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

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

Abstract

Youth participation in employability programs is usually low. This paper studies the first pure test of a randomized conditional cash transfer linked to participation in labor activation measures: 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 jobseekers. However, 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.

JEL classification

J68, J64, C93

Keywords

randomized experiment, conditional cash transfer, labor market activation

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1 Introduction

Many youths enter the job market with few qualifications and little to no knowledge of the job market. Globally, 20.4% of youths aged between 15 and 24 years are not in employment, education, or training (NEET) (ILO, 2024). 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 (Pignatti and Van Belle, 2018; Quintini et al., 2007). This paper evaluates the first randomized conditional cash transfer (CCT) linked to participation in an employability program. 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 employability 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 (Romero and Kuddo, 2019). 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 effects are often temporary and program beneficiaries may displace other workers (Caliendo and Schmidl, 2016; Crépon et al., 2013).

A key potential explanation why a diverse range of programs produces limited effects is that youths do not participate in them, or drop out early. 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 such programs makes them rush to find employment to avoid attending. Worse still, LaLonde (2003) and Ivry and Doolittle (2003) argue that those youths who would benefit most from assistance programs do not enroll, and that the majority of those who do enroll invest little energy in the programs (see e.g. Decker et al., 2000).

Theoretical models of human capital investments identify several potential factors in the demand for employability investments. Seminal models highlight the role of expected returns of employability investments (Heckman et al., 1999), which in turn depend on labor market conditions, program quality, and opportunity costs. They also underscore the central role of financial constraints in the decision to invest: Youths may not be able to afford training costs, or may not be able to give up subsistence activities (such as low-skill or informal work) in order to make time for job search

[1] A 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.

and training. These constraints typically interact with parents' incomes. Furthermore, recent work in behavioral economics shows that individuals frequently exhibit non-standard preferences and beliefs in their labor market choices (reviewed by Babcock et al., 2012 and DellaVigna, 2009). First, perceived returns rather than real returns determine investment in job search and training, and these perceptions are often biased (Spinnewijn, 2015). The need to choose from a portfolio of services adds an additional layer of complexity (Babcock et al., 2012). In addition to beliefs about program returns or labor market conditions, recent evidence points to the importance of individuals' subjective beliefs about their ability to affect outcomes and achieve goals: psychological concepts known as locus of control and self-efficacy strongly predict training and job search choices (Caliendo et al., 2015, 2022; McKelway, 2024). Lastly, intertemporal preferences for the *here and now* matter (DellaVigna and Paserman, 2005; Paserman, 2008): young people may systematically favor low-paying, insecure jobs, which are easy to find, and procrastinate on investments in human capital.

How can governments increase participation in employability programs, given the wide range of potential barriers in beliefs, incentives, and financial constraints? A candidate solution comes from the large body of work on cash transfers conditional on school attendance or health clinic visits in low and middle income countries (reviewed by Baird et al., 2014 and Lagarde et al., 2007): individuals could be paid for their participation in the programs. Such short-term incentives may be effective both in the case of pessimistic beliefs and present bias, and can also address financial constraints. Yet, they are infrequently used in labor market contexts: Where governments do provide sizable transfers to unemployed youths, these typically take the form of minimum welfare benefits, which are by nature unconditional.[2] Such unconditional cash transfers may alleviate financial constraints, but they do not address a lack of motivation (due to pessimism or present bias) to invest in human capital. In France, youths below 25 years are not eligible for welfare payments, and thus have no guaranteed minimum income. This provides a unique setting for the introduction of cash transfers which are conditional on employability investments.

This paper studies the randomized introduction of a cash transfer conditional on participation in the French national career guidance program. NEET young adults in France are offered enrollment in a national career guidance program, the "Contrat d'Insertion dans la Vie Sociale" ("Social Integration Contract", hereafter, the guidance program). It is administered in Job Youth Centers (JYCs) by trained job counselors. Following low attendance and high dropout rates, the French government agreed to trial monthly cash transfers: among the 5498 youths who registered for the guidance program in February and March 2011 in participating JYCs, a random half were offered monthly payments. Conditional on complying with the program, a €250 transfer was paid monthly during the first year. The amount decreased gradually during the second year (€240 in the first quarter down to €60 in the last quarter). If a participant found employment, the monthly

[2]Youths with little or no work experience do not usually have access to unemployment insurance. Austria, Germany, Portugal, and Sweden provide minimum welfare benefits based on parental income while the young person is still dependent upon them, and on their own income otherwise. In Denmark, Finland and Netherlands, a minimum benefit is paid based solely on the young person's income. Some countries couple welfare systems with earned income tax credit to maintain incentives to find a job, including the United States, the UK, and Canada.

transfer decreased at a rate of 24 cents per additional Euro earned. A participant with no labor income could thus receive up to €4,800 in cash transfers during the two-year program. If the youth failed to attend meetings or comply with the tasks stipulated by the program, the JYC could decide to suspend the transfers. In practice, the personalized nature of the guidance program meant that tasks were diverse and partially non-contractable. As a result, the key contractable behavior was to attend regular meetings with the counselor. Those assigned to the control group participated in the guidance program without cash transfers or specific requirements for participation.

Results show a significant increase in program participation and a drastic decrease in the drop-out rate: the average number of months spent in the program increases by 52 percent (from 13.1 to 19.9 months) and the number of meetings with a counselor increases by 62 percent (from 8.2 to 13.3 per participant). More meetings translate to a 55 percent increase in the services offered to the participants by their counselors, including information and matches with jobs, training, and career planning services. Transfers received from the JYC increase steeply by €1,528 (from a base of €237).[3] Despite this noted improvement in participation, recipients do not invest more in their employability: we observe null effects on a wide range of outcome behaviors, from enrollment in the trainings proposed by the counselors during the meetings 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 amount. In addition to income-based tapering of benefits, we observe substitution with other income sources, such as transfers from family or friends. Lastly, variables collected to measure social integration show no improvement. A notable exception is increased trust towards government institutions, including the JYC and the education system.

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, counselor quality, or labor market conditions as key mechanisms explaining 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 counselor. Our findings emphasize the importance of conditioning incentives directly on outcomes of interest (such as trainings or job search), rather than only on intermediary steps (like meetings with the counselor).

Our paper contributes to and connects the literatures on conditional cash transfers and labor

[3]We show that the shortfall relative to the maximum of €4,800 is primarily due to the income taper rule.

market activation. First, to the authors' knowledge, we provide the first pure test of a randomized conditional cash transfer which conditions on participation in labor market activation.

An extensive and rigorous literature studies the effects of conditional (as well as unconditional) cash transfers on educational and health outcomes (see Garcia and Saavedra, 2023, Glewwe and Muralidharan, 2016 and Millán et al., 2019 for recent reviews). Starting with the well-known PROGRESA program in Mexico in the 1990s, CCT programs are traditionally conditioned on children's school attendance or health clinic visits. Recent work investigates the labor supply effects of CCT programs (Baird et al., 2018), often on parents. Returning to the domain of labor economics, unemployment insurance in high-income countries is essentially a cash transfer conditional on *not* being employed. A large non-experimental literature studies how the amount and duration of unemployment benefits impact the length of time individuals remain unemployed (see Schmieder and Von Wachter (2016) for a recent review). While more rarely feasible, a few studies exploit randomization of benefits or earnings subsidies with employment conditions (Card and Hyslop, 2005; Verlaat et al., 2023), or test the removal of conditionality (Verho et al., 2022). However, these strands of literature do not allow insights on how governments can address low participation in labor activation measures, increase adult human capital investment, or help those who do not have access to unemployment insurance.

Closer to our activation focus, a recent literature studies the effects of bundling cash transfers with job search assistance and other support policies. Del Boca et al. (2021) and Aparicio Fenoll and Quaranta (2022) study a randomized experiment in Italy where low-income families with young children received lump-sum cash transfers for attending training courses on parenting, job search, and money management, assigned by a social worker based on the family's needs. Control group participants were not assigned to courses. The authors show positive effects of the cash and training bundle on fathers' employment, but no or negative effects on mothers. Close to our setting, Filippucci (2022) studies the combined effect of cash and activation measures in a sample of NEETs in France. The labor market policy he evaluates (*Garantie Jeunes*) succeeded ours, and differs in its non-experimental nature, the provision of activities specific to cash recipients, and the fact that participants were selected on demonstrated motivation as well as specific eligibility criteria. Using a difference-in-difference strategy which compares jobseekers who registered in JYCs before and after program introduction, he estimates positive effects on employment. Finally, Chiodi et al. (2023) study the randomized introduction of lump-sum cash transfers bundled with job search assistance, housing assistance and rent guarantees, suggesting that neighborhood effects interact with labor market dynamics.

Our study differs fundamentally in its design and objective: Cash transfers are used to incentivize participation in activities that *all jobseekers* should be doing: regularly meeting their caseworker, and engaging with the proposed trainings, workshops, or job offers. This is reminiscent of the original CCT literature on school attendance and health clinic visits, but not commonly

evaluated in labor markets.[4] We innovate by isolating the effect of a randomized cash transfer conditional on participation in activities which are accessible (and recommended) for all jobseekers.

Finally, our paper relates to a small literature on search cost subsidies: transfers that are paid to all jobseekers to reimburse specific expenses, such as transport costs or bus tickets to travel to the city (Abebe et al., 2021; Bryan et al., 2014; Franklin, 2018; Banerjee and Sequeira, 2020). These subsidies are typically small (9.3 USD in Abebe et al., 2021), and have been effective in increasing employment and seasonal migration in low-income countries. Arguably, the search frictions they address (e.g. lack of online labor markets) are distinct from the high-income context we consider. The CCT we study further differs by increasing jobseekers' overall budget set, rather than subsidizing a specific expense. It may thus allow participants to address a wider range of financial constraints.

