

You Get What You Pay For: Evidence from a Jobseeker Conditional Cash Transfer Program in France *

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

Keywords: Conditional cash transfer, youth unemployment, randomized experiment

JEL: J68, J64, C93.

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1 Introduction

Many youth enter the job market with few qualifications and little to no knowledge of the job market. Across OECD countries, 38.4 million youths aged between 16 and 29 years (18% of their age group) are not in employment, education, or training (NEET). Two-thirds of them are not looking for a job, and only one in six has any tertiary education (Carcillo et al., 2015). Helping these young people to develop and achieve professional goals, as well as increase their overall human capital and employability, has been a policy priority across countries (Quintini et al., 2007). This paper evaluates the first randomized conditional cash transfer program in a labor market context. It assesses whether a monthly cash transfer conditional on participation in a large national employment program creates effective incentives to participate and engage with the program, increase employability investments, and improve employment outcomes.

Designing programs which are effective and attractive is not an easy task. Existing programs across major industrialized countries have focused on skill assessments, career planning, general or specialized training, job search assistance and employment experience through internships and subsidized job contracts. Whatever lever is used, such programs have a positive short-term effect at best, but do not build human capital to the degree necessary to improve long-term employment outcomes: In a recent meta-analysis of 113 impact evaluations, only one third of youth employment programs show positive effects on employment or earnings – and most of these are in low-income countries (Kluge et al., 2019).¹ Programs that help job seekers find work tend to yield better results, but benefits are often temporary and may displace workers who are not supported by the programs (Crépon et al., 2013).

Available empirical evidence also points to a lack of attractiveness of these programs, resulting in low participation levels and high dropout rates. Heckman et al. (2000) have shown that participation rates in assistance programs are low, and Behaghel et al. (2014) found that less than 50% of those assigned to assistance programs in France actually attend.² Black et al. (2003) even show that assigning job seekers to this type of program makes them rush to find employment to avoid attending. Certain studies also report that a majority of youth invest little energy in the programs offered, as they judge them to be ill-suited to their needs or do not see the point of a medium-term commitment to training or skills-building.³

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

²Schochet et al. (2008) reports high levels of dispersion in the length of time spent in the program in the Job Corps program. The assessment by Bloom et al. (1997) of the 1982 Job Training Partnership Act (JTPA) mention a participation rate of just two-thirds of those assigned.

³See LaLonde (2003). Ivry and Doolittle (2003) explain that: “the mixed results from studies of existing youth programs can be explained largely by the low enrollment of key subgroups of young people, inconsistent

On a theoretical level, models of human capital investments and of behavioral economics have identified several potential factors likely to affect demand for employability investment. Heckman et al. (1999) propose a model which encompasses all services designed to improve job-market readiness, highlighting the role of opportunity costs and expected returns in terms of the perceived productivity of job searches and of the expected remuneration and stability of jobs.⁴ They also underscore the central role of financial constraints in the decision to invest, including when these constraints come from parents' incomes. In this case, paying a transfer to a young person with financial difficulties relieves him or her of those constraints. Behavioral economics models identify several reasons why the intrinsic motivation of young people can be low.⁵ Babcock et al. (2012) summarize the lessons to be learned from these findings when designing labor market policy. The first obstacle is that the perceived benefit of this investment plays a central role in the decision to enroll but is very difficult to gauge correctly.⁶ The second obstacle is that choosing an orientation strategy adds an extra degree of complexity to the task of assessing the benefits of a training program. Lastly, intertemporal preferences for the here and now can also be a determining factor in demand: young people may systematically favor low-paying, insecure jobs, which are easy to find, and delay investments in human capital to a later time. In this context, paying a young person a conditional transfer re-shifts the priority to choosing investments in employability. Providing a transfer is one way of alleviating active financial constraints. Many countries have opted for the payment of a minimum benefit, which gives young people more leeway in their choice of a career track.⁷ Nevertheless, such systems can undermine incentives to get into and stay in the job market.⁸ For this reason, some transfer systems have evolved

participation among enrollees, and high rates of attrition. Many of the young people who could benefit most from program services do not enroll at all, and a large proportion of those served do not participate long enough to earn education credentials, improve their work readiness and life management skills, and acquire the technical skills needed to compete effectively in the job market”.

⁴Cunha et al. (2006) also highlight similarities between human capital investments carried out in childhood and early adulthood and underscore the resulting poor outcomes expected from remedial programs such as those studied here. Recent research on the role played by psychological traits highlights the role of an external locus of control – the belief that life outcomes are determined by external factors. Underskilled youth could be more likely to have an external locus of control, which would limit their intrinsic drive to invest in programs offered. See Cobb-Clark (2014) for a review of the literature on locus of control.

⁵See Křeps (1997), for example, on the links between extrinsic incentives and intrinsic motivation.

⁶This phenomenon has been proven empirically by Spinnewijn (2015), who demonstrates that job seekers tend to underestimate the return on job seeking activities. Also noteworthy is the mistrust young people can feel towards public institutions, which skews assessments of program participation benefits. Many youth have already experienced alienation by leaving school without a diploma.

⁷Various approaches exist: Austria, Germany, Portugal, Sweden and others have systems based on parental income when the young person is still dependent upon them, and on the young person's own income when they no longer depend on their parents. In Denmark, Finland and Netherlands, a minimum benefit is paid based solely on the young person's income regardless of whether they depend on their parents or not.

⁸Moffitt (2003) reviews the research conducted on the Aid to Families with Dependent Children program

to ensure that incentives to paid work remain central.⁹ Unconditional transfers run the risk of being inefficient if low participation rates among young people are due to weak intrinsic motivation, which is linked, for example, to an undervaluation of program returns or a biased preference for the here and now. In this paper we look at the payment of a minimum transfer which attempts to support human capital investment incentives for young people by making payment conditional on participation in a national career guidance program. We compare two cohorts selected randomly from a sample of young adults enrolled in the national program, differing only in that one is given a transfer which is conditional on attending the program.

The young adults targeted by the service have poor job prospects. Typically, they have had serious issues at school and have dropped out or failed multiple times. Most live in social environments and areas which condemn their chances of integration, such as the isolated housing estates found in major cities. When they are *Not in Employment, Education or Training* (NEET), these young people are usually offered enrollment in a national career program, the “Contrat d’Insertion dans la Vie Sociale (CIVIS)”, called hereinafter the *standard program*. The CIVIS program is characterized by low attendance and a high dropout rate. The experiment consisted in offering 3,000 of these young adults a place in a new program: the “Revenu Contractualisé d’Autonomie”, called hereinafter the “experimental program”, identical to the standard program except for a monthly benefit payment. A €250 transfer is paid monthly the first year.¹⁰ The amount decreases gradually the second year (€240 in the first quarter down to €60 in the last quarter). In total, young adults can theoretically receive up to €4,800 over the course of the two-year program. The benefit is paid as long as the youth complies with the guidance program. If the youth fails to attend meetings or comply with the tasks stipulated by the program, his or her counselor may decide to suspend payment of the transfer in coordination with the Job Youth Center (JYC) director. Due to the diverse and partially non-contractable nature of tasks in a personalized guidance program, the key contractable behavior in practice was attendance of the meetings.

Results show a significant increase in program participation. Because of the benefit, the program’s drop-out rate diminishes drastically. Young adults remain in the program for a longer period of time and have more meetings with their counselors: the number of months spent in the program went from 12.1 (in the standard program, without the transfer) to 21.7

in the United States.

⁹This is true of the Earned Income Tax Credit (EITC), the Working Income Tax Benefit (WITB) in Canada, and the Working Tax Credit (WTC) in the U.K.

¹⁰In 2011, €250 equaled 23% of the net minimum wage (SMIC in France), and 54% of the minimum welfare benefit (Revenu de solidarité active, RSA), which young adults are only entitled to when they turn 25. The amount is the maximum allowed, since the program includes a taper rule: transfer amounts decrease as job revenue increases. The implicit tax rate associated with this rule is 24%.

months (in the experimental program, with the transfer) and the total number of interviews with a counselor increased from 8.1 to 14.6. Transfers received increased steeply by €1,868 (\$ 2,577) to a total of €2,132 (approximately \$ 2,942 in 2011).¹¹ This additional individual expense, though diminished, nevertheless applies to a potentially large group – 170,000 young adults in 2011 – thus representing a non-negligible direct financial commitment of about €318 i.e. \$ 438 million.

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

Moreover, in the first six months, there is a decrease in full-time employment equal to three percentage points.¹² This effect is consistent with the disincentives traditionally associated with transfer payments and taper rules. Relatedly, we find that income increased by less than half of the theoretical transfer amounts. Benefits received as part of the program are initially diminished through the taper rule. We additionally observed substitution with other income sources, primarily employment income and transfers from friends or family. Lastly, variables collected to measure social integration show no notable improvement except in the confidence young people had in the JYC.

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

¹¹This amount falls quite short of the €4,800 announced. We show that this is primarily due to the income taper rule.

¹²While this decrease could be linked to a locking-in effect caused by increased participation in the program, this is unlikely: meetings with counselors are spaced out, and there is no evidence of enhanced commitment to defining career goals.

intermediary steps.

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 principle results regarding program participation, employability investment, employment, income and social integration. Section 5 assesses results found in each sub-sample of participants to determine whether there is heterogeneity in program outcomes. Section 6 analyses the robustness of the results and section 7 provides a conclusion.

2 Programs and Participants

2.1 The Study Population

A considerable number of young people exit the education system quickly with little in the way of qualifications and have a very hard time entering the job market. In France, the “Generation 2007” survey carried out in 2010 targeted young adults who finished school in 2007 and examined employment outcomes. Table 1 presents descriptive statistics obtained from the survey, which provide a more clear description of the sample studied here. It reveals a high prevalence of young people exiting the school system with few skills: 18 % leave school with no diploma and 17 % only complete the equivalent of junior high. The table also reveals difficulties in finding employment: 21.7 % of those surveyed have mostly been unemployed (9.3 %) or inactive (12.5 %) in the three-year period after leaving school.¹³ A lack of qualifications and problems finding work are linked: 58.6 % of the young people surveyed who had trouble entering the job market were also unskilled (column 2).¹⁴

Assistance to youth between the ages of 16-25 who encounter problems finding work is provided by 450 Job Youth Centers (JYCs) located throughout France.¹⁵ 20.6 % of young people who finished school in 2007 went to a JYC at least twice (column 1). Column 3 lists young people who went to a JYC at least twice in the three years after they left school. The data shows that people with basic diplomas are particularly prone to visiting JYCs: 63.7 % had no diploma or only a junior-high equivalent. They left the education system at a younger age and repeated a year at the primary level more frequently than most children. Their parents are more likely to be immigrants and they are more likely to live in deprived neighborhoods

¹³Those surveyed were also asked about their month-to-month employment status between November 2006 and July 2010, with multiple categories of answers possible. A typology of labor market trajectories emerges, which we divide into four categories.

¹⁴The data in this column also shows that 41.4 % of young adults which experienced the same difficulties have higher qualifications and, in particular, 23.5 % had a post-secondary degree. Crépon et al. (2013) looks at a career guidance program targeted to this specific group of young, qualified job seekers.

¹⁵These include local missions and drop-in/information/guidance centres (PAIO).

Not all young people who have trouble finding a job go to JYCs however. Column 2 shows that only 41.6 % of subjects went to a JYC at least twice, even if the proportion is higher among the unskilled. Taken alone, 52.3 % the subsample of young adults with few qualifications (less than a high school diploma) who have trouble finding work went to a JYC at least twice. Inversely, only 27.1 % of those with a high school diploma or higher and who have trouble finding work went to a JYC at least twice.

2.2 The Standard Program (CIVIS)

JYCs offer a guidance program to young adults who have the most trouble on the labor market: the *Contrat d'Insertion dans la Vie Sociale* (CIVIS), which we will refer to as the 'standard program'. Approximately 170,000 young adults enrolled in this program in 2011. It is a one-year program which may be repeated once, aimed at helping those enrolled to establish a career plan (in the first three months) and then implement it.¹⁶ Participation is enshrined by the signature of a contract. Modest cash transfers may be paid in certain cases to cover job search costs. In theory, meetings are offered at least once a month. Time is provided for training or business internships if necessary (these are offered by other organizations). In theory, then, the program acts as a platform to steer participants towards the best suited training courses, career workshops, subsidized job contracts, or job shadowing at companies. If they enroll in a course or find short-term work, participants remain in the program and are expected to remain in touch with their counselor. They leave the program at the end of the contract (one year later), or when they find a minimum six-month long job contract, or when the contract is revoked, which is what occurs if an individual drops out. One major problem with the standard program is that participants invest little effort, and drop-out rates are high: in 2011, only 27 % of all program participants found long-term employment; 15 % left the program after being enrolled the maximum period of time; 24 % dropped out and 34 % were not re-enrolled by their counselor after the first year (these may be de facto drop outs) (Dares, 2014).

2.3 The Experimental Program (RCA)

Our analysis of this program, designed by the Ministry of Youth, should begin with an overview of the context in which it was developed. In late 2008, a youth research fund was launched to test innovative initiatives which address difficulties faced by young adults. The French government's 2009 Green Paper on Youth (*Livre Vert de la Jeunesse* (2009)) published under the leadership of Martin Hirsch, High Commissioner for Youth at the time,

¹⁶Very hard-to-place job seekers are allowed to enroll more than twice. They are offered an enhanced version of the program which includes more meetings with their JYC counsellor.

identified a lack of financial independence as an important concern, especially given that in France, unlike many other countries, there is no minimum income system in place for young adults.¹⁷ The Green Paper recommended that new forms of youth cash transfers be tested to solve this problem. Several options were on the table, including a monthly transfer conditional on enrollment in an employment support program or an endowment provided for investments in human capital.

Policymakers chose a monthly transfer conditional on enrollment in the standard program. A new program was designed, identical to the existing one except for the provision of financial assistance. The new experimental program includes a monthly transfer of €250 the first year and a digressive amount the second year (€240 monthly the first quarter; €180 monthly the second quarter, €120 the third and €60 the fourth), for a maximum total of €4,800 over two years (see 1 (a)). Like the previous program, a career plan must be drawn up within the first three months of counseling and then implemented by way of training courses, jobs, internships or job searches based on the needs of the job seeker.

An important feature of the program is that transfer payments are subject to attendance. Like the previous program, the new one is designed as a contract and transfers are paid only if the individual participates in the program. Rules clearly state the conditions in which a contract may be terminated: “[t]he 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 with acknowledgement of receipt[.]”

Another feature of the program distinguishes it quite clearly from the original outline described in the 2009 Green Paper on Youth: the transfer can only be partially combined with employment income. The amount of the transfer is tapered off in relation to employment income and is no longer paid once a participant makes €1,050 – the minimum monthly salary as of April 2011 (see Figure 1 (b)). This type of structure implies an implicit tax on employment income of $250/1050=24\%$. This figure is not particularly high given the tapers involved in social transfer systems in general. The RSA benefit – the minimum income benefit in France – implies an implicit tax rate of 38 %; the rate is approximately 35 % for housing benefits – the primary form of transfer in France.¹⁸ In the U.S. the implicit tax rate

¹⁷The guaranteed minimum income scheme in France is called the Revenu de Solidarité Active (RSA) to which only adults over 25 are entitled.

