What Do NEETs Need?
The Effect of Combining Activation Policies and Cash Transfers

Francesco Filippucci*

December 2021

Job Market Paper
Latest version available here

Abstract

Activation policies and cash transfers are often used jointly, but the literature has only evaluated them one conditional on the other. This paper evaluates an innovative French program that provided a year of cash transfers and intensive activation measures to disadvantaged youth Not in Employment Education or Training (NEETs). I develop a difference-in-difference methodology that extends De Chaisemartin and D'Haultfœuille (2020a) to a setting where rolling over a third dimension is needed. While no significant effect was found when participants are enrolled, after completion of the program compliers reported an increase of 33 percentage points in the probability of employment and of 72 hours worked on a quarterly basis. No effect was detected on wages. I investigate the mechanisms using the timing of activation measures, the phase-out of the cash transfer, and a framework with discrete labor supply and search frictions. I find that the zero effect during enrollment arises from a negative reaction to implicit taxation from transfer phase-out, lock-in from training, and a counterbalancing positive effect of activation. This finding suggests potential complementarities between cash transfers and activation measures. Moreover, it shows that disadvantaged NEETs have low job finding rates at baseline, large elasticity of labor supply, and significant time constraints.

Keywords: active labor market policies, cash transfers, NEETs, job search, difference-in-difference

JEL Codes: J64, J68, C23

*Paris School of Economics and EHESS. francesco.filippucci@psemail.eu

I am particularly grateful to Marc Gurgand for continuous advice which was crucial for the development of this paper, and to Philippe Zamora for the support during the early phase of the work. I also thank Luc Behaghel, David Card, Veronica Escudero, Francois Fontaine, Xavier D'Haultfoeuille, Hilary Hoynes, Eric Maurin, Aprajit Mahajan, Andrea Merlo, Benjamin Nefussi, Paolo Pinotti, Jesse Rothstein, Emmanuel Saez, and Chris Walters for useful help and suggestions. Finally, I thank all the participants at *Chaire Travail* seminars, seminars at UC Berkeley and at the Paris School of Economics, EEA-ESEM Congress, ADRES Doctoral conference, and EALE Conference for their constructive comments. This research has been possible thanks to technical support by the French Ministry of Labor and Social Affairs (*DARES*). The author acknowledge the financial support of the Norface Dynamics of Inequality Across the Life-course (DIAL) Joint Research Programme (file number 462-16-090), "Human Capital and Inequality During Adolescence and Working Life".

1 Introduction

Youths who are neither in employment, education or training (NEETs) are a persisting problem in Europe. NEET rates in the last decade for youths aged 15-24 ranged between 12% and 22% in countries such as Spain or Italy, and were persistently above 10% in others, such as France. Higher levels were reported for women, less educated persons and foreign-born individuals. Economists have long wondered about the possible causes. Given that disadvantaged youths are more likely to become NEETs (Carcillo and Königs, 2015), some have posited that NEETs face significantly higher job search frictions, lacking networks and soft-skills[1]. NEET spells can thus become a poverty trap. In fact, unemployment has proven to be "scarring", in the sense that it can permanently harm one's employability (Oreopoulos et al., 2012; Schwandt and Von Wachter, 2019; Rothstein, 2019) as much as does prematurely dropping out of formal education (Brunello and De Paola, 2014).

Can combining cash transfers and activation policies break this vicious cycle of under-employment? "Passive" policies such as cash transfers risk decreasing individuals' labor supply and search effort (Moffitt, 1985). This could happen through pure moral hazard or liquidity effects (Card et al., 2007; Chetty, 2008), but also through distortionary implicit taxation generated by benefit reduction with job earnings (Le Barbanchon, 2020). A possible solution is to couple passive with active labor market policies, a policy mix which is often advocated by international institutions and more and more common across states (OECD, 2013; Pignatti and Van Belle, 2018). The literature has mostly evaluated the effect of activation measures (i.e. training, job search assistance, and subsidized employment) conditional on cash transfers, and vice versa. Activating receivers of social protection has been found to improve employability in the medium term (Card et al., 2018). Offering cash transfers in addition to activation measures might finance the opportunity cost of participation in the program (Heckman et al., 1999), although this doesn't always translate to more effort and more successful job search (Aeberhardt et al., 2020). Yet, to the best of my knowledge, a combined increase in cash transfers and activation has never been considered in the literature to date. There are reasons to believe that complementarities might arise when cash transfers and activation are jointly provided if it were the case, for example, that activation functions as a monitoring device (Boone et al., 2007), or if cash transfers relax time and credit constraints of youths participating to activation measures.

This paper fills this gap in the literature by evaluating an innovative French program, *Garantie Jeunes*, that targets disadvantaged NEETs between 16 and 25 years old. The program combines a year of cash transfers equivalent to the French minimum income (€485 per month in 2018) with intensive activation measures, i.e. soft-skills training for a month, regular counseling and short-term job experiences. For this purpose, I create a novel dataset by merging two administrative sources: the informational system of youth employment centers, and social security data about any employment spell of youths during 2013-2017. The results highlight that, while no significant effect is observed during enrollment in the program, a strong positive effect of *Garantie Jeunes* occurs after completion. In the second year of exposure to the program, employment is 1.6 points higher on average and youths work 4.3 hours more per quarter on average. The effect is driven by the share of youths who have completed the program, while no significant effect is detected when youths are still receiving cash transfers and activation. Finally, there is no significant change in wages. Because not all youths are eligible and participants are further selected, take-up rates are low (6% on average after a year that a cohort of youths could enroll in the program). Hence, intent-to-treat (ITT) effects translate into high

[1]The debate on the causes of higher NEET rates considered supply-side explanations such as education or training system, demand shocks such as trade or technical change generating skill-mismatch, or unintended consequences of policies such as the minimum wage (Quintini, 2011; Eichhorst et al., 2012; Cahuc et al., 2013).

local average treatment effects (LATEs) on takers: +26 points in the probability of employment (on a mean rate of 49%) and +71 in quarterly hours worked. It should be noted that the effect comes overwhelmingly from fixed-term contracts and agency jobs. Also, the program has considerable costs for public finances, almost as large as the benefits it generates, such that the Marginal Value of Public Funds (Hendren and Sprung-Keyser, 2020) is slightly above one (1.15).

