$ rather than on the true value.
3. **Internal beliefs and locus of control:** An increasing body of evidence points to the importance of agents' beliefs about themselves for economic behaviour (Bernard et al. 2018; McKelway 2018; Haushofer et al. 2019). Jobseekers may believe that their life is not in their own hands, and that hiring decisions depend on external factors rather than their own actions. They may also believe that they would not be able to complete a given investment (say, an apprenticeship) in the first place. In this simple model, such beliefs about agency and ability are also captured in $\Delta\tilde{\pi}$.
4. **Labor market conditions:** Finally, labour market conditions for the target population may be difficult, for instance due to an excess supply of low-skilled workers. Labor market conditions enter the model through the probability of finding a job, π_0 and π_2 . A low baseline probability π_0 does not affect the model in any way, conditional on the return to investment $\Delta\pi$.¹⁵ Labor market conditions become relevant to the extent that they affect the return to investment, $\Delta\pi = \pi_2 - \pi_0$.

¹⁵This holds true even with risk aversion, as expected utility is linear in probability.

5.4.1 Adding Conditional Cash Transfers

With a low believed return $\Delta\tilde{\pi}$, employability investments e_2 are unlikely to be optimal:

$$\Delta\tilde{\pi}(\bar{Y} - \underline{Y}) \geq \psi_1 + \psi_2, \quad (11)$$

Objective underinvestment depends on whether $\Delta\tilde{\pi}$ reflects the true return $\Delta\pi$. Offering a transfer t conditional on meeting attendance e_1 changes the investment condition to

$$\Delta\tilde{\pi}(\bar{Y} - \underline{Y}) \geq \psi_2. \quad (12)$$

As in previous subsections, the transfer moves the agent closer to the margin by covering the cost ψ_1 , regardless of the exact size of the transfer.

5.4.2 Theoretical Predictions: Returns to Effort

The predictions of the model can be summarized as follows:

1. Any variable that affects perceived or real returns to effort negatively predicts human capital investment and job search. Examples include program quality, perceived quality and trust, locus of control and internal beliefs, as well as labor market conditions which affect the return to search effort.
2. Low real returns, but not low perceived returns, predict the absence of a relationship between human capital investment and employment.
3. Transfers conditional on meeting attendance will increase human capital investment, job search, and employment, at the margin.
4. To the extent that agents with low (perceived) returns are farther away from the margin of investment, there will be treatment effect heterogeneity by measures of (perceived) returns to effort. Low (perceived) returns predict smaller treatment effects.

5.4.3 Empirical Evidence: Program Quality

Since all participants are exposed to the career guidance program (G), the program itself cannot be evaluated directly. However, program quality is largely determined by the information and the services provided by the caseworkers. We derive a proxy for caseworker quality, using the idea that jobseekers will not return to a caseworker who provides poor or unsuitable information, does not listen to the jobseekers' situation, or does not target services to their specific needs. We obtain the universe of administrative jobseeker records from 2010, the year before the experiment started, and group jobseekers by the caseworker they

were paired with. Caseworker quality is then measured as the proportion of jobseekers who drop out of the program after first meeting their caseworker, one year before the experiment. We de-mean this proportion at the JYC level to allow for different jobseeker populations, and exploit the fact that the allocation of jobseekers to caseworkers within a JYC is quasi-random. "High quality" then indicates that a caseworker had a below-average proportion of drop-outs, relative to his or her JYC. Unfortunately, our pre-experimental administrative data can only be matched to the caseworkers of two thirds of our participants, which reduces the sample size for this analysis. Table A13 shows treatment effect heterogeneity using the caseworker quality proxy.

Prediction 1 of Subsection 5.4.2 finds support in the data: Average human capital investment and average search are higher with high-quality caseworkers. This difference is significant for human capital investments (controlling for JYC fixed effects), but not for search. In contrast to Prediction 2, there is a very strong positive relationship ($p < 0.01$, not shown in the table) between human capital investment and employment volume, both after 1 and 2 years. This is consistent with high real program returns, assuming that (i) the observed human capital investments are those recommended by the caseworkers, and (ii) abstracting from the obvious endogeneity of human capital investment. We find no support for Predictions 3 and 4.

5.4.4 Empirical Evidence: Locus of Control

Table A14 studies heterogeneity by a measure of locus of control, which captures participants' internal beliefs about their returns to exerting effort. In line with Prediction 1, mean human capital investment is significantly higher for those with an internal locus of control – i.e., for those who believe their life is shaped by their own actions, rather than by external factors. Interestingly, and similar to the findings for patience, search effort is significantly lower for those with an internal locus, consistent with a more long-term focus on building human capital. Prediction 2 does not apply to perceived, as opposed to real, returns. Again, there is little support for Predictions 3 and 4 in the data, though those with an internal locus are proposed and matched with more trainings, likely due to more openness towards them. Short-term employment decreases by 10 percent (0.25 months) for those with an internal locus, consistent with a short-term focus on human capital building rather than immediate job search. Employment after 2 years is unaffected. Outside the predictions of the model, it is notable that income increases from the transfer are entirely driven by participants with an external locus of control. This makes sense: Those with an internal locus are more likely to actively manage their income sources, and potentially crowd out or supplement income as needed. In contrast, those with an external locus are more likely to surrender their financial situation to external circumstances, in this case receipt of the conditional cash transfer.

5.4.5 Empirical Evidence: Labor market conditions

In order to proxy the labor market conditions faced by our jobseekers, we obtained administrative records of the local youth unemployment rate, specific to each JYC catchment area. The variation is substantial: The unemployment rate for 16-25 year olds ranges from 12.7 to 58.4 percent across JYCs, with the median jobseeker facing an unemployment rate of 25 percent. In terms of the model, the local unemployment rate is a determinant of π_0 , and potentially affects $\Delta\pi$. Table A15 studies heterogeneity by whether jobseekers face a local unemployment rate above or below the median.

We find that mean human capital investment does not differ significantly with the local unemployment rate. However, search effort is substantially higher in tougher labor market conditions, consistent with a lower $\Delta\pi$ but strong income effects. Consistent with tougher conditions forcing youths to search more, more search effort negatively predicts employment in the overall sample (not shown in the table). As with other candidate mechanisms, we find little support for heterogeneous treatment effects of cash transfers on effort and employment. However, meeting attendance increases significantly more in areas with high unemployment, and income increases only in those areas. The evidence suggests that jobseekers in high-unemployment areas have few alternatives to searching for employment, and are heavily reliant on external financial help.

5.4.6 Empirical Evidence: Labor market connectedness

We additionally examined heterogeneity in terms of a jobseeker's connection with the labor market. We measured how connected participants were based on the fact that the guidance program has two tracks: a standard track and an intensive one, with more frequent meetings and closer monitoring. The intensive track is reserved for young adults identified by case-workers as having particularly serious integration issues when they enroll. We thus consider subjects enrolled in the intensive guidance program to be more disconnected from the labor market. Table A16 presents our findings. As in the previous table, estimated effects in both groups are very similar. A key exception is human capital investment: While disconnected jobseekers invest less at baseline than better connected jobseekers, they invest significantly more when provided with cash transfers. The difference is large enough to compensate for the baseline gap, i.e. jobseekers in the intensive track catch up with those in the standard track, if they are additionally provided with transfers. However, this effect does not translate into higher employment after 24 months.

To test for possible heterogeneity in outcomes between men and women, we conducted analogous heterogeneity analyses by gender (Table A17). We find no significant difference

between men and women with respect to the considered outcome variables.

5.5 Summary of Evidence on Mechanisms

Summing up the available evidence, we find that employability investments are significantly predicted by measures of patience and locus of control, the presence of financial constraints, and proxies for caseworker quality. All of these may act as barriers to enrollment in vocational training and other human capital investments.

Yet, we observe little evidence that these barriers are significantly affected by conditional cash transfers. Two possible explanations for the absence of effects are insufficient conditionality, and insufficient transfer size.

Recall that the transfer acts by eliminating the cost of guidance program participation (meetings) from the incentive constraint. Guidance by caseworkers is assumed to be a pre-requisite for identifying suitable employability investments. Transfers will fail to increase investment if the cost of program participation is insubstantial relative to the cost of the investments themselves. An example are large and indivisible investments such as vocational training programs and apprenticeships: The jobseeker may now attend the guidance meetings and identify suitable investments. However, the perceived cost of the investment itself may outweigh the perceived benefits, due to barriers including impatience and present bias, or an external locus of control. This argument is less applicable for job search effort, which is more divisible. However, guidance program participation is arguably less of a pre-requisite for engaging in job search than it is for human capital investments. If jobseekers do not rely on guidance to apply for jobs, then the incentive constraint for job search will not be affected by the transfers. Either of these cases boils down to *insufficient conditionality*: Only a conditioning of transfers directly on employability investments will meaningfully change behaviour. Note that an increase of transfers by itself will have no effect.

A second, qualitatively distinct explanation are financial constraints in combination with *insufficient transfers*: Financial constraints can explain the lack of an effort response if income-generating activities are not perfectly divisible, and the cash transfer covers less than the subsistence needs. In support of this, we find evidence that mean human capital investment is lower for financially challenged youths, and that transfers reduce financial constraints (though perhaps not sufficiently). If financial constraints are the *only* barrier to investment, then conditionality is unnecessary.

The evidence presented in this section cannot conclusively distinguish between insufficient conditionality and insufficient transfer size in explaining the absence of treatment effects. However, the strong predictive power of barriers including impatience and locus of control on one side, and financial constraints on the other, suggests that jobseekers may face mul-

multiple barriers to investment. In this case, a combination of higher transfers with stronger conditionality may be indicated. This prediction is supported by the French government's choice of a successor policy, the "Garantie Jeune", which nearly doubled the transfer size while imposing much stronger conditions. Similar policy directions have been observed in other countries, including Germany and Australia.

6 Robustness

The midline and endline surveys provide useful information in addition to the administrative records, but their response rates are weaker and differential between the treatment and the control group (see Table 6). This calls into question the internal and external validity of the results obtained from the surveys.

This issue can be addressed by looking at Tables 1 and 6. Table 6 uses a post-double-selection Lasso approach to show that the response rate is significantly linked to individual characteristics found in the administrative records. However, treatment does not interact with observable characteristics in predicting attrition. Table 1 confirms that treatment and control groups are balanced on observables in both surveys, based on administrative records before the study began. This means that external validity may be compromised to some extent, while internal validity is maintained.

We proceed by carrying out additional analyses. Results appear in Table A2. The first set of columns recalls the results of the specification discussed above: the estimate of equation (1) when control variables are introduced, the results of which are presented in certain tables in section 4. The second set of columns presents the results of the estimate of equation (1) without control variables. The third set presents the results obtained by correcting for sample selection bias resulting from non-response, using the method developed by Behaghel et al. (2015). In this procedure, only individuals who were reached after a certain number of attempts are included in the treatment group (which has a higher response rate), so that final response rates in both groups are identical (18 calls for the midline survey, with a 59 % response rate in treatment and control groups – see Figure A1 b). The two last sets of columns present bound estimates, as developed by Lee (2009).¹⁶

Results show that the Lee bounds are not very informative: estimated intervals are very large. Most of the time, they include zero, and when they do not, it is clear, given the standard errors, that the confidence interval for at least one bound would systematically include zero. Results converge well with the three alternate estimation methods used. Rebalancing response rates in the treatment and control groups, in particular, yielded very similar results

¹⁶These last estimates do not include control variables or JYC dummies.

to those obtained without doing so. Lastly, results obtained using estimates without control variables are coherent with those obtained for the two other procedures (not significantly different), but point estimates differ slightly. Nevertheless, the same conclusions apply to all variables in the Table.