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 presents a principal-agent model and 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. In 2010 – one year before our CCT program started – a representative national survey on labor market integration was conducted with youth who exited the education system in 2007 (Table A1). According to the survey, 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 is provided by 450 Job Youth Centers (JYCs) located throughout France. 20.6 % of those who finished school in 2007 went to a JYC at least twice by 2010. Youths without a high-school diploma were disproportionately represented (63.7 %) among those who sought help at the JYC. Compared to the population as a whole, JYC attendees were 55 percent (9.6 percentage points) more likely to have repeated a year in primary school, and 89 percent (19.3pp) more likely to have left school before their 18th birthday. They were 21 percent

[4] Unemployment benefit programs commonly impose some conditions regarding job applications or training, with possible sanctions in case of non-compliance. We are not aware of any study which causally identifies the effect of such conditional transfers on participation in activation measures. van den Berg et al. (forthcoming) find positive effects from formalizing such conditions in Integration Agreements in Germany. However, unemployment benefit levels and factual conditions on job search and training were the same in treatment and control groups.

(4.6pp) more likely to have immigrant parents, and 51 percent (4.2pp) more likely to live in deprived suburbs.

2.2 The Guidance Program (G)

The JYCs offer a guidance program (G) to facilitate labor market integration: the *Contrat d'Insertion dans la Vie Sociale*. 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 aims to identify and steer participants towards the 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.[6] If participants enroll in a course or find short-term work, they remain in the program, and are expected to stay 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).

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. We refer to the resulting program as 'guidance + conditional cash transfer (G+CCT).' Participants received €250 per month in the first year, equal to 23% of the French

[5] Very 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 counselor.

[6] Programs which tailor services individually to participants' needs have been shown to be most effective by Kluve et al., 2019.

monthly minimum wage and 54% of the guaranteed minimum income scheme for adults over 25. 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 most enforceable criterion in practice was the attendance of the monthly meetings with the counselor.

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 legal minimum monthly salary as of April 2011. The tapering implied a linear tax on employment income of 250/1050=24% (see Figure 2b). Employment revenue includes wages, unemployment insurance and training compensation. The tapering of the program thus interacts with employability investments: A participant who starts a certified training course would earn €325 per month, but see their CCT 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 CCTs on participation in the guidance program, dropout rates, employability investments, and employment. An invitation to take part in the study was sent 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 as control group participants within the same JYC would have learned about the transfers, and complained about preferential treatment by the JYC. Randomization at the JYC level would have yielded limited statistical power, and created endogenous selection in who registers for the guidance program (while a given jobseeker can only register at the JYC for their geographical area, the registration decision would be affected by whether this JYC offers cash transfers). We solve these selection and power issues by randomizing *within* each JYC and by cohorts, based on whether individuals signed up for the guidance program in February or in March 2011. This was done as follows: First, registrations for the 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 cash transfer experiment, 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 3 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, 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: all the offers made to participants while in the program, including information about job offers, opportunities for training or career building services, proposals and matching. This allows us to assess 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. Administrative records are tied to participants having contact with the JYC. We thus rely on administrative data mainly to measure effects on program participation and opportunities offered by the JYC.

In addition, 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 on average, 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, Table 1 shows 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. As discussed above, we verify the randomization is balanced in all three samples that are relevant for the analysis. 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 program in February and March 2011 are 19.7 years old (program eligibility was restricted to age 18-22, so that transfers would complete by age 24). Participants have few qualifications, with most leaving school at the high school or basic vocational level. Only 30 % have a driver's license, which is an important but expensive asset for social inclusion in France (see footnote 10). 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 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 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 a Lasso post-double-selection procedure to the model:

$$y_{m,i} = \alpha + \beta_{ITT} T_i + \gamma X_i + \lambda_m + \epsilon_{m,i} \tag{1}$$

where y is the outcome of interest for individual i, T 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-pair 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 guidance program (G) without cash transfers.

Finally, to look at heterogeneity in our results in relation to a subsample identified by a dummy variable I (see Section 5), we estimate an equation in which the treatment group variable interacts with the I dummy and the $(1 - I)$ dummy:

$$y_{m,i} = \alpha + \beta_{ITT,I} T_i \times I_l + \beta_{ITT,1-I} T_i \times (1 - I_l) + \delta I_l + \gamma X_i + \lambda_m + \epsilon_{m,i} \quad (2)$$

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. We test for equality $\beta_{ITT,I} = \beta_{ITT,1-I}$.

4 Results

4.1 Increased Program Attendance

Figure 4 shows the estimated ITT effects on enrollment and participation in the program. As outlined in Section 2.2, participants left the program when they found stable employment, or when the counselor terminated their contract due to lack of participation. Figure 4a shows enrollment rates, estimated using equation 1. Month 0 is April 2011, when randomization occurred and cash transfers started. The dotted line is the mean enrollment in the control (group G) for a given month. The shaded area 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.

Figure 4a shows a steady decline in enrollment rates throughout the first year of the guidance program (G), culminating in a sharp drop in months 11 and 12. Enrollment then stabilizes at 20% 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 common that participants who miss several appointments 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 remains around 70 percent throughout the second year.[7]

Figure 4b shows the average number of counselor meetings by month per jobseeker. Unlike program enrollment, average monthly meetings decrease at a steady rate over the two years in

[7]The counselors' discretion may introduce confounds into the re-registration decision. For instance, altruistic counselors may be reluctant to terminate contracts because they do not want to deprive jobseekers of the cash transfers. We thus put more weight on objective measures of participation: registered meetings and other exchanges.

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 is consistently higher until the end of the program. This confirms with objective measures that the cash transfers had real impacts on program participation. The rate of overall exchanges with the JYC counselor (including emails and calls, available upon request) closely mirrors the rate of meetings, and also shows consistent treatment effects throughout the program duration. 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.

The first panel of Table 2 summarizes the estimated Intent-to-Treat (ITT) effects on program participation. Among youths assigned to the treatment group, 82% accepted to convert their guidance program contract to one with cash transfers (see Section 3), while nobody in the control group had access. The offer of the cash transfer program increased the number of meetings over the two-year period by 62 percent (or 5.1 meetings from a base of 8.2). Average months of program enrollment increased by 69 percent (by 7.9 months from a base of 11.4, not in Table). The probability to remain enrolled after one year doubles (from a base of 40 percent).[8] The panel ends with the total payments received from the JYC during the program: control group participants received €237 from the JYC over the two-year period (mostly reimbursements of job search costs), while youths assigned to the cash transfers received €1,765 on average.

To summarize, Table 2 as well as Figures 4a and 4b 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 €300 (= 1528/5.1) 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

[8]The JYC records can be cross-validated using the midline survey from April 2012: respondents were asked how many meetings were held in the last three months (months 10 to 12 of the program). Treatment significantly increased JYC meetings in three months by 0.77, from a control mean of 1.58. There is no substitution 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).

in which youths were matched with providers of these job offers, training courses, and career services. Matching goes beyond information provision in that the counselor facilitates a first contact between the participant and the provider.

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 top panel 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 4.5 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 2.14 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 (job offers, training, or career planning, shown in Table A3), 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, rates of matching with training and job offers 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 outcomes regarding training enrollment and employment, 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 suggested 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) and Anderson (2008) in establishing an index for each outcome category.[9]

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 jobseeker's individual situation, rather than promoting general 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 have obtained or started to prepare their driver's license since entering the guidance program is 2.66 percentage points higher in the treatment group, with a mean of 41.9 % in the control group.[10] We estimate a precise zero effect on the overall index of investment,and conclude with high statistical power that the treatment has no impact on human capital investment.

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 for an open-ended job contract. The bottom panel of Table 3 shows that treatment does not affect job search behavior on average. 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

[9] We standardize variables by subtracting the control group mean and dividing by the control group standard deviation. We then sum the standardized variables to form an index for each outcome category. Consistent with Kling et al. (2007), we do not re-standardize the summed variables to avoid artificially deflating standard errors. This implies that our indices will be mean zero, but will have standard deviations larger than one.

[10] 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 disadvantaged neighbourhoods from town centres, a driver's licence is seen as a key asset in the search for employment.

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. Estimated using equation 1, Figure 4c presents the rate of employment on full-time contracts, while Figure 4d 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 of the first year, 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 employment (formal or informal), on subsidized jobs, on whether it is full-time or part-time, if fixed-term (typically six months or less) or open-ended, 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).

4.5 Limited Substitution Between Income Sources

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 the month preceding the midline (endline) survey in April 2012 (2013).

Overall, we find that treatment group income in month 11 after randomization (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 11 of the program, subjects

were still entitled to the maximum theoretical transfer amount of €250 per month. 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 €48 in other sources, consisting of employment income (€21, not significant), family and friends (€9), other government transfers (€8) 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: Income from activity (control mean €405) and from other government transfers (control mean €78) reduced transfer amounts at a rate of 24%, accounting for a transfer gap of €116. The remaining difference is explained by the fact that only 82% of participants accepted the cash transfer program, and 6.4% of these dropped out of the program before month 12.

4.6 Transfers Are Used to Increase Savings

The results above provide insights into how transfers were used. If participants had used the transfer to replace low-skill or insecure work with human capital investments, we would expect JYC income to crowd out employment income. Instead, employment income changes little, and many participants received reduced transfers because they continued to earn income from work or other sources. This pattern is consistent with two interpretations. First, financial constraints may not be the main reason why young people remain in low-skill employment. Second, financial constraints may matter, but the transfer may have been too small to allow participants to give up earnings while investing in human capital. In this case, participants may have remained in low-skill or insecure work out of subsistence needs, rather than choice. 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 (see 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 (such as full-time employment contracts) or human capital investments (such as apprenticeships) to be indivisible. We explore this and other mechanisms theoretically and empirically in Section 5.