To identify the effect of the program, I exploit its staggered adoption across different regions, between the last quarter of 2013 and the beginning of 2017. For estimation, I develop a new "rolling" diff-in-diff methodology, which extends De Chaisemartin and D'Haultfœuille (2020a) defining estimators separately for given levels of a third dimension. This necessity arises because in my setting individuals enter the population (i.e. they register at youth employment centers, YECs) in cohorts, and are subsequently exposed to treatment at different times since registration with YECs. Then, when their cohort becomes exposed to treatment, individuals can enroll or not in the program. This poses two challenges. First, only a particular selection of individuals can be willing to take-up treatment after longer time since registration with YECs, such that distinguishing between individuals treated at different times since registration becomes crucial. Second, my identifying event is treatment adoption, but the possible dynamic of the effects arises over individuals' enrollment into treatment. Because some cohorts can register at YECs after the treatment has been already adopted by the YEC, treatment effect since *adoption* can be a misleading proxy of the effect since actual *exposure* of a cohort (or since actual *enrollment* of an individual). Rolling over a third dimension – which in my case is time since registration with YECs – allows me to identify first stage and reduced form effects (ITT) since exposure, since every group-cohort-time since registration cell corresponds to a unique level of exposure. In a second step of my methodology, I regress cell-specific ITT effects on the share of youths at different stages of program enrollment in that specific cell,recovering LATEs since enrollment.

I disentangle the mechanisms behind my effects by exploiting the timing of the activation measures and the phase-out of the cash transfer. In fact, time-consuming activities such as intensive training and job immersions are concentrated in the first semester of *Garantie Jeunes*. In addition, the cash transfer is cumulative with job earnings only up to €300, while it decreases by 0.55 cents for every euro earned between €300 and about €1100. I estimate the LATE for youths at different stages of the program (first semester, second semester and after completion) on the probability of having job earnings below €300, between €300 and €1100, and above €1100. During the first semester of enrollment, when youths are involved in intensive training and receive the cash transfer, I find a decrease in the probability of having job earnings below €300 or between €300 and €1100. In the second semester, when youths are out of the training but continue receiving the transfer, the decrease is concentrated in jobs earning €300-1100, where transfers are only partially cumulative with job earnings.

I interpret this heterogeneity through the lens of a simple model of labor supply with discrete hours choice and search frictions. In the model, youths choose their optimal level of employment, including part-time, based on idiosyncratic skills and on if they are receiving the cash transfer (generating implicit taxation of job earnings) of *Garantie Jeunes*. Then, they face a probability of actually finding their optimal job that depends on their time availability to search and on if they received the activation policy. Hence, the model fully disentangles the effect of cash transfers (acting through labor supply) and activation measures (acting through the probability of finding the chosen job). I estimate that cash transfers reduce employment mostly due to implicit taxation, while the magnitude of the reduction in labor supply due to moral hazard/liquidity effect is much smaller. This is consistent with disadvantaged youths having a large elasticity of labor supply with respect to net-of-tax rate, but suggest that youths have limited scope for moral hazard. In addition, time constraints are shown to be a relevant problem for disadvantaged youth, as lock-in from training dents the probability of finding a job by 36%. Finally, the estimated parameters of the model imply that activation compensates these negative effects by doubling the probability of finding the chosen job thanks to improved search technology.

The main contribution of this work is to the literature on unemployment policies by offering evidence on the joint effect of active and passive labor market policies. Prior work in this field only evaluated one component conditional on the other. An example of a study of cash transfers to receivers of activation measures in developed countries is Aeberhardt et al. (2020), who experimentally evaluates a cash transfer of similar value and in the same context as that of this paper. The authors found a significant increase in attendance for compulsory counseling (not as intense as in *Garantie Jeunes*) but a non-significant effect on job-search and a small negative effect on employment. By contrast, an extensive literature evaluates programs that increase activation measures, but not cash support. In the French context, some working papers indicate a large positive effect of job search assistance in YECs (Crépon et al., 2013b) and of collective counseling sessions (van den Berg et al., 2015) to disadvantaged youth. Card et al. (2018) meta-analyzes the literature on active labor market policies in Europe and finds that programs with a work-first approach can have a positive impact 1-2 years after activation. The main estimates of this paper indicate a positive effect of activation measures when combined with cash transfers. Yet, the magnitude is large compared to the effect of activation programs in isolation (i.e. with no cash transfers involved) reviewed in Card et al. (2018)[2], and the effect is observed only after cash transfers terminate. Disentangling the role of cash transfers and activation with a simple framework shows that cash transfers affect labor supply through implicit taxation and increasing the relative utility of partial employment, compensated for by activation. This suggests potential complementarities between cash transfers and activation, either because activation functions as a monitoring device for conditional cash benefits (Boone et al., 2007), or because cash benefits finance effort and attendance at activities.

Secondly, the results provide empirical insights for the literature on labor supply and job search behavior of welfare recipients, focusing on a disadvantaged and young population. As is the literature that studies labor supply reaction to income tax (Le Barbanchon, 2020; Saez et al., 2012), I highlight significant effects of implicit taxation from the phase-out of the cash transfer. Back-of-the-envelope calculations indicate an elasticity of earnings to net-of-tax rate between .4 and .8, which is consistent with larger reactions observed in sub-populations less attached to work (Card and Hyslop, 2005). Conversely, the relative increase in utility arising from receiving the cash transfers when not working is estimated to be smaller, suggesting that the moral hazard/liquidity effect of cash transfers (Chetty, 2008) is limited in this case. Turning to activation measures, Gautier et al. (2018) suggest that they affect job search through search efficiency and time available for search. I empirically confirm the role of both aspects, finding that time-consuming activation measures generate lock-in and that activation increases search efficiency and job finding. An implication of the large observed effect of activation is that job finding is estimated low for untreated takers. This speaks to different streams of the literature that show how lack of networks, geographical isolation and low soft skills can dramatically limit job search efforts on the part of disadvantaged youth[3].