¹⁸The marginal implicit tax rates of the RSA and housing benefits are combined for many low-income households, resulting in a marginal implicit rate of over 75 %.

of the EITC is 21 %, but the AFDC had a rate of 66 % and was replaced by the TANF with a rate of 50 %. Other transfer systems have lower rates, like the WITB in Canada (15 %) or higher rates like the WTC in the U.K. (55 %), which was recently replaced by the Universal Credit with an implicit tax rate of 65 %. Employment revenue includes wages, unemployment insurance and training compensation. People between 18-25 who take part in certified training courses in France receive €325. An apprentice earns €470 the first year. The implicit tax rate therefore reduces the amount a young person would receive while completing a training course (€78) or working as an apprentice (€113).¹⁹

3 Experimental Study Design and Data

3.1 Experimental Study Design

A randomized study was implemented to assess the impact of substituting the standard program by the experimental program. We developed an original design, where individuals are randomly assigned at the aggregate level of the JYCs. This design has a detection capacity close to the level achieved by randomizing individuals from within the JYCs. With this design, it is also possible to isolate the impact of the program at the intensive margin and prevent problems linked to the potential existence of a magnet factor within the program. The new program was launched for up to 4,000 participants. A call for applications to take part in the study was sent to the 450 structures offering the standard program and 82 centers replied favorably. The relatively large number of participants and participating centers enabled us to design a study protocol capable of detecting program impacts. A theoretically simple protocol would have been to randomly assign the RCA participants from among the young adults coming to the JYCs for a standard CIVIS program. However this solution was not implemented for several reasons. First, it was considered as a potential risk for the experiment as participants assigned to the control group could have considered it as unfair. Second, the burden of the registration process for the study would have been quite high for counselors, who were also reluctant to be perceived by participants as an additional source of randomness in their life. The evaluation team was specifically asked to find a protocol that would guarantee that randomization would not take place at the individual level. The initial goal as conveyed by the agency in charge of the program was for some of the JYCs to implement the RCA experimental program while others kept implementing the CIVIS standard program. The treatment and control groups would consist in the newly

¹⁹It is interesting to note that the actual content of this initiative partially contradicts the ideas which spurred its development: to provide financial resources on a conditional basis or to support progress in efforts to find employment.

recruited young adults coming to the two types of JYCs. However, this would have led to two major issues for the study design. First, a selection issue and second a statistical power issue.

The selection issue arises from the fact that even if each JYC has its own designated area, not all young people from this area come to the JYC, and the ones who do come to the JYC are not a random sample of all the youth in the area. Therefore, such a protocol would have been likely to attract different populations depending on whether they offered the standard or the experimental contract with transfer payments. This would therefore not ensure that both groups would be statistically similar because the young adults interested in the experimental program would probably not be the same ones interested in the standard program. Indeed, a new group of participants would probably emerge who would be more interested in the additional cash transfer given during the experimental program. To overcome this difficulty, the evaluation was not based on the flow of new enrollees but on the stock of young people who had signed a standard contract in February or March 2011.

Randomization took place on April 11, 2011. It assigned young job seekers who had signed a standard contract between February 1st and March 31 to one of two groups, a) Treatment group: participants were offered to sign a transfer contract instead of their recently signed standard one, and b) Control group: participants kept their standard contract. This way, since the subjects had no way of knowing in advance whether they would be offered a transfer contract, this assignment to treatment was exogenous. In fact, they had almost no way of even knowing whether they had a chance to be offered such a contract. The drawback is that individuals assigned to the program with transfers are only given the opportunity to turn their standard program into the program with transfers. In fact, 20 % of participants did not accept and preferred to stay in the standard program. There is therefore an imperfect compliance issue. We can estimate both an Intention To Treat (ITT) parameter and a Local Average Treatment Effect parameter (LATE), using in this latter case the assignment variable as an instrument for participation in the program with transfers. Moreover, because participants assigned to the control group could not gain access to the program with transfers, the LATE parameter can be interpreted as a Treatment On the Treated (TOT) parameter (Bloom, 1984; Angrist and Pischke, 2009).

The statistical power issue is that even if 82 JYCs arguably constitute a sufficiently large sample, it might not be enough to reach high levels of statistical power. This would be the case if the heterogeneity at the JYC level were significant, even when the number of youth per JYC is high. In order to increase power, the treatment was randomly assigned within each JYC, to young adults registered in February or registered in March. This raises the power level to an almost sufficient degree as randomization at the individual level, while

avoiding the practical problems associated with this randomization strategy.

More precisely, the JYCs were first paired according to existing characteristics, such as the number of youths per counselor or the proportion of youths with a high school degree. Members of each pair were then randomly assigned to either group A or B. They discovered their group as soon as they provided their list of contracts signed in February and March to the evaluation team (first week of April 2011). Group A JYCs had to contact all the subjects who had signed a standard contract in March 2011 and offer them a transfer contract while group B JYCs had to contact those who had signed a standard contract in February 2011. Figure 2 shows the map of France and locates the two types of JYCs. As shown in table 2, there were 5,498 youth registered in the study: 2,871 came from Type A JYCs and 2,671 from Type B JYCs. In Type A JYCs 1,372 individuals registered in the standard program in March and were therefore assigned to the treatment group. 1,455 registered in February and were assigned to the control group. In Type B JYCs 1,289 individuals registered in February and were therefore assigned to the treatment group while 1,382 registered in March and were assigned to the control group. In the end, 2,661 youth were assigned to the treatment group and 2,837 to the control group.

3.2 Data

The empirical analysis uses both administrative data from the Job Youth Center records and two surveys.

Job Youth Centers are organized in a network of over 450 branches. Much individual data regarding participants are recorded in a central record known as “Parcours 3”. Available information is collected when youths register for the first time at the Youth Center. This includes demographics as well as information regarding housing, resources and past experience in the labor market. The administrative records provide information which helps assess the most important questions we have regarding the impact of cash transfers on incentives to invest in employability. Administrative data traces all exchanges between registered youths and their counselor (meetings, phone calls, email) as well as the details of these exchanges (dates, main content). This will allow us to determine whether the experimental track fosters the participation of youth in the program.

The records also contain other key information: the content of the program and all the offers made to participants while in the program, including job offers, opportunities for training or career building services, proposals and matching. This is important as it allows us to measure whether more exchanges were productive in the sense that they led to more opportunities for participants. The records also provide information about the situation of young people noted during meetings: unemployment, training, employment. We are therefore able to assess

whether offers made to participants and initiated matches were followed by real action. The main drawback is that this only worked with young people who stayed in contact with the Youth Center. One difference between this records and Public Employment Service (PES) records is that participants remain registered in the records and still stay in touch with their caseworker even if they return to employment. Even if the quality of these labor market outcomes is better than what can usually be obtained from PES records, we can consider it as a reliable outcome variable to measure the impact of program assignment only for opportunities, offers and actions carried out via counselors

In addition to information recorded in the administrative records, two annual and individual telephone surveys were carried out: a midline survey (April 2012) and an endline survey (April 2013).²⁰ These two 25-minute long surveys allowed us to collect a significant amount of data usually not available when studying cash transfer program impacts. The survey included questions on socio-demographic variables, detailed labor market outcomes concerning employment, training, career building or searches, and also included information about income, sources of income, expenses, social integration and psychological traits (locus of control). Employment outcomes, for example, included all employment events (full and part time) in the past twelve months and indicated the duration over the month.

Table 2 provides information about the surveys. As can be seen, the surveys have two main drawbacks: response rates are not very high; nor are they balanced between treatment and control. Response rates to the midline and endline surveys are only 60 % and 40 % in the control group and there is a persistent 5-6 % differential in response rates between treatment and control groups even after controlling for pairs or covariates.

Both the internal and external validity of the study are therefore at risk when it comes to using outcome variables from surveys. We implement several robustness checks (see section 6). The first tests for balancing using information from administrative records for the whole sample but also for respondents to the midline and endline surveys (see subsection 3.3). Furthermore, several outcome variables key to our analysis are available for the whole population thanks to the administrative records. Using these meaningful variables, we can test whether estimating impact based on the whole sample or on the two subsamples of respondents to the two surveys would have led to significant changes. We also checked survey outcomes to see whether adding covariates changes our results and whether implementing the method proposed in Behaghel et al. (2015) also leads to significant changes. This method is based on using information about the number of calls needed to reach individuals to balance response

²⁰A baseline survey was also supposed to be carried out before randomization. However delays in carrying out the survey resulted in the survey being mostly carried out after randomization and most of the time one month after participants were contacted to turn their usual standard contract into the cash transfer contract. See appendix C

rates between treatment and control groups, removing some of the observations in the group with the highest response rate. Finally, we also implemented Lee bounds (Lee, 2009).

3.3 Balance Check and Sample Description

Table A1 provides means in the control group and the treatment-control difference for the variables collected. For soundness of the analysis, three specifications are shown. The first set of columns considers the whole sample of the baseline respondents, the second set considers only the respondents to the midline survey and the last one considers the respondents to the endline survey.

Most of the variables in the table were recorded when subjects registered at the JYC. It is noteworthy that all participants in the study, whether in the control or treatment groups, enrolled in the standard program in February and March 2011 but most had already been in contact with the JYC beforehand, so the variables reflect their situation at that time. As a result, variables only trace a subject's status at the outset of the study with partial success and therefore are useful mainly to balance out the sample.

Variables are indeed well balanced between both groups. Across the full sample, we only reject the equality of means between the treatment and control group in two of the 44 variables studied. In the sample of respondents to the midline survey, this figure increases slightly: only three variables have different averages at the 10 % significance level; one at the 5 % level and another at the 1 % level. In the sample of respondents to the endline sample, only one variable is not balanced at the 10 % significance level. Similarly, for each sample, we test for the joint nullity of all coefficients by regressing the assignment variable on linearly independent variables in the table. We cannot reject the joint nullity hypothesis at the 5 % significance level. We can conclude that the sample composed via randomization of the month when individuals enrolled in the standard program worked well and produced two statistically similar groups of young adults. We also note a reassuring result: the differential attrition between the groups does not jeopardize this balance for the population of respondents to the mid- and endline surveys. Differences do exist, however, in the characteristics of respondents to these two surveys and those of the entire sample. This is particularly the case for living conditions and education level. The hypothesis of equality of means between each of the respondent populations and the entire sample is strongly rejected. These results suggest that internal validity appears to be only somewhat affected by the survey process; external validity could be more affected, however.

Young people who enrolled in the standard program in February and March 2011 are young compared to the age range required for enrollment in the experimental program (18-22 years old). Most are between 18 and 20. Participants in the study have few qualifications and

most dropped out of school at the high school or basic vocational level. Furthermore, only 30 % of them have a driver’s license, an expensive “diploma” in France. Data in the records show that only 62 % still live with their parents and that nearly 20 % live with another family member, with friends, or in unstable living conditions. When they registered, they had very little personal income: €77 a month on average.

Some of this data is consistent with previous information noted with the case worker in the first quarter of 2011, and includes contacts, meetings, periods of employment or training declared during this period. We note that the young adults enrolled in the study are highly disconnected from the job market. The number of days spent in employment (resp. training) in the three months preceding randomization is 6.7 (resp. 6.4). Only 14.5 % of participants declare having worked during that quarter, and 13.5 % were in training.

3.4 Estimation

Given an outcome variable y , we estimate “Intention To Treat” (ITT) parameters applying ordinary least squares to the model:

$$y_{m,i} = a + c \times Z_i + \sum_m \alpha_m I_m + u_{m,i} \quad (1)$$

where Z is a dummy variable for assignment to the treatment group (the group which had a standard contract and was offered an experimental one). As such, it is the variable directly derived from randomization, independent from potential outcomes. m represents the JYC and i the index of the individual. Standard errors are clustered at the JYC level.

We can also control for observed characteristics, to either improve the precision of estimators, or to account for the existence of residual differences between the treatment and control groups:

$$y_{m,i} = a + c_{ITT} \times Z_i + xb + \sum_m \alpha_m I_m + u_{m,i} \quad (2)$$

Generally, the list of control variables is the same from one outcome variable to another and corresponds to the one in Table A1. One way of gauging the extent of impact is to compare it with a counterfactual mean – what the average situation of the treatment group would have been if the program had not been implemented. It is estimated using the average situation of the control group.

However our study wanted to look at the program’s impact on participants with a contract

that included a transfer. To obtain such a parameter, we used this model instead

$$y_{m,i} = a + c \times T_i + xb + \sum_m \alpha_m I_m + u_{m,i} \quad (3)$$

where T represents the dummy variable for participants who chose the transfer program when it was offered. This variable, unlike the Z variable, is a decision variable and therefore potentially correlated to the residual, excluding the use of the ordinary least squares model for estimate. We estimate this parameter using an instrumental variables method and use the Assignment variable as an instrument. This is equivalent to the Local Average Treatment Effect (LATE) parameters ((Angrist et al. (1996)). These are equivalent to “Treatment on the Treated” parameters in that control group subjects cannot take part in the transfer program ((Bloom, 1984; Angrist and Pischke, 2009).

Another way of gauging the extent of impact is to compare it to a counterfactual mean – what the average situation would have been for those taking part in the RCA program if the program had not been implemented. This counterfactual mean is easily identified using the data (Imbens and Rubin, 1997). Here, we estimate using the average situation of the control group minus the impact estimated using the instrumental variables method.

4 Results

4.1 Increased Program Attendance

Figure 3 (a) presents the duration of participants’ enrollment in the program (Intention to Treat (ITT)). The rate of enrollment each month in one of the two programs (standard or experimental) is represented as a dotted line for the control group and a solid line for the treatment group. The control group line is highlighted by a zone equivalent to a 95 % confidence interval as estimated by equation (2). As such, when the solid line falls outside the highlighted zone, the difference estimated by equation (2) is statistically significant at the 5 % level.²¹ Months are numbered from early 2011 onwards, even though program enrollment occurred at months 2 and 3 and randomization began in month 4. Figures 3 (b) and 3 (c) provide the same information for the number of monthly meetings with a JYC counselor (Panel B) or simply exchanges (including meetings but also email exchanges and telephone calls). Figure 3 (d) shows the proportion of participants who had no further contact with

²¹More specifically, we only report the average rate of enrollment observed for the control group (or, more generally speaking, the average for the outcome variable under consideration). We represent the evolution of the treatment group using equation 2: the program enrollment rate is equal to that of the control group plus the effect of the program – the c_{ITT} coefficient of the Z variable derived from the equation 2 estimate. This treatment group enrollment rate differs slightly from that seen in the data due to the inclusion of JYC dummy variables and control variables in equation 2 which improve estimate accuracy.

the JYC after the specified month and until the end of the study period (month 30, or June 2013).

In total, Figure 3 clearly shows the significant effect of the experimental program on program enrollment and meetings/other exchanges with the JYC. Figure (a) shows the steep decline in standard program enrollment rates in the first year and a leveling off of rates at 20 % in the second year. Conversely, enrollment rates for the experimental program drop to a much lower level. The difference is quite spectacular in the second year. To understand the difference, it is important to note that counselors are supposed to meet with participants regularly and exclude them from the program when they miss several appointments in a row (in both the standard and experimental programs). However, in practice, counselors maintain many people in the program even though they miss appointments and only exclude them when re-enrollment is necessary at the end of a given year. It is therefore likely that the actual enrollment rate is lower than reported using administrative records.²² Figures (b) to (d) clearly show the outcome of longer enrollment in the program in terms of meetings and other exchanges. The average number of exchanges with the JYC or meetings with counselors is considerably larger in the treatment group than in the control group. Similarly, the proportion of participants no longer in touch with the JYC after a specified month is considerably lower in the treatment group. On average, then, treatment group subjects have more frequent exchanges with the JYC for longer periods of time than the control group.