5 Evidence for Mechanisms

The results in the preceding section show that the intervention affected the incentivized margin, but not the downstream behaviors it was intended to promote. Conditional cash transfers substantially increase participation in the guidance program, generating more meetings with counselors and substantially more exposure to employment services. Yet participants do not increase their employability investments and employment outcomes do not improve. The key institutional feature of the program is that transfers can be conditioned on participation in counseling activities, but not directly on the employability investments that ultimately determine labor market success. Attendance at meetings is observable and contractible, whereas subsequent investments such as training, apprenticeships or job search effort are much harder to monitor and enforce.

The following section studies the mechanisms behind the observed treatment effects (or the lack thereof). We introduce a theoretical framework using a modified principal-agent model. Labor market integration is modelled as a two-step process. The first step consists of participation in meetings with counselors (e_1). The second step consists of the actual employability investments undertaken by the jobseeker (e_2). The transfer directly affects the incentive to attend counseling meetings, while employment outcomes depend primarily on subsequent employability investments. The theoretical framework clarifies under which conditions the transfer may affect e_2, either through increased participation in meetings or through a direct relaxation of constraints on investment. We discuss 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. Figure 1 summarizes the logic of the intervention.

5.1 Theoretical Framework: Benchmark Case

Unlike standard models of human capital investment, the planner cannot directly contract on the productive action of interest. In our setting, counselors can observe and reward participation in meetings, but they cannot perfectly observe or verify the employability investments subsequently undertaken by jobseekers. This distinction mirrors the institutional reality of many labor market

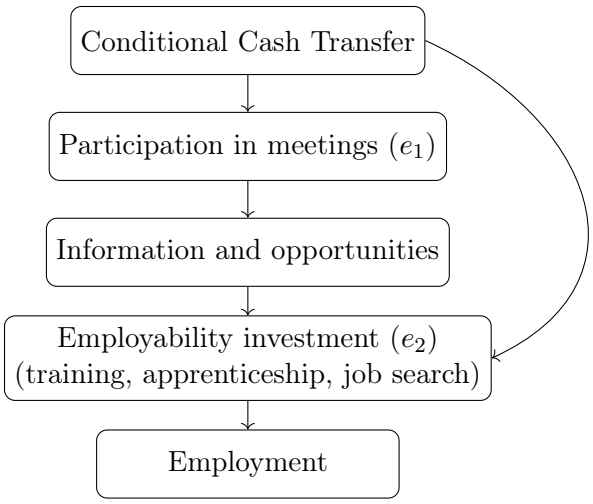

Figure 1: Conceptual framework

activation programs. We therefore model labor market integration as a two-step effort process. First, the agent needs to meet with the counselor (principal or planner), $e_1 \in \{0, 1\}$, incurring a cost ψ_1. During the meeting, she learns about the required second step to find a job, $e_2 \in \{0, 1\}$, which costs an additional ψ_2. Effort e_2 can be thought of as the specific training, apprenticeship, or direct job search that is most suitable for the jobseeker.[11] Jointly exerting e_1 and e_2 results in a probability of employment π_2, while the baseline probability of finding a job is π_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: $\pi_1 = \pi_0$. 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. Employment outcomes are realized in the period following an initial search period. In a static, risk-neutral benchmark case, the agent invests in effort e_1 and e_2 if

$$\Delta\pi(\bar{Y} - \underline{Y}) \geq \psi_1 + \psi_2 \tag{3}$$

Inequality 3 represents the optimality condition for a risk-neutral social planner, who shares the agent's valuation of employment as well as effort disutility. It will be used to evaluate underinvestment in effort. While distinct from a principal-agent model in that the agent directly cares about Y, there are parallels in that the planner and the agent may disagree about the optimal effort level.

5.2 Causes of Underinvestment

Two classes of mechanisms predict that the agent will underinvest in effort, relative to the preferences of a patient and risk-neutral social planner. The first class is a misalignment in the valuation of returns to investment, $\Delta\pi(\bar{Y} - \underline{Y})$. The agent may discount the expected gains from employability

[11] We keep effort cost ψ_2 deterministic and constant here. Allowing for stochastic draws from the effort cost distribution would mostly predict heterogeneity in treatment effects by effort type: "low-cost effort" like web search and job applications should increase, while "high-cost 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.

investments due to time preferences or risk aversion. Alternatively, her beliefs about the returns $\Delta\pi$ themselves may diverge. A second class of mechanisms concerns the agent's constraints. For instance, subsistence constraints may prevent an agent from investing even when her preferences are aligned with those of the social planner. We briefly discuss key mechanisms in the context of the model below, and present the empirical evidence for them in Section 5.4.

Risk and Time Preferences

Effort costs are certain, finding a job is risky. Also, the benefits of employment are likely to accrue with some delay. Allowing for concave utility of consumption $u(c)$, and discounting returns in τ periods by D_τ, returns to human capital investment are reduced to $D_\tau \Delta\pi[u(\bar{Y}) - u(\underline{Y})]$. Underinvestment relative to the planner's preferences increases in discounting D_τ and risk aversion $u''(c) < 0$.

Returns to Effort

A large class of possible frictions is contained in the return to employability investments, captured in $\Delta\pi$. The agent and the social planner may disagree about $\Delta\pi$ for various reasons:

Program quality: The model captures program quality in the information which the counselors give to the jobseekers. If the counselors recommend human capital investments which are not suitable for the jobseeker, 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.

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 counselor in particular, to have the jobseeker's interests in mind. As a result, she may believe that the human capital investments suggested by the counselor are not privately profitable. The perceived $\Delta\tilde{\pi}$ will be low.

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 2024; John and Orkin 2022). Jobseekers may believe that their life is not in their own hands, and that hiring decisions depend largely on external factors. They may also believe that they would not be able to complete a given investment (say, an apprenticeship). Such beliefs about agency and ability are also captured in perceived $\Delta\tilde{\pi}$.

Labor market conditions: Labour market conditions for the target population may be difficult, for instance due to an excess supply of low-skilled workers. Labor market conditions become relevant to the extent that they affect the return to investment, $\Delta\pi = \pi_2 - \pi_0$.

Financial Constraints

Constraints may prevent the agent from investing even when her preferences are aligned with those of the planner. Suppose the agent needs to work in informal or low-skill work to finance subsistence

consumption $\underline{c}$, with $u(c) = -\infty$ for $c < \underline{c}$. Informal work yields income $\underline{Y} \geq \underline{c}$. However, time constraints prevent the agent from doing informal work and investing in human capital (such as vocational training) at the same time.[12] The agent needs to give up $\underline{Y}$ during the search period in order to gain $\Delta\pi(\bar{Y} - \underline{Y})$ in the employment period. For a patient planner, the optimality condition becomes

$$\Delta\pi(\bar{Y} - \underline{Y}) \geq \underline{Y} + \psi_1 + \psi_2 \tag{4}$$

However, the subsistence constraint makes investment impossible for the agent: Investment is risky given $\pi_2 < 1$, and any chance to incur $U(c) = -\infty$ is unacceptable.

5.3 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: $t \gg \psi_1$.[13] The transfer is paid during the initial search period, but not during the subsequent period of employment realizations.[14] Allowing flexibly for time discounting $D_\tau \leq 1$, risk aversion $u''(c) \leq 0$, and perceived returns $\Delta\tilde{\pi}$, the agent is willing to additionally exert e_2 iff

$$D_\tau[\Delta\tilde{\pi}(u(\bar{Y}) - u(\underline{Y}))] \quad \geq \quad \psi_2. \tag{5}$$

Condition 5 differs from the condition without transfers simply by eliminating the meeting cost ψ_1 from the right-hand side. Note that the transfer t drops out of the incentive constraint. As long as t is large enough to compensate the agent for attending the meetings, the exact size of the transfer is irrelevant. What is the effect on e_2? 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. Starting from a no-transfer benchmark where nobody invests, the less risk averse, more patient, and less pessimistic agents are more likely to respond to the transfer, as they are closer to the margin.

Predictions are qualitatively different if agents are constrained in their choices, e.g. due to subsistence needs. Two cases emerge: In the first case, for sufficiently large transfers $t \geq \underline{c}$, the subsistence condition is satisfied with meeting attendance alone. Since human capital investment has higher returns than low-skilled labor (inequality 4), the agent exerts e_1 and e_2. In the second, and likely more plausible case, the transfer covers only part of the subsistence constraint, $t < \underline{c}$. With the current assumption that informal work is indivisible (such as low-skill or seasonal work projects with a minimum time commitment), the cash transfer has no effect on human capital investment. As the agent cannot afford to give up $\underline{Y}$, she cannot invest in e_2), and thus she maintains her status

[12] This time constraint can be formalized by interpreting ψ_1 and ψ_2 as time costs, and allowing a time cost ψ_L for informal work. The agent has a time budget T, with $\psi_1 + \max(\psi_2, \psi_L) \leq T < \psi_1 + \psi_2 + \psi_L$.

[13] The formal condition is $u(\underline{Y} + t) - \psi_1 > u(\underline{Y})$.

[14] The model thus abstracts from the role of transfers as unemployment insurance. 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 observed in our data, but they are temporary – transfers are limited in time. This modeling choice thus represents a long-term view of human capital investment and job search.

quo regarding informal or low-skill work. What if low-skill work commitments are divisible, akin to an hourly wage? Then, for sufficiently large transfers, the agent can reduce subsistence work to a level that leaves enough time for human capital investment (which is assumed to be indivisible).[15] Finally, if both low-skill work and human capital investment are divisible, any size transfer will lead to higher investments in human capital, given its higher proportional returns.