My final contribution is methodological. The rolling diff-in-diff approach I propose is generalizable to other settings where the researcher needs to apply estimators by De Chaisemartin and D'Haultfœuille (2020a) but rolling over a third dimension. An example can be schools undergoing a shock which differentially affects

[2]Our 50% increase in employment of takers is in the top 5% of the effects considered in the meta-analysis

[3]See Ioannides and Datcher Loury (2004); Pellizzari (2010); Dustmann et al. (2016); Cingano and Rosolia (2012); Kramarz and Skans (2014); Marinescu and Rathelot (2018); Mendolia and Walker (2014)

students in different grades, as in Martorell et al. (2016). Another instance is firms adopting policies which differentially affect workers of different tenures. In fact, in these situations, units enter the population (a school, a firm, ...) in cohorts and are subsequently exposed to treatment at different times since entering the population. If the true effect is heterogeneous according to the time elapsed between entrance in the population and exposure – an hypothesis which is hard to exclude in many applications – then the effect since adoption as in De Chaisemartin and D'Haultfœuille (2020a) can be a mix of the effect on units exposed at different times since entering the population. A second instance is if the treatment effect on the units (i.e. students, workers, ...) within treatment groups (schools, firms,...) is dynamic. Because units might enter the population (or enter treatment) after treatment adoption in their group, the effect since adoption will be a mix of different effects since exposure (or since enrollment), which is inevitably hard to interpret. Rolling over time since entering the population allows to flexibly estimate dynamic effects since exposure, and I propose a second-step regression to recover effects since enrollment.

The relevance of these results for policy is considerable. First, the paper offers a successful case of a labor market policy promoting employability of a vulnerable population. However, the gain is concentrated in very precarious jobs, and it concerns a population of takers which is selected based on motivation. These aspects caution about the external validity of my results. Second, the analysis supports the importance of providing active and passive labor market policies jointly. The effect of activation is shown to be strong enough to compensate for lock-in and distortive effects of the cash transfers, while part of it might be due to complementarities between cash transfers and activation measures. Third, given the negative effect I find of implicit taxation from the phase-out of cash transfers, policy makers might consider allowing cash transfers to be fully cumulated with job earnings. In turn, the jump in employment subsequent to the end of cash transfers highlights the importance of limited benefit duration. While bearing in mind the limits in terms of external validity of my results, the insights I find are interesting also for policies using different combinations of the same ingredients, such as minimum income schemes or unemployment insurance with activation requirements.

The paper is constructed as follows. Section 2 provides the relevant institutional background and describes the program. Section 3 describes the data and sample selection process, and outlines the main identification strategy. Section 4 presents the results in terms of ITT and LATE, their heterogeneity according to contract type and youths characteristics, and the cost-benefit analysis. Section 5 exploits differences in timing of the program and the cash transfer phase-out to disentangle the mechanisms, namely the effect of activation, lock-in, disincentives from cash transfers and implicit taxation. Section 6 discusses the results in comparison with related studies. Section 7 draws policy implications and conclusions.

2 Institutional Background

Garantie Jeunes was part of the European Union Youth Guarantee, which financed a number of national programs aimed at promoting youth employment, sharing the same name but having very different characteristics[4]. The French version of the program was launched in October 2013, co-financed by the French

[4]The concept of Youth Guarantee derives actually from a Nordic tradition of establishing a right to employment or training for youth entering the labor market. The EU channeled part of the European Social Fund (6.6 billions) toward financing nationally-defined implementation programs aiming at supporting employment of disadvantaged youth. There was quite some variability in focus and kind of the implementation programs at national level (Escudero and López, 2017; Escudero and Mourelo, 2018). Other counterfactual evaluations of country-specific initiatives have been produced (Bratti et al., 2017; Pastore

government, and targeted disadvantaged NEETs aged 16-25. The program lasts one year, and is renewable for 1-6 months in exceptional cases (2% of participants eventually renew). At acceptance, the participant is required to sign a contract of engagement, including penalties for not participating in the mandated activities. The early activation part consists of a six-week period of collective courses provided by 2 counselors, with 10-20 participants per class. The training is centered on job search and search frictions (*freins à l'emploi*) covering soft skills linked to job search (presentation skills, job search strategies, applications, CVs, motivation letters) but also on personal habits and self confidence (learn to be timely, manage your health, plan your week, ...). There follows a ten-month period of regular counseling, with a personal counselor and interviews held once every 21 days on average. This second part is characterized by a "work-first" approach, i.e. frequent proposals of internships and short work experiences of at most a month called "job immersions"[5], during which the youth works on small tasks in a partner firm with the aim of learning about the working environment and the industry. Meanwhile, youths receive a monthly cash transfer equal to the amount provided by the French minimum income scheme (RSA), which is annually updated. For example, it was €484.82 gross in April 2018. Importantly, the cash transfers do not decrease with employment income earned while enrolled in *Garantie Jeunes*, until €300. For earnings above €300, cash transfers decrease proportionally with earnings until they reach zero at 80% of the French gross monthly Minimum wage (i.e. between €1,143 and €1,174). Sanctions are possible if the engagement contract is not respected, including suspension from the program. 23% of participants quit the program in advance, almost all in the last quarter of enrollment. Among those who quit, roughly one-third quits because they found employment, one-third quit for exogenous reasons (age, relocation), and the remainder were expelled for not adhering to the terms of of the contract (3% of the total number of participants).

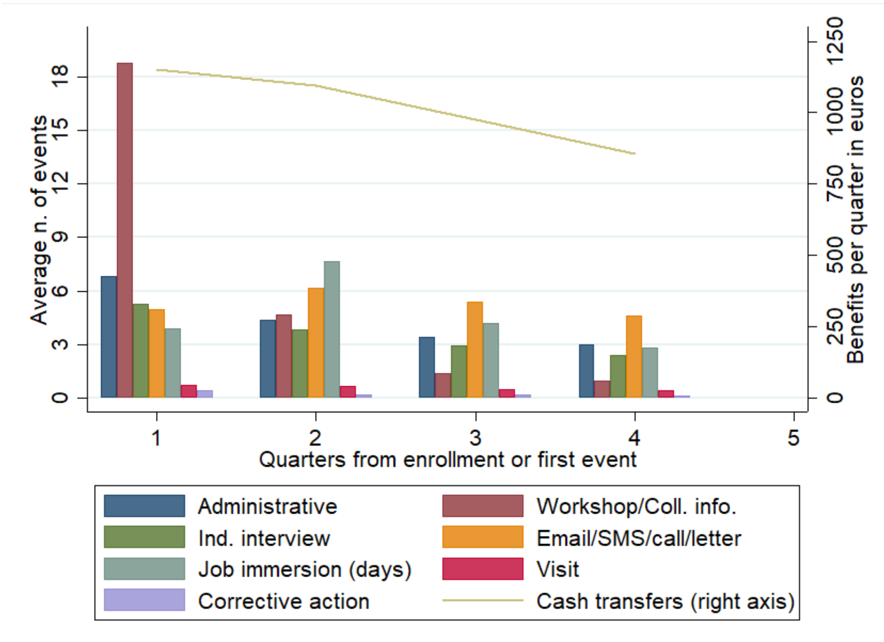
The combination of activation policies and generous cash transfers is considered quite innovative in the French landscape. The design of *Garantie Jeunes* was done in light of evidence from previous experimental programs and was supported by a working group of experts who wrote down guidelines for monitoring and implementation (Gurgand and Wargon, 2013). While implementation details may vary in different youth centers, the core elements of the program are uniformly prescribed. Although minor deviations from the guidelines are reported in the qualitative evaluation reports (Gautié, 2018), the timeline of activities and income benefits observed in the data aligns quite well with the national guidelines (Figure 1)[6].

and Pompili, 2019).