7 Conclusion

In this study, we examine the impact of a conditional cash transfer introduced in 2011 to supplement the traditional career guidance program offered by JYCs to young, unskilled French jobseekers. The French government implemented the cash transfer program to encourage these jobseekers to take advantage of the JYCs' services, as well as to push them to invest in their job searches and in improving their human capital. The program thus represents an attempt to provide these jobseekers with the financial means to invest in their employability, thus breaking their reliance on low-skill jobs offering subsistence-level wages and little by way of new experience.

Our results demonstrate that receiving the conditional cash transfer does induce higher participation among jobseekers in the career guidance program offered by the JYC. Specifically, jobseekers attend more meetings with their assigned career counselor. As a result, they are offered more opportunities to partake in trainings and other activities designed to improve their employability. However, cash transfer recipients do not seize them: Despite receiving more placement offers and recommendations from their counselors, no statistically significant difference exists between the takeup rates of transfer recipients and jobseekers participating in the JYC guidance program without this monetary benefit.

We also observed a lower rate of participation on the labor market in the first six months of the program. While this trend may be due to a well-known "locking-in" effect, the fact that no difference is observed in participants' real commitment to the program in terms of taking part in training courses or career planning activities suggests that transfers curiously act as a disincentive to finding new employment. The program is very expensive nevertheless. The program's additional transfer alone increases the per-person cost of the program by €1,868, from €264 to €2,132. Approximately 170,000 people enroll in the program every calendar year. The additional cost is therefore $1,868 * 170,000 = €318$ million. The program also offered 6.5 additional meetings per participant, increasing the total from 8.1 to 14.6. For 170,000 jobseekers, this would represent an additional 1.1 million meetings were offered. We were unable to quantify the cost of these additional meetings for the JYCs.

One of the main findings of the study was the key effects of financial incentives on the behavior of young jobseekers in the program. Participation in the program is one example;

limited rates of employment in the early stages of the program is another. Program impact stops where the incentives stop and no differences are found between subjects with financial difficulties and subjects without. In theory, the conditional nature of transfers should have affected commitment to the program, career planning and the completion of the various steps of the career plan. In practice, however, implementing transfers on a conditional basis is quite complicated. The only real conditionality associated with the transfers stemmed from the requirement to meet with counselors.

An alternative model could take conditionality to the next level, for example by paying part of the transfer only once significant steps towards finding employment are accomplished. Babcock et al. (2012) suggest using such a mechanism in the more general context of unemployment insurance. Our results are consistent with this mechanism in that they illustrate the risk of incentives remaining a half measure. Nevertheless the actual form this strategy should take is not clear. Results obtained on a theoretical level by Benabou and Tirole (2003), as well as evidence found in an experimental setting (Ariely et al., 2009) suggest that a financial reward for efficiency can be counterproductive. Providing incentives sends a signal which people try to interpret, inferring things about a hidden part of themselves or about what they are being encouraged to do. Political discourse conveys both the idea of making transfers conditional and of giving young seekers independence. One of the initial models presented in the 2009 Green Paper on Youth involved providing young jobseekers with a lump sum paid when they achieved specific stages of their career plan, to both increase the perceived return and to make career-related achievement easier.

Nor is it certain that conditional transfers are the only option to explore. Blattman et al. (2014) show that in Uganda, providing poorly educated young people with transfers to finance existing projects that have been identified as promising yields very good results. Recipients benefit from training, invest in the physical capital of a revenue-generating activity and increase their long-term income substantially. Of course, these results are linked to different populations and contexts and cannot be directly transposed onto the context of young school drop-outs in underprivileged French suburbs. They are proof, however, that alternative methods can work. The initial findings of studies conducted on this same sample do tend to confirm that healthcare, housing and mental health support programs – accessible to young jobseekers while they are in the labor market and form an opinion of public institutions – can have a significant impact on training and integration.

The main conclusion of our study is that the right way to improve incentives to invest in employability remains to be found. This is a crucial issue with serious implications for society and various alternative models must be tested rigorously. Stopping in midstream, as does the program studied here, may come with more drawbacks than advantages.

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Table 1: Balance across Survey Samples

	Total participants			Midline Survey			Endline Survey		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Demographics</u>									
Age	19.7 (1.3)	0.0 (0.0)	5,488	19.8 (1.3)	0.0 (0.1)	3,413	19.8 (1.3)	-0.0 (0.1)	2,309
Male	48.2 (50.0)	0.5 (1.4)	5,488	45.7 (49.8)	1.5 (1.8)	3,413	46.2 (49.9)	0.9 (2.2)	2,309
Foreigner	4.6 (20.9)	0.2 (0.5)	5,488	4.3 (20.3)	-0.0 (0.6)	3,413	3.7 (18.8)	1.1 (1.0)	2,309
Non married	92.2 (26.8)	-0.5 (0.9)	5,488	92.1 (26.9)	0.2 (1.0)	3,413	93.6 (24.5)	-0.3 (1.1)	2,309
Has children	4.0 (19.6)	1.3** (0.6)	5,488	3.3 (18.0)	2.4*** (0.9)	3,413	3.2 (17.7)	1.1 (0.8)	2,309
<u>Housing and resources</u>									
Parents	62.1 (48.5)	-1.5 (1.4)	5,488	65.5 (47.6)	-0.7 (1.7)	3,413	68.5 (46.5)	-1.6 (2.3)	2,309
Other family	9.9 (29.9)	0.6 (0.8)	5,488	9.3 (29.0)	0.2 (0.9)	3,413	8.8 (28.4)	-0.7 (1.1)	2,309
Self	15.7 (36.4)	0.7 (1.1)	5,488	15.0 (35.7)	0.6 (1.3)	3,413	13.2 (33.9)	1.6 (1.6)	2,309
Friends	5.6 (23.0)	-0.4 (0.8)	5,488	4.8 (21.4)	-0.6 (0.8)	3,413	4.3 (20.3)	-0.7 (0.8)	2,309
Precarious	3.4 (18.1)	0.4 (0.6)	5,488	2.4 (15.3)	0.3 (0.7)	3,413	1.7 (12.9)	0.7 (0.6)	2,309
Has resources	16.0 (36.7)	0.8 (1.0)	5,488	16.2 (36.9)	0.0 (1.3)	3,413	15.6 (36.3)	0.5 (1.6)	2,309
Amount	74.6 (215.9)	1.6 (5.4)	5,488	78.9 (223.6)	-7.5 (7.8)	3,413	75.5 (215.3)	3.0 (9.7)	2,309
Medical insurance	43.0 (49.5)	-1.5 (1.2)	5,488	44.7 (49.7)	-2.2 (1.5)	3,413	46.6 (49.9)	-1.6 (1.8)	2,309
<u>Diploma</u>									
Driving license	30.6 (46.1)	-1.8 (1.1)	5,488	34.3 (47.5)	-3.6** (1.4)	3,413	34.9 (47.7)	-2.9 (1.8)	2,309
Above high-school	2.4 (15.2)	-0.4 (0.3)	5,488	2.9 (16.8)	-0.9** (0.5)	3,413	3.2 (17.7)	-1.4** (0.6)	2,309
High-school diploma and eq	29.6 (45.7)	-0.7 (1.2)	5,488	34.1 (47.4)	-1.3 (1.8)	3,413	37.2 (48.3)	-1.3 (2.3)	2,309
Vocational	26.4 (44.1)	0.6 (1.2)	5,488	28.1 (45.0)	-0.9 (1.8)	3,413	28.2 (45.0)	-1.4 (2.4)	2,309
Dropout vocational high-school	33.9 (47.3)	-0.1 (1.4)	5,488	29.4 (45.6)	1.9 (1.9)	3,413	25.7 (43.7)	3.5 (2.4)	2,309
Leave school at 16	7.7 (26.6)	0.6 (0.7)	5,488	5.4 (22.5)	1.3 (0.8)	3,413	5.6 (23.1)	0.5 (1.1)	2,309

Administrative records. The table has three set of columns. In each set the control mean variable appears first, then the difference between treatment and control resulting from the estimation of equation (1) and lastly the number of observations. Below each variable, its standard deviation appears into parentheses. The first set of column considers the whole sample, the second set respondents to the midline survey and the last one respondents to the endline survey. Standard errors are robust to heteroskedasticity and are clustered at the Job Youth Center level. * corresponds to parameter significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 2: Results from Administrative Data

	Trimester 1			Semester 1			Year 1			Total after 2 years		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Program enrollment and meetings</u>												
Individual meetings	1.82 (1.94)	1.25*** (0.11)	5,482	2.92 (2.81)	2.02*** (0.17)	5,482	5.82 (4.56)	3.12*** (0.24)	5,482	8.23 (7.17)	5.07*** (0.43)	5,482
Still in program	95.54 (20.64)	0.88 (0.58)	5,482	88.19 (32.28)	5.85*** (1.13)	5,482	39.78 (48.95)	39.99*** (4.22)	5,482	15.77 (36.45)	51.04*** (1.81)	5,482
Final cumulative transfers from JYC							236.53 (381.94)	1,528.32*** (87.29)				5,482
<u>Services received from the JYC</u>												
Information about a job offer	3.02 (5.10)	1.60*** (0.29)	5,482	4.61 (6.82)	2.45*** (0.36)	5,482	7.42 (10.46)	4.09*** (0.55)	5,482	11.40 (16.58)	6.31*** (0.90)	5,482
Information about a training	0.89 (1.55)	0.36*** (0.08)	5,482	1.51 (2.34)	0.71*** (0.13)	5,482	2.64 (3.86)	1.16*** (0.22)	5,482	3.96 (5.70)	1.79*** (0.33)	5,482
Match with a job offer	0.47 (1.23)	0.13*** (0.05)	5,482	0.72 (1.66)	0.23*** (0.06)	5,482	1.10 (2.30)	0.45*** (0.10)	5,482	1.69 (3.29)	0.70*** (0.14)	5,482
Match with a training	0.15 (0.48)	0.05*** (0.02)	5,482	0.29 (0.69)	0.11*** (0.03)	5,482	0.53 (1.10)	0.17*** (0.04)	5,482	0.82 (1.65)	0.22*** (0.06)	5,482
<u>Offers taken up and employment as recorded by the JYC</u>												
Training	0.50 (1.02)	0.01 (0.02)	5,482	0.83 (1.62)	0.03 (0.05)	5,482	1.57 (2.67)	0.11 (0.07)	5,482	0.02 (0.03)	0.00 (0.00)	5,482
Human capital investment	0.62 (1.09)	0.02 (0.03)	5,482	1.09 (1.80)	0.04 (0.05)	5,482	2.27 (3.29)	0.18 (0.11)	5,482	0.02 (0.03)	0.00 (0.00)	5,482
Months with employment	0.70 (1.13)	0.04 (0.03)	5,482	1.53 (2.13)	0.06 (0.06)	5,482	2.77 (3.69)	0.17* (0.09)	5,482	0.03 (0.04)	0.00* (0.00)	5,482

Data: Administrative records.