Table 3 summarizes information in Figure 3, indicating the average number of exchanges over the two-year program. It also shows the payments received from the JYC, with impacts measured both in terms of ITT and ToT. The top panel acts as a first-step estimator and shows that participants assigned to the control group didn't have access to the program and that 82 % of the participants assigned to the treatment group decided to convert their standard contract into an experimental contract. The table also shows that the average number of months of program enrollment increases by 9.6 from an average of 12.1 and that the number of meetings over the two-year period increases by 6.5 from an average of 8.1. The table also shows the cost of the program in terms of payments to participants: whereas only €264 on average would be paid during the entire program without the transfer program, participants receive an additional €1,868, bringing the total amount received to €2,132.

To summarize, Figure 3 and Table 3 show that the young adults in the programs are extremely responsive to the financial incentives provided as part of the experimental contract: they remain enrolled in the program for longer, and they maintain a more active relationship

²²In the midline survey conducted in April 2012, participants specified whether they were still in the program or not. Enrollment rates are logically lower than what appears in administrative records: 31 % of participants in the control group and 61 % of those in the treatment group declare enrollment in a program on this date.

with their JYC. The logical consequence is a sharp increase in the amount of cash received by participants.²³

4.2 Participants Are Offered More Opportunities But Do Not Seize Them

Table 4 presents a second central outcome of our assessment. It shows that treatment group participants received more information on topics related to important program goals (career plan design, training, employment); they are offered more training and job opportunities, but do not take more initiative.

Administrative records register and encode the content of meetings and exchanges with each enrollee at the JYC. This data can be used to assess the “productivity” of these meetings in that more meetings effectively expose participants to more information on the job market and to more opportunities. Our variables address information on career planning services, training courses, and job opportunities. The data further records instances in which youth were matched with providers of these services, courses and job offers.

Records also provide information on periods of employment, training, internships and apprenticeships, which is recorded every time an exchange occurs, or retroactively to update a participant’s file. Information can also be about a job, course or service obtained via a channel other than the JYC. Given that the experimental program involves more meetings and exchanges with the JYC, there is a discrepancy between the situation of treatment and control group participants as they appear in the records. It is the situation as reported to the counselor which appears, not the real one. Impacts we can measure with these variables are skewed upwards due to the increased number of meetings and exchanges for the treatment group participants.

In the following order, Table 4 presents program impact (ITT) on the information a counselor gives to a participant (top panel), service matching (middle panel) and action actually taken (bottom panel). The table also indicates average participation rates in the program and the number of meetings, in order to establish any possible discrepancies in employment and training status between the two groups. The table presents impacts linked to actions and situations recorded in the first three months, then the first six months, then the first year of the program. After the first year, discrepancies in program enrollment rates between treatment and control groups are such that comparisons provide little information.

²³To illustrate this, the additional transfer paid to people in the experimental contract can be compared to the number of additional meetings held. For each additional meeting, €287 is paid (=1 868/6.5). Of course, this is a simplistic calculation which does not account for other exchanges at the JYC in conjunction with the meetings, such as attending workshops, for example). Furthermore, providing young people at the JYC with additional resources in order to boost their independence was one of the program’s goals

The table clearly shows a link between program participation and increased exposure to information on services available in order to establish a career plan, on training and on employment opportunities (top panel). The number of all types of actions recorded by counselors – including health and housing services – is 5.2 per young adult in the control group in the first quarter; program impact is +2.8 actions per participant, which is largely significant: it is a steep increase of 48 % which is relatively consistent with the increase in the number of interviews (69 %, or 1.26/1.82). The same increase is observed for each type of service – employment, training and career planning – and a similar increase is observed regarding information received after the first quarter (first semester and first year).

The table also shows that this additional information is followed by an increase in service matching (middle panel). In the first three months of the program, service matches for career planning are +0.14 higher per participant in the treatment group, compared to an average of 0.14 in the control group. Rates of service matching for training and job offers in the treatment group are also statistically superior than in the control group: +0.05 for training compared to an average of 0.15 in the control group, and +0.13 for job offers compared to an average of 0.47. Differences between the treatment and control groups subsist over the longer periods studied (six months and one year).

The midline survey conducted in April 2012 (one year after the study began) provides additional information about the assistance received by participants, from their point of view. In particular, respondents are asked to specify how many meetings were held between January and March 2012 at the JYC but also with other traditional employment structures: the Public Employment Service and temp agencies. Like in the administrative records, the results, which do not appear in the tables, show a definitely significant increase in the number of meetings with the JYC in the treatment group: +0.77 for meetings from an average of 1.58 in the control group. Interestingly, there is no drop in the number of meetings via other channels: in the control group, between January and March 2012, 0.33 meetings were held with the Public Employment Service and 1.27 with temporary employment agencies; neither of the estimated impacts of the programs – 0.00 and -0.02 respectively – are significant.

At this point results show that greater participation in the program entails an increased series of opportunities and actual solid offers made.

The bottom panel of Table 4 reveals the other key result: the young adults in the program do not seize these opportunities: the rates of re-employment or 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. This occurred in the first three months but also in the first six months and the first year. Again, these rates are those noted in the administrative records and reflect not the true situation of participants but

the one reported to counselors during meetings. Counselors do, nevertheless, monitor the service matching they offer: even if reported information is not indicative of real situations, increased service matching can be expected to improve reported situations, if participants seize these opportunities.

4.3 No Impact on Employability Investments

The bottom panel of Table 4 looks at human capital investments made and reported in national JYC records. The surveys carried out in April 2012 and April 2013 (one and two years after the study began) provide additional information about these investments without the 'filter' of reporting to counselors. Table 5 includes two panels: the top one contains outcome variables for human capital investments and the bottom one the outcome variables for job seeking activities. The left side of each panel provides the results of the 2012 midline survey (conducted one year after the study began) while the right side looks at the 2013 endline survey (two years after the study began). Following Kling et al. (2007), we established two indices which summarize impact on a series of outcome variables. The first column lists the variables used in the index.²⁴

We looked at a wide range of information to assess employability investments. We used variables for objective aspects, like apprenticeship programs, internships, number of courses completed, certified training and obtaining a driver's license. Other variables are for subjective aspects, such as having an established career plan, or the self-assessment of prospects of finding suitable employment. This wide range of outcome variables provides an overview of the many ways an individual can improve their employability which could be linked to program participation. For example, the program may give a young participant time to invest in less formal investments than training courses. Results almost unanimously confirm the initial picture provided by administrative records: only two selected variables improve as an effect of the program. The number of participants who start a driver's course to obtain their license or who had obtained it is 3 points higher in the treatment group, based on a rate of 41.9 % in the control group.²⁵ The index which summarizes employability investments

²⁴Following Kling et al. (2007), we standardize variables by subtracting the control group mean and dividing by the control group standard deviation before summing them. In addition, unlike in the paper quoted, we also standardize the outcome variable again for a standard deviation of 100 in the control group. Doing so gives a clearer picture of the detection capacity of the evaluation protocol. A standardized variable demonstrates a minimum detectable effect 2.8 times the standard deviation estimated by the variable with a statistical power of 80 % and p-value of 5 % (Bloom, 1995). In essence, this means that our protocol is able to detect a minimum effect of between 9 and 10 % of a standard deviation: a weak minimum detectable effect compared to the literature (9.3 % = 3.5 x 2.83 for the job seeking effort index and 9.9 % = 3.3 x 2.82 for human capital investment index).

²⁵Obtaining a driver's licence in France – a long process – is difficult. Learners must first pass a demanding theory exam and then complete a large number of driving lessons, register for the driving exam and wait for a spot to open. It is an expensive investment that costs upwards of €1,500. Due to the distance of

includes the major categories of outcome variables. No effect was detected. Likewise, the estimated standard error is 3.5. The minimum detectable effect is therefore 2.83×3.5 , or 9.9 for a variable for which the standard deviation is standardized to 100 (see footnote ??). We conclude that the study detects no impact on employability investments with high statistical power.

The study also provides information about job search behavior: actively seeking work, using the usual search channels, the distance participants are willing to travel to accept a job and their willingness to move to take an indefinite term job contract. As Table 5 clearly shows, being assigned to the treatment group does not change job search behavior at all. When we use the same process to establish an index for human capital investments, we see a similar lack of impact despite a significant detection capacity.²⁶

4.4 Short-Term Negative Impact on Employment

Employment is a key outcome variable in our study. Several effects can be expected. The ultimate long-term goal of the program is to give participants improved access to high-quality jobs. This was to be achieved by increasing employability investments and job seeking activities, which may have initially and temporarily reduced employment rates (the “locking-in” effect). On the other hand, transfers weaken the incentive to work (classic income effect). Furthermore, the scheme involves an implicit tax rate of 24 % since the monthly payment amount decreases as employment income increases; the first year, tapering begins at €250, and transfers stop when a participant earns €1,050 (the equivalent of a full-time minimum wage salary). This is likely to reinforce disincentives to work and encourage part-time work over full-time work.

The surveys were used to find out whether and how much participants had worked from month to month and on what basis. For each month of the study, we can see whether surveyed participants worked, if the job(s) lasted the whole month or not, and if it was (they were) primarily full- or part-time. We combined both surveys to establish a two-year timeline of employment. It should be noted that results for the first 12 months are obtained from answers to the April 2012 survey while results for the months after that are obtained from the April 2013 survey. Results are presented in Figure 4 as they are for program participation: the monthly employment access rate for those in both groups is accompanied by the same

underprivileged neighbourhoods from town centres, a driver’s licence is seen as a key asset in a young adult’s search for employment. (Gallo et al., 2014) have assessed the impact of a pilot project in France in which 10,000 young people enrolled at JYCs were given the chance to earn their driver’s licence for free. The share of youth with a driving license remained at a low 45 % two years later, despite a 15 percentage point increase due to funding.

²⁶Another interesting outcome – not reported in the tables but mentioned in section 4.2 – is that there is no difference in how other labour market intermediaries such as the PES and temp agencies are used.

shaded area used to determine the 95 % confidence interval for the difference between the treatment and control group (see section 4.1). Figure (a) presents the rate of employment for that month; figure (b) the rate of full-time employment, figure (c) the rate of part-time employment. Lastly, figure (d) combines all this information to establish an employment index of 1 for participants who worked full-time the entire month; 2/3 for those who worked part-time the whole month or full-time for part of the month; 1/3 for those who worked part-time for part of the month, and 0 for those who did not work that month.

Results clearly show that the experimental program has a negative impact in the first six months, on all forms of employment except part-time work, on which it has no effect. This result is therefore consistent with the disincentive effect of the transfer on employment.²⁷

The effect is not substantial, however. Table 7 presents the results in ITT (left panel) and ToT (right panel) for the first six months, the first year, and the second year. In the employment 'volume' index, there is a decrease of -0.28 for participants in the experimental program, compared to a counterfactual mean of 3.97. This is equal to a 7 % decrease only. The surveys also measured the type of job held at the time (table 7). Like the other tables, the left panel presents results from the midline survey and the right panel results from the endline survey. Various aspects of heterogeneity are studied: type of contract (short-term, permanent, contracts through temp agencies, apprenticeship or internship), formal or informal employment, subsidized jobs and type of employer (public or private). The experimental program has next to no impact on the type of job found, with the exception of a slightly higher rate of informal employment and public-sector employment (impacts significant at the 10 % level only).

4.5 Income and Social Integration

Another goal of the experimental program is to provide young job seekers with additional financial means in order to increase their resources and support their integration. The surveys measure the impact of the program on these factors. Here again, the program appears to be ineffective, with no significant impact on income, or improvement in social integration. The only notable result detected is that participants in the treatment group save more, once more highlighting the importance of financial factors in their behavior.

Table 8 presents the impact of the experimental program on the different income sources of participants in March 2012 and March 2013. The table is composed of three panels. The left and middle panels present results in ITT for the midline (left panel) and endline (middle

²⁷This may be attributable to a locking-in effect, but a lack of impact on employability investments or job searches, together with the slow frequency of interviews included in the program (one a month), suggests this is not the case.

panel) surveys, to compare all control group subjects (who signed a standard contract) with all treatment group subjects (82 % of whom signed an experimental contract and 18 % a standard contract). The right panel also looks at the midline survey but presents results in ToT, on subjects who signed an experimental contract.

One surprising outcome is that in March 2012, treatment group income was only €40 higher than that of the control group (€602 on average). The program only marginally increases the resources of participants, despite the fact that, in March 2012, 11 months after the study began, subjects were still entitled to the maximum theoretical transfer amount (€250 per month for those who did not work). The table contains a second surprising outcome: income earned by treatment group participants from the JYC only increased by an average of €87. There is significant substitution between different income sources: JYC income increased by €87 but other forms of income fell by €47. This is a general decrease that affects all income sources: employment income (€26, but not significantly different from zero) and other sources (€21). Income from parents drops significantly but modestly (€10).

This trend is primarily attributable to the 24 % implicit tax rate on work income: JYC income only increased by €87 for treatment group subjects. In fact, if we look at the ToT results, we see that experimental contract participants earned €102 more than the counterfactual mean of €23; these subjects earned €125 (102 + 23) on average from the JYC. This amount still falls short of the theoretical maximum of €250. This is due to the fact that subjects earned work income – €435 on average for experimental contract holders (compared with a counterfactual mean of €465). The amount of implicit tax on this income is therefore $0.24 \times €435 = €104$. This €104, added to the €125 provided on average by the JYC, makes €229, which is only slightly less than the expected €250. The remaining difference is explained by the fact that some participants dropped out of the program.

Table 9 shows that a modest increase in income does not significantly affect participants' spending habits.²⁸ In particular, there was no increase in temptation good spending or big purchases. Saving, however, was more frequently observed in treatment group subjects, who in Q1 2012 saved €37 more than the control group (which saved €211). Neither was there a change in perceived financial constraints. The midline survey asked participants about the various aspects of these constraints: trouble paying the bills, rent, taxes, spending a day without eating, or incapacity to afford health care. These financial problems are clearly real; 20 % of subjects on average said they feel these kinds of constraints. Nevertheless, the program does not appear to lessen these constraints for the participants.

One last important factor is social integration, several aspects of which can be examined via

²⁸The table only includes spending reported in the midline survey; expenses were not examined in the endline survey.

the surveys. Results appear in Table 10. The first aspect we looked at is mobility, viewed as a major problem for young adults that can make integration harder. We asked participants about their usual mode of transport. The top panel of Table 10 shows that there is little variation between treatment and control group participants in terms of transport.²⁹

Another aspect of integration we looked at was confidence – in public institutions, for example: school, the health care system, the JYC, the justice system. Confidence in the JYC is sustainably higher for treatment group participants (+8 percentage points more than the control group for the endline survey).³⁰ This is notable progress. The young adults involved in the study are known to be highly mistrustful of public institutions. Building trust with them is notoriously difficult. It is often the first pitfall that programs come up against, and some even implement special schemes, like job search clubs, to overcome this obstacle.³¹

The last factor of integration studied is a more incongruous one. It looks at personality traits such as a locus of control, present bias, social interaction such as the number of friends a participant has, and how happy a subject generally is with his or her life. The table shows that very few of these aspects are seriously affected.

5 Heterogeneity

Several heterogeneity factors are potentially pertinent. The first is related to the existence of financial constraints. One of the program’s main objectives is to allow financially vulnerable participants to invest in their employability. We also studied heterogeneity in relation to how disconnected subjects were from the job market. Certain participants are far more disconnected than others; in their case, human capital investments are a key issue. Lastly we looked at heterogeneity in relation to gender. Results do not reveal a significant heterogeneity factor and do not invalidate results discussed above.