[5]The technical name of the contract used for these instances is *période de mise en situation en milieu professionelle* (PMSMP)

[6]It should be noted that according to Gautié (2018) the number of events reported in the administrative data of YECs under-estimates the number of effective events

Figure 1: Average number of events, by kind of event, and average benefits for participants in *Garantie Jeunes*.



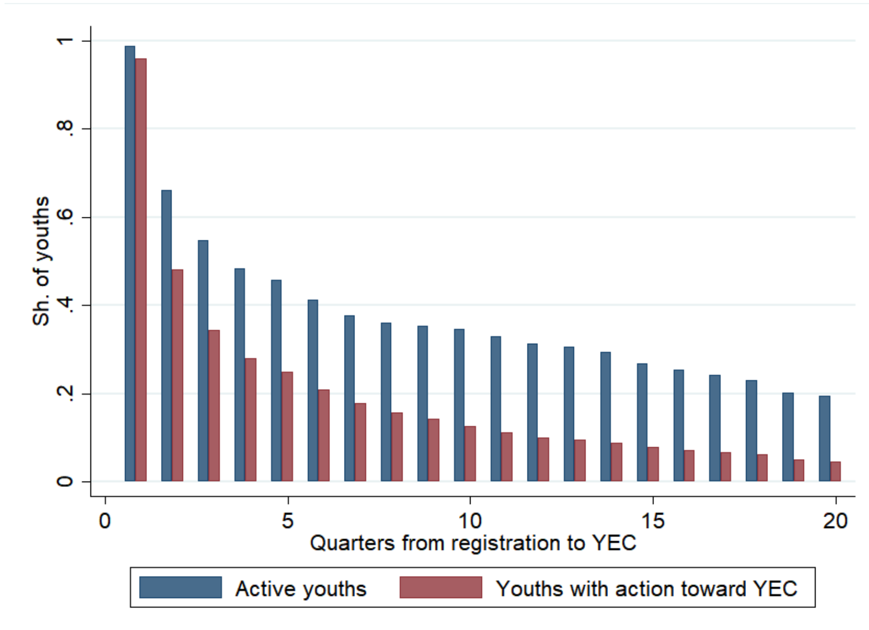
Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from enrollment in *Garantie Jeunes*. The cash transfers series plots instead the average amount of benefit to youths participating in *Garantie Jeunes*, basing on when the actual transfer of money is recorded in the information system I-Milo.

French local Youth Employment Centers (YECs)[7] are in charge of the administration of the program. These employment centers were introduced in the 1990s. They have a decentralized structure (they are created and managed by groups of municipalities) and focus specifically on youths between 16 and 25. Usually, even before *Garantie Jeunes* was introduced, YECs offer youths information on training and job offers, a number of sporadic events and a standard job search assistance program (*Contrat d'insertion dans la vie sociale*, CIVIS) with a modest number of required activities (Figure 16 in Appendix). Once youths register at YECs there is no formal de-registration, so they can remain in contact with YECs for a variable amount of time, depending on the youth's and the YEC's reliability, even without participating in any program. Figure 2 indicates that 31.4% of youths are still considered active in a specific cohort of registration – meaning youths for whom the YEC records at least one action on their *dossier* during a quarter – 3 years from the time of registration [8].

[7] *Missions Locales* in French

[8] However, after 3 years since registration only 10.1% of the youth still records an action "youth toward YEC"[9], e.g. an email sent by the youth, an interview, or another activity with participation by the youth.

Figure 2: Share of youth considered active at the YEC and youths who actually undertake action toward a YEC from time of registration.



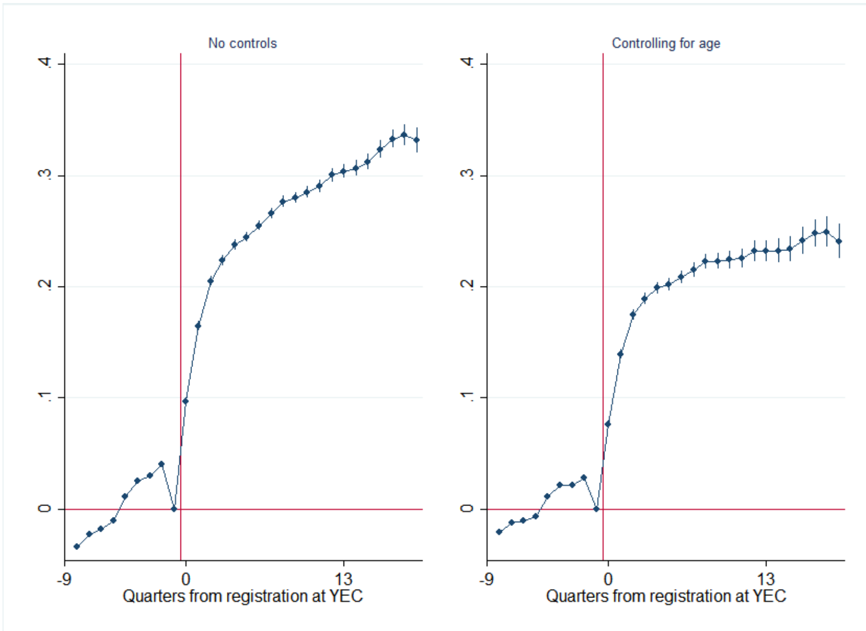
Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from registration at the YEC. "Active youths" are considered those whose *dossier* records any kind of action in the quarter. The red series reports instead youths for which a "youth toward YEC" action is recorded.

YECs are organized into 459 centers[10] with more than 9000 local offices. The assignment to a YEC is based on one's municipality of residence. There are about half a million youths registering at YECs every year, out of about 9 million youths aged 16-25 in France, suggesting that more than one-third of French youth register to YECs at some point[11]. At the time of registration, youths are likely just out of school and are beginning to enter the labor market. Figure 3 shows the average employment trajectory of a youth when registering with a YEC: it is evident that registering with a YEC corresponds to a rise in the probability of employment of the youth (of course, this is not caused only by the activities underwent at YECs).