Notes: The table provides Intention To Treat estimates for variables related to services provided by the Job Youth Center (JYC). Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has four vertical panels. The left panel provides events recorded over the first quarter following random assignment, the second panel information recorded over the first semester, the third panel over the first year, and the last panel over the two years. A first horizontal panel gives basic information about engagement with the program. The second horizontal panel describes the services provided by the JYC, encompassing information about and matches with job offers or training opportunities. The third horizontal panel deals with the actual response of the beneficiaries to these offers, with human capital investment and employment measures recorded by the JYC. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Still in program") are multiplied by 100 so that the results can be interpreted in percentage terms. See Table A3 in appendix for more details.

Table 3: Human Capital Investment and Job Search

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Human capital</u>						
Number of trainings over 1 y. ($\times 100$)	56.14 (72.65)	-2.33 (2.55)	3,409	48.08 (70.22)	-4.52 (2.95)	2,308
Ongoing apprenticeship	6.85 (25.27)	0.95 (0.86)	3,409	6.43 (24.55)	0.23 (0.99)	2,308
Ongoing internship	2.62 (15.98)	-0.21 (0.57)	3,409	1.43 (11.88)	-0.59 (0.52)	2,308
Driving license	41.90 (49.35)	2.66** (1.30)	3,409	42.54 (49.46)	1.83 (1.89)	2,308
Perceived employment prospect	32.78 (80.68)	3.00 (2.67)	3,409	29.76 (80.47)	1.28 (3.82)	2,308
Has a career plan	45.23 (49.79)	-0.50 (1.58)	3,409	48.17 (49.99)	-0.92 (2.13)	2,308
Has necessary diploma	18.47 (38.82)	-1.77 (1.33)	3,409	21.98 (41.43)	-0.64 (1.59)	2,308
Human capital index	0.00 (3.47)	0.05 (0.13)	3,409	0.00 (3.33)	-0.06 (0.13)	2,308
<u>Search behavior</u>						
Search for a job	56.14 (49.64)	0.16 (1.46)	3,409	51.56 (50.00)	2.87 (2.54)	2,308
Intensity of use of channels						
Web search	19.31 (34.92)	-2.04* (1.12)	3,409	21.76 (37.92)	1.68 (1.85)	2,308
Index for other channels	-0.00 (252.18)	-0.95 (9.02)	3,409	-0.00 (259.09)	-0.49 (12.72)	2,308
Number of firms contacted	4.81 (8.24)	-0.21 (0.27)	3,409	4.45 (8.12)	-0.06 (0.34)	2,308
At least one interview	20.86 (40.64)	1.03 (1.44)	3,409	16.09 (36.76)	1.90 (1.74)	2,308
Search index	0.00 (5.30)	-0.05 (0.17)	3,409	-0.00 (5.42)	0.14 (0.27)	2,308
<u>Flexibility in search</u>						
Acceptable commuting duration	35.91 (21.37)	0.89 (0.72)	3,409	36.03 (21.63)	0.45 (1.08)	2,308
Accept to move if indefinite term	19.96 (39.99)	1.33 (1.40)	3,409	20.02 (40.03)	0.85 (1.88)	2,308

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for outcomes related to participants' human capital investment and job search response to the program. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. The human capital index encompasses a count of the trainings attended by the respondent and binary variables indicating whether she was doing an apprenticeship or an internship (two variables), whether she attended at least one training delivering a certification, whether she prepared or obtained the driving license after the beginning of the program, whether she deems her chances of finding a fitting job have improved, whether she has a career plan or ideas (two variables), and whether she holds the necessary diploma(s) for her targeted career plan. The job search index covers a binary variable indicating whether the respondent was looking for a job, a count of the firms contacted, a dummy indicating whether these contacts led to at least one interview, and a set of binary variables describing the job search means mobilized (web, temporary help agency, sending resumes, or direct job applications). Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Driving license") are multiplied by 100 so that the results can be interpreted in percentage terms. See Tables A4 and A5 in appendix for more details.

Table 4: Employment and Income

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Employment status</u>						
Months employed over 1st sem.	2.42 (2.34)	-0.20*** (0.07)	3,409	3.11 (2.54)	-0.01 (0.09)	2,308
Months employed over 2nd sem.	2.82 (2.51)	-0.02 (0.08)	3,409	3.20 (2.59)	-0.03 (0.10)	2,308
Currently employed	45.41 (49.80)	2.44 (1.63)	3,409	52.10 (49.98)	0.09 (1.93)	2,308
<u>Employment quality</u>						
Employment type						
Formal	42.49 (49.45)	1.14 (1.60)	3,409	49.78 (50.02)	-0.05 (1.87)	2,308
Subsidized formal	9.24 (28.96)	-0.24 (1.13)	3,409	8.67 (28.15)	1.58 (1.24)	2,308
Informal	2.86 (16.67)	1.40** (0.70)	3,409	2.59 (15.90)	0.26 (0.69)	2,308
Employment volume						
Full-time	26.82 (44.31)	1.42 (1.19)	3,409	33.33 (47.16)	-0.27 (1.90)	2,308
Part-time	18.59 (38.92)	1.03 (1.36)	3,409	18.86 (39.13)	0.03 (1.60)	2,308
Contract duration						
Indefinite term	9.95 (29.95)	-0.12 (0.94)	3,409	14.39 (35.11)	0.31 (1.59)	2,308
Fixed term	18.00 (38.43)	0.64 (1.32)	3,409	19.39 (39.55)	-1.61 (1.67)	2,308
Employer type						
Private	32.90 (47.00)	-0.49 (1.56)	3,409	36.46 (48.15)	0.94 (1.97)	2,308
Public	8.28 (27.57)	1.58* (0.95)	3,409	10.10 (30.14)	-0.34 (1.43)	2,308
<u>Income</u>						
Any type						
From JYC	602.20 (489.42)	38.70** (15.33)	3,409	731.51 (521.39)	-13.57 (20.04)	2,307
From activity	32.83 (110.78)	87.57*** (5.72)	3,409	8.41 (50.35)	6.24** (2.91)	2,308
Other government transfers	405.36 (478.28)	-21.29 (14.48)	3,409	537.73 (530.61)	-17.82 (21.47)	2,308
Family and friends	78.36 (214.02)	-7.54 (6.32)	3,409	138.47 (270.00)	4.86 (12.18)	2,308
Other	36.16 (105.11)	-9.37*** (3.52)	3,409	41.09 (113.14)	-11.73** (4.92)	2,308
	49.49 (154.68)	-10.35** (4.85)	3,409	5.16 (147.58)	2.41 (4.03)	2,307

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for participants' job market outcomes and income. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Currently employed") are multiplied by 100 so that the results can be interpreted in percentage terms. See Tables A6 and A7 in appendix for more details.

Table 5: Expenditures, Mobility, and Integration

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Expenditures and savings</u>						
Financial constraint index	0.00 (4.11)	-0.08 (0.16)	3,409	0.00 (2.26)	0.05 (0.08)	2,308
Temptation goods index	0.00 (2.25)	-0.03 (0.08)	3,215			
Saved money	45.47 (49.81)	4.70** (2.06)	3,409			
Amount saved	211.19 (427.36)	36.54** (17.76)	3,295			
Owes money to relatives	16.39 (37.03)	-2.02* (1.08)	3,409			
<u>Mobility</u>						
Parents	2.80 (16.50)	-1.25** (0.53)	3,409	1.61 (12.59)	-0.16 (0.59)	2,308
Other mobility means index	0.00 (1.04)	0.02 (0.04)	3,409	-0.00 (1.05)	0.05 (0.05)	2,308
<u>Integration</u>						
Trust index	0.00 (2.47)	0.32*** (0.10)	3,409	0.00 (2.53)	0.05 (0.13)	2,308
Personality index	0.00 (1.73)	0.05 (0.06)	3,247	0.01 (1.69)	-0.11 (0.07)	2,253
No friends	5.42 (22.65)	2.04** (0.91)	3,409	6.43 (24.55)	0.16 (0.98)	2,308

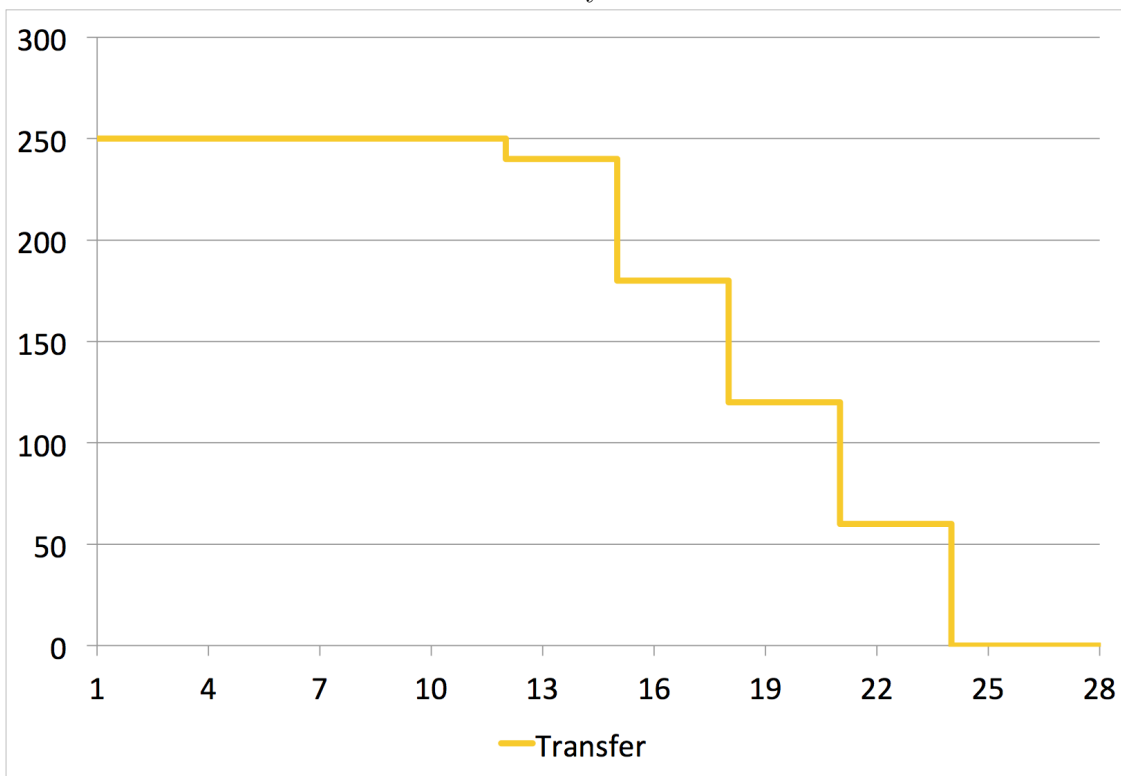
Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for variables related to participants' expenditures and savings, mobility means, and personality traits. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. Note that the set of variables included in the financial constraint survey varies between both surveys for data availability reasons. In the left-hand side panel, the index gathers binary variables indicating difficulties paying bills, rent, or taxes (three distinct variables), whether the respondent had to forego a training, whether she had to spend at least one day without a meal, whether she had to forego healthcare, and whether she was in bank overdraft. In the right-hand side panel, the index is restricted to the last three outcomes (meal, healthcare, and overdraft). The temptation goods index covers the following expenditures: restaurants, nights out, phone, and tobacco. The index for other mobility means covers the following options: foot, bike, public transportation, scooter, and car. The trust index encompasses trust in school, the healthcare system, the Job Youth Center (JYC), and the justice system. Eventually, the personality index covers a variable measuring the time the respondent is willing to wait for a €250 gain versus an immediate €200, a life satisfaction scale, and a locus of control variable. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Saved money") are multiplied by 100 so that the results can be interpreted in percentage terms. See Tables A8, A9, and A10 in appendix for more details.