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

$$y_{m,i} = a + c_{ITT,I} \times Z_i \times I_l + c_{ITT,1-I} \times Z_i \times (1 - I_l) + dI + xb + \sum_m \alpha_m I_m + u_{m,i} \quad (4)$$

²⁹Independence in terms of transportation increases, however, from one survey to the next (one year later): the number of participants who take public transport most of the time dropped 18 points in both groups between April 2012 and April 2013, while the rate of participants who drove increased considerably.

³⁰Other survey questions asked participants what they thought of the JYC concerning other aspects. The responses confirm that the regular transfer payment builds a longer-lasting relationship of trust between the JYC and young adults.

³¹See for example the FIXME Club Jeune Zus

Regression also uses the I variable if necessary (i.e. if it is not a control variable already) as an additional control variable. The $c_{ITT,I}$ and $c_{ITT,1-I}$ coefficients represent the impact of being assigned to the program on the $I = 1$ subsample and on the $I = 0$ subsample.

Three tables of results appear in Appendix B. The left panel of each table lists the results for participants without the criteria for heterogeneity (few financial constraints in Table B1) and the right panel lists results for those who meet this criteria (serious financial constraints). The last column lists the results from the test of a same effect in the two groups. We only studied a smaller set which included the main variables of the study: a composite employment index for the first year and second year; (see Table 6 for more information about the questions used), composite indices for human capital investment and job hunting (see Table 5), income in March 2013, levels of savings in March 2013, perceived financial constraints, and a few variables from the administrative records related to services provided by the JYC.

The first aspect we studied was financial constraints. We expected that the most financially-vulnerable participants would benefit the most from the program, especially in terms of employability investments. Identifying the subsample of participants most likely to experience financial programs is no easy task. We used the financial constraints index developed using information on that subject collected in the midline survey (see Table 9 for more information about the questions used). For control group subjects, we looked at how this index can be predicted from variables in the administrative records using only variables that existed prior to the study. We considered that a subject experienced serious financial difficulties if the predicted value of the composite constraints index was higher than the median value of the index in the control group. Where this did not happen, the subject was considered to have few financial constraints. Results appear in Table B1. Effects in both groups are very similar. We observed that the decreased volume of employment in the first year – which was greater for financially challenged subjects – was only significant for them alone. However the difference between the two groups is not very strong and not significant. We also noticed that subjects likely to experience financial problems rank much lower on the human capital investment index than the other subjects, and that program effects are positive for them but negative for the others. No effect or difference is significant, however. The only noteworthy and significant difference is observed among the most financially vulnerable subsample, whose difficulties are drastically reduced. This subsample also has significant savings. This lack of specific effects on financially vulnerable participants is important: one of the reasons a transfer is paid is that certain young adults have financial difficulties and cannot commit to demanding investments in human capital or job searches. A lack of impact on the most financially vulnerable subjects does not confirm that this is a major cause of their lack of investment.

We also examined heterogeneity in terms of gender and a subject's connection with the labor market. We measured how connected participants were based on the fact that the standard 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. Subjects enrolled in the standard program or intensive experimental program are considered as being more disconnected from the labor market. Tables B2 and B3 present our findings. As in the previous table, estimated effects in both groups are very similar. This is particularly striking in the table on gender where no significant difference is found between men and women. Some significant differences in human capital investments do appear where labor market status is concerned. Subjects who were the most disconnected from the labor market invested more than less disconnected subjects in the treatment group, while they invested less in the control group. It is an interesting outcome. An analysis of the different components of the index shows that the biggest improvements are found in self-assessments of employment prospects.

6 Robustness

The midline and endline surveys provide very useful information in addition to the administrative records, but have weaker response rates, which are higher in the treatment group than the control group (see Table 2). This raises a doubt as to the internal and external validity of the results obtained via these surveys.

The issue can be addressed by looking at Tables 2 and A1. Table 2 shows that the response rate is significantly linked to individual characteristics found in the administrative records. Table A1 shows that for each survey, both groups of respondents in the treatment and control groups are balanced (based on variables from the administrative records, recorded before the study began). These results can lead to two conclusions: on the one hand, survey respondents are specific, which implies that external validity may be compromised. On the other hand, both groups of respondents in the treatment and control groups are identical, which suggests that internal validity is less challenged.

We pursued our study by carrying out additional analyses. We were fortunate to have at our disposal many pertinent outcome variables from the administrative records and therefore for the entire sample. We could therefore measure program impact on three different subsamples: the entire sample, midline survey respondents and endline survey respondents. We formally tested for possible differences in program impact between these three groups using a statistical test built based on the estimate of the model below:

$$\begin{aligned}
y_{m,i} = & a + cZ_i + dZ_iM_i(1 - E_i) + eZ_i(1 - M_i)E_i + fZ_iM_iE_i \\
& + g_1M_i(1 - E_i) + g_2(1 - M_i)E_i + g_3M_iE_i + x_ib + \sum_m \alpha_m I_m + u_{m,i}
\end{aligned} \tag{5}$$

where M_i and E_i are dummies for respondents of the midline and endline surveys respectively, and Z_i is always the assignment variable for the treatment group. The c coefficient is the effect on the entire sample. The d coefficient measures the difference between participants who took part in the midline but not the endline survey and overall effect. The coefficients e and f measure the corresponding quantities for the groups which took part in the endline but not the midline (e) and in both the mid- and endline surveys (f). The hypothesis $d = e = f = 0$ therefore equals the lack of effect of response behavior on estimated impact. If the hypothesis is not rejected, neither internal nor external validity are compromised.

Results appear in Table A2. The first set of columns lists the results of the estimate on the entire sample, while the second and third sets present the results of the estimate when analysis is limited to either respondents in the midline or endline survey, respectively. The last column presents results from the previously described test (on the nullity of the d , e and f coefficients in model 5. Each line represents a variable. We selected the central variables from the administrative records: number of meetings with a caseworker, and the total number of actions recorded by the caseworker. Actions are recorded by type: employment and training; putting a participant in touch with someone concerning job offers or training opportunities, and jobs and courses already started or completed. These variables are calculated either for the first quarter (top panel), the first semester (middle panel) or the first year (bottom panel). We found that impacts stemming from assignment to the treatment group are very similar for all variables. Test results confirm this convergence. Most of the time, the tested hypothesis is widely accepted. It was only rejected once at the 5 % significance level and twice at the 10 % level, for all 24 tests performed.

Figure A1 (a) presents the same type of results: the impact of the experimental program on the average number of meetings per month either for the entire sample or for survey respondents only. The graph clearly shows that the estimated profile for program impact on the number of meetings is very close for each estimate (standard errors are not included in the graph to keep it simple).

A last, more traditional series of tests looked at alternative estimators for a set of variables selected from the midline and endline surveys. Results appear in Table A3. The first set of columns recalls the results of the specification discussed above: the estimate of equation (2) when control variables are introduced, the results of which are presented in certain tables

in section 4. The second set of columns presents the results of the estimate of equation (1) (without control variables). The third set presents the results obtained by correcting for sample selection bias resulting from non-response, using the method developed by Behaghel et al. (2015). In this procedure, only individuals who were reached after a certain number of attempts are included in the treatment group (which has a higher response rate), so that final response rates in both groups are identical (18 calls for the midline survey, with a 59 % response rate in treatment and control groups – see A1 b). The two last sets of columns present bound estimates, as developed by Lee (2009).³²

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 (not significantly different), but point estimates differ slightly. Nevertheless, the same conclusions apply to all variables in the Table.

7 Conclusion

This paper presents the results of a large-scale randomized controlled trial conducted in France to assess the increased investments young job seekers make in their employability. The sample was composed of young, unskilled job seekers. This population has significant problems finding its place on the labor market. Deciding to invest in one’s employability, however, is not simple. These job seekers are often trapped in a circuit of multiple low-skill jobs which offer little new experience. We looked at a career guidance program aimed at helping these individuals establish a career plan and follow the necessary steps to achieve it. The program is characterized by a high drop-out rate and poor levels of commitment on the part of participants.

The study involved the creation of a monetary transfer system conditional on the participation of enrollees in the support program. Our assessment was a unique opportunity to measure the impact and scope of financial incentives in the demands for employability investments of young, unskilled job seekers.

Results fall quite short of expectations. Physically speaking, the young job seekers do participate more; they go to the job center offering the program more often, and schedule more

³²These last estimates do not include control variables or JYC dummies.

meetings. But their increased involvement remains superficial. They are offered more opportunities for training and services to improve their employability, as well as actual job opportunities which would provide experience, but they do not seize them.

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

One of the main findings of the study was the key effects of financial incentives on the behavior of young job seekers in the program. Participation in the program is one example; limited rates of employment in the early stages of the program is another. Program impact stops where the incentives stop and no differences are found between subjects with financial difficulties and subjects without. In theory, the conditional nature of transfers should have affected commitment to the program, career planning and the completion of the various steps of the career plan. In practice, however, implementing transfers on a conditional basis is quite complicated. The only real incentives provided by the program were related to the meetings with counselors.

An alternative model could take conditionality to the next level, for example by paying part of the transfer only once significant steps towards finding employment are accomplished. Babcock et al. (2012) suggest using such a mechanism in the more general context of unemployment insurance. Our results are consistent with this mechanism in that they illustrate the risk of incentives remaining a half measure. Conditional transfer payments have been the subject of many studies of school enrollment in developing countries. Baird et al. (2011) show that making a payment conditional enhances participation. Barrera-Osorio et al. (2011) show that making a payment partly conditional on school marks has an effect on results. Nevertheless the actual form this strategy should take is not clear. Results obtained on a theoretical level by Benabou and Tirole (2003), as well as evidence found in an experimental setting (Ariely et al., 2009) suggest that a financial reward for efficiency can be counterproductive. Providing incentives sends a signal which people try to interpret, inferring things about a hidden part of themselves or about what they are being encouraged to do. Political

discourse conveys both the idea of making transfers conditional and of giving young seekers independence. One of the initial models presented in the 2009 Green Paper on Youth involved providing young job seekers with a lump sum paid when they achieved specific stages of their career plan, to both increase the perceived return and to make career-related achievement easier.

Nor is it certain that conditional transfers are the only option to explore. For instance Benhassine et al. (forthcoming) show that in Morocco, to improve education, a non-conditional transfer system which is labeled however – provided to recipients with a clear message that it is meant to improve their children’s participation at school – produces better results than a standard conditional transfer system. Blattman et al. (2014) show that in Uganda, providing poorly educated young people with transfers to finance existing projects that have been identified as promising yields very good results. Recipients benefit from training, invest in the physical capital of a revenue-generating activity and increase their long-term income substantially. Of course, these results are linked to different populations and contexts and cannot be directly transposed onto the context of young school drop-outs in underprivileged French suburbs. They are proof, however, that alternative methods can work. The initial findings of studies conducted on this same sample do tend to confirm that healthcare, housing and mental health support programs – accessible to young job seekers while they are in the labor market and form an opinion of public institutions – can have a significant impact on training and integration.³³

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

³³See the experiment which involved offering a doctor’s services at each JYC to detect health problems, along with health and health care-related information: <http://www.povertyactionlab.org/evaluation/supporting-18-25-year-olds-through-long-term-mentoring-plus-financial-assistance-france-p>

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Table 1: 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 .

Table 2: Youth assignment and Surveys

	JYC			Survey response rates					
	Type A	Type B		Differential response rates					
				#	Control	Controlling for Paires and Covariates			
Test	2661	1372	1289	Midline	3413	59.3	6.0***	5.8***	6.1***
Control	2837	1455	1382	Endline	2310	39.5	5.3***	4.7***	5.0***
Total	5498	2827	2671	Admin.	5487	99.8	0.02	-0.02	-0.004

Experiment records, midline and endline surveys

In its left panel the table gives the number of youth in the Treatment and Control groups and the number of them coming from Type A JYCs (at which youth registered in March are assigned to treatment) or Type B ones. In its right panel the table provides information about the surveys. Midline survey information is on the first row and endline information on the second row. The table first gives the response rate in the control group and then the differential response rate as well as its significativity under various specifications. The last specification includes control variables which are the variables listed in table A1. Standard errors are robust to heteroskedasticity and are clustered at the Job Youth Center level. * corresponds to a parameter significant at the 10 % level, ** at the 5 % level and *** at the 1 % level.

Table 3: Impact of the Cash program on the number of month in program and transfers received from the JYC

	Obs	Mean	Participation	std	sign
Take up Cash Program	5492	0.00	0.82	0.01	***
Intention To Treat parameter					
	Obs	Mean	Coefficient	std	sign
Months in program	5486	11.4	7.9	0.3	***
Total number of meetings	5492	7.3	5.3	0.5	***
Transfer from JYC	5492	237	1530	89	***
Treatment On the Treated parameter					
	Obs	Counterfactual mean	Coefficient	std	Sign.
Months in program	5486	12.1	9.6	0.4	***
Total number of meetings	5492	8.1	6.5	0.5	***
Transfer from JYC	5492	264	1868	91	***

Administrative records

The first column gives the number of individuals, then the control group mean, the coefficient of the treatment parameter, its standard error and its significativity. The upper panel provides Intention To Treat estimates obtained from an OLS regression including the test variable, a set of JYC dummy variables and the set of control variables listed in table A1 (see equation 2). The lower panel provides information about the Treatment On the Treated parameter in which the Cash Program participation variable is instrumented by the assignment variable (see equation 3). The control group mean is in this case an estimate of the counterfactual mean. The first line of the upper panel provides results of the corresponding first stage regression.

Standard errors are robust to heteroskedasticity and are clustered at the Job Youth Center level. * corresponds to parameter significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 4: Services received from JYC

	# obs	1st quarter			1st semester			1st year					
		Mean	Coef	std	sign	Mean	Coef	std	sign	Mean	Coef	std	sign
Number of events in which information about different type of services were provided – JYC file													
Any type	5492	5.22	2.84	0.35	***	8.12	4.48	0.48	***	13.48	7.15	0.70	***
Related to jobs	5492	3.03	1.64	0.29	***	4.62	2.51	0.37	***	7.45	4.15	0.56	***
Related to training	5492	0.89	0.37	0.09	***	1.51	0.71	0.14	***	2.64	1.16	0.22	***
Related to career	5492	0.94	0.63	0.11	***	1.38	0.96	0.17	***	2.37	1.39	0.29	***
Number of matching with services – JYC file													
Job offer	5492	0.47	0.13	0.05	**	0.72	0.23	0.06	***	1.09	0.46	0.11	***
Training	5492	0.15	0.05	0.02	***	0.29	0.11	0.03	***	0.53	0.16	0.04	***
Career planning	5492	0.14	0.14	0.04	***	0.21	0.17	0.04	***	0.33	0.21	0.06	***
Number of human capital investments and training courses started and month with reported employment – JYC file													
# training started	5492	0.50	0.02	0.02	.	0.83	0.03	0.05	.	0.83	0.03	0.05	.
# HC investment started	5492	0.62	0.02	0.03	.	1.09	0.04	0.05	.	2.27	0.19	0.10	*
# months with employment	5492	0.70	0.04	0.03	.	1.52	0.07	0.06	.	2.77	0.18	0.09	**
Still in program	5492	0.95	0.01	0.01	*	0.88	0.06	0.01	***	0.40	0.40	0.04	***
# individual meetings	5492	1.82	1.26	0.11	***	2.92	2.04	0.17	***	5.82	3.15	0.24	***
From midline survey													
# months with employment	3413	1.08	-0.09	0.04	**	2.41	-0.18	0.07	**	5.23	-0.18	0.12	.