[10]These are total numbers in the data available; currently 436 centers and 6800 offices are active, because some offices have been closed, and some are "artificial" in our data, i.e. correspond to special administrative cathegories

[11]The probability that a youth registers over the 10 years of eligibility is $1 - (1 - 500,000/9,000,000)^{10} = 43.6\%$.

Figure 3: Average employment rates in the quarters precedent/following registration at YEC, controlling or not for age.

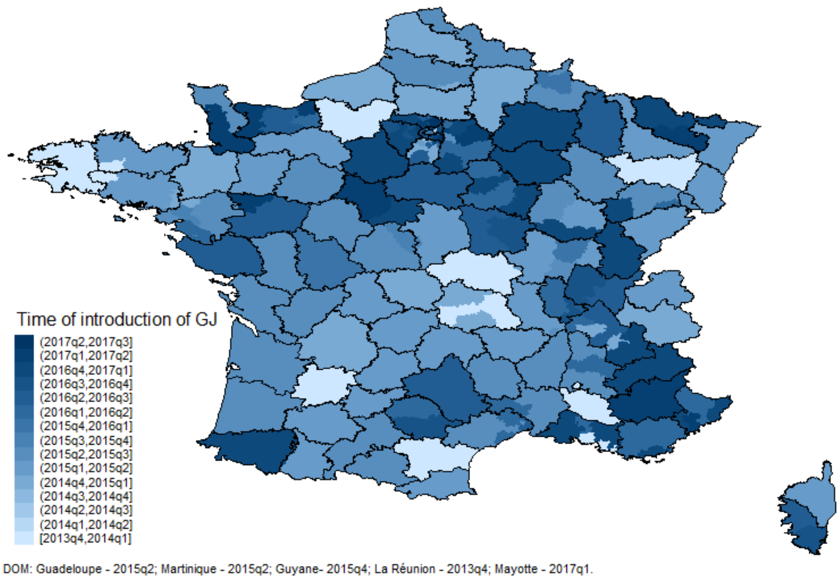


Notes. The figure plots coefficients of a regression of an employment dummy on quarters from registration, cohort and YEC fixed-effects (left panel), adding age fixed effects (right panel).

The introduction of *Garantie Jeunes* was progressive, which provides our source of identification. A pilot wave was launched in October 2013 in a number of areas selected as those with the highest reported NEETs rate among a set of volunteer territories. The program was then extended in six waves until it reached all volunteer territories in January 2016. Finally, after a preliminary evaluation, the program was extended to the whole French territory in January 2017. Figure 4 maps this process. Beside the seven official waves of extension,[12] some YECs delayed the introduction of the program, so that between 2013q3 and 2017q2 in every quarter except one there were some YECs adopting the program for the first time.

[12]The pilot wave started in October 2013 q4, a second commenced in January 2015, a third in April 2015, a fourth in September 2013 q3, a fifth in March 2016, a sixth in September 2016, and, finally, the whole territory in January 2017.

Figure 4: Progressive extension of *Garantie Jeunes*.



Notes. French municipalities (black borders correspond to *départments*) by date of first case of enrollment in *Garantie Jeunes* in their corresponding YEC. Overseas territories (DOM) are reported at the *départment* level.

Garantie Jeunes represented a challenge for YECs: the program was much more organizationally demanding than their standard activities, and political attention by the central government was high, with regular reporting requirements. YECs receive additional funding for administering *Garantie Jeunes* conditional on the number of youths enrolled (70% of the funding), on the number of youths who complete the program successfully (20%), and 10% conditional on the provision of complete data in their information system and proof of their correctness (e.g. enrollment documentation).

Among youths registered at YECs (aged 16-25), not all youths are eligible to apply, and not all who apply are selected for the program. On the one hand, in order to be eligible, youths must either live in a household below the minimum income (RSA) threshold, have quit their parents or receive no support from them, have dropped out of school without a qualifying secondary school diploma, or be convicted[13]. Note that in France minimum income is not available for youths younger than 25 with no kids. On the other hand, to enroll in *Garantie Jeunes*, youths must demonstrate a condition of "fragility" and "motivation" through an application process. Qualitative reports describing this process argue that the first selection mechanism involved selective targeting of youths by YECs, which often themselves organized information sessions and pitched the program to a selected group of registered youths. After an individual applies, the decision on the application is made by local independent commissions[14]. There are thus two possible layers of selection between potentially eligible youths (estimated to number between 187,000-189,000 in 2016) and actually enrolled youths, who are roughly half of the eligible population according to Gaini et al. (2018).

Garantie Jeunes is nowadays a large program: since 2013 more than 400,000 youths have participated in

[13]Young parents are not expected to be the target of *Garantie Jeunes*, since they are eligible to the French minimum income program RSA – guaranteed also to any individual in poverty from 25 years old onward – but are nonetheless not prevented to participate and 5% of *Garantie Jeunes* participants are reported to have kids.

[14]These commissions are composed by a president appointed by the local *Prefecture*, one representative of the government of the *département*, presidents of local YECs, and other members named by the local *Prefecture*.

it, with yearly costs estimated at 354 million Euros in 2018, when it reached full extension. Prior to this paper, the program underwent a qualitative evaluation by Gautié (2018) and a pilot evaluation by Gaini et al. (2018). Also in light of the evidence provided by this article, *Garantie Jeunes* is considered a successful model in France. The policy is currently at the center of political debate for being expanded with a new name, *Contrat d'Engagement Jeunes*, which should cover all individuals earning below minimum income, with little or no upfront screening on motivation.

3 Empirical strategy

3.1 Data Sources, Sample Selection and Data Structuring

I build a novel dataset using two administrative sources. The first source is the administrative system of YECs, called I-Milo. This dataset includes abundant information provided by youths when registering at YECs. For most individuals, I have information on housing difficulties, access to child-care services, mean of transportation used, and financial resources. For 94% of youth we can also calculate the distance between youths' declared residency and the local YEC main office or satellite office[15]. In addition, the dataset reports details of programs and activities undertaken by the youth at the YEC or with partner firms, including the dates and duration of the events attended. Finally, it reports employment and resources as declared by the youth, as long as the youth is in contact with the YEC. The dataset covers all YECs from late 2010 until the present.