Summary of Testable Predictions

The predictions of the model can be summarized as follows:

1. Any mechanism that reduces the expected returns to effort negatively predicts human capital investment and job search. This includes discounting of returns due to time preferences, and risk aversion towards uncertain payoffs. Direct mechanisms affecting expected returns 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. If real returns to effort are low, human capital investment should not predict employment in the data. Low *perceived* returns to effort do not generate a prediction for the relationship between investment and employment.
3. Transfers conditional on meeting attendance will increase human capital investment, job search, and employment only for agents close to the investment margin, for whom the cost of attending counselor meetings prevents the incentive constraint from being satisfied. As a result, treatment effects will be positive if at least some agents are at the margin.
4. Heterogeneity in treatment effects depends on *who* is at the margin. If no investments are made in the absence of transfers, then those with a higher valuation of the expected returns will react first. This includes more patient, less risk-averse, and less pessimistic agents.

In the presence of binding financial constraints, the model predicts that:

5. Financial constraints negatively predict human capital investment and job search, with a stronger impact on more time-consuming activities.
6. If income-generating activities during guidance program participation are indivisible, human capital investment will not react to a transfer that does not fully cover subsistence consumption.
7. If income-generating activities during guidance program participation are divisible, then *divisible* employability investments (like job search) will respond to the conditional cash transfer, while *indivisible* investments (like apprenticeships) will respond only if the transfer is sufficiently large.

Applied to the data, Predictions 1 and 5 should appear as differences in baseline investment behaviour across subgroups, independent of treatment status. Prediction 2 requires a correlation between investment outcomes and employment outcomes. Prediction 3 concerns overall treatment effects on employability investments. The limited evidence for such effects in Section 4 is therefore

[15] Formally, suppose that low-skill work $\lambda\psi_L$ yields $\lambda\underline{c}$ for $\lambda \leq 1$. Effort e_2 will respond iff $t > c_L(1 - \frac{(T-\psi_1-\psi_2)}{\psi_L})$.

consistent with few agents being close to the investment margin. Predictions 4, 6 and 7 should appear as heterogeneous effects of the cash transfer program.

5.4 Empirical Evidence

The following section discusses the available evidence for the presence of key barriers to human capital investment, and their interaction with the cash transfer intervention. We test the predictions derived in the previous section, and provide an analysis of heterogeneous treatment effects.

5.4.1 Time Preferences

Appendix D shows heterogeneous treatment effects across available measures of candidate mechanisms, estimated using equation 2. Unfortunately, we do not have a measure of risk preferences. However, theoretical predictions are closely aligned for risk and time preferences. For time preferences, we use a simple proxy for patience from the initial available data : participants were asked whether they were willing to wait a given amount of time (between one day and 6 months) 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.

Before looking at a potential heterogeneity in treatment effects, Prediction 1 implies that the control means between the impatient and patient subgroups in Table A11 should differ. Indeed, using our normalized human capital investment index (with mean zero across the whole sample), more patient jobseekers invest significantly more in human capital (through apprenticeships or trainings), reflected in means of 0.17 standard deviations for patient versus -0.18 standard deviations for impatient jobseekers (see footnote 9 on index construction). In contrast, they invest significantly less into job search (-0.20 versus 0.23 standard deviations). This goes beyond our highly stylized model, but it is intuitive when allowing for 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 Prediction 4:Key outcomes (human capital, search and employment) do not increase with the transfers, and there is no heterogeneity across patient and impatient subgroups. We find support for a crowd-out of employment among patient participants, and patient participants are also more often matched with specific investment opportunities by their counselors (suggesting a better perceived fit than for impatient participants), but neither difference is significant. Overall, the available evidence does not support time preferences as a binding constraint to human capital investment.

5.4.2 Returns to Effort

Program or Counselor Quality Since all participants are exposed to the career guidance program (G), the program cannot be evaluated directly. However, program quality is largely determined by the information and the services provided by the counselors. We derive a proxy for counselor quality, using the idea that jobseekers will not return to a counselor who provides poor or unsuitable information, does not listen to the jobseekers' situation, or does not target services to their specific needs. To avoid contamination of the quality measure with the study, we obtain the universe of administrative jobseeker records from 2010, the year before the experiment started, and group jobseekers by counselor. We exploit the fact that jobseekers are allocated quasi-randomly to counselors within a JYC. Counselor quality is then measured as the proportion of jobseekers who drop out of the program after first meeting their counselor, one year before the experiment. We de-mean this proportion at the JYC level to allow for different jobseeker populations. "High quality" then indicates that a counselor 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 counselors of two thirds of our participants, which reduces the sample size for this analysis. Table A12 shows treatment effect heterogeneity using the counselor quality proxy.

Prediction 1 of Section 5.3 finds support in the data: comparing the control means across subgroups, average human capital investment and average search are higher with high-quality counselors. 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 at midline and employment status at endline. This is consistent with high real program returns, assuming that (i) the observed human capital investments are those recommended by the counselors, and (ii) abstracting from the obvious endogeneity of human capital investment. We find no support for Prediction 4, i.e. jobseekers with high-quality counselors do not react more strongly to cash transfers than jobseekers with low-quality counselors. This is consistent with the interpretation that jobseekers valued additional meetings primarily as a condition for receiving the transfer, rather than as high-quality opportunities for employability investment.

Locus of Control Table A13 studies heterogeneity by a measure of locus of control, which captures participants' internal beliefs about their returns to exerting effort. It was measured using six adapted items from the German Socio-Economic Panel (Goebel et al., 2019). 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 (0.17 versus -0.18, $p = 0.041$)) . 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 (-0.27 versus 0.29, $p = 0.036$)). As with other mechanisms, there is little support for heterogeneous treatment effects (Prediction 4) in the data, though those with

an internal locus are proposed and matched with more trainings by their counselors, 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.

Labor market conditions Labor market conditions may affect both the baseline probability to find a job, and the return to investing in employability. 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. Table A14 studies heterogeneity by whether jobseekers face a local unemployment rate above or below the median. Comparing the control means across subgroups, 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 at midline negatively predicts employment at endline in the overall sample (not shown in the table). Again, 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.

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 counselors 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. Since all study participants enroll into the standard guidance program (G) before the randomization to the cash transfer program (G+CCT) is conducted (see Section 3.1), the counselor's decision to assign a participant to the intensive track is independent of (and precedes) assignment to treatment. Table A15 presents our findings relative to labor market disconnectedness. 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 than better connected jobseekers when comparing control

means across subgroups, disconnected jobseekers 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.

5.4.3 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 A16 shows treatment effect heterogeneity by probability to face financial constraints. Effects are broadly similar across groups. In line with Prediction 5, mean human capital investment is weakly lower for financially challenged youths ($p = 0.15$). This difference reverses for job search ($p < 0.001$), consistent with a more immediate need to secure income for subsistence. In line with Prediction 6, but not Prediction 7, human capital and search effort do not respond to cash transfers, with no differential effect. This is what we would expect if income-generating activities used for subsistence were indivisible. 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, with low-skill work being indivisible, and transfers being too small to cover subsistence needs.

To test for possible heterogeneity in outcomes between men and women, we conducted analogous heterogeneity analyses by gender (Table A17). We find no significant heterogeneity in treatment effects 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 counselor quality. While these represent only correlations, all of these may plausibly act as barriers to enrollment in vocational training and other human capital investments. Yet, we observe little evidence for heterogeneous treatment effects of conditional cash transfers by measures of the barriers discussed, suggesting that cash transfers do not mediate any potential impact such barriers have on investment. 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 (meet-

ings) from the incentive constraint. Guidance by counselors 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. Yet, guidance program participation is likely less of a pre-requisite for engaging in job search than it is for making human capital investments. In other words, jobseekers may be less reliant on advice from counselors to apply for jobs than they are for identifying suitable training opportunities. In this case, 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 would 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 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 were the *only* barrier to investment, then conditionality would be 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 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 (Filippucci, 2022). Similar policy directions have been observed in other countries, including Germany and Australia (Van den Berg et al., 2022; Chan et al., 2024).

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. We address such concerns in 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 main specification: the estimate of equation (1) when control variables are introduced, the results of which are presented 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 (which is higher in the control group), 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, 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 to those obtained without doing so. Lastly, results obtained using estimates without control variables are coherent with those obtained for the two other procedures, but point estimates differ slightly. Nevertheless, the same conclusions apply to all variables in the Table.

7 Conclusion

This paper evaluates a randomized conditional cash transfer linked to participation in a national career guidance program for young, low-skilled jobseekers in France. The intervention was designed to address two constraints at the same time: low participation in the guidance program, and possible financial constraints preventing jobseekers from investing in their employability. Formally, the transfer was conditional on broad program compliance: participants could lose the contract if they failed to meet their commitments, missed counselor appointments without justification, or refused training or employment opportunities consistent with their career plan. In practice, however, because career plans and recommended activities were individualized, many of these conditions were difficult to define and enforce. The main enforceable condition was therefore attendance at meetings.

The transfer changed the behavior it directly incentivized. Jobseekers assigned to the cash transfer group remained enrolled for longer, attended more meetings, and maintained more contact with their counselors. This increased participation also changed the supply of opportunities: counselors proposed more job offers, training opportunities, and career planning activities. Yet these additional opportunities did not translate into higher take-up of employability investments or

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

greater job search. We estimate precise null effects on a wide range of investment and search outcomes, and find a short-term reduction in employment during the first six months of the program. The intervention therefore increased procedural participation, but did not increase the downstream behaviors the program ultimately sought to promote.