Table 6: Attrition

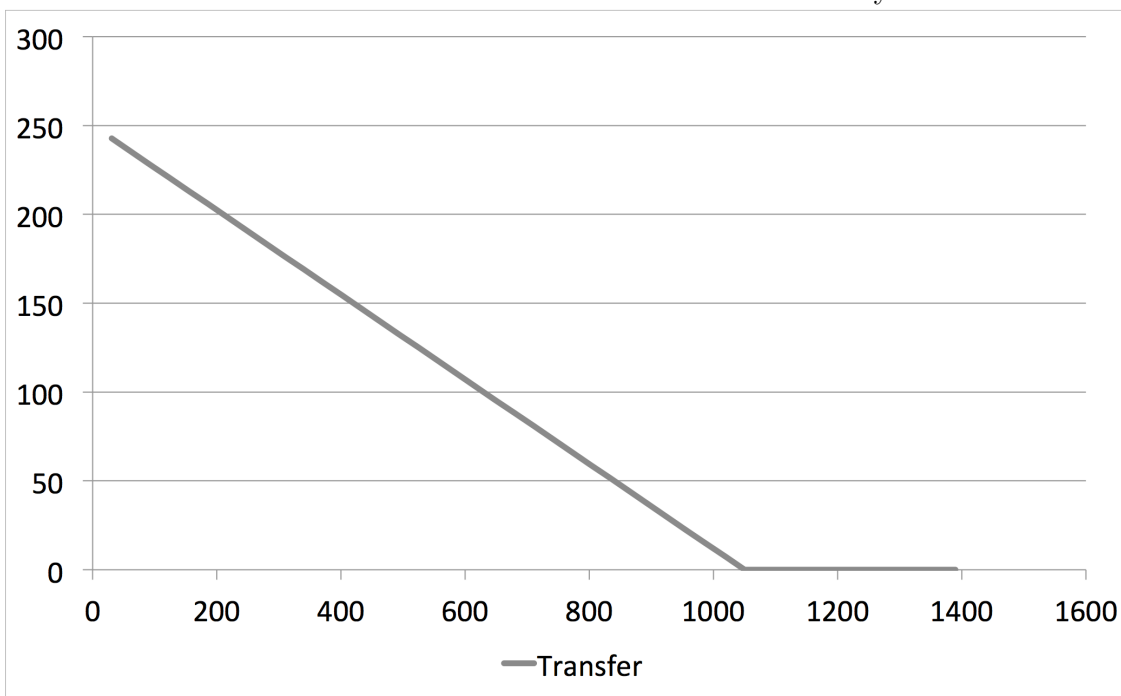
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline
Treatment	0.059*** (0.013)	0.052*** (0.014)	0.057*** (0.013)	0.046** (0.014)	0.063*** (0.013)	0.054*** (0.014)	0.024 (0.029)	0.024 (0.031)
Reinforced Civis					-0.100*** (0.015)	-0.106*** (0.016)	-0.132*** (0.022)	-0.126*** (0.022)
Precarious housing					-0.165*** (0.032)	-0.162*** (0.030)	-0.152** (0.057)	-0.173*** (0.046)
Baccalaureat Level					0.062*** (0.017)	0.083*** (0.016)	0.047 (0.025)	0.065* (0.025)
T (×) Reinforced Civis							0.069 (0.035)	0.045 (0.038)
T (×) Precarious housing							-0.017 (0.083)	0.024 (0.063)
T (×) Baccalaureat Level							0.030 (0.039)	0.037 (0.038)
N	5,488	5,488	5,488	5,488	5,482	5,482	5,488	5,488
Estimation	OLS	OLS	OLS	OLS	Lasso PDS	Lasso PDS	OLS	OLS
FE	No	No	Yes	Yes	Yes	Yes	No	No
Controls	No	No	No	No	Yes	Yes	No	No

Figure 1: Cash transfer schemes
Scheduled month by month transfer



(a)

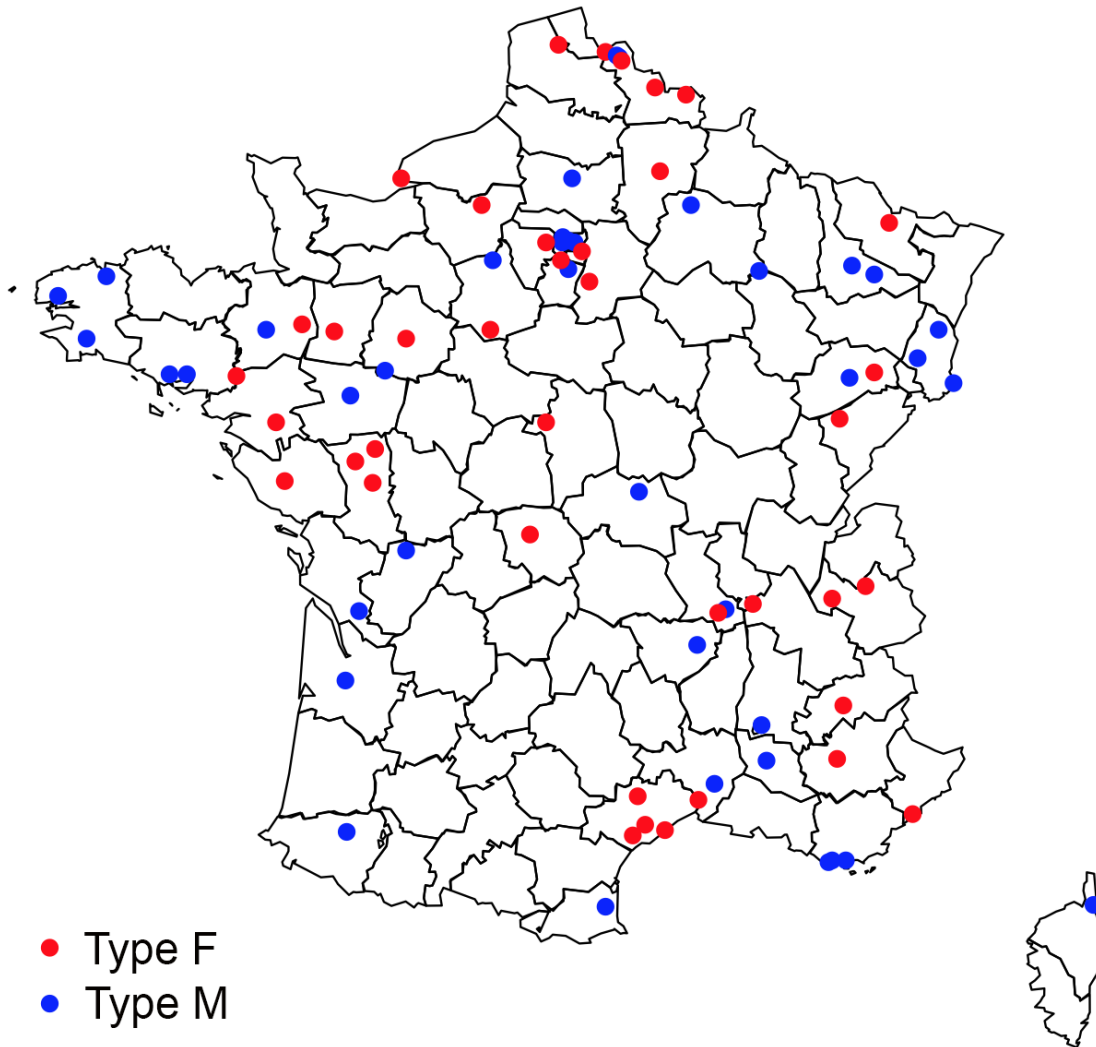
Transfer as a function of incomes from activity



(b)

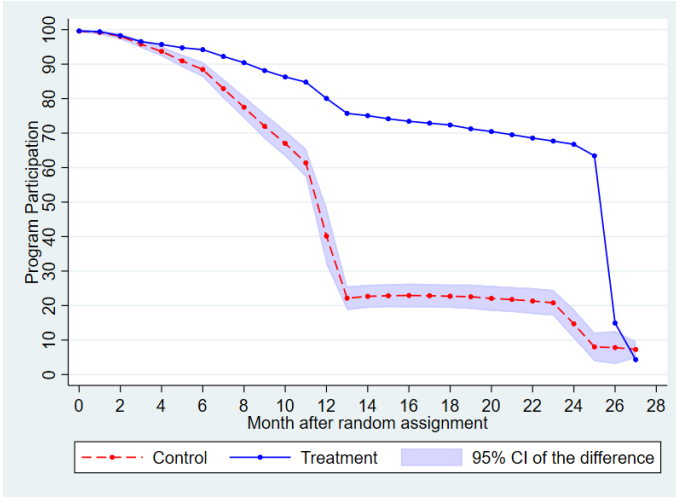
The graph on the upper panel presents the pattern of maximum possible transfers related to the transfer program. The graph on the lower panel presents actual transfers as a function of income from activity. These incomes include wages, unemployment benefits, and internship and training allowances. The upper limit to receive a positive transfer corresponds to the level of the 2011 minimum wage

Figure 2: JYC Map

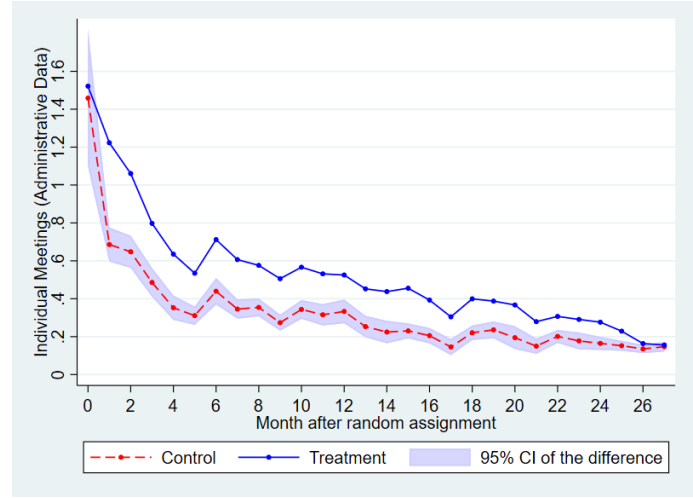


Map indicating JYC participating in the experiment. Blue dots identify type M JYC where youth registered in March were assigned to the Cash program and youth registered in February to the control group. Red dots identify type F JYC where youth registered in February were assigned to the transfer program and youth registered in March to the control group. Randomization was implemented the 1st of April after all lists were closed.