To obtain information on youths also when they are not in contact with YEC, I merge the dataset with data extracted from French social security records[16]. The matching was performed by the French Agency for Social Security under an agreement with the French Labor Ministry. The resulting dataset includes information on all contracts signed during the period 2013-2018 by all youths who registered in YECs between 2013 and 2017. The available information includes date of start and termination of the contract, type of contract, total earnings and hours worked. I can derive information on hourly earnings indirectly by dividing earnings by the number of hours; henceforth, I will simply refer to this as "wage").

After cleaning[17], the population which we observe in our dataset consists of all youths who registered in YECs between January 2013 and December 2016, a panel of 1,967,000 individuals over 2013-2017. The percentage of youth aged 16-25 in our sample who earned less than secondary vocational qualification is similar to the that of the overall French population, but a larger share of youth in our sample have at most a secondary diploma (about half, against a national mean of 44%). With respect to all youths 16-25 in France, the population of YECs is not significantly different in terms of share of females and French nationals. However, the population is characterized by early experience of activities which are typical of adulthood. 35% of youth in YECs, on average, have experienced at least one employment episode in the quarter preceding registration (national mean 30%). 37% of youth in YECs, on average, live independently (national mean 23%). 8% of

[15] The dataset also contains information on French or foreign language proficiency, skills, and hobbies, but only for smaller samples (respectively 14%, 6%, and 4% of individuals, overlapping for 1.7% of our sample of interest).

[16] The dataset source is DADS and DSN (which replaced DADS in 2017). Correspondence between the individual identifiers in the two datasets was provided by the French Labor Ministry. I correctly match 17,084,219 contracts to corresponding youths in the sample from the YEC information system, while 477,733 youths in our sample report no employment contracts, and are thus coded as not employed.

[17] I drop individuals who are older than 25 in January 2013, which are too old to ever be able to enroll in *Garantie Jeunes*. I winsorize extreme values in earnings and hours (those above 99th percentile).

youth in YECs, on average, have children of their own (national mean 4%). Youths entering *Garantie Jeunes* are not easily distinguishable in terms of these general characteristics, except that they have a much lower employment rate in the quarter before registration, suggesting lower employability.

Table 1: Characteristics of the overall population, of youth in YECs (sample observed), of youth registering in the standard program of YECs, and in *Garantie Jeunes*.

	All youth 16-25 (Census)	Youth in YECs	Youth in standard pr.	Youth in *Garantie Jeunes*
Number of youth (total youth stock)	9327476	1967000	444309	113085
Number of youth (avg. quarter inflow)		125689	41471	14899
Lower than secondary educ.	0.394	0.373	0.424	0.467
Upp. secondary edu. diploma	0.434	0.519	0.541	0.507
Avg. age	20.3	20.1	19.7	18.8
Female	0.491	0.491	0.511	0.463
French nat.	0.915	0.912	0.919	0.929
Empl. last quarter	0.297	0.349	0.335	0.212
Lives independently	0.230	0.365	0.369	0.354
Has children	0.0390	0.0838	0.0878	0.0496

Notes. The table compares the characteristics of youths in different population. The first column concerns all youths aged 16-25 in France, as reported by the Census in years 2013-2016. The second column reports all youths in the sample, namely all youths who registered at YECs in the 2013-2016 period. The third and fourth columns report, respectively, information on youth enrolling in the standard program offered at YECs, CIVIS, and on those enrolling in *Garantie Jeunes* at some point of their stay at YECs. All information from second to fourth column is measured at the quarter of registration at YECs.

For simplicity, I collapse time variables by quarters. I can then define a dummy for the probability of being employed in a specific quarter, corresponding to the probability of having at least one hour of work reported in the quarter. I will refer to this dummy as "employment" throughout the paper. Then, I define the cohort of registration at YEC as the quarter in which the youth first checks in at her YEC, and the wave of introduction of *Garantie Jeunes* as the quarter in which the first enrollment in *Garantie Jeunes* occurs in the YEC[18].

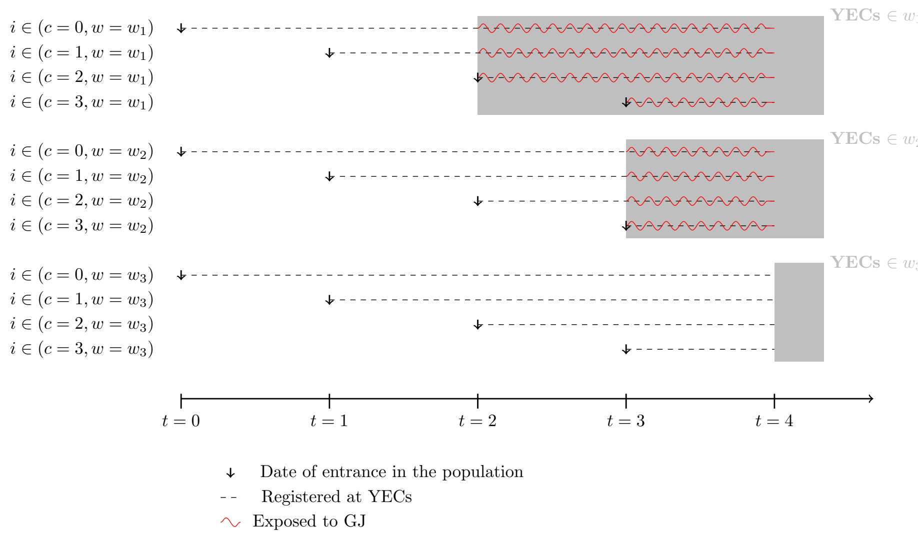
3.2 Identification

3.2.1 Intuition

My identifying shock is the staggered adoption of *Garantie Jeunes* by different employment centers over time. Each youth in the population belongs to a cohort of registration with the YECs, $c \in \{1, ..., \overline{c}\}$, based on the period when she enters the population. Each YEC belongs to a wave of introduction of *Garantie Jeunes*, $w \in \{1, ..., \overline{w}\}$. In my data, I am then able to follow each individual over cardinal time t or, equivalently, over time since registration at the YECs $h = t - c + 1, h \in \{1, ...\overline{h}\}$, with $h = 1$ at time of registration. Figure 5 reports an illustration of my setting simplified by including only 12 youths, in 4 cohorts and 3 waves. Identification stems from the fact that some cohorts of youths are in treated cells at the time of registration, while others will only subsequently be exposed to treatment, later in their tenure.

[18] Table 9 provides some descriptive statistics of the cohorts entering our panel. In the Appendix I further describe how the number of youths enrolling in *Garantie Jeunes* (Table 10) and the number of youths registering to YECs (Table 11) is distributed across waves and cohorts.