Administrative records and midline survey April 2012.

The table provides Intention To Treat estimation of variables related to services provided by the JYC. Estimates are obtained in the same way as described in table 7. The table has three panels. The left panel provides events recorded over the first quarter following random assignment, the second panel information recorded over the first semester and the last panel over the first year. The upper panel provides the number of events in which general information about services was received, the intermediate panel provides the number of matches with job offers or human capital investments that occurred through the JYC. The third panel provides the number of human capital investments registered at the JYC and months with reported employment. On the last line the table also provides employment information from the midline survey.

Table 5: Human Capital and Search

	April 2012 Survey – 3417				April 2013 Survey – 2310				
Index	Mean	Coef	std	Sign.	Mean	Coef	std	Sign.	
Human capital investment									
Apprenticeship	1	6.8	0.8	0.8	.	6.4	0.4	1.0	.
Internship	1	2.6	-0.3	0.6	.	1.4	-0.6	0.6	.
Training									
Number over 1 year ($\times 100$)	1	56.0	-2.2	2.6	.	48.0	-4.6	2.9	.
At least one certified	1	30.8	-0.6	1.5	.	17.5	-1.0	1.8	.
Forwent for financial reason		13.7	-0.4	1.3	.	12.6	2.2	1.5	.
Driver's license	1	41.9	3.0	1.3	**	42.6	1.8	2.1	.
Perceived Employment Prospects									
Improved	1	44.3	3.5	1.8	*	46.2	3.1	2.2	.
Same		24.3	-0.3	1.7	.	26.5	-1.6	1.7	.
Reduced		21.5	-1.7	1.5	.	21.9	-0.4	1.9	.
Career Plan									
Has one	1	45.2	-0.4	1.7	.	48.2	-0.6	2.1	.
Has ideas	1	36.9	0.3	1.6	.	34.0	0.7	2.0	.
No idea		17.8	0.2	1.4	.	17.4	-0.5	1.6	.
Has necessary diploma		18.4	-1.4	1.4	.	22.0	-0.5	1.6	.
Human Capital Index		0.0	1.9	3.5	.	0.0	-1.0	4.1	.
Search behavior									
Search for a job		56.2	-0.0	1.5	.	51.5	2.8	2.6	.
Intensity of use of channels									
Web search	1	19.3	-2.0	1.1	*	21.7	1.5	1.8	.
Temporary help agency	1	20.9	-1.0	1.4	.	20.3	0.2	1.7	.
Send resumes	1	36.7	1.2	1.5	.	33.6	0.1	2.4	.
Direct job application	1	28.1	-1.0	1.7	.	27.3	0.0	2.1	.
Number of firms	1	4.8	-0.2	0.3	.	4.5	-0.0	0.3	.
Search Index		-0.0	-2.6	3.3	.	-0.0	1.4	4.9	.
Maximum commute time		35.9	0.7	0.7	.	36.1	0.3	1.1	.
Move if indefinite term contract		20.0	1.1	1.4	.	20.1	0.8	1.9	.

Midline and endline surveys April 2012 and April 2013.

Estimates are obtained applying OLS to equation 2, adding a whole set of JYC dummy variables and the set of control variables listed in table A1. The table also presents human capital and job search indexes. They are obtained by only selecting relevant components in the table which are then standardized and summed (the sum is standardized again so as to get a better idea of the power).

Table 6: Impact on employment : Intention To Treat and Treatment On the Treated

	Intention To Treat parameter		Treatment On the Treated parameter		Relative to mean (d/c)
	Control mean (a)	Coef (b)	Counter. mean (c)	Coef (d)	
First semester					
Any type	2.41	-0.18	2.31	-0.21	0.08 ***
Employment index	1.89	-0.20	1.79	-0.23	0.07 ***
First year					
Any type	5.23	-0.18	5.10	-0.21	0.14 .
Employment index	4.09	-0.24	3.97	-0.28	0.12 **
Second year					
Any type	6.31	-0.03	6.24	-0.03	0.19 .
Employment index	5.05	0.00	4.97	0.00	0.18 .

Midline and endline surveys April 2012 and April 2013.

The table has a left and a right panel. The left panel provides results about Intention to Treat estimation see table while the right panel provides information about Treatment On the treated estimation See table 3 for details. The table also includes in each case the impact relative to the mean in the control group (last column).

Table 7: Quality of Employment

	April 2012 Survey: 3417				April 2013 Survey: 2310			
	Mean	Coef	std	Sign.	Mean	Coef	std	Sign.
Employed	45.4	2.5	1.6	.	52.1	0.4	2.0	.
Type of Contract								
Indefinite term	9.9	-0.0	1.0	.	14.4	0.6	1.6	.
Fixed term	18.0	0.7	1.4	.	19.5	-1.7	1.6	.
Temporary help	5.7	0.8	0.8	.	5.2	1.5	1.0	.
Internship	2.6	-0.3	0.6	.	1.4	-0.6	0.6	.
Apprenticeship	6.8	0.8	0.9	.	6.4	0.4	1.0	.
Other	2.3	0.5	0.6	.	4.1	0.2	1.0	.
With/Without Contract								
Subsidized	9.2	-0.2	1.2	.	8.7	1.7	1.3	.
With contract	42.1	1.2	1.6	.	49.6	-0.1	1.9	.
Without contract	2.9	1.4	0.7	*	2.6	0.4	0.7	.
Type of Employer								
Private	32.8	-0.3	1.5	.	36.5	1.0	2.0	.
Public	8.3	1.7	1.0	*	10.1	-0.3	1.3	.

Midline and endline surveys April 2012 and April 2013.

The table provides results of Intention To Treat estimations of outcome variables related to the quality of employment. If there is no employment the variable is zero. See table 5. Standard errors are robust to heteroskedasticity and are clustered at the Job Youth Center level. * corresponds to a parameter significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 8: Income March 2012 and March 2013

	Intention To Treat parameter				Treatment On the Treated parameter								
	Midline Survey		Endline Survey		Midline Survey		Endline Survey						
	Control mean	Coef	std	Sign.	Control mean	Coef	std	Sign.					
Any type	602	40	16	**	731	-10	20	.	569	47	18	***	8.3
From JYC	33	87	6	***	8	6	3	*	23	102	6	***	448.1
Not from JYC	569	-46	16	***	723	-16	20	.	547	-54	18	***	-10.0
From Activity	483	-26	16	.	676	-5	21	.	465	-30	18	*	-6.4
Wage	372	-11	15	.	491	-5	24	.	359	-13	17	.	-3.6
Odd jobs	18	-3	3	.	24	-6	6	.	19	-4	3	.	-21.3
Unempl. benefits	78	-7	7	.	138	5	12	.	72	-9	8	.	-12.3
Training allowance	15	-4	3	.	22	2	4	.	16	-4	3	.	-26.3
Neither work nor YC	86	-21	6	***	47	-10	7	.	81	-24	7	***	-30.1
Parents	37	-10	3	***	41	-12	5	**	39	-12	4	***	-30.0
Other	49	-11	5	**	6	2	4	.	43	-13	6	**	-30.2

Midline survey April 2012 and endline survey April 2013

The table has three panels. The first two panels provide ITT estimates on income variables following the same procedure as described in table 5. The last panel provides instrumental variation estimates of Treatment On the Treated parameters following the same procedure as described in table 3. The table also includes in each case the impact relative to the mean in the control group (last column).

Table 9: Expenditures – April 2012 Survey

	Obs	Mean	Coef	std	Sign.
Temptation goods over last month					
Number of restaurants	3304	2.2	0.0	0.1	.
Nights out	3276	2.1	-0.1	0.1	.
Tobacco	3413	29.6	1.5	1.8	.
Phone	3390	55.6	-3.4	5.8	.
Temptation goods index	3219	0.0	0.0	3.6	.
Largest purchase in last 12 months					
Largest purchase in last 12 months	3117	660.3	33.6	44.7	.
Saved money in last 12 months	3413	45.4	5.0	2.1	**
Amount saved 1st quarter 2012	3299	210.8	36.8	17.3	**
Financial constraints over last 12 months					
Pbs paying bills	3413	27.7	-0.2	1.7	.
Pbs paying rent	3413	18.1	-0.8	1.7	.
Pbs paying taxes	3413	8.7	-0.1	1.0	.
A day without a meal	3413	19.4	-1.1	1.4	.
Forewent medical care	3413	24.4	-0.7	1.4	.
Bank overdraft	3413	45.0	-1.7	1.7	.
Forwent training for financial reason	3413	13.7	-0.4	1.3	.
Budget constraint index ^a	3413	0.0	-2.8	4.0	.

Midline survey April 2012

The table provides Intention To Treat estimates on various types of expenses in March 2012. The estimation procedure is the same as described in table 5. *a* – sum of standardized variables appearing in the “Financial constraints over last 12 months” panel rescaled to have a 100 std

Table 10: Mobility and Integration

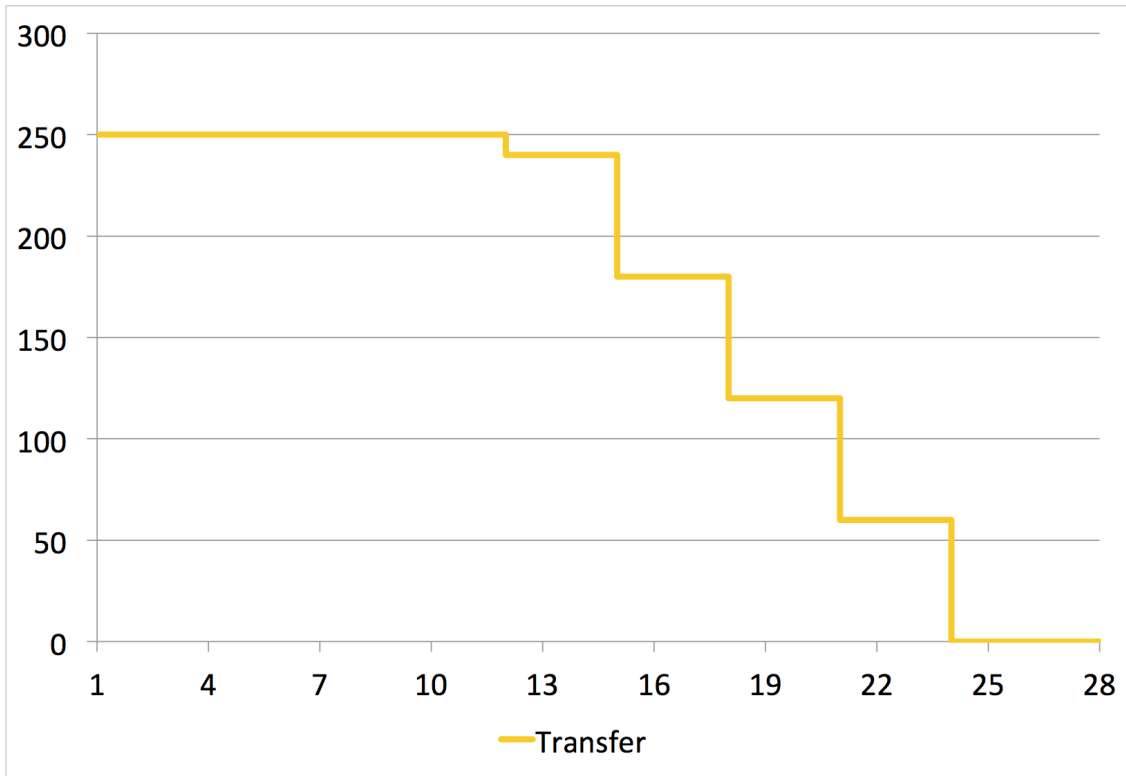
	April 2012 Survey – 3417				April 2013 Survey – 2310			
	Mean	Coef	std	Sign.	Mean	Coef	std	Sign.
Mobility : mainly uses								
Foot	9.3	-0.5	1.2	.	7.9	0.0	1.1	.
Bike	1.7	0.3	0.5	.	1.9	0.2	0.8	.
Public transport	37.5	2.8	2.0	.	29.6	3.3	2.0	*
Parents	2.8	-1.1	0.5	**	1.6	-0.2	0.6	.
Scooter	5.1	-0.9	0.8	.	3.6	0.7	0.8	.
Car	43.7	-0.5	1.4	.	55.4	-4.1	2.0	**
Trust								
School	64.0	3.9	1.7	**	66.4	0.3	2.2	.
Health Care System	84.1	1.1	1.6	.	82.7	-2.0	1.6	.
JYC	81.2	6.6	1.2	***	69.9	7.8	1.9	***
Justice system	53.8	2.3	1.6	.	56.3	-2.9	2.3	.
Sum	2.83	0.14	0.04	***	2.75	0.03	0.06	.
Personality traits and integration								
Number of days								
ready to wait for 20%	97.7	0.4	3.2	.	101.9	-6.8	3.9	*
Locus of control [/20]	10.8	0.0	0.1	.	10.1	-0.0	0.2	.
Life satisfaction	71.2	0.9	0.6	.	71.7	-0.5	0.9	.
No friends	5.4	1.9	0.9	**	6.4	0.0	1.0	.
Number of friends	4.0	-0.1	0.1	.	4.2	0.1	0.1	.
Owes money to relatives	16.4	-2.5	1.0	**	15.4	0.8	1.6	.

Midline and endline surveys April 2012 and April 2013.

The table provides Intention To Treat estimates on outcome variables related to mobility (panel 1), trust (panel 2), some personality traits and integration (panel 3) in March 2012 and March 2013. The estimation procedure is the same as described in table 5.