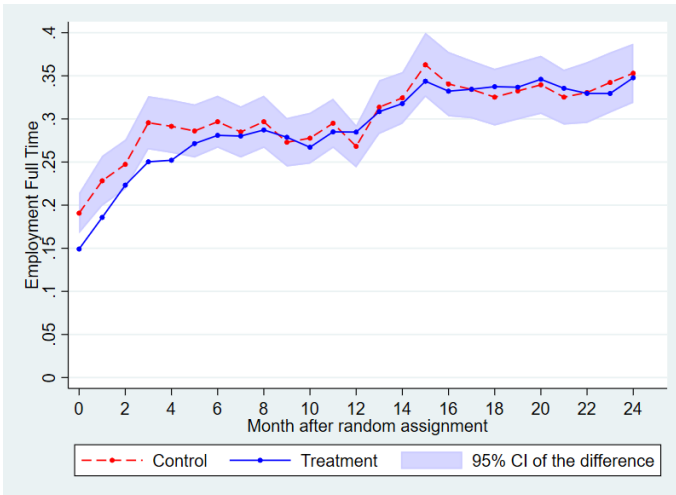
Figure 3: Participation in the program and month by month employment



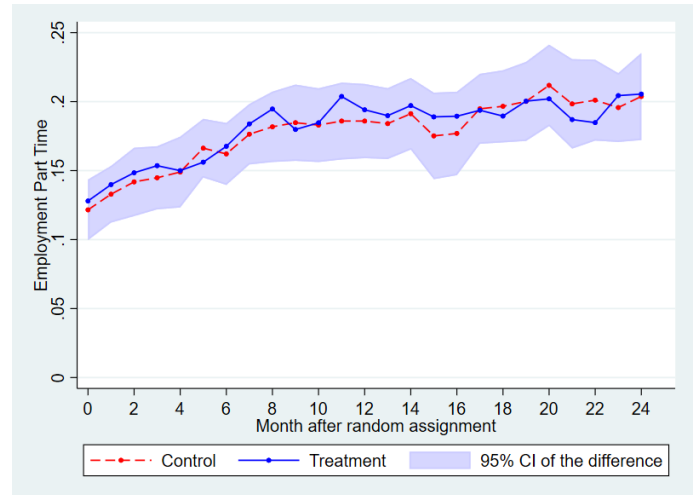
(a)



(b)



(c)



(d)

Graph (a) and (b) present the profile of the monthly mean of the considered variable for the two groups of youth: youth assigned to the transfer program (blue line) and youth assigned to stay in the *standard* program (red line). The shaded area around the red line corresponds to the confidence interval at the 5 % level resulting from estimation of equation 1 for the monthly variable considered. Actually the blue line is obtained by adding the mean in the control group (reported on the red line) to the estimated treatment parameter.

(a) : Month by month variable indicating whether the youth is still officially registered in either the career program or the transfer program

(b) : Month by month number of meetings with a caseworker at the JYC

Graph (c) and (d) present the monthly profiles of employment for youth in the two assignment groups. Information used for month 0 (April 2011) to 12 (April 2012) comes from the retrospective calendar in the midline survey. Information used for month 13 (May 2012) to 24 (April 2013) also comes from the endline survey

(c) : 1 if in employment with a full-time contract at least once during the month

(d) : 1 if in employment with a part-time contract at least once during the month

Online Appendix

A Context

Table A1: Youth Diploma and Labor Market integration

	Whole sample (1)	Difficult LM integration (2)	Attended JYC (3)
Repeated at least one year in primary school	17.5	27.7	27.1
No diploma	18.0	36.3	37.3
Junior high school diploma	17.0	22.3	26.4
High school diploma	23.0	17.8	21.4
Above high school	42.0	23.5	15.0
Left school			
At or before 16	3.0	6.7	5.9
At 17 or 18	18.6	32.8	35.0
At 19 or 20	27.4	27.7	33.6
At 21 or 22	21.7	15.6	16.4
Older than 22	29.3	17.2	9.1
Environment			
Both parents born abroad	12.1	17.8	15.7
At least one parent born abroad	21.9	27.7	26.5
Father works	80.7	74.2	77.4
Father clerical or blue collar worker	53.6	67.0	70.5
Live in deprived suburbs	8.3	12.4	12.5
Attended JYC at least twice	20.6	41.9	100.0
Employment path			
Direct access to stable employment	57.2	0.0	23.5
Delayed access to employment	12.0	0.0	20.4
Long-term unemployment	9.3	42.6	24.6
Inactivity and labor market dropout	12.5	57.4	19.6
Back to school or training	9.0	0.0	11.9
# observations	24579	21.7	20.6

The Generation 2007 survey is a large representative national survey about youth labor market integration for youth exiting the educational system in 2007. The survey was conducted in 2010, three years after youth left the educational system. Column (1) provides averages for the entire sample, column (2) the averages for youth experiencing either long-term unemployment or a shift to inactivity during the three years between 2007-2010, column (3) provides averages for youth who attended Job Youth Centers twice or more in the three-year period .

B Robustness to Attrition

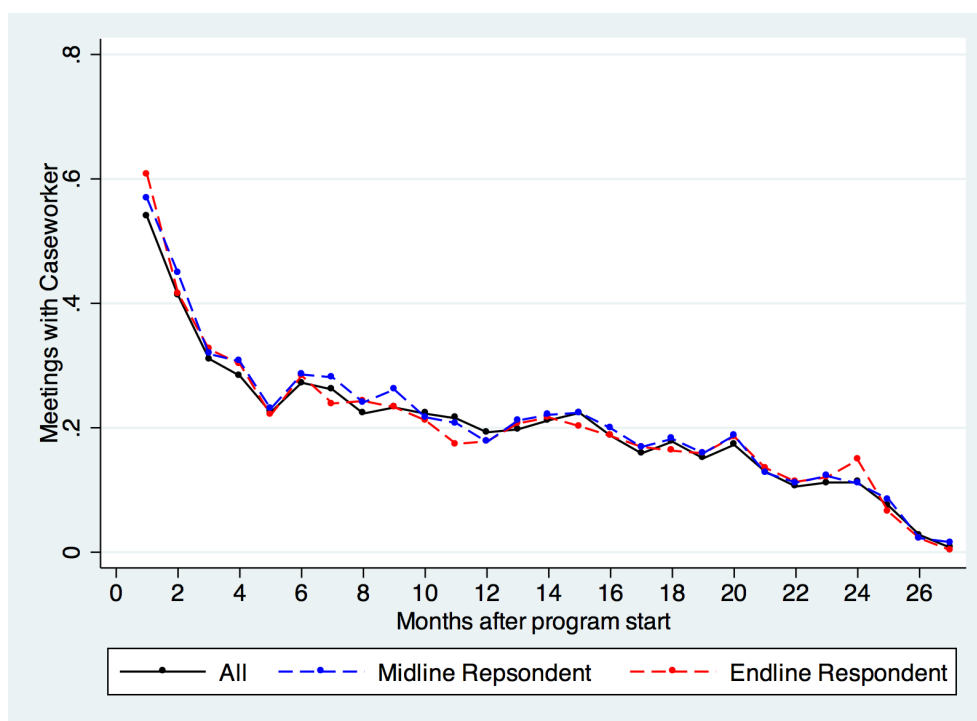
Table A2: Bounding of treatment effects

	Control Mean	Actual Effect	N	Without Controls Effect	N	Same Response Rate Effect	N	Lee Bounds Lower	Upper
<u>Employment</u>									
Months employed over 1st sem.	2.42 (2.34)	-0.20*** (0.07)	3,409	-0.25 (0.07)	3,413	-0.23 (0.07)	3,211	-0.54 (0.12)	0.06 (0.10)
Employed after 24 months	52.10 (49.98)	0.09 (1.93)	2,308	-0.64 (1.93)	2,309	0.61 (1.93)	2,229	-6.35 (2.81)	6.71 (2.90)
<u>Investment</u>									
Human capital index	0.00 (3.47)	0.05 (0.13)	3,409	0.10 (0.13)	3,413	0.05 (0.13)	3,211	-0.63 (0.16)	0.54 (0.15)
Search index	0.00 (5.30)	-0.05 (0.17)	3,409	-0.04 (0.17)	3,413	-0.01 (0.18)	3,211	-1.11 (0.26)	0.35 (0.22)
<u>Income</u>									
Any type	602.20 (489.42)	38.70** (15.33)	3,409	35.03 (16.99)	3,413	38.37 (16.06)	3,211	-53.15 (22.52)	99.77 (21.62)
From activity	405.36 (478.28)	-21.29 (14.48)	3,409	-31.87 (15.87)	3,413	-21.94 (15.08)	3,211	-128.77 (24.83)	9.54 (19.17)
Amount saved	211.19 (427.36)	36.54** (17.76)	3,295	26.68 (18.01)	3,299	41.38 (18.75)	3,103	-92.88 (20.49)	49.61 (16.83)

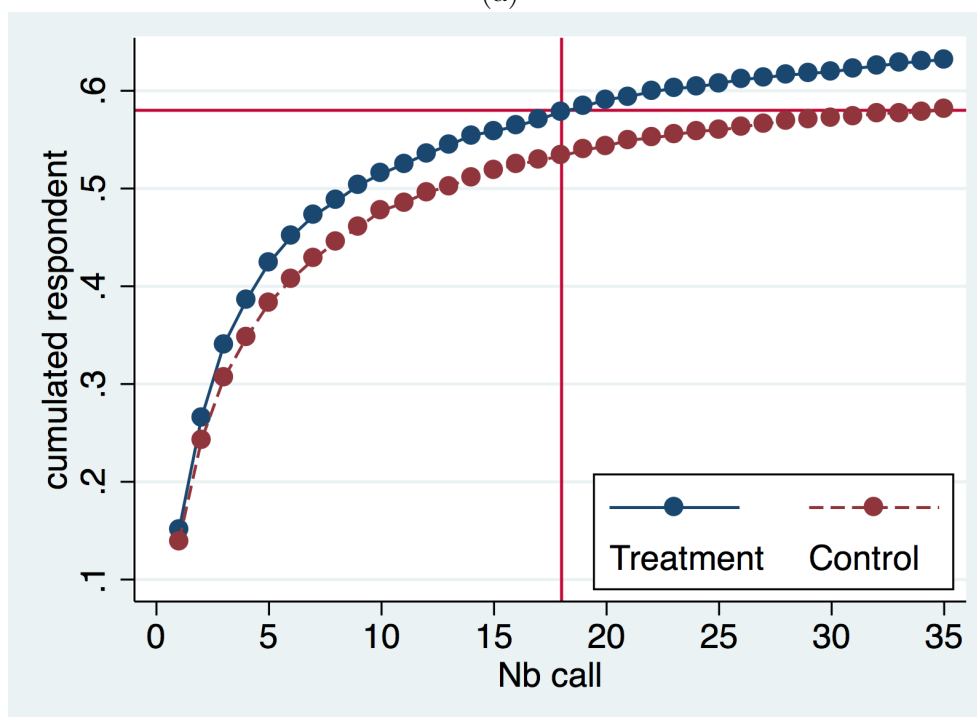
Data: Midline survey (April 2012) if not explicitly specified; endline survey (April 2013) when specified.

Notes: The table provides various estimates of Intention to Treat parameters. Columns under “Actual” provide the benchmark results obtained with a Lasso post-double-selection procedure and control variables listed in Table A1. Columns under “Without Controls” show OLS estimates without the control variables (but keeping Job Youth Center indicator variables). Columns under “Same Response Rate” replicate the results obtained with the Lasso post-double-selection procedure and control variables while removing individuals reached in the treatment group after more than 18 calls. Eliminating these “most difficult to reach individuals” from the treatment group leads to identical response rates in treatment and control groups. The last columns provide Lee bounds.