Figure 5: A simplified illustration of the setting.



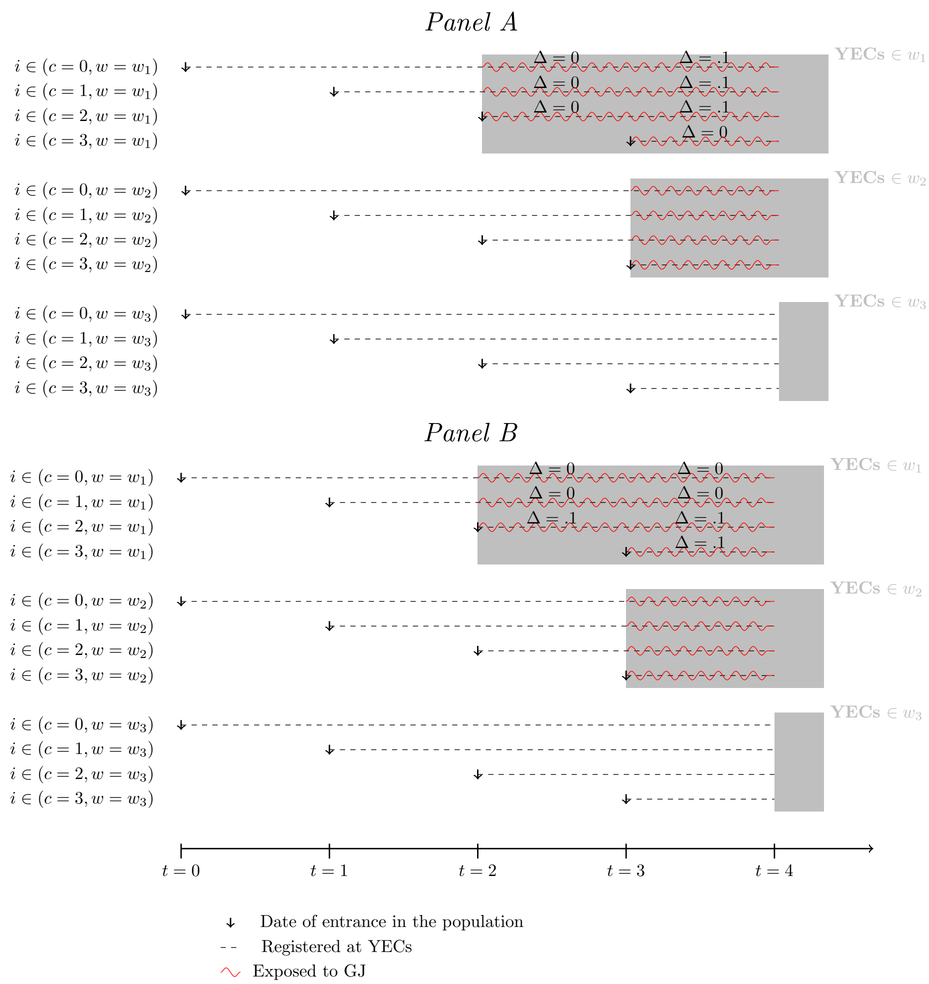
De Chaisemartin and D'Haultfœuille (2020b) show that, in staggered adoption designs, event studies using two-way fixed effects or first-difference estimators heavily rely on homogeneous treatment effects, and are otherwise biased due to negative weighting of the effect in some groups. They propose a version of the diff-in-diff approach as a solution, and in De Chaisemartin and D'Haultfœuille (2020a) adapt their methodology to the staggered adoption case, similarly to Callaway and Sant'Anna (2018). The building block for this kind of diff-in-diffs is basically a cell-specific estimator of the effect for youths in treatment wave w at time t, which in the notation of my setting would be

$$DID_{w,t}^{DCDH} = Y_{w,t} - Y_{w,t'} - \sum_{w' \in \Omega_w} \frac{n_{w',t}}{N_{\Omega_{w,t}}}(Y_{w',t} - Y_{w',t'})$$

Where $Y_{w,t}$ is the empirical average of the outcome of interest in cell w, t, t' is the period before w gets treated, $n_{w',t}$ is the number of units in cell w', t and $N_{\Omega_{w,t}}$ is the number of youths in all cells w, t such that treatment at t is still zero. If the program has been adopted at time T_w, units in cell w, t are $t - T_w$ periods away from adoption of the program, so that $DID_{w,t}$ identifies the treatment effect *since adoption*. However, direct application of this to the context of *Garantie Jeunes* is problematic.

The first problem arises if there are dynamic effects of the program. Suppose by now that there is full take-up of the program, then any dynamic effect over *enrollment* in the program will emerge over *exposure* of single cohorts to the program. Intuitively, since some cohorts are registering after the introduction of the program in their YEC, $DID_{w,t}^{DCDH}$ will be an average of cohorts with different levels of exposure. Panel A of Figure 6 exemplifies this case. Suppose by now that all youths enter who take-up the program enter immediately after exposure, so that there is no difference between exposure and enrollment, and let $G_{w,c}^h$ be the number of periods of exposure/enrollment in the program for youths in a particular (h, w, c) cell. Note, in fact, that all youths sharing the same time since registration, cohort and wave are exposed to treatment for the same amount of time. Suppose the true dynamic effect of the program is $\Delta = 0$ when $G_{w,c}^h = 1$ and $\Delta = .1$ when $G_{w,c}^h = 2$, so that the true program effect is increasing with exposure/enrollment. The average effect after two periods of *exposure*, whenever $G_{w,c}^h = 2$, is then 0.1. However, the average effect after 2 periods of *adoption* is $DID_{w_1,t=4}^{DCDH} = 0.075$. Such estimate is neither wrong nor biased, but simply targets an estimand (the effect since program adoption) which is not informative about the relevant dynamic of treatment.

Figure 6: When the effect since adoption is different than the average effect since exposure.



A second problematic case arises if time since registration is a source of selection into treatment, hence of potential heterogeneity. In my setting, this can actually be a concern. In fact, youths remain in contact with YEC for long after their registration, so that when *Garantie Jeunes* is introduced in a particular YEC, both youths who just registered and youths who registered some time before will be able to take-up the program. These two groups might not be comparable, however, since the latter will be composed only of those youths who have not found a job or a formal training during the time they have been in contact with the YEC. Hence, treatment effect might be heterogeneous across these groups. For instance, in Panel B of Figure 6 the true treatment effect is $\Delta = 0$ if $G^h_{w,c} > 0$, $h > G$, i.e. there is no treatment effect for youths who registered before treatment introduction and are exposed later. Instead, $\Delta = .1$ if $G^h_{w,c} > 0$, $h = G$, i.e. there is a positive 0.1 treatment effect for youths who registered at the moment of introduction of the program or later. The average effect when $G^h_{w,c} = 2$ is 0.03, but the effect two periods since adoption $DID^{dCDC}_{w_1,t=2} = 0.05$.