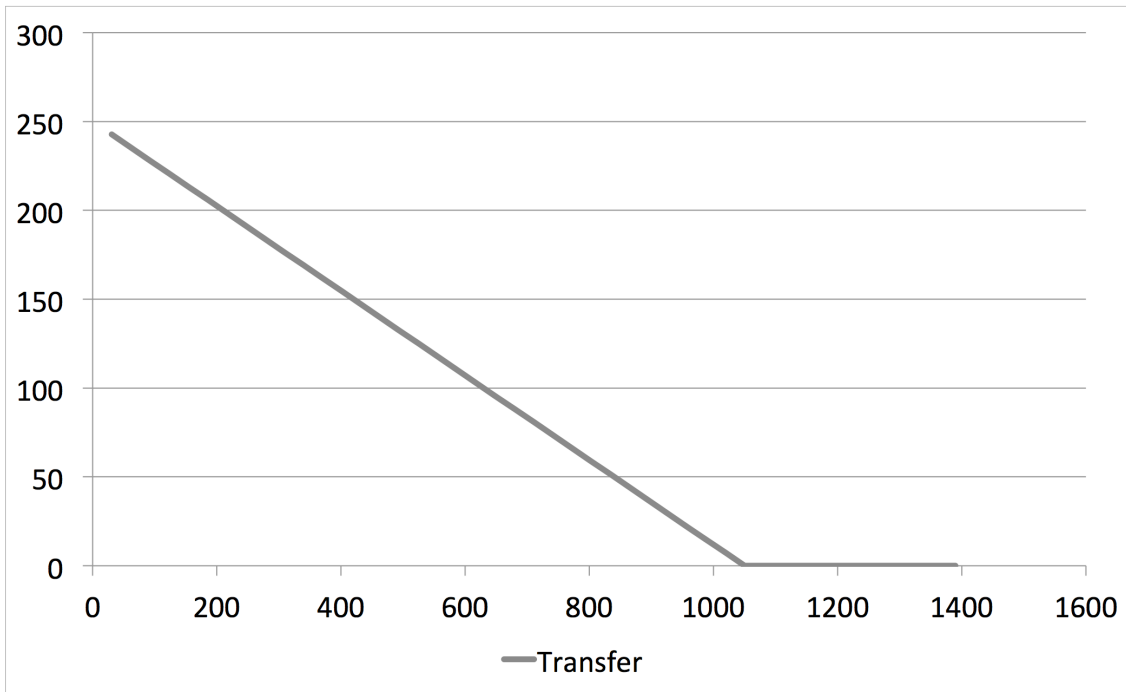
Figure 1: Cash transfer schemes

Scheduled month by month transfer



(a)

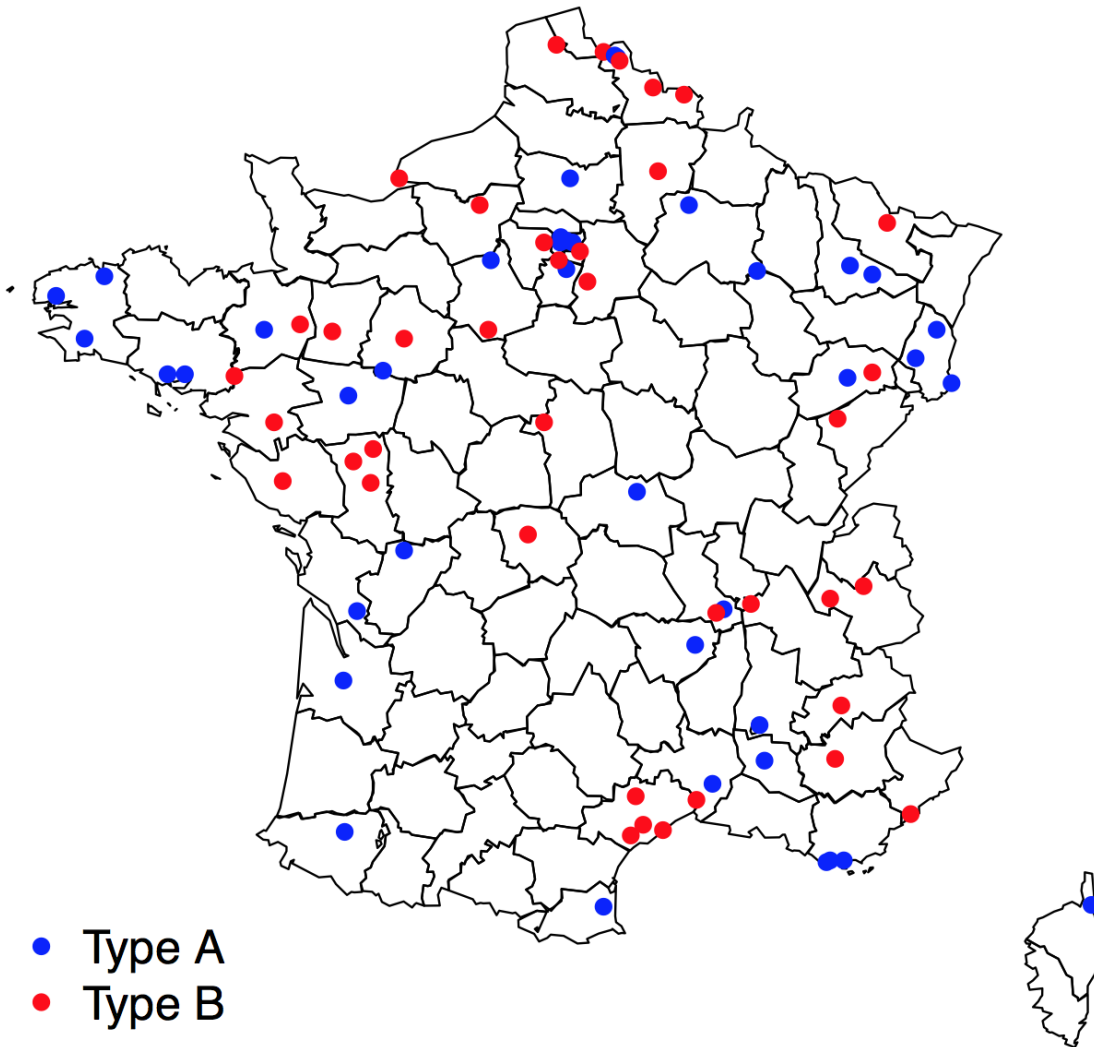
Transfer as a function of incomes from activity



(b)

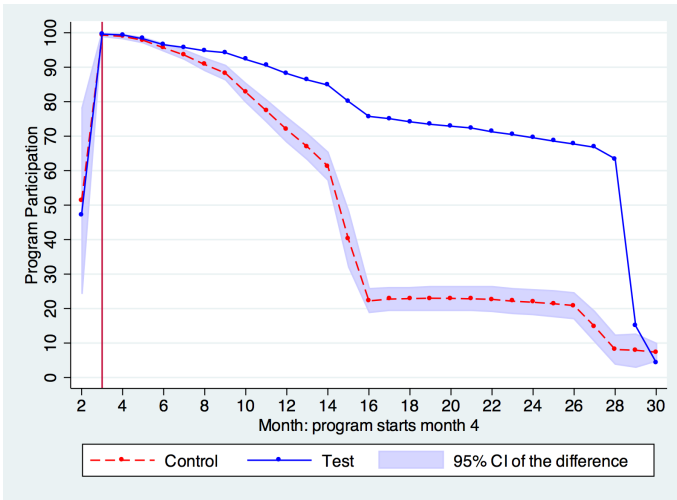
The graph on the upper panel presents the pattern of maximum possible transfers related to the transfer program. The graph on the lower panel presents actual transfers as a function of income from activity. These incomes include wages, unemployment benefits, and internship and training allowances. The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

Figure 2: JYC Map

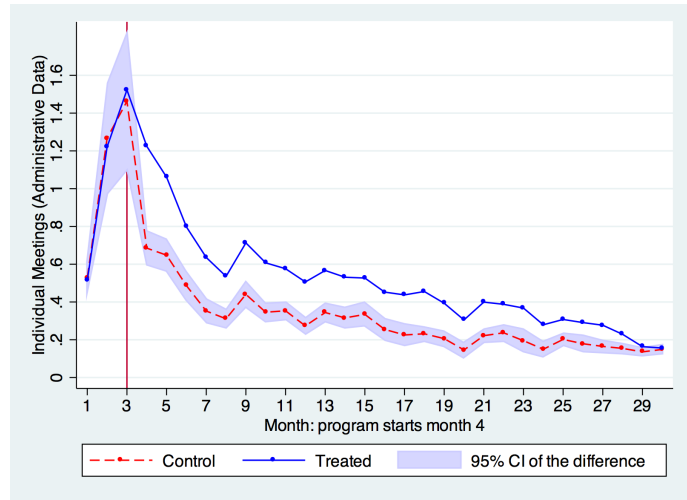


Map indicating JYC participating in the experiment. Blue dots identify type A JYC where youth registered in March were assigned to the Cash program and youth registered in February to the control group. Red dots identify type B JYC where youth registered in February were assigned to the transfer program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

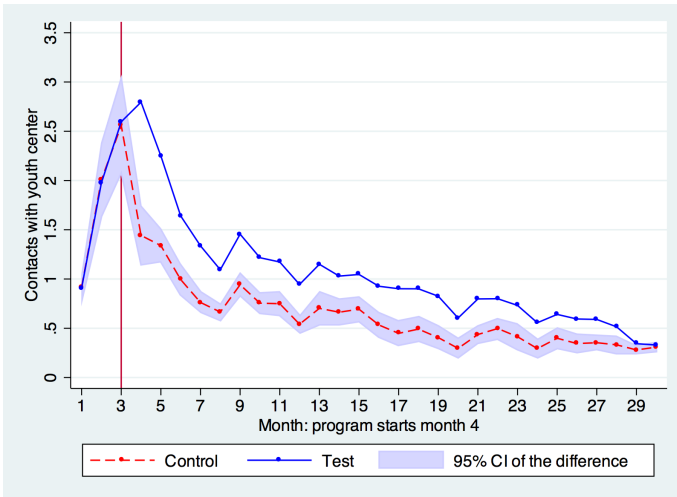
Figure 3: Participation in the program: dropout - meeting - contacts with JYC



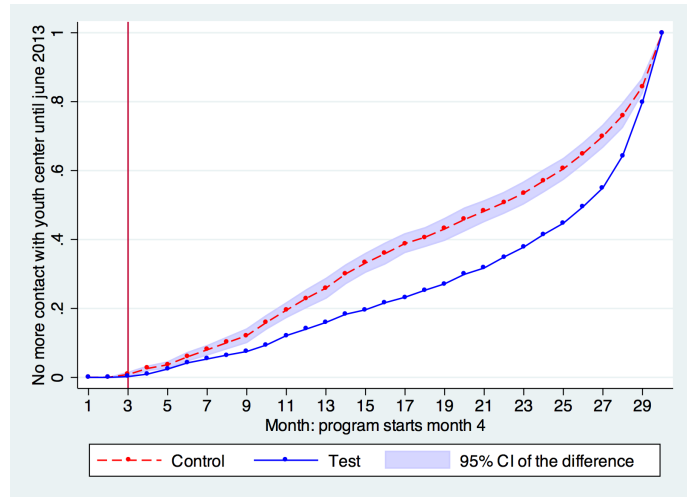
(a)



(b)



(c)



(d)

Administrative records.

Each graph presents 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 2 for the monthly variable considered. Actually the blue line is obtained by adding the mean in the control group (reported on the red line) to the estimated treatment parameter.

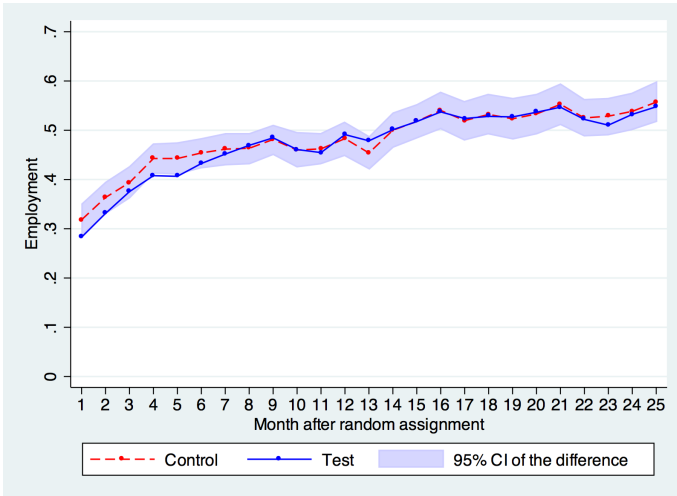
(a) : Month by month variable indicating whether the youth is still officially registered in either the career program or the transfer program

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

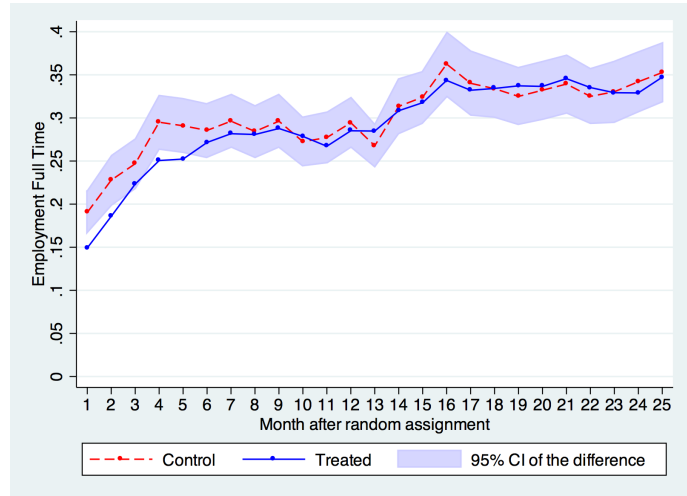
(c) : Month by month number of registered exchanges with a caseworker at the JYC

(d) : Share of youth that will have no more contact with the JYC after the date considered up to June 2013

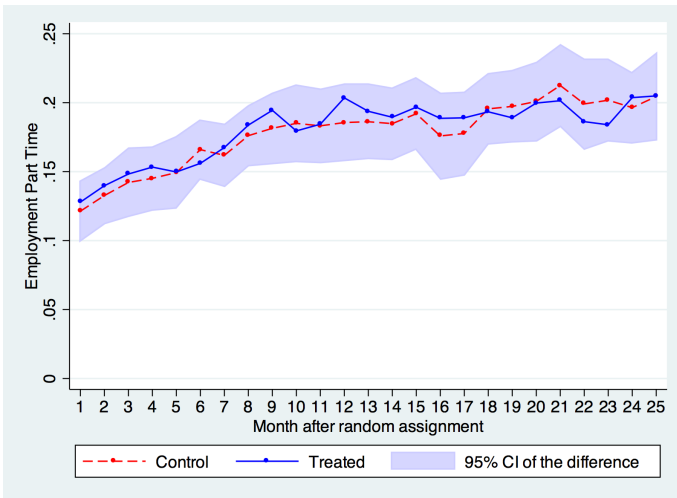
Figure 4: Month by month Employment



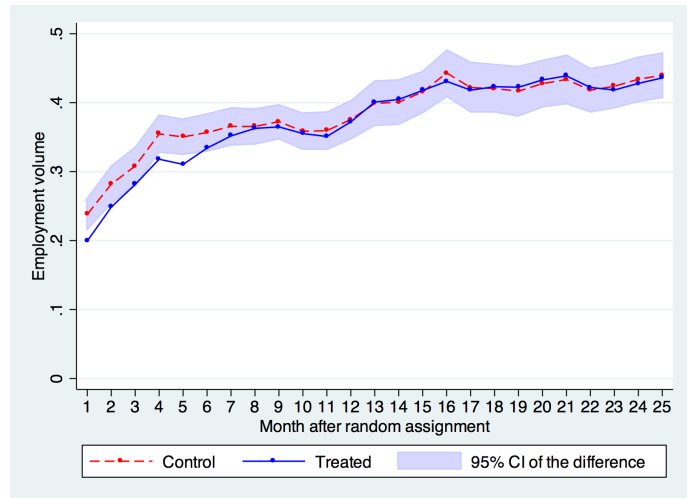
(a)



(b)



(c)



(d)

Midline survey April 2012 and endline survey April 2013

The graph presents the monthly profiles of employment for youth in the two assignment groups, following the procedure described in Figure 3. Information used for month 1 (April 2011) to 13 (April 2012) comes from the retrospective calendar in the midline survey. Information used for month 14 (May 2012) to 25 (April 2013) also comes from the endline survey

(a) : 1 if in employment at least once during the month

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

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

(d) : Employment index 1 if in employment with a full-time contract for the whole month; 2/3 if in employment with a part-time contract for the whole month or had at least one full-time contract but not for the whole month; 1/3 if had at least on part-time contract but not for the whole month and no full-time employment during the month.