This distinction is central for policy design. The results do not imply that financial incentives are ineffective. On the contrary, they show that young jobseekers respond strongly to incentives. The problem is that the response occurs on the incentivized margin. A transfer conditional on meetings increases the value of attending meetings. It does not necessarily increase the private return to enrolling in training, completing an apprenticeship, searching more intensively, or accepting employment. If the policy objective is employability investment or employment, incentives should therefore be tied as directly as feasible to those outcomes, or to milestones that are much closer to them. This is difficult in practice. Contractible outcomes must be observable, verifiable, and sufficiently under the control of the jobseeker. They must also avoid perverse incentives, such as rewarding low-quality training, perfunctory job applications, or unstable job matches. These constraints are well known in labor market policy. U.S. reemployment bonus experiments and the Canadian Self-Sufficiency Project provide examples of incentives tied more directly to employment or earnings outcomes (Meyer, 1995; Card and Hyslop, 2005). More broadly, activation regimes in countries such as Germany and Australia illustrate how benefit receipt can be linked to job search requirements and sanctions (Van den Berg et al., 2022; Chan et al., 2024).

A second lesson concerns transfer size. If cash transfers are intended to relax financial constraints, they need to be large enough to let participants satisfy subsistence needs while investing in human capital. In this experiment, realized income increased only modestly. Many participants continued to earn income from work or other sources, the transfer was partly offset by the earnings taper, and measures of financial hardship changed little. Treated jobseekers increased savings, but did not increase consumption or employability investment. This pattern is consistent with the transfer being large enough to affect meeting attendance, but too small to allow participants to give up low-skill work or finance indivisible investments such as training.

The evidence does not allow us to fully distinguish between insufficient conditionality and insufficient transfer size. The two explanations also need not be mutually exclusive. Young jobseekers may face several constraints at once: present bias, low perceived agency, limited trust in institutions, and binding subsistence needs. In such a setting, a higher transfer without stronger conditionality may relax financial constraints but fail to change investment behavior. Stronger conditionality without a sufficient transfer may require investments that participants cannot afford. A more promising design may therefore combine a larger transfer with conditions linked more closely to employability investments or employment outcomes.

The French government's successor policy, the Garantie Jeunes, moved in this direction by combining a larger transfer with a more intensive and structured activation program (Filippucci, 2022). This does not prove that the combined model is optimal, but it is consistent with the

interpretation that meeting-based conditionality alone is too weak. Future experiments should vary both the size of transfers and the object of conditionality, and should distinguish between attendance, investment take-up, investment completion, job search quality, employment, and earnings. The main lesson from this study is therefore not that cash transfers cannot support labor market integration. It is that partial conditionality may successfully buy attendance without changing the investments that matter.

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

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.
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Table 2: Results from Administrative Data

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

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 1. 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
Human capital						
Number of trainings over 1 y. ($\times 100$)	56.14	-2.33	3,409	48.08	-4.52	2,308
	(72.65)	(2.55)		(70.22)	(2.95)	
Ongoing apprenticeship	6.85	0.95	3,409	6.43	0.23	2,308
	(25.27)	(0.86)		(24.55)	(0.99)	
Ongoing internship	2.62	-0.21	3,409	1.43	-0.59	2,308
	(15.98)	(0.57)		(11.88)	(0.52)	
Driving license	41.90	2.66**	3,409	42.54	1.83	2,308
	(49.35)	(1.30)		(49.46)	(1.89)	
Perceived employment prospect	32.78	3.00	3,409	29.76	1.28	2,308
	(80.68)	(2.67)		(80.47)	(3.82)	
Has a career plan	45.23	-0.50	3,409	48.17	-0.92	2,308
	(49.79)	(1.58)		(49.99)	(2.13)	
Has necessary diploma	18.47	-1.77	3,409	21.98	-0.64	2,308
	(38.82)	(1.33)		(41.43)	(1.59)	
Human capital index	0.00	0.05	3,409	0.00	-0.06	2,308
	(3.47)	(0.13)		(3.33)	(0.13)	
Search behavior						
Search for a job	56.14	0.13	3,409	51.56	2.87	2,308
	(49.64)	(1.47)		(50.00)	(2.54)	
Intensity of use of channels index	0.00	-0.07	3,409	0.00	0.05	2,308
	(3.17)	(0.10)		(3.32)	(0.16)	
Number of firms contacted	4.81	-0.21	3,409	4.45	-0.06	2,308
	(8.24)	(0.27)		(8.12)	(0.34)	
At least one interview	20.86	1.03	3,409	16.09	1.90	2,308
	(40.64)	(1.44)		(36.76)	(1.74)	
Search index	0.00	-0.05	3,409	-0.00	0.14	2,308
	(5.30)	(0.17)		(5.42)	(0.27)	
Flexibility in search						
Acceptable commuting duration	35.91	0.89	3,409	36.03	0.45	2,308
	(21.37)	(0.72)		(21.63)	(1.08)	
Accept to move if indefinite term	19.96	1.33	3,409	20.02	0.85	2,308
	(39.99)	(1.40)		(40.03)	(1.88)	

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

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 1. 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
Expenditures and savings						
Financial constraint index	0.00	-0.08	3,409	0.00	0.05	2,308
	(4.11)	(0.16)		(2.26)	(0.08)	
Temptation goods index	0.00	-0.03	3,215			
	(2.25)	(0.08)				
Savings index	-0.03	0.26***	3,295			
	(2.13)	(0.08)				
Mobility and integration						
Mobility means index	0.00	-0.06	3,409	-0.00	0.03	2,308
	(1.14)	(0.04)		(1.22)	(0.06)	
Trust index	0.00	0.32***	3,409	0.00	0.05	2,308
	(2.47)	(0.10)		(2.53)	(0.13)	
Personality index	0.00	0.05	3,247	0.01	-0.11	2,253
	(1.73)	(0.06)		(1.69)	(0.07)	

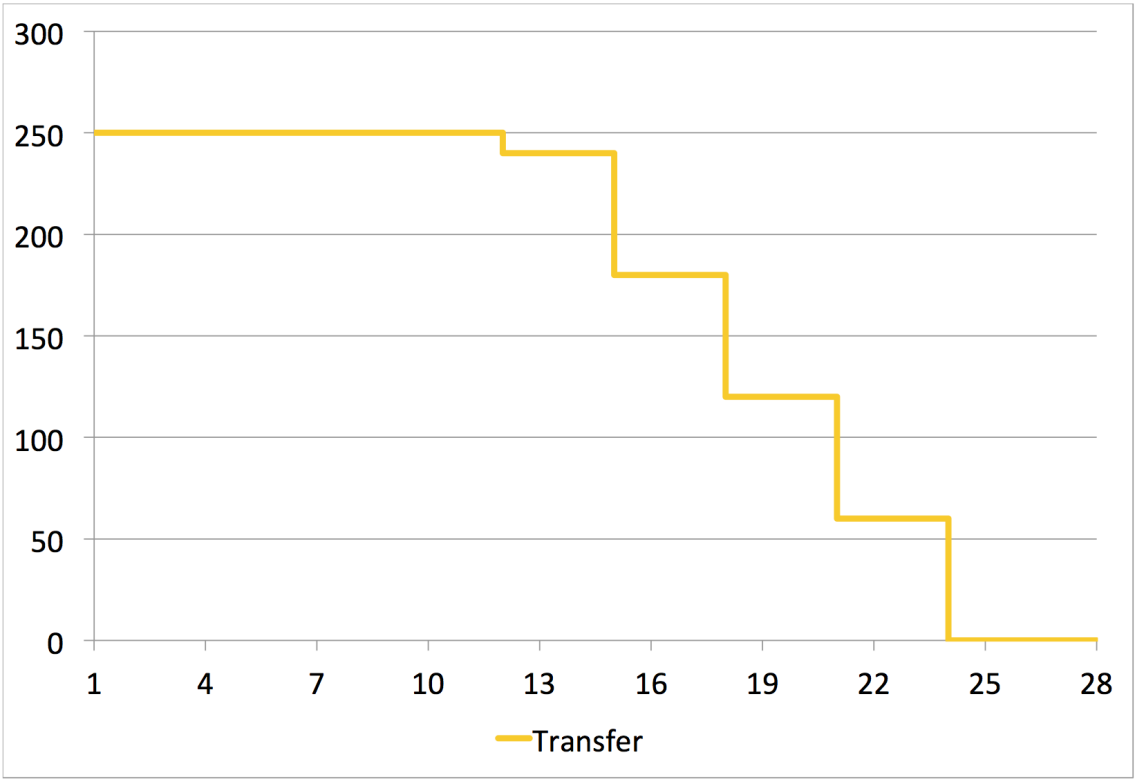
Notes: Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table 1. Indices are obtained by standardizing (subtracting the mean and dividing by the standard error) a set of relevant outcomes and summing them, without standardizing the sum. The financial constraints 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. The index for other mobility means covers going by 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. Finally, the personality index includes 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.