Figure A1: Robustness



(a)



(b)

Administrative records, midline and endline survey, April 2012 and April 2013.

The top graph presents the monthly impact of being assigned to the transfer program on the total number of meeting using three samples: the whole sample (in black) the sample of respondent to the midline survey (blue) and the sample of respondent to the endline survey (red)

The bottom graph presents the response rate in both assignment groups as a function of the number of calls. The sample used in the robustness table A2 is obtained by selecting in the treatment group individuals answering after a number of attempts lower or equal to 18.

C Detailed Treatment Effects

C.1 Administrative Data

Table A3: Administrative Outcomes (detailed)

	Trimester 1			Semester 1			Year 1			Total after 2 years		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Program enrollment and meetings</u>												
Individual meetings	1.82 (1.94)	1.25*** (0.11)	5,482	2.92 (2.81)	2.02*** (0.17)	5,482	5.82 (4.56)	3.12*** (0.24)	5,482	8.23 (7.17)	5.07*** (0.43)	5,482
Still in program	95.54 (20.64)	0.88 (0.58)	5,482	88.19 (32.28)	5.85*** (1.13)	5,482	39.78 (48.95)	39.99*** (4.22)	5,482	15.77 (36.45)	51.04*** (1.81)	5,482
Months in program	2.93 (0.37)	0.01 (0.01)	5,476	5.66 (1.07)	0.13*** (0.04)	5,476	9.67 (3.02)	1.34*** (0.15)	5,476	13.11 (6.16)	6.83*** (0.30)	5,476
Final cumulative transfers from JYC							236.53 (381.94)	1,528.32*** (87.29)				5,482
<u>Services received from the JYC</u>												
Information about a job offer	3.02 (5.10)	1.60*** (0.29)	5,482	4.61 (6.82)	2.45*** (0.36)	5,482	7.42 (10.46)	4.09*** (0.55)	5,482	11.40 (16.58)	6.31*** (0.90)	5,482
Information about a training	0.89 (1.55)	0.36*** (0.08)	5,482	1.51 (2.34)	0.71*** (0.13)	5,482	2.64 (3.86)	1.16*** (0.22)	5,482	3.96 (5.70)	1.79*** (0.33)	5,482
Information about project building	0.94 (1.97)	0.62*** (0.12)	5,482	1.38 (2.65)	0.95*** (0.17)	5,482	2.36 (4.49)	1.38*** (0.29)	5,482	3.56 (7.11)	2.11*** (0.40)	5,482
Match with a job offer	0.47 (1.23)	0.13*** (0.05)	5,482	0.72 (1.66)	0.23*** (0.06)	5,482	1.10 (2.30)	0.45*** (0.10)	5,482	1.69 (3.29)	0.70*** (0.14)	5,482
Match with a training	0.15 (0.48)	0.05*** (0.02)	5,482	0.29 (0.69)	0.11*** (0.03)	5,482	0.53 (1.10)	0.17*** (0.04)	5,482	0.82 (1.65)	0.22*** (0.06)	5,482
Match with a project	0.14 (0.55)	0.13*** (0.04)	5,482	0.21 (0.69)	0.17*** (0.04)	5,482	0.34 (0.96)	0.21*** (0.06)	5,482	0.49 (1.41)	0.24*** (0.07)	5,482
<u>Offers taken up and employment as recorded by the JYC</u>												
Training	0.50 (1.02)	0.01 (0.02)	5,482	0.83 (1.62)	0.03 (0.05)	5,482	1.57 (2.67)	0.11 (0.07)	5,482	0.02 (0.03)	0.00 (0.00)	5,482
Human capital investment	0.62 (1.09)	0.02 (0.03)	5,482	1.09 (1.80)	0.04 (0.05)	5,482	2.27 (3.29)	0.18 (0.11)	5,482	0.02 (0.03)	0.00 (0.00)	5,482
Months with employment	0.70 (1.13)	0.04 (0.03)	5,482	1.53 (2.13)	0.06 (0.06)	5,482	2.77 (3.69)	0.17* (0.09)	5,482	0.03 (0.04)	0.00* (0.00)	5,482

Data: Administrative records.

Notes: The table provides Intention To Treat estimates for further variables related to services provided by the Job Youth Center (JYC). Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has four vertical panels. The left panel provides events recorded over the first quarter following random assignment, the second panel information recorded over the first semester, the third panel over the first year, and the last panel over the two years. A first horizontal panel gives basic information about engagement with the program. The second horizontal panel describes the services provided by the JYC, encompassing information about and matches with job offers or training opportunities. The third horizontal panel deals with the actual response of the beneficiaries to these offers, with human capital investment and employment measures recorded by the JYC. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Still in program") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 2 in the main text.

C.2 Survey data

Table A4: Human Capital Investment (detailed outcomes)

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Training</u>						
Number of trainings over 1 y. ($\times 100$)	56.14 (72.65)	-2.33 (2.55)	3,409	48.08 (70.22)	-4.52 (2.95)	2,308
At least one certified	30.81 (46.18)	-0.66 (1.47)	3,409	17.52 (38.03)	-0.88 (1.78)	2,308
Forewent for financial reason	13.71 (34.40)	-0.27 (1.29)	3,409	12.60 (33.20)	2.37 (1.58)	2,308
Ongoing apprenticeship	6.85 (25.27)	0.95 (0.86)	3,409	6.43 (24.55)	0.23 (0.99)	2,308
Ongoing internship	2.62 (15.98)	-0.21 (0.57)	3,409	1.43 (11.88)	-0.59 (0.52)	2,308
Driving license	41.90 (49.35)	2.66** (1.30)	3,409	42.54 (49.46)	1.83 (1.89)	2,308
<u>Employment prospects</u>						
Perceived employment prospect	32.78 (80.68)	3.00 (2.67)	3,409	29.76 (80.47)	1.28 (3.82)	2,308
Improved	44.40 (49.70)	3.29* (1.77)	3,409	46.29 (49.88)	2.74 (2.20)	2,308
Same	24.20 (42.84)	-0.39 (1.62)	3,409	26.45 (44.13)	-1.68 (1.63)	2,308
Reduced	21.51 (41.10)	-1.32 (1.44)	3,409	21.89 (41.37)	0.31 (1.97)	2,308
Already satisfying job	9.89 (29.87)	-1.70* (1.02)	3,409	5.36 (22.54)	-1.37 (1.08)	2,308
<u>Career plan</u>						
Has a career plan	45.23 (49.79)	-0.50 (1.58)	3,409	48.17 (49.99)	-0.92 (2.13)	2,308
Has necessary diploma	18.47 (38.82)	-1.77 (1.33)	3,409	21.98 (41.43)	-0.64 (1.59)	2,308
Has ideas	36.83 (48.25)	0.67 (1.51)	3,409	34.05 (47.41)	0.94 (2.02)	2,308
No idea	17.82 (38.28)	-0.02 (1.43)	3,409	17.43 (37.95)	-0.29 (1.55)	2,308
<u>Index</u>						
Human capital index	0.00 (3.47)	0.05 (0.13)	3,409	0.00 (3.33)	-0.06 (0.13)	2,308

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for further outcomes related to participants' human capital investment and job search response to the program. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. The human capital index encompasses a count of the trainings attended by the respondent and binary variables indicating whether she was doing an apprenticeship or an internship (two variables), whether she attended at least one training delivering a certification, whether she prepared or obtained the driving license after the beginning of the program, whether she deems her chances of finding a fitting job have improved, whether she has a career plan or ideas (two variables), and whether she holds the necessary diploma(s) for her targeted career plan. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Apprenticeship") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 3 in the main text.

Table A5: Job Search (detailed outcomes)

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Search behavior</u>						
Search for a job	56.14 (49.64)	0.16 (1.46)	3,409	51.56 (50.00)	2.87 (2.54)	2,308
Intensity of use of channels						
Web search	19.31 (34.92)	-2.04* (1.12)	3,409	21.76 (37.92)	1.68 (1.85)	2,308
Index for other channels	-0.00 (252.18)	-0.95 (9.02)	3,409	-0.00 (259.09)	-0.49 (12.72)	2,308
Temporary help agency	20.86 (39.18)	-0.90 (1.39)	3,409	20.29 (39.05)	0.02 (1.69)	2,308
Send resumes	36.62 (47.14)	1.46 (1.47)	3,409	33.60 (46.44)	-0.17 (2.39)	2,308
Direct job application	28.10 (43.63)	-0.81 (1.67)	3,409	27.35 (43.27)	-0.11 (2.06)	2,308
Number of firms contacted	4.81 (8.24)	-0.21 (0.27)	3,409	4.45 (8.12)	-0.06 (0.34)	2,308
At least one interview	20.86 (40.64)	1.03 (1.44)	3,409	16.09 (36.76)	1.90 (1.74)	2,308
Search index	0.00 (5.30)	-0.05 (0.17)	3,409	-0.00 (5.42)	0.14 (0.27)	2,308
<u>Flexibility in search</u>						
Acceptable commuting duration	35.91 (21.37)	0.89 (0.72)	3,409	36.03 (21.63)	0.45 (1.08)	2,308
Accept to move if indefinite term	19.96 (39.99)	1.33 (1.40)	3,409	20.02 (40.03)	0.85 (1.88)	2,308

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for further outcomes related to participants' human capital investment and job search response to the program. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. The job search index covers a binary variable indicating whether the respondent was looking for a job, a count of the firms contacted, a dummy indicating whether these contacts led to at least one interview, and a set of binary variables describing the job search means mobilized (web, temporary help agency, sending resumes, or direct job applications). Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Apprenticeship") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 3 in the main text.

Table A6: Employment Quality (detailed outcomes)

	Mean	Control Effect	Treatment	N Mean	Control Effect	Treatment	N
<u>Employment status</u>							
Months employed over 1st sem.		2.42 (2.34)	-0.20*** (0.07)	3,409	3.11 (2.54)	-0.01 (0.09)	2,308
Months employed over 2nd sem.		2.82 (2.51)	-0.02 (0.08)	3,409	3.20 (2.59)	-0.03 (0.10)	2,308
Currently employed		45.41 (49.80)	2.44 (1.63)	3,409	52.10 (49.98)	0.09 (1.93)	2,308
<u>Contract type</u>							
Indefinite term		9.95 (29.95)	-0.12 (0.94)	3,409	14.39 (35.11)	0.31 (1.59)	2,308
Fixed term		18.00 (38.43)	0.64 (1.32)	3,409	19.39 (39.55)	-1.61 (1.67)	2,308
Temporary help		5.72 (23.23)	0.61 (0.77)	3,409	5.18 (22.18)	1.55 (0.99)	2,308
Internship		2.62 (15.98)	-0.21 (0.57)	3,409	1.43 (11.88)	-0.59 (0.52)	2,308
Apprenticeship		6.85 (25.27)	0.95 (0.86)	3,409	6.43 (24.55)	0.23 (0.99)	2,308
Other		2.26 (14.88)	0.40 (0.57)	3,409	4.11 (19.86)	0.05 (0.96)	2,308
<u>Employment quality</u>							
Employment type							
Formal		42.49 (49.45)	1.14 (1.60)	3,409	49.78 (50.02)	-0.05 (1.87)	2,308
Subsidized formal	Δ	9.24 (28.96)	-0.24 (1.13)	3,409	8.67 (28.15)	1.58 (1.24)	2,308
Informal		2.86 (16.67)	1.40** (0.70)	3,409	2.59 (15.90)	0.26 (0.69)	2,308
Employment volume							
Full-time		26.82 (44.31)	1.42 (1.19)	3,409	33.33 (47.16)	-0.27 (1.90)	2,308
Part-time		18.59 (38.92)	1.03 (1.36)	3,409	18.86 (39.13)	0.03 (1.60)	2,308
Employer type							
Private		32.90 (47.00)	-0.49 (1.56)	3,409	36.46 (48.15)	0.94 (1.97)	2,308
Public		8.28 (27.57)	1.58* (0.95)	3,409	10.10 (30.14)	-0.34 (1.43)	2,308

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for additional job market outcomes and income variables. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Temporary help") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 4 in the main text.