A Appendix Tables and Figures

Table A1: Balancing: Whole sample and respondent to midline and endline surveys

	Whole sample			Respondents to survey					
	Cont	Coef	Sign.	Midline			Endline		
				Cont	Coef	Sign.	Cont	Coef	Sign.
Demographics									
Aged 18	23.2	-2.1	.	21.8	-1.4	.	21.3	-1.3	.
Aged 19	22.8	1.4	.	22.3	2.8	*	22.5	2.8	.
Aged 20	22.7	0.2	.	24.2	-1.9	.	24.8	-2.7	.
Aged 21	18.6	0.3	.	18.9	0.0	.	18.0	1.9	.
Aged 22	12.7	0.2	.	12.7	0.5	.	13.3	-0.7	.
Male	48.2	0.4	.	45.8	1.3	.	46.2	0.8	.
Foreigner	4.6	0.2	.	4.3	0.0	.	3.7	1.2	.
Non married	92.2	-0.5	.	92.1	0.1	.	93.6	-0.4	.
Has children	4.0	1.4	**	3.3	2.4	***	3.2	1.1	.
Diploma									
Driver's license	30.5	-1.8	.	34.3	-3.6	**	34.8	-2.8	.
Above high-school	2.4	-0.3	.	2.9	-0.9	*	3.2	-1.3	*
High-school diploma and eq	29.5	-0.6	.	34.1	-1.3	.	37.1	-1.3	.
Vocational	26.4	0.6	.	28.1	-1.0	.	28.2	-1.4	.
Dropout vocational high-school	34.1	-0.2	.	29.5	1.8	.	25.8	3.4	.
Left school at 16	7.6	0.6	.	5.4	1.3	.	5.6	0.5	.
Housing and resources									
Parents	62.2	-1.5	.	65.6	-0.7	.	68.6	-1.6	.
Other family	9.9	0.6	.	9.3	0.2	.	8.8	-0.7	.
Self	15.7	0.7	.	14.9	0.6	.	13.2	1.7	.
Friends	5.6	-0.4	.	4.8	-0.6	.	4.3	-0.6	.
Precarious	3.4	0.4	.	2.4	0.3	.	1.7	0.7	.
Has resources	16.0	0.9	.	16.2	0.0	.	15.6	0.5	.
Amount	74.4	1.8	.	78.7	-7.3	.	75.4	3.0	.
Medical insurance	43.0	-1.4	.	44.7	-2.2	.	46.6	-1.7	.
Type of program and reasons for joining the JYC									
Enhanced program	42.3	0.9	.	35.6	3.1	.	32.1	3.8	.
Administration	10.8	0.8	.	9.7	1.0	.	9.9	0.2	.
PES	27.5	0.3	.	28.7	1.2	.	29.8	0.6	.
Relatives	31.8	-1.0	.	32.7	-2.4	.	31.8	-0.8	.
Self	17.0	0.0	.	16.3	0.3	.	16.8	-0.7	.
Relationship with JYC, employment and training 1st quarter 2011									
# Contacts	5.5	-0.0	.	5.5	0.1	.	5.4	0.1	.
# Meetings	3.3	0.0	.	3.3	0.1	.	3.3	0.1	.
# days in employment	6.7	0.1	.	7.0	0.2	.	7.4	0.6	.
# days in training	6.4	1.0	.	6.5	0.8	.	6.5	-0.1	.
Started job	11.2	-0.1	.	11.7	-0.7	.	11.3	1.2	.

Continued on next page...

... table A1 continued

	Cont	Coef	Sign.	Cont	Coef	Sign.	Cont	Coef	Sign.
In employment	14.5	0.3	.	14.9	0.1	.	15.5	1.8	.
In FT employment	10.6	0.0	.	11.4	-0.7	.	11.2	1.5	.
In ITC employment	1.7	-0.2	.	1.2	0.6	.	1.5	0.0	.
In subsidized employment	3.0	0.4	.	3.3	0.2	.	3.8	0.2	.
Started training	8.7	2.0	*	8.6	2.4	*	8.8	1.5	.
In training	13.4	2.1	.	13.4	2.0	.	12.8	1.8	.
Started apprenticeship	0.3	-0.1	.	0.2	-0.1	.	0.2	0.0	.
In apprenticeship	1.0	-0.0	.	0.7	0.1	.	0.5	0.2	.
Started internship	4.9	-0.5	.	5.1	-0.6	.	5.0	-0.0	.
In internship	5.0	-0.5	.	5.2	-0.6	.	5.0	-0.0	.
In School	0.7	0.3	.	0.8	0.3	.	0.6	0.5	.
Global Test ($\chi^2(40)$ under H_0)	5492	68.3	.	3413	12.3	.	2310	79.2	.
Sign Transfer Program	0.0	81.9	***	0.1	85.0	***	0.0	86.5	***

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 test result. At the bottom of the table also appears the p-value for the joint significant test and the result of the proportion of youth assigned to test group signing for the cash program. The first set of column considers the whole sample, the second set respondents to the midline survey and the last one respondents to the endline survey. Standard errors are robust to heteroskedasticity and are clustered at the Job Youth Center level. * corresponds to parameter significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A2: Robustness: ITT impact on administrative variables estimated on various selected samples

	Whole Sample				Midline Respondents				Endline Respondents				Test
	Mean	coef	std	sign	Mean	coef	std	sign	Mean	coef	std	sign	p-value
Outcome variables computed over the first quarter													
Number of action from JYC													
# meetings	1.82	1.26	0.11	***	1.86	1.34	0.12	***	1.91	1.35	0.12	***	14.4
Any type	5.22	2.84	0.35	***	5.30	2.89	0.38	***	5.43	3.01	0.46	***	44.7
Job	3.03	1.64	0.29	***	3.11	1.61	0.30	***	3.29	1.57	0.36	***	95.4
Training	0.89	0.37	0.09	***	0.94	0.35	0.10	***	0.96	0.41	0.12	***	54.4
# of matching initiated by JYC													
Job offer	0.47	0.13	0.05	**	0.48	0.11	0.05	*	0.49	0.13	0.07	*	97.7
Training	0.15	0.05	0.02	***	0.15	0.04	0.02	**	0.16	0.03	0.02	.	84.3
# of action started													
Emp P3	0.70	0.04	0.03	.	0.74	0.05	0.04	.	0.74	0.09	0.05	*	44.7
Training	0.50	0.02	0.02	.	0.50	0.02	0.03	.	0.50	0.02	0.05	.	63.6
Outcome variables computed over the first semester													
Number of action from JYC													
# meetings	2.92	2.04	0.17	***	3.05	2.16	0.18	***	3.11	2.16	0.19	***	10.1
Any type	8.12	4.48	0.48	***	8.42	4.56	0.51	***	8.56	4.68	0.59	***	39.0
Job	4.62	2.51	0.37	***	4.85	2.44	0.39	***	5.00	2.46	0.47	***	88.9
Training	1.51	0.71	0.14	***	1.63	0.71	0.15	***	1.70	0.71	0.18	***	37.9
# of matching initiated by JYC													
Job offer	0.72	0.23	0.06	***	0.76	0.20	0.07	***	0.77	0.22	0.09	**	97.1
Training	0.29	0.11	0.03	***	0.30	0.10	0.03	***	0.33	0.06	0.04	*	28.3
# of action started													
Emp P3	1.52	0.07	0.06	.	1.61	0.08	0.07	.	1.67	0.10	0.09	.	99.4
Training	0.83	0.03	0.05	.	0.86	0.04	0.06	.	0.88	-0.00	0.08	.	68.1
Outcome variables computed over the first year													
Number of action from JYC													
# meetings	5.82	3.15	0.24	***	6.09	3.37	0.27	***	6.17	3.24	0.27	***	1.1
Any type	13.48	7.15	0.70	***	14.27	7.28	0.73	***	14.32	7.20	0.73	***	36.8
Job	7.45	4.15	0.56	***	7.90	4.25	0.61	***	7.96	4.18	0.63	***	87.9
Training	2.64	1.16	0.22	***	2.90	1.10	0.25	***	2.98	1.07	0.27	***	22.1
# of matching initiated by JYC													
Job offer	1.09	0.46	0.11	***	1.16	0.45	0.12	***	1.13	0.49	0.14	***	81.7
Training	0.53	0.16	0.04	***	0.57	0.13	0.04	***	0.58	0.10	0.05	*	8.4
# of actions started													
Emp P3	2.77	0.18	0.09	**	2.97	0.23	0.12	*	3.11	0.21	0.17	.	79.9
Training	0.83	0.03	0.05	.	0.86	0.04	0.06	.	0.88	-0.00	0.08	.	50.1
Nb obs	5492				3413				2310				

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

The table provides Intention To Treat estimates on administrative variables. The table has three panels and a last column. Each panel provides ITT results obtained in the same way as described in table 7. The first panel provides results obtained on the whole sample registered in the study. The second panel provides results obtained for the subsample of midline respondents and the last panel results for endline respondents. The last column is related to the ITT estimation of an equation extending equation 2 to include a variable corresponding to individuals responding both surveys, individuals responding only to the midline survey and individuals only responding only to the endline survey as well as their interactions with the treatment variable. The results reported in the last column provides the p-value of the F-test corresponding to the joint nullity of those interacted variables $\chi^2(3)$.

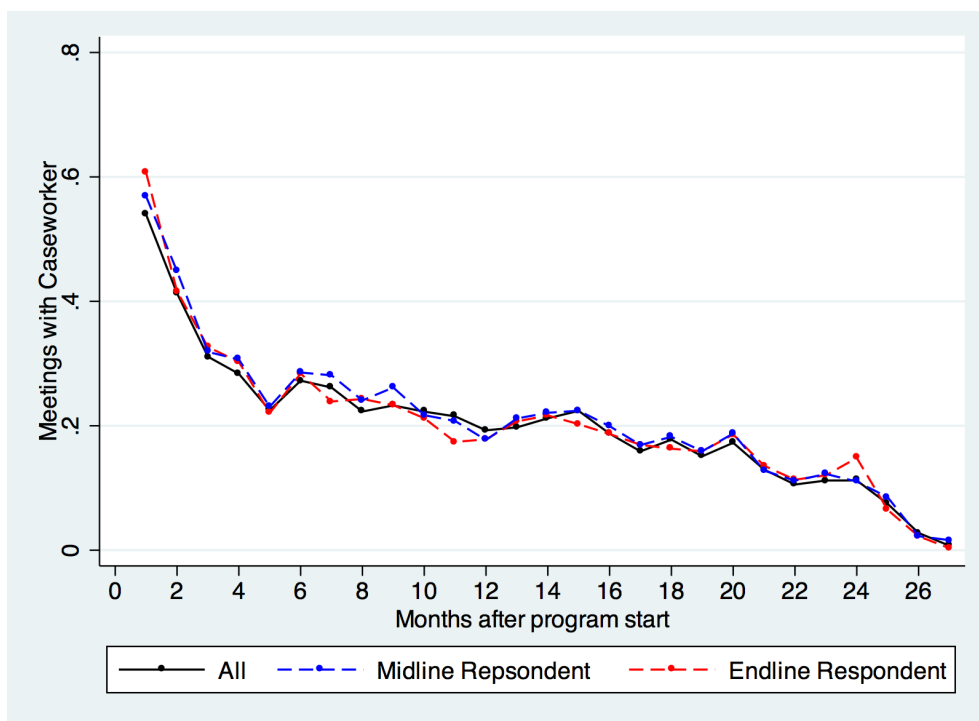
Table A3: Main survey variables - results under various estimation methods

	Actual		Without control		Same Resp. rate		Lee bounds			
	Coef	std	Coef	std	Coef	std	Lower		Upper	
	Coef	std	Coef	std	Coef	std	Coef	std	Coef	std
Employment										
Employment index	-23.7	10.8	-36.3	11.6	-26.1	11.0	-93.8	17.4	13.5	15.6
Investment										
Human Capital	1.9	3.5	3.1	3.8	1.5	3.6	-19.3	4.5	18.1	4.4
Job seeking	-2.6	3.3	-2.0	3.2	-1.8	3.5	-22.6	5.0	4.9	3.9
Income										
All	40.3	15.8	35.1	17.0	41.2	16.5	-54.4	22.5	100.8	21.6
From work	-22.0	15.6	-36.5	16.5	-24.5	16.3	-127.9	24.3	14.4	20.1
Savings	36.8	17.3	27.0	18.0	41.6	18.2	-92.8	20.7	49.9	16.8

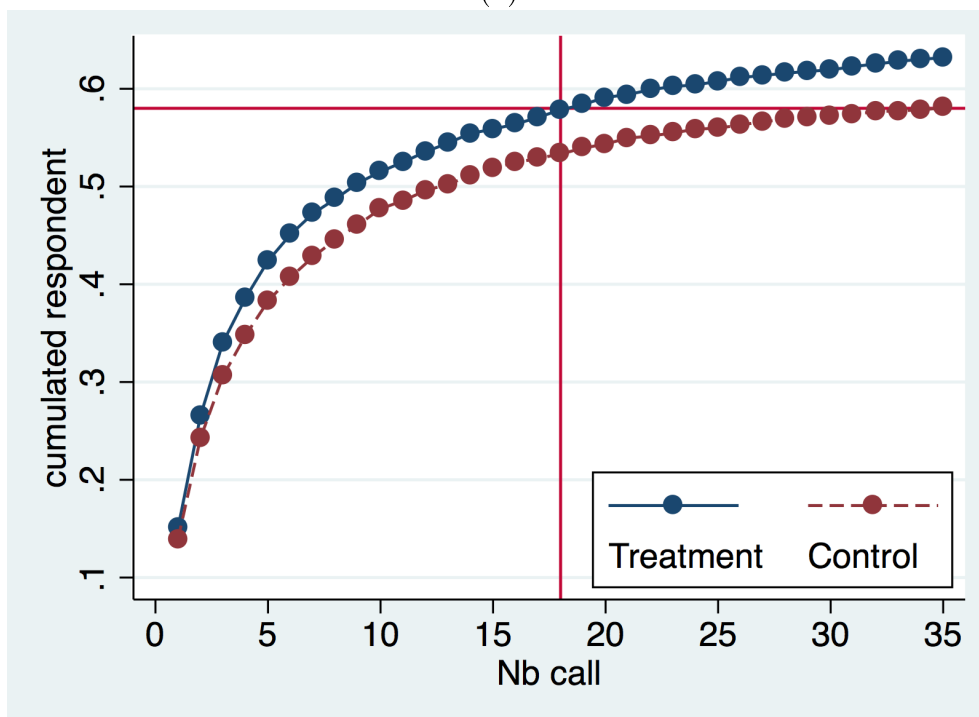
Midline survey April 2012

The table provides various estimates of ITT parameters. The first panel provide actual results obtained following the procedure described in table 7. The second panel provides results obtained by removing control variables (but keeping JYC dummy variables). The third panel provides results obtained by removing individuals reached in the treatment group after more than 18 calls. Eliminating these “most difficult to reach” individuals in the treatment group leads to identical response rates in treatment and control groups (see figure A1 (b)). The last panel provides 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 A3 is obtained by selecting in the test group individuals answering after a number of attempts lower or equal to 18.