Table 6: Attrition

	(1) Midline	(2) Endline	(3) Midline	(4) Endline	(5) Midline	(6) Endline	(7) Midline	(8) Endline
Treatment	0.059***	0.052***	0.057***	0.046**	0.063***	0.054***	0.024	0.024
	(0.013)	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)	(0.029)	(0.031)
Intensive guidance track					-0.100***	-0.106***	-0.132***	-0.126***
					(0.015)	(0.016)	(0.022)	(0.022)
Precarious housing					-0.165***	-0.162***	-0.152**	-0.173***
					(0.032)	(0.030)	(0.057)	(0.046)
Baccalaureat Level					0.062***	0.083***	0.047	0.065*
					(0.017)	(0.016)	(0.025)	(0.025)
T (×) Intensive track							0.069	0.045
							(0.035)	(0.038)
T (×) Precarious housing							-0.017	0.024
							(0.083)	(0.063)
T (×) Baccalaureat Level							0.030	0.037
							(0.039)	(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

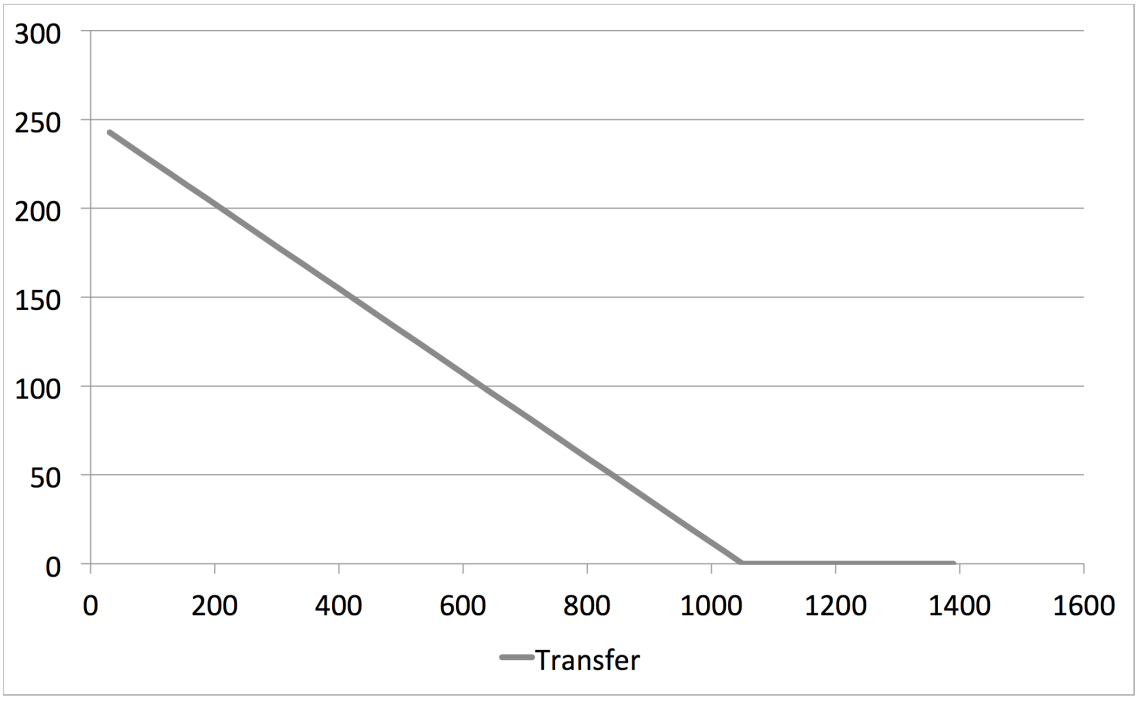
Figure 2: Cash transfer schemes

Scheduled month by month transfer



(a)

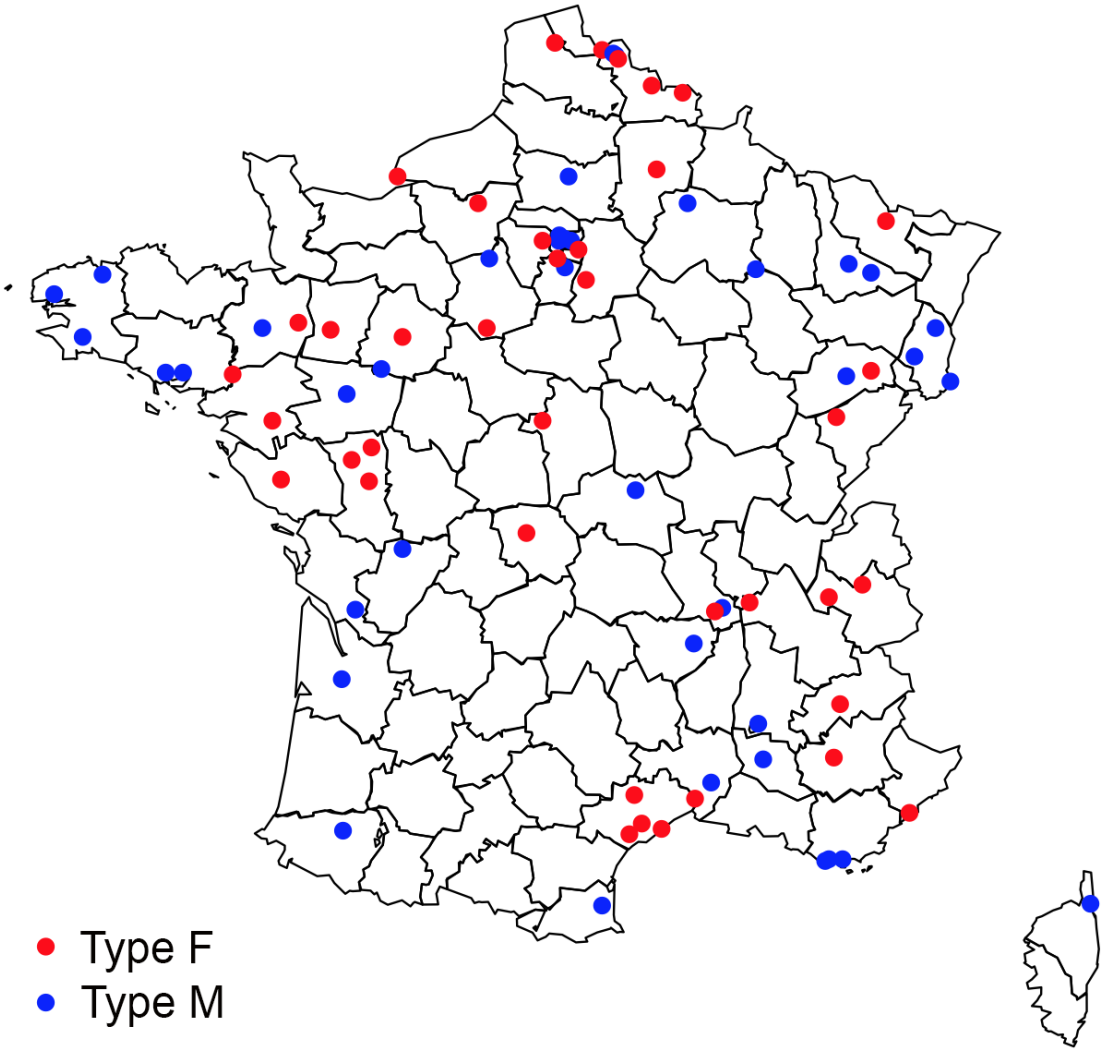
Transfer as a function of incomes from activity



(b)

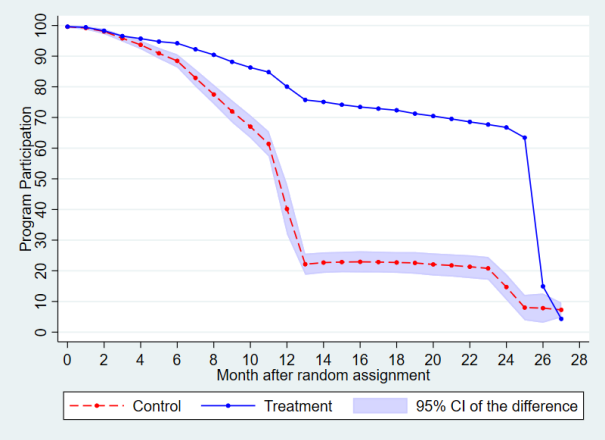
The graph on the upper panel presents the pattern of maximum possible transfers related to the transfer program. The graph on the lower panel presents actual transfers as a function of income from activity. These incomes include wages, unemployment benefits, and internship and training allowances. The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

Figure 3: JYC Map

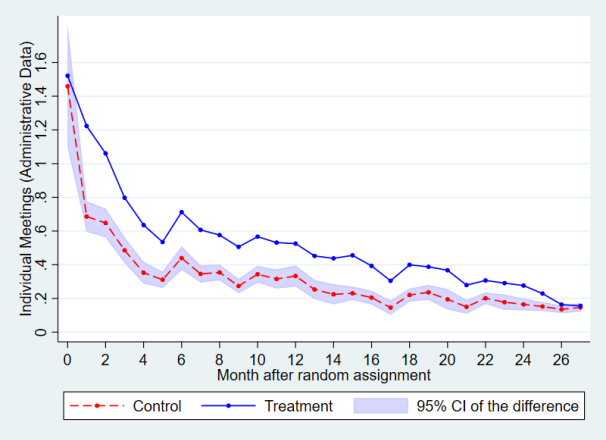


Map indicating JYC participating i the experiment. Blue dots identify type M JYC where youth registered in March were assigned to the Cash program and youth registered in February to the control group. Red dots identify type F JYC where youth registered in February were assigned to the transfer program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

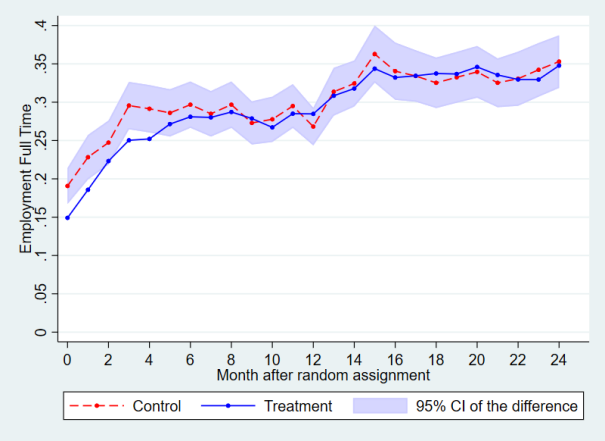
Figure 4: Participation in the program and month by month employment



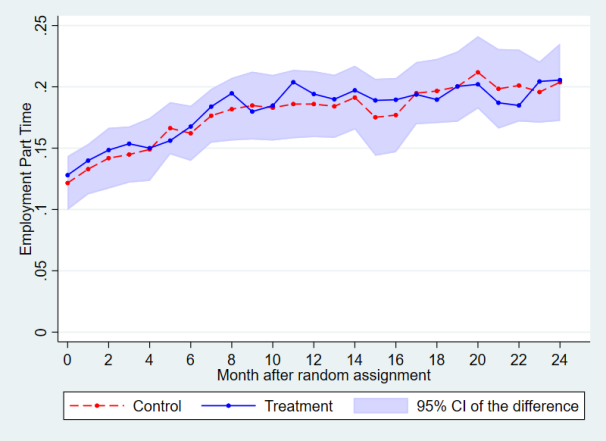
(a)



(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 counselor 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 .