Table A7: Income (detailed outcomes)

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>General income outcomes</u>						
Any type	602.20 (489.42)	38.70** (15.33)	3,409	731.51 (521.39)	-13.57 (20.04)	2,307
From JYC	32.83 (110.78)	87.57*** (5.72)	3,409	8.41 (50.35)	6.24** (2.91)	2,308
Not from JYC	569.37 (495.10)	-48.94*** (15.26)	3,409	723.09 (524.06)	-19.55 (19.43)	2,307
<u>Income outside JYC</u>						
From activity	405.36 (478.28)	-21.29 (14.48)	3,409	537.73 (530.61)	-17.82 (21.47)	2,308
From Pôle Emploi	78.36 (214.02)	-7.54 (6.32)	3,409	138.47 (270.00)	4.86 (12.18)	2,308
Family and friends	36.16 (105.11)	-9.37*** (3.52)	3,409	41.09 (113.14)	-11.73** (4.92)	2,308
Other	49.49 (154.68)	-10.35** (4.85)	3,409	5.16 (147.58)	2.41 (4.03)	2,307
<u>Income from activity</u>						
Wage	372.52 (476.99)	-14.10 (14.64)	3,409	491.02 (533.15)	-12.86 (22.98)	2,308
Odd jobs	17.57 (77.37)	-4.29* (2.53)	3,409	24.28 (123.03)	-5.15 (5.75)	2,308
Fellowship	15.26 (79.06)	-2.95 (2.93)	3,409	22.44 (96.15)	1.51 (4.03)	2,308

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for additional job market outcomes and income variables. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Temporary help") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 4 in the main text.

Table A8: Expenditures (detailed outcomes)

		Midline Survey (April 2012)		
		Control Mean	Treatment Effect	N
<u>Financial constraints over last 12 monts</u>				
Financial constraint index		0.00 (4.11)	-0.08 (0.16)	3,409
Any financial difficulty		64.42 (47.89)	0.82 (1.52)	3,409
Pbs. paying bills		27.71 (44.77)	0.11 (1.72)	3,409
Pbs. paying rent		18.12 (38.53)	-0.45 (1.64)	3,409
Pbs. paying taxes		8.76 (28.28)	0.19 (1.03)	3,409
A day without a meal		19.37 (39.53)	-0.71 (1.47)	3,409
Forewent medical care		24.37 (42.95)	-0.60 (1.45)	3,409
Bank overdraft		44.93 (49.76)	-1.19 (1.72)	3,409
Forewent training		13.71 (34.40)	-0.27 (1.29)	3,409
<u>Temptation goods over last month</u>				
		Â		
Temptation goods index		0.00 (2.25)	-0.03 (0.08)	3,215
Number of restaurants		2.21 (2.73)	-0.00 (0.09)	3,300
Nights out		2.06 (2.72)	-0.12 (0.10)	3,272
Phone		55.08 (156.52)	-2.81 (5.74)	3,386
Tobacco		29.68 (49.86)	1.75 (1.78)	3,409
Largest purchase		661.55 (1,410.07)	21.42 (45.62)	3,113
<u>Saving behavior</u>				
		Â		
Saved money		45.47 (49.81)	4.70** (2.06)	3,409
Amount saved		211.19 (427.36)	36.54** (17.76)	3,295
Owes money to relatives		16.39 (37.03)	-2.02* (1.08)	3,409

Data: Midline survey (April 2012).

Notes: The table provides Intention To Treat estimates for further variables related to participants' financial constraints and expenditures. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has one vertical panel, based on the midline survey of April 2012. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. The financial constraint index gathers binary variables indicating difficulties paying bills, rent, or taxes (three distinct variables), whether the respondent had to forego a training, whether she had to spend at least one day without a meal, whether she had to forego healthcare, and whether she was in bank overdraft. The temptation goods index covers the following expenditures: restaurants, nights out, phone, and tobacco. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Pbs. paying bills") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 5 in the main text.

Table A9: Mobility (detailed outcomes)

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Mobility means</u>						
Parents	2.80 (16.50)	-1.25** (0.53)	3,409	1.61 (12.59)	-0.16 (0.59)	2,308
Other mobility means index	0.00 (1.04)	0.02 (0.04)	3,409	-0.00 (1.05)	0.05 (0.05)	2,308
Foot	9.24 (28.96)	-0.30 (1.24)	3,409	7.95 (27.07)	0.08 (1.07)	2,308
Bike	1.67 (12.81)	0.31 (0.46)	3,409	1.88 (13.58)	0.25 (0.76)	2,308
Public	37.54 (48.44)	2.95 (1.93)	3,409	29.58 (45.66)	3.77* (1.94)	2,308
Scooter	5.07 (21.94)	-0.95 (0.80)	3,409	3.57 (18.57)	0.71 (0.75)	2,308
Car	43.68 (49.61)	-0.63 (1.43)	3,409	55.41 (49.73)	-4.22** (1.97)	2,308

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for further variables related to participants' obility means, personality traits, and social integration. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. The index for other mobility means covers the following options: foot, bike, public transportation, scooter, and car. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Foot") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 5 in the main text.

Table A10: Integration (detailed outcomes)

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
<u>Trust</u>						
Trust index	0.00 (2.47)	0.32*** (0.10)	3,409	0.00 (2.53)	0.05 (0.13)	2,308
School	63.89 (48.05)	4.06** (1.63)	3,409	66.40 (47.26)	0.28 (2.30)	2,308
Health care system	84.15 (36.53)	0.78 (1.52)	3,409	82.75 (37.80)	-2.42 (1.66)	2,308
Job Youth Center (JYC)	81.17 (39.11)	6.49*** (1.23)	3,409	69.88 (45.90)	8.18*** (1.83)	2,308
Justice system	53.87 (49.86)	2.50 (1.64)	3,409	56.39 (49.61)	-3.41 (2.24)	2,308
<u>Personality traits</u>						
Personality index	0.00 (1.73)	0.05 (0.06)	3,247	0.01 (1.69)	-0.11 (0.07)	2,253
Number of days ready to wait for 20% premium	97.76 (78.28)	0.15 (2.99)	3,373	101.86 (78.21)	-6.90* (3.85)	2,300
Locus of control	0.00 (1.00)	0.01 (0.03)	3,270	0.00 (1.01)	0.01 (0.04)	2,260
Life satisfaction	71.19 (20.49)	0.64 (0.61)	3,406	71.69 (20.10)	-0.63 (0.81)	2,308
Growth mindset	-0.00 (1.00)	0.00 (0.04)	3,355	0.01 (1.00)	-0.03 (0.04)	2,288
Number of friends	4.03 (2.84)	-0.08 (0.10)	3,409	4.18 (2.88)	0.05 (0.11)	2,308
No friends	5.42 (22.65)	2.04** (0.91)	3,409	6.43 (24.55)	0.16 (0.98)	2,308

Data: Midline and endline surveys (respectively April 2012 and April 2013).

Notes: The table provides Intention To Treat estimates for further variables related to participants' ability means, personality traits, and social integration. Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. The table has two vertical panels. The left panel is based on the midline survey of April 2012, while the right panel relies on the endline survey of April 2013. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. The trust index encompasses trust in school, the healthcare system, the Job Youth Center (JYC), and the justice system. The personality index covers a variable measuring the time the respondent is willing to wait for a €250 gain versus an immediate €200, a life satisfaction scale, and a locus of control variable. Standard errors are robust to heteroskedasticity and clustered at the JYC level. * indicates that the parameter is statistically significant at the 10% level, ** at the 5% level, and *** at the 1% level. Binary outcomes (e.g., "Foot") are multiplied by 100 so that the results can be interpreted in percentage terms. This table complements Table 5 in the main text.

D Appendix Heterogeneity Tables

Table A11: Heterogeneity – Time preferences

	<u>Impatient</u>		<u>Patient</u>		N	Equal (p-value)
	Control Mean	Treatment Effect	Control Mean	Treatment Effect		
<u>Midline variables</u>						
Months employed over 1st sem.	2.33 (2.32)	-0.16 (0.10)	2.51 (2.36)	-0.18** (0.09)	3,373	0.87
Human capital index	-0.18 (3.53)	0.10 (0.15)	0.17 (3.40)	0.08 (0.18)	3,373	0.94
Search index	0.23 (5.28)	0.01 (0.26)	-0.20 (5.31)	-0.25 (0.25)	3,373	0.50
Income	580.52 (482.08)	40.80* (21.34)	629.25 (496.10)	40.77* (23.56)	3,373	1.00
Amount saved	171.10 (394.29)	39.25* (22.96)	252.89 (456.14)	35.90 (24.49)	3,268	0.92
Financial constraint index	0.47 (4.33)	-0.15 (0.22)	-0.44 (3.84)	-0.09 (0.22)	3,373	0.84
<u>Endline variables</u>						
Employed after 24 months	50.12 (50.06)	2.37 (2.84)	55.19 (49.78)	0.33 (2.96)	1,906	0.62
<u>Administrative variables</u>						
Months in program	11.94 (6.64)	7.86*** (0.43)	12.23 (6.69)	8.05*** (0.47)	3,370	0.72
Total number of meetings	8.13 (7.69)	5.00*** (0.57)	7.77 (7.31)	6.04*** (0.60)	3,373	0.07
Training over the first quarter						
Proposed	0.94 (1.58)	0.31** (0.12)	0.95 (1.53)	0.38*** (0.10)	3,373	0.53
Matched	0.16 (0.46)	0.03 (0.03)	0.16 (0.47)	0.06** (0.02)	3,373	0.33
Started	0.48 (1.01)	0.04 (0.04)	0.53 (1.06)	0.00 (0.04)	3,373	0.42

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).

Notes: TO BE COMPLETED.