B Appendix Heterogeneity Tables

Table B1: Heterogeneity – Financial constraints

	Obs	Mean	Probability of financial constraint							
			Low			High				
			Coef	std	sign	Mean	Coef	std	sign	sign
Midline variables										
Employment volume	3413	4.41	-0.19	0.15	.	3.75	-0.29	0.15	*	.
Human Capital index	3413	4.16	-2.92	4.21	.	-4.42	6.78	5.27	.	.
Search index	3413	-4.21	-2.91	4.62	.	4.47	-2.26	4.37	.	.
Income	3413	604	38	21	*	600	42	22	*	.
Amount saved	3299	272	22	25	.	146	52	23	**	.
Financial constraints index	3413	-23.45	4.20	4.47	.	24.92	-9.88	5.91	*	**
Endline variables										
Employment volume	2310	5.37	0.02	0.19	.	4.67	-0.02	0.21	.	.
Administrative variables										
Months in program	5486	11.56	8.01	0.34	***	11.15	7.76	0.41	***	.
Total number of meetings	5492	7.06	5.28	0.47	***	7.53	5.37	0.53	***	.
Training over first quarter										
Proposals	5492	0.86	0.33	0.09	***	0.93	0.39	0.11	***	.
Matching	5492	0.14	0.05	0.02	**	0.16	0.05	0.02	**	.
Start	5492	0.46	0.03	0.04	.	0.53	0.00	0.03	.	.

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

The table presents the results of the estimation of equation 4. Standard errors are clustered at the JYC level. The left part of the table presents the results for young adults who are least likely to perceive financial constraints and the right part the results for those who are most likely to 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 the the estimated standard error as well as the result of the test of a null effect. The variables we consider are mainly from the midline survey (employment, training and search indexes, income, savings and perceives constraints) although we also consider the employment index for the endline survey. We also consider some administrative variables corresponding to proposals, matching and training courses started as well as the number of months registered in the program and the number os meetings.

Table B2: Heterogeneity – Gender

	Obs	Mean	Female			Male				
			Coef	std	sign	Mean	Coef	std	sign	sign
Midline variables										
Employment volume	3413	3.71	-0.18	0.15	.	4.53	-0.31	0.16	*	.
Human Capital index	3413	-4.41	-0.64	4.87	.	5.21	4.72	5.30	.	.
Search index	3413	2.55	-4.48	4.93	.	-3.01	-0.49	4.52	.	.
Income	3413	568	46	20	**	642	34	25	.	.
Amount saved	3299	195	35	20	*	230	39	28	.	.
Financial constraints index	3413	3.33	-2.49	4.67	.	-3.94	-3.16	5.92	.	.
Endline variables										
Employment volume	2310	4.56	0.10	0.22	.	5.62	-0.10	0.25	.	.
Administrative variables										
Months in program	5486	11.61	7.63	0.32	***	11.07	8.15	0.43	***	.
Total number of meetings	5492	7.51	5.26	0.49	***	7.08	5.40	0.54	***	.
Training over first quarter										
Proposals	5492	0.93	0.40	0.11	***	0.85	0.33	0.10	***	.
Matching	5492	0.16	0.05	0.02	**	0.15	0.05	0.02	**	.
Start	5492	0.52	0.01	0.04	.	0.47	0.03	0.03	.	.

Administrative records, midline and endline survey, April 2012 and April 2013.
see note in table B1

Table B3: Heterogeneity – Disconnect from labor market

	Obs	Mean	Low			High				
			Coef	std	sign	Mean	Coef	std	sign	sign
Midline variables										
Employment volume	3413	4.56	-0.25	0.13	*	3.24	-0.21	0.19	.	.
Human Capital index	3413	6.55	-5.61	4.02	.	-11.84	14.30	5.97	**	***
Search index	3413	-3.90	-1.28	3.84	.	7.05	-4.83	5.87	.	.
Income	3413	646	43	18	**	521	36	26	.	.
Amount saved	3299	254	34	22	.	133	41	25	.	.
Financial constraints index	3413	-9.89	-1.14	4.03	.	17.87	-5.56	6.12	.	.
Endline variables										
Employment volume	2310	5.54	-0.05	0.18	.	4.03	0.11	0.30	.	.
Administrative variables										
Months in program	5486	11.15	8.22	0.35	***	11.63	7.43	0.42	***	**
Total number of meetings	5492	6.82	5.54	0.49	***	7.96	5.06	0.51	***	.
Training over first quarter										
Proposals	5492	0.83	0.32	0.09	***	0.98	0.43	0.12	***	.
Matching	5492	0.13	0.04	0.02	**	0.18	0.07	0.03	***	.
Start	5492	0.42	0.01	0.03	.	0.61	0.02	0.03	.	.

Administrative records, midline and endline survey, April 2012 and April 2013.
see not of table B1

C Appendix Baseline Table and Figure

This appendix will be published online

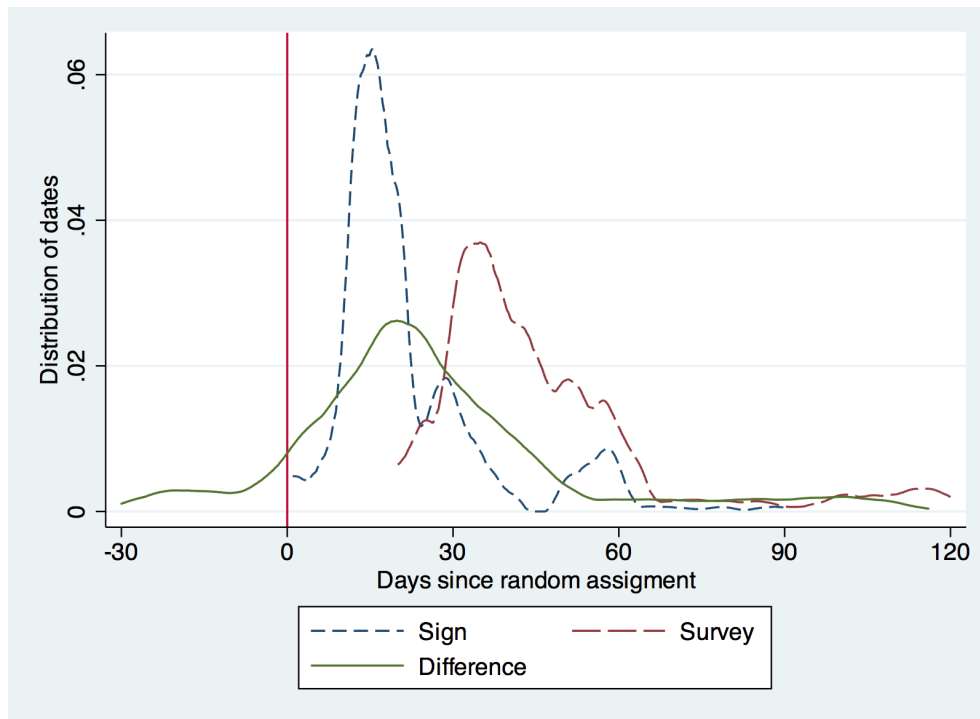
This appendix presents the results of balance checks between treatment and control samples using variables collected in the baseline survey instead. Results appear in Table C1. A quick review of the table shows that balancing properties are not as successful as could be expected. Differences exist for several variables which are consistently modest but often significant. We looked at approximately one hundred variables. For about twenty of these, differences were significant at the 10 % level. This is far more than expected. Overall, we found that treatment group subjects came from less privileged backgrounds and had fewer resources. Even if their experience on the job market is comparable to that of the control group, they were also found to spend less time in work and more time looking for work at the time of the survey. Comparing the two samples reveals a stark contrast with results obtained using the administrative records, which show a well-balanced division of the sample for variables where differences were noted which are identified here, for example in terms of housing.

This survey presented a significant logistical problem: it should have been conducted gradually, as young job seekers enrolled in the standard program, but actually only began after randomization. Given the time needed to reach participants, the survey was often conducted after the counselor offered treatment group subjects the possibility of converting their standard contract into an experimental one. Figure C1 shows the distribution of dates on which surveys were conducted and new contracts signed. Contracts were converted either approximately 15 days after randomization, or approximately one month after, and in the case of a last group, approximately two months after randomization. A peak in survey completion was observed a little more than a month after randomization and continued over a two-month period. When we look at the distribution of the lags between dates on which surveys were conducted and on which contracts were signed, most occur just after contracts are signed. This discrepancy may explain the existing imbalance in employment status at the time of the survey. For most subjects, this is their status in May or June 2011, one month after signing their contract. Therefore it is not surprising to observe such a discrepancy in the average responses in both groups. This is also consistent with midline survey results, when participants have a better understanding of the actual program.

Another important reason for this discrepancy is the existence of the RSA (Revenu de solidarité active) – the minimum guaranteed income in France. The name of this benefit is very similar to that of the experimental program – RCA, or (revenu contractualisé d'autonomie).

RSA is widely distributed in France: 2.3 million people received it in 2013. It is a means-tested benefit which is only paid if combined household income is less than 1.04 times the minimum wage, and eligibility is re-examined every three months. Given the similarities between the two benefits, treatment group responses may have been biased by respondents believing that their answers may affect their eligibility for the transfer. This may explain the discrepancy observed in reported income in March, as well as what respondents said about parents' employment status and living with their parents.

Figure C1: Surveys and Signature dates



Administrative records, baseline survey.

The graph presents the distribution of dates when new contracts were signed, the distribution of the dates the survey was conducted and the distribution of the difference of the two.

Table C1: Balancing properties based on baseline survey

Variable Names	# observations	Control	Difference	std	sign
<i>Baseline survey response rate</i>					
Response rate	5498	76.00	4.82	1.22	***
<i>General variables</i>					
Above High school	4310	27.55	-1.98	1.30	.

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... table C1 continued

Variable Names	# observations	Control	Difference	std	sign
Low level of education	4310	33.86	-0.37	1.52	.
No education	4310	20.36	1.19	1.12	.
Driving license	4310	38.45	-3.24	1.40	**
Couple	4310	80.24	0.17	1.33	.
Has children	4310	5.43	2.01	0.90	**
Smoker	4310	45.36	0.70	1.55	.
Father employed	4310	54.17	-2.35	1.73	.
Father unemployed	4310	10.67	0.84	1.05	.
Father out of LF	4310	9.60	0.55	0.96	.
Father ill	4310	6.12	-0.25	0.84	.
Father other	4310	19.43	1.21	1.25	.
Mother employed	4310	51.25	-5.94	1.62	***
Mother unemployed	4310	9.74	2.86	1.06	***
Mother out of LF	4310	26.81	1.79	1.52	.
Mother ill	4310	5.29	0.78	0.72	.
Mother other	4310	6.91	0.51	0.78	.
Housing parents	4310	66.51	-3.28	1.57	**
Housing family	4310	5.61	1.31	0.85	.
Housing other people	4310	6.12	1.16	0.76	.
Housing rent	4310	19.02	0.62	1.31	.
Housing precarious	4310	2.46	-0.18	0.40	.
Locus of control	4289	73.25	-1.25	0.66	*
Very low self esteem	4176	16.28	0.66	1.10	.
Low self esteem	4176	54.43	-2.91	1.51	*
Medium self esteem	4176	24.41	1.06	1.29	.
High self esteem	4176	4.88	1.19	0.81	.
<i>Variables defined over passed 12</i>					
Large expense herself paid for	4310	38.64	-0.99	1.61	.
Large expense parents paid for	4310	38.50	-0.91	1.61	.
Has Saved	4310	42.35	-4.47	1.62	***
Problem paying bill	4310	27.69	-0.12	1.42	.
Problem paying taxes	4310	7.79	-1.25	0.74	*
Problem paying rent	4310	18.04	-0.45	1.26	.
Problem bank withdrawal	4310	22.03	1.93	1.18	.
Banned from holding bank account	4310	4.36	1.22	0.66	*
Has health insurance	4310	77.37	-1.80	1.42	.
Spend day without a meal	4310	22.08	0.93	1.43	.
Has already worked	4310	71.29	0.80	1.39	.
Month of work experience	4310	891.00	-22.85	30.79	.
Registered at JYC for job	4310	61.92	-0.67	1.39	.
Registered at JYC for money	4310	9.88	2.16	0.95	**
Registered at JYC for training	4310	24.68	-0.77	1.23	.
<i>Variables defined for March 2011</i>					
Wage from job	4310	23.61	-4.01	1.40	***

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... table C1 continued

Variable Names	# observations	Control	Difference	std	sign
Wage internship	4310	4.31	-0.58	0.58	.
Wage from small job	4310	18.97	-2.58	1.09	**
Transfer from parents	4310	37.34	-3.81	1.46	**
Scholarship	4310	1.86	-1.02	0.34	***
Transfer from PES	4310	16.88	0.67	1.33	.
Transfer from JYC	4310	12.94	1.35	2.38	.
March total income	4304	336.54	-52.80	11.65	***
<i>Variables defined over passed mo</i>					
Intensity of internet use	4278	70.50	-1.80	0.99	*
Intensity of meetings with frien	4278	68.52	-0.41	1.05	.
Intensity of sport practice	4277	49.99	-0.42	1.30	.
Search using motivation letter	4310	49.12	-0.19	1.78	.
Search using Q46 CV	4310	56.73	1.29	1.81	.
Search using spontaneous applica	4310	34.69	2.47	1.25	*
Search using web channel	4310	33.72	-1.87	1.39	.
Search using news paper job offe	4310	11.46	-0.87	0.97	.
Search using registration on web	4310	31.22	0.05	1.37	.
Search using temporary help agen	4310	34.79	-0.19	1.37	.
Give money	4310	30.43	0.73	1.45	.
Help with money	4310	15.12	0.50	1.19	.
Restaurant Last Month	4310	15.21	0.85	1.04	.
Pay restaurant Last Month	4310	46.10	-0.41	1.53	.
Night out Last Month	4310	12.94	0.10	1.15	.
Pay night out Last Month	4310	41.70	-1.55	1.55	.
Owes money to bank	4310	6.73	1.28	0.81	.
Owes money to individuals	4310	14.70	1.31	1.35	.
Owes money to store	4310	3.57	0.24	0.65	.
<i>Variables defined currently</i>					
Know Job	4310	49.95	2.33	1.63	.
Has idea about job	4310	38.17	-2.45	1.47	*
No idea about job	4310	11.87	0.13	1.05	.
Need training	4310	64.47	0.00	1.41	.
Need diploma	4310	55.15	1.31	1.38	.
Need experience	4310	68.60	0.13	1.33	.
Plan to start training	4310	64.15	0.78	1.53	.
Intent to move	4310	27.27	1.77	1.54	.
Mobility foot	4310	8.86	-0.14	0.72	.
Mobility byke	4310	2.55	0.85	0.62	.
Mobility public transport	4310	42.58	2.73	1.49	*
Mobility scooter	4310	7.42	-0.09	0.75	.
Mobility parents	4310	2.60	0.58	0.57	.
Mobility car	4310	35.44	-4.04	1.69	**
Employed	4310	31.45	-3.81	1.38	***
Search for job	4310	73.65	3.40	1.35	**

Continued on next page...

... table C1 continued

Variable Names	# observations	Control	Difference	std	sign
Take a job at 30mn	4310	65.17	1.53	1.48	.
Take a job at 45mn	4310	46.66	1.11	1.47	.
Take a job at 60mn	4310	26.81	1.67	1.24	.
Move for a job	4310	39.94	4.11	1.51	***
Take 100 immediately	4310	27.18	0.37	1.29	.
Number of days ready to wait for	4250	1.75	-0.17	0.13	.
Satisfied with his life	4295	65.46	-1.08	0.74	.

Administrative records and baseline survey.

The table presents balancing properties of the sample of respondents to the baseline survey. The control mean variable appears first, then the difference between treatment and control resulting from the estimation of equation (1) and lastly the test result. Outcome variables are divided into several panels. First appears demographic information. Then we consider variables corresponding to past behavior over a long period. It is followed by information about income in March, a month before randomization. Then appear results for variables corresponding to a type of behavior over one month before the survey. The last panel considers outcome variables measured at the moment the survey was conducted.