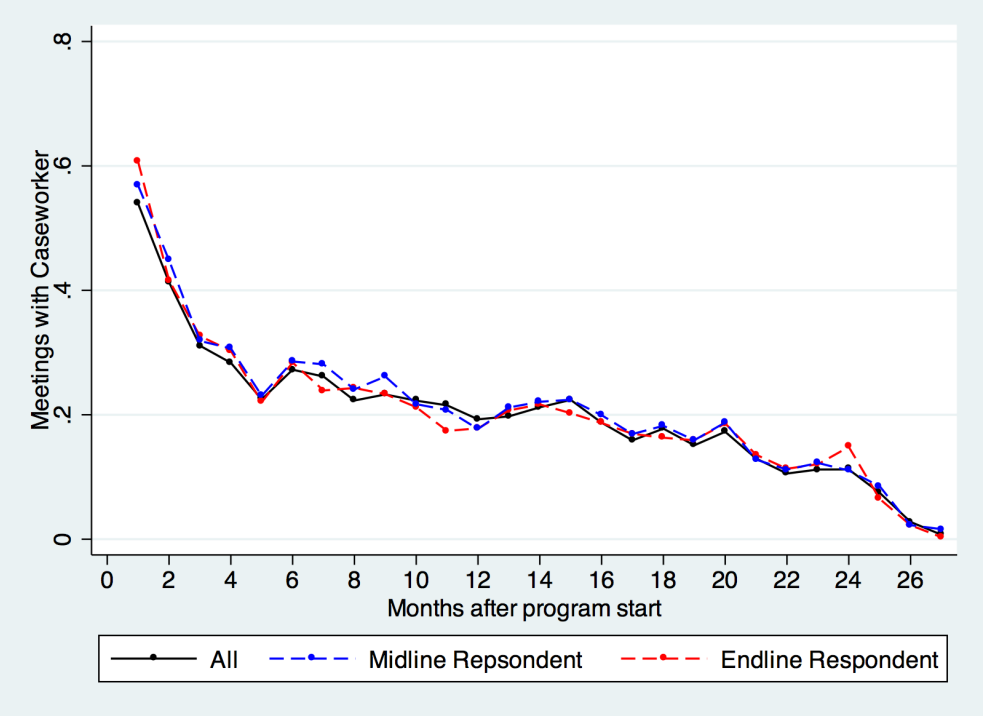
B Robustness to Attrition

Table A2: Bounding of treatment effects

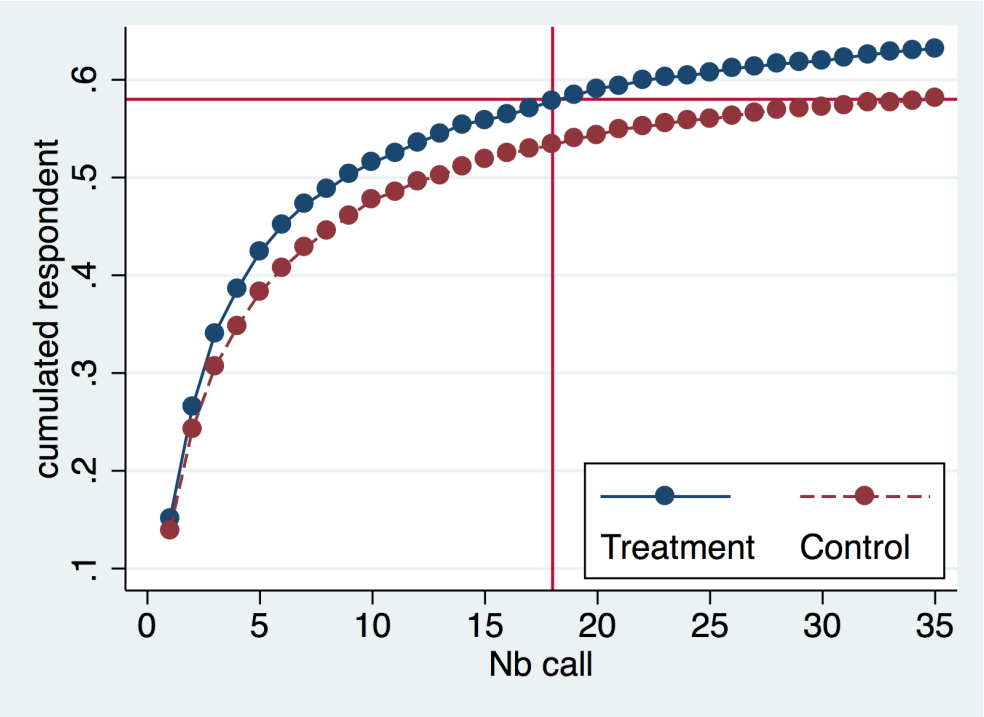
	Control Mean	Actual		Without Controls		Same Response Rate		Lee Bounds	
		Effect	N	Effect	N	Effect	N	Lower	Upper
Employment									
Months employed over 1st sem.	2.42	-0.20***	3,409	-0.25***	3,413	-0.23***	3,211	-0.54	0.06
	(2.34)	(0.07)		(0.07)		(0.07)		(0.12)	(0.10)
Employed after 24 months	52.10	0.09	2,308	-0.64	2,309	0.61	2,229	-6.35	6.71
	(49.98)	(1.90)		(1.93)		(1.89)		(2.81)	(2.90)
Investment									
Human capital index	0.00	0.05	3,409	0.10	3,413	0.05	3,211	-0.62	0.54
	(3.47)	(0.12)		(0.13)		(0.13)		(0.16)	(0.15)
Search index	0.00	-0.05	3,409	-0.04	3,413	-0.01	3,211	-1.11	0.35
	(5.30)	(0.17)		(0.17)		(0.17)		(0.26)	(0.22)
Income									
Any type	602.20	38.70**	3,409	35.03**	3,413	38.37**	3,211	-53.15	99.77
	(489.42)	(15.14)		(16.99)		(15.85)		(22.52)	(21.62)
From activity	405.36	-21.29	3,409	-31.87**	3,413	-21.94	3,211	-128.77	9.54
	(478.28)	(14.30)		(15.87)		(14.89)		(24.83)	(19.17)
Amount saved	211.19	36.54**	3,295	26.68	3,299	41.38**	3,103	-92.88	49.61
	(427.36)	(17.54)		(18.01)		(18.50)		(20.49)	(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)

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

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

Notes: Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. 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 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, whether she deems her chances of finding a 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
Search behavior						
Search for a job	56.14	0.13	3,409	51.56	2.87	2,308
	(49.64)	(1.47)		(50.00)	(2.54)	
Intensity of use of channels index	0.00	-0.07	3,409	0.00	0.05	2,308
	(3.17)	(0.10)		(3.32)	(0.16)	
Web search	19.31	-2.04*	3,409	21.76	1.68	2,308
	(34.92)	(1.12)		(37.92)	(1.85)	
Temporary help agency	20.86	-0.90	3,409	20.29	0.02	2,308
	(39.18)	(1.39)		(39.05)	(1.69)	
Send resumes	36.62	1.46	3,409	33.60	-0.17	2,308
	(47.14)	(1.47)		(46.44)	(2.39)	
Direct job application	28.10	-0.81	3,409	27.35	-0.11	2,308
	(43.63)	(1.67)		(43.27)	(2.06)	
Number of firms contacted	4.81	-0.21	3,409	4.45	-0.06	2,308
	(8.24)	(0.27)		(8.12)	(0.34)	
At least one interview	20.86	1.03	3,409	16.09	1.90	2,308
	(40.64)	(1.44)		(36.76)	(1.74)	
Search index	0.00	-0.05	3,409	-0.00	0.14	2,308
	(5.30)	(0.17)		(5.42)	(0.27)	
Flexibility in search						
Acceptable commuting duration	35.91	0.89	3,409	36.03	0.45	2,308
	(21.37)	(0.72)		(21.63)	(1.08)	
Accept to move if indefinite term	19.96	1.33	3,409	20.02	0.85	2,308
	(39.99)	(1.40)		(40.03)	(1.88)	

Notes: Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. 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)