Table A12: Heterogeneity – Financial constraints

	Control Mean	<u>Low</u> Treatment Effect	Control Mean	<u>High</u> Treatment Effect	N	Equal (p-value)
<u>Midline variables</u>						
Months employed over 1st sem.	2.59 (2.35)	-0.20* (0.10)	2.23 (2.31)	-0.16* (0.09)	3,409	0.81
Human capital index	0.14 (3.40)	-0.10 (0.15)	-0.15 (3.54)	0.23 (0.18)	3,409	0.14
Search index	-0.32 (5.22)	-0.09 (0.24)	0.34 (5.36)	-0.08 (0.23)	3,409	0.97
Income	603.73 (480.47)	38.28* (21.13)	600.56 (499.08)	42.41* (22.51)	3,409	0.89
Amount saved	271.53 (493.31)	21.59 (24.68)	146.61 (331.37)	52.17** (23.61)	3,295	0.37
Financial constraint index	-0.94 (3.50)	0.17 (0.18)	1.00 (4.46)	-0.39* (0.23)	3,409	0.04
<u>Endline variables</u>						
Employed after 24 months	55.11 (49.78)	0.54 (2.74)	48.41 (50.02)	0.51 (3.14)	2,308	1.00
<u>Administrative variables</u>						
Months in program	11.56 (6.45)	8.01*** (0.34)	11.16 (6.41)	7.74*** (0.41)	5,476	0.48
Total number of meetings	7.06 (6.77)	5.28*** (0.46)	7.51 (7.88)	5.38*** (0.53)	5,482	0.79
Training over the first quarter						
Proposed	0.86 (1.49)	0.33*** (0.09)	0.92 (1.60)	0.39*** (0.11)	5,482	0.54
Matched	0.14 (0.46)	0.05** (0.02)	0.17 (0.51)	0.05** (0.02)	5,482	0.92
Started	0.46 (1.00)	0.03 (0.04)	0.53 (1.05)	0.00 (0.03)	5,482	0.56

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).

Notes: TO BE COMPLETED.

Table A13: Heterogeneity – Caseworker quality

	<u>Low quality</u>		<u>High quality</u>		N	Equal (p-value)
	Control Mean	Treatment Effect	Control Mean	Treatment Effect		
<u>Midline variables</u>						
Months employed over 1st sem.	2.45 (2.34)	-0.18 (0.13)	2.48 (2.30)	-0.16 (0.12)	2,315	0.93
Human capital index	-0.26 (3.38)	0.33 (0.22)	0.33 (3.55)	-0.24 (0.23)	2,315	0.12
Search index	-0.24 (5.04)	0.15 (0.30)	0.12 (5.46)	-0.32 (0.27)	2,315	0.24
Income	606.49 (488.16)	50.89* (29.40)	590.63 (478.78)	51.38* (28.61)	2,315	0.99
Amount saved	192.95 (418.31)	55.83* (28.98)	241.37 (461.88)	33.17 (31.12)	2,237	0.55
Financial constraint index	-0.03 (4.15)	-0.21 (0.25)	-0.15 (4.19)	-0.00 (0.27)	2,315	0.54
<u>Endline variables</u>						
Employed after 24 months	49.10 (50.07)	4.33 (3.49)	54.44 (49.87)	-0.58 (3.90)	1,576	0.40
<u>Administrative variables</u>						
Months in program	11.46 (6.24)	7.87*** (0.36)	11.46 (6.65)	8.22*** (0.50)	3,710	0.51
Total number of meetings	6.97 (7.39)	5.37*** (0.53)	7.75 (7.60)	5.15*** (0.44)	3,710	0.68
Training over the first quarter						
Proposed	0.78 (1.41)	0.41*** (0.11)	0.88 (1.60)	0.25*** (0.08)	3,710	0.22
Matched	0.14 (0.47)	0.05** (0.03)	0.15 (0.46)	0.06** (0.03)	3,710	0.89
Started	0.48 (1.00)	-0.01 (0.04)	0.52 (1.04)	0.03 (0.05)	3,710	0.51

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).

Notes: TO BE COMPLETED.

Table A14: Heterogeneity – Locus of control

	External		Internal		N	Equal (p-value)
	Control Mean	Treatment Effect	Control Mean	Treatment Effect		
<u>Midline variables</u>						
Months employed over 1st sem.	2.24 (2.31)	-0.14 (0.11)	2.62 (2.35)	-0.25** (0.10)	3,270	0.49
Human capital index	-0.18 (3.43)	0.19 (0.17)	0.17 (3.47)	-0.05 (0.17)	3,270	0.31
Search index	0.29 (5.21)	-0.01 (0.30)	-0.27 (5.34)	-0.17 (0.22)	3,270	0.68
Income	557.96 (472.72)	69.94*** (22.86)	650.52 (496.59)	7.69 (20.25)	3,270	0.04
Amount saved	202.38 (420.76)	37.82* (22.10)	225.93 (438.97)	33.54 (24.12)	3,170	0.89
Financial constraint index	0.31 (4.39)	-0.23 (0.21)	-0.32 (3.80)	0.02 (0.18)	3,270	0.30
<u>Endline variables</u>						
Employed after 24 months	51.72 (50.03)	0.91 (3.39)	54.99 (49.80)	0.62 (3.42)	1,851	0.96
<u>Administrative variables</u>						
Months in program	12.27 (6.63)	7.76*** (0.46)	11.82 (6.67)	8.30*** (0.42)	3,268	0.25
Total number of meetings	8.17 (7.74)	6.02*** (0.66)	7.60 (7.23)	5.34*** (0.50)	3,270	0.21
Training over the first quarter						
Proposed	1.03 (1.67)	0.24* (0.13)	0.86 (1.44)	0.46*** (0.11)	3,270	0.07
Matched	0.17 (0.53)	0.01 (0.03)	0.14 (0.41)	0.07*** (0.02)	3,270	0.11
Started	0.51 (1.04)	-0.02 (0.05)	0.50 (1.02)	0.06 (0.04)	3,270	0.26

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).

Notes: TO BE COMPLETED.

Table A15: Heterogeneity – Local youth unemployment rate

	Control Mean	<u>Low</u> Treatment Effect	Control Mean	<u>High</u> Treatment Effect	N	Equal (p-value)
<u>Midline variables</u>						
Months employed over 1st sem.	2.76 (2.35)	-0.18* (0.10)	2.10 (2.28)	-0.17* (0.09)	3,380	0.95
Human capital index	-0.08 (3.38)	0.06 (0.18)	0.08 (3.55)	0.06 (0.16)	3,380	1.00
Search index	-0.32 (5.40)	0.09 (0.23)	0.31 (5.21)	-0.21 (0.23)	3,380	0.37
Income	674.00 (491.19)	19.05 (23.29)	539.01 (479.44)	57.23*** (19.05)	3,380	0.20
Amount saved	216.47 (427.53)	37.79 (25.22)	208.56 (429.74)	33.90 (24.23)	3,269	0.91
Financial constraint index	0.22 (4.23)	-0.24 (0.24)	-0.18 (4.00)	-0.00 (0.20)	3,380	0.45
<u>Endline variables</u>						
Employed after 24 months	54.39 (49.85)	0.27 (3.13)	49.91 (50.04)	0.55 (2.49)	2,288	0.94
<u>Administrative variables</u>						
Months in program	11.13 (6.31)	7.40*** (0.36)	11.65 (6.51)	8.31*** (0.54)	5,426	0.16
Total number of meetings	6.74 (6.86)	4.16*** (0.45)	7.82 (7.79)	6.58*** (0.63)	5,432	0.00
Training over the first quarter						
Proposed	0.67 (1.29)	0.27*** (0.09)	1.08 (1.72)	0.46*** (0.14)	5,432	0.28
Matched	0.10 (0.36)	0.06** (0.02)	0.20 (0.57)	0.05* (0.03)	5,432	0.78
Started	0.50 (1.03)	0.01 (0.03)	0.50 (1.03)	0.02 (0.04)	5,432	0.90

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).

Notes: TO BE COMPLETED.

Table A16: Heterogeneity – Disconnect from labor market

	Control Mean	<u>Low</u> Treatment Effect	Control Mean	<u>High</u> Treatment Effect	N	Equal (p-value)
<u>Midline variables</u>						
Months employed over 1st sem.	2.69 (2.36)	-0.19** (0.09)	1.92 (2.22)	-0.17* (0.10)	3,409	0.92
Human capital index	0.22 (3.50)	-0.20 (0.14)	-0.41 (3.38)	0.49** (0.21)	3,409	0.01
Search index	-0.25 (5.34)	-0.01 (0.20)	0.45 (5.19)	-0.21 (0.30)	3,409	0.56
Income	646.84 (483.50)	42.89** (18.22)	521.36 (490.11)	36.23 (26.14)	3,409	0.83
Amount saved	254.11 (461.61)	34.26 (22.08)	133.95 (344.70)	40.65 (25.28)	3,295	0.84
Financial constraint index	-0.40 (3.95)	-0.05 (0.17)	0.72 (4.29)	-0.22 (0.24)	3,409	0.47
<u>Endline variables</u>						
Employed after 24 months	57.50 (49.47)	-0.54 (2.44)	40.67 (49.19)	2.69 (4.08)	2,308	0.52
<u>Administrative variables</u>						
Months in program	11.15 (6.36)	8.21*** (0.35)	11.63 (6.52)	7.43*** (0.42)	5,476	0.04
Total number of meetings	6.82 (6.81)	5.54*** (0.49)	7.94 (8.01)	5.07*** (0.51)	5,482	0.20
Training over the first quarter						
Proposed	0.83 (1.45)	0.31*** (0.08)	0.97 (1.67)	0.43*** (0.12)	5,482	0.26
Matched	0.14 (0.45)	0.04** (0.02)	0.18 (0.52)	0.07*** (0.03)	5,482	0.18
Started	0.42 (0.95)	0.01 (0.03)	0.61 (1.11)	0.02 (0.03)	5,482	0.84

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).

Notes: TO BE COMPLETED.

Table A17: Heterogeneity – Gender

	Female		Male		N	Equal (p-value)
	Control Mean	Treatment Effect	Control Mean	Treatment Effect		
<u>Midline variables</u>						
Months employed over 1st sem.	2.34 (2.34)	-0.13 (0.10)	2.51 (2.32)	-0.24** (0.09)	3,409	0.44
Human capital index	-0.16 (3.35)	-0.02 (0.17)	0.19 (3.59)	0.15 (0.18)	3,409	0.49
Search index	0.16 (5.34)	-0.13 (0.27)	-0.19 (5.25)	-0.04 (0.25)	3,409	0.83
Income	567.70 (444.55)	46.59** (20.01)	643.17 (535.29)	33.33 (25.21)	3,409	0.68
Amount saved	194.76 (402.73)	35.15* (20.37)	230.60 (454.26)	38.41 (27.86)	3,295	0.92
Financial constraint index	0.17 (4.27)	-0.09 (0.20)	-0.20 (3.91)	-0.14 (0.23)	3,409	0.87
<u>Endline variables</u>						
Employed after 24 months	51.16 (50.03)	1.30 (2.65)	53.19 (49.95)	-0.29 (2.65)	2,308	0.65
<u>Administrative variables</u>						
Months in program	11.61 (6.41)	7.62*** (0.32)	11.07 (6.45)	8.14*** (0.44)	5,476	0.19
Total number of meetings	7.51 (7.66)	5.26*** (0.49)	7.05 (7.02)	5.41*** (0.54)	5,482	0.73
Training over the first quarter						
Proposed	0.93 (1.61)	0.39*** (0.10)	0.84 (1.47)	0.33*** (0.10)	5,482	0.54
Matched	0.16 (0.49)	0.05** (0.02)	0.15 (0.48)	0.05** (0.02)	5,482	0.84
Started	0.52 (1.04)	0.00 (0.03)	0.47 (1.00)	0.03 (0.03)	5,482	0.64

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).

Notes: TO BE COMPLETED.