	Midline Survey (April 2012)			Endline Survey (April 2013)		
	Control Mean	Treatment Effect	N	Control Mean	Treatment Effect	N
Employment status						
Months employed over 1st sem.	2.42	-0.20***	3,409	3.11	-0.01	2,308
	(2.34)	(0.07)		(2.54)	(0.09)	
Months employed over 2nd sem.	2.82	-0.02	3,409	3.20	-0.03	2,308
	(2.51)	(0.08)		(2.59)	(0.10)	
Currently employed	45.41	2.44	3,409	52.10	0.09	2,308
	(49.80)	(1.63)		(49.98)	(1.93)	
Contract type						
Indefinite term	9.95	-0.12	3,409	14.39	0.31	2,308
	(29.95)	(0.94)		(35.11)	(1.59)	
Fixed term	18.00	0.64	3,409	19.39	-1.61	2,308
	(38.43)	(1.32)		(39.55)	(1.67)	
Temporary help	5.72	0.61	3,409	5.18	1.55	2,308
	(23.23)	(0.77)		(22.18)	(0.99)	
Internship	2.62	-0.21	3,409	1.43	-0.59	2,308
	(15.98)	(0.57)		(11.88)	(0.52)	
Apprenticeship	6.85	0.95	3,409	6.43	0.23	2,308
	(25.27)	(0.86)		(24.55)	(0.99)	
Other	2.26	0.40	3,409	4.11	0.05	2,308
	(14.88)	(0.57)		(19.86)	(0.96)	
Employment quality						
Employment type						
Formal	42.49	1.14	3,409	49.78	-0.05	2,308
	(49.45)	(1.60)		(50.02)	(1.87)	
Subsidized formal	9.24	-0.24	3,409	8.67	1.58	2,308
	(28.96)	(1.13)		(28.15)	(1.24)	
Informal	2.86	1.40**	3,409	2.59	0.26	2,308
	(16.67)	(0.70)		(15.90)	(0.69)	
Employment volume						
Full-time	26.82	1.42	3,409	33.33	-0.27	2,308
	(44.31)	(1.19)		(47.16)	(1.90)	
Part-time	18.59	1.03	3,409	18.86	0.03	2,308
	(38.92)	(1.36)		(39.13)	(1.60)	
Employer type						
Private	32.90	-0.49	3,409	36.46	0.94	2,308
	(47.00)	(1.56)		(48.15)	(1.97)	
Public	8.28	1.58*	3,409	10.10	-0.34	2,308
	(27.57)	(0.95)		(30.14)	(1.43)	

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

Notes: Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. 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
Financial constraints over last 12 monts			
Financial constraint index	0.00	-0.08	3,409
	(4.11)	(0.16)	
Any financial difficulty	64.42	0.82	3,409
	(47.89)	(1.52)	
Pbs. paying bills	27.71	0.11	3,409
	(44.77)	(1.72)	
Pbs. paying rent	18.12	-0.45	3,409
	(38.53)	(1.64)	
Pbs. paying taxes	8.76	0.19	3,409
	(28.28)	(1.03)	
A day without a meal	19.37	-0.71	3,409
	(39.53)	(1.47)	
Forewent medical care	24.37	-0.60	3,409
	(42.95)	(1.45)	
Bank overdraft	44.93	-1.19	3,409
	(49.76)	(1.72)	
Forewent training	13.71	-0.27	3,409
	(34.40)	(1.29)	
Temptation goods over last month			
Temptation goods index	0.00	-0.03	3,215
	(2.25)	(0.08)	
Number of restaurants	2.21	-0.00	3,300
	(2.73)	(0.09)	
Nights out	2.06	-0.12	3,272
	(2.72)	(0.10)	
Phone	55.08	-2.81	3,386
	(156.52)	(5.74)	
Tobacco	29.68	1.75	3,409
	(49.86)	(1.78)	
Largest purchase	661.55	21.42	3,113
	(1,410.07)	(45.62)	
Saving behavior			
Savings index	-0.03	0.26***	3,295
	(2.13)	(0.08)	
Saved money	45.47	4.70**	3,409
	(49.81)	(2.06)	
Amount saved	211.19	36.54**	3,295
	(427.36)	(17.76)	
Owes money to relatives	16.39	-2.02*	3,409
	(37.03)	(1.08)	

Notes: 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
Mobility means						
Mobility means index	0.00	-0.06	3,409	-0.00	0.03	2,308
	(1.14)	(0.04)		(1.22)	(0.06)	
Foot	9.24	-0.30	3,409	7.95	0.12	2,308
	(28.96)	(1.24)		(27.07)	(1.07)	
Bike	1.67	0.31	3,409	1.88	0.25	2,308
	(12.81)	(0.46)		(13.58)	(0.76)	
Parents	2.80	-1.25**	3,409	1.61	-0.16	2,308
	(16.50)	(0.53)		(12.59)	(0.59)	
Public	37.54	2.95	3,409	29.58	3.77*	2,308
	(48.44)	(1.93)		(45.66)	(1.94)	
Scooter	5.07	-0.95	3,409	3.57	0.71	2,308
	(21.94)	(0.80)		(18.57)	(0.75)	
Car	43.68	-0.63	3,409	55.41	-4.22**	2,308
	(49.61)	(1.43)		(49.73)	(1.97)	

Notes: Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. 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
Trust						
Trust index	0.00	0.32***	3,409	0.00	0.05	2,308
	(2.47)	(0.10)		(2.53)	(0.13)	
School	63.89	4.06**	3,409	66.40	0.28	2,308
	(48.05)	(1.63)		(47.26)	(2.30)	
Health care system	84.15	0.78	3,409	82.75	-2.42	2,308
	(36.53)	(1.52)		(37.80)	(1.66)	
Job Youth Center (JYC)	81.17	6.49***	3,409	69.88	8.18***	2,308
	(39.11)	(1.23)		(45.90)	(1.83)	
Justice system	53.87	2.50	3,409	56.39	-3.41	2,308
	(49.86)	(1.64)		(49.61)	(2.24)	
Personality traits						
Personality index	0.00	0.05	3,247	0.01	-0.11	2,253
	(1.73)	(0.06)		(1.69)	(0.07)	
Number of days ready to wait for 20% premium	97.76	0.15	3,373	101.86	-6.90*	2,300
	(78.28)	(2.99)		(78.21)	(3.85)	
Locus of control	0.00	0.01	3,270	0.00	0.01	2,260
	(1.00)	(0.03)		(1.01)	(0.04)	
Life satisfaction	71.19	0.64	3,406	71.69	-0.63	2,308
	(20.49)	(0.61)		(20.10)	(0.81)	
Growth mindset	-0.00	0.00	3,355	0.01	-0.03	2,288
	(1.00)	(0.04)		(1.00)	(0.04)	
Number of friends	4.03	-0.08	3,409	4.18	0.05	2,308
	(2.84)	(0.10)		(2.88)	(0.11)	
No friends	5.42	2.04**	3,409	6.43	0.16	2,308
	(22.65)	(0.91)		(24.55)	(0.98)	

Notes: Regressions are estimated via a Lasso post-double-selection procedure and the specification includes the control variables listed in Table A1. 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

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

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).
Notes: Standard errors are clustered at the JYC level. The left part of the table presents the results for more impatient young adults and the right part the results for those who are more patient. The last column of the table corresponds to the test of a same effect in the two groups. Each panel first provides the mean over the population considered in the control group then the estimated value of the impact coefficient and below the estimated standard error.

Table A12: Heterogeneity – counselor quality

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

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).
Notes: Counselor quality is measured as the proportion of jobseekers who drops out of the program after one year of participation into the program, using administrative records from the universe of jobseekers in 2010 (the year before the experiment started). "High quality" indicates that a conuselor had a below-average proportion of drop-outs, relative to his or her JYC.

Table A13: Heterogeneity – Locus of control

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

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).
Notes: Standard errors are clustered at the JYC level. The left part of the table presents the results for young adults who have external locus of control and the right part the results for those who have internal locus of control. The last column of the table corresponds to the test of a same effect in the two groups. Each panel first provides the mean over the population considered in the control group then the estimated value of the impact coefficient and below the estimated standard error.

Table A14: Heterogeneity – Local youth unemployment rate

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

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).
Notes: Standard errors are clustered at the JYC level. The left part of the table presents the results for young adults who face a local unemployment rate below the median with respect to the JYC catchment area and the right part the results for those who face a local unemployment rate above the median. The last column of the table corresponds to the test of a same effect in the two groups. Each panel first provides the mean over the population considered in the control group then the estimated value of the impact coefficient and below the estimated standard error.

Table A15: Heterogeneity – Disconnect from labor market

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

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).
Notes: Standard errors are clustered at the JYC level. The left part of the table presents the results for young adults who enrolled in the standard track and the right part the results for those enrolled in the intensive one, with more frequent meetings and closer monitoring. The last column of the table corresponds to the test of a same effect in the two groups. Each panel first provides the mean over the population considered in the control group then the estimated value of the impact coefficient and below the estimated standard error.

Table A16: Heterogeneity – Financial constraints

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

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).
Notes: Standard errors are clustered at the JYC level. The left part of the table presents the results for young adults who are less likely to perceive financial constraints and the right part the results for those who are most likely to perceive financial constraints. The last column of the table corresponds to the test of a same effect in the two groups. Each panel first provides the mean over the population considered in the control group then the estimated value of the impact coefficient and below the estimated standard error.

Table A17: Heterogeneity – Gender

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

Data: Administrative records; midline and endline surveys (respectively April 2012 and April 2013).
Notes: Standard errors are clustered at the JYC level. The left part of the table presents the results for females and the right part the results for males. The last column of the table corresponds to the test of a same effect in the two groups. Each panel first provides the mean over the population considered in the control group then the estimated value of the impact coefficient and below the estimated standard error.