To address these two concerns, I propose an estimator which resembles the one by De Chaisemartin and D'Haultfœuille (2020a) but rolls over time since registration h, $DID^h_{w,c}$. This introduces a third dimension, beside treatment wave w and cohort of registration with YECs c. Note that focusing on outcomes of individuals at the same point in cardinal time t or on individuals from the same cohort of registration with YECs c is equivalent, given the same time since registration, h. Because $(w, c|h) \to G^h_{w,c}$, $DID^h_{w,c}$ will deliver estimates of the effect after *exposure* to the program. However, this granularity comes at a cost. In fact, one needs to observe many more cohorts of youths registering before the adoption of the program in order to assess long-term dynamic effects. Moreover, coefficients further in exposure will be necessarily estimated using only those exposed early after their YEC registration, which implies large standard errors and requires caution in interpretation[19].

3.2.2 Formal Identification of ITT

Let $G^h_{w,c}$ denote the treatment status – i.e. the number of periods exposed to *Garantie Jeunes* – and $\{Y^h_i(g)\}_{\forall g}$ be the set potential outcomes when youth i was registered to YECs since h quarters:

$$Y^h_i(G^h_{w,c}) = \begin{cases} Y^h_i(g) & \forall g > 0 \\ Y^h_i(0) \end{cases}$$

The first parameter of interest is the intention-to-treat (ITT) effect, i.e. the average causal change in the outcomes as a function of the number of periods of exposure to *Garantie Jeunes* (g). This corresponds to the expectation over w, c, h such that $G^h_{w,c} = g$ of the difference in outcome when treatment exposure is g

[19] In the simplified example of Panel B in Figure 6, treatment effect after one period of exposure can be estimated using as treated cells individuals $i \in (c = 1, w = w_1)$ at $t = 3$, $i \in (c = 2, w = w_1)$ at $t = 3$, $i \in (c = 1, w = w_2)$ at $t = 4$, $i \in (c = 2, w = w_2)$ at $t = 4$, and $i \in (c = 3, w = w_2)$ at $t = 4$, subtract to each of them the latest cohort in the same w sill untreated at h (first difference) and the evolution in untreated cohorts in other waves (second difference). However, for treatment effect after two periods of exposure I can only estimate it for $i \in (c = 2, w = w_1)$ at $t = 4$, subtracting $i \in (c = 0, w = w_1)$ at $t = 2$, and subtracting as second difference the difference between $i \in (c = 2, w = w_3)$ at $t = 4$ and $i \in (c = 0, w = w_3)$ at $t = 2$. Even though $i \in (c = 1, w = w_1)$ at $t = 4$ are exposed for two periods, I have no cohorts in the data which, after the same tenure, are still untreated (I would need to observe cohorts $c = -1$ in w_1). Although my estimate is not biased, if one compares the average effect for one and two periods of exposure the dynamic between these two includes treatment heterogeneity between youths immediately exposed and those exposed only later. Hence, one should check that the heterogeneity across youths exposed to treatment at different times since registration is not so strong before aggregating estimates from youths exposed to treatment at different times since registration.

and when not exposed[20] :

$$\Delta^{ITT}(g) = \quad \mathbb{E}(Y^h_{w,c}(g) - Y^h_{w,c}(0)) \qquad (w,c,h) : G^h_{w,c} = g > 0$$

Where I denote $Y^h_{w,c} := \mathbb{E}(Y^h_i|w,c)$, the empirical mean in the h,w,c cell. Consider a set of assumptions typical of diff-in-diff settings.

Assumptions 1-4.

1. *(Independent groups)* Treatment (i.e. *Garantie Jeunes* introduction) of one wave doesn't influence the evolution of potential outcomes of others, i.e. $\mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)|G^h_{w,c}, G^h_{w',c}) = \mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)|G^h_{w,c})$ for each wave w, given YEC-tenure h;

2. *(Strong exogeneity)* Treatment (i.e. *Garantie Jeunes* introduction) is independent from the evolution of mean potential outcomes when non-treated: $G^h_{w,c} \perp\!\!\!\perp \mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)), \forall c, c'$, given YEC-tenure h;

3. *(No anticipation)* Mean potential outcomes $Y^h_{w,c}$ in a cohort at a specific point in time are independent from treatment status in the next period $G^h_{w,c+1} = G^{h+1}_{w,c}$, so that outcomes when treated depend only on past exposure $Y^h_i = Y^h_i(G^h_{w,c})$;

4. *(Common trends)* Expected variation in potential outcomes when non-treated doesn't vary across waves, given YEC-tenure h: $\mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)) = \mathbb{E}(Y^h_{w',c}(0) - Y^h_{w',c'}(0))$.

Analogously to Callaway and Sant'Anna (2018); De Chaisemartin and D'Haultfœuille (2020a), I first target cell-specific $\Delta^{ITT}(h,w,c)$, which will be the building block for identification of more aggregate parameters. For each h,w,c such that $G^h_{w,c} = g > 0$, define

$$\Delta^{ITT}(h,w,c) = Y^h_{w,c}(g) - Y^h_{w,c}(0) \qquad \forall\, given\, (w,c,h) : G^h_{w,c} = g > 0$$

Consider an estimator of $\Delta^{ITT}(h,w,c)$:

$$DID^h_{w,c} := Y^h_{w,c} - Y^h_{w,c'} - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_{w,c}}}(Y^h_{w',c} - Y^h_{w',c'}) \qquad \forall\, given\, (w,c,h) : G^h_{w,c} = g > 0 \tag{1}$$

Where $G^h_{w,c'} = 0$ but $G^h_{w,c'+1} = 1$, and $\Omega_{w,c}$ is the set of waves such that $G^h_{w',c} = G^h_{w',c'} = 0$, for each $w' \neq w$ and $c' \neq c$. $n_{w'}$ is the number of individuals of cohort c in wave w' while $N_{\Omega_{w,c}}$ is the total number of individuals of cohort c in all waves $w' \in \Omega_{w,c}$. Proposition 1, proven in the Appendix, yields cell-specific unbiased estimators $DID^h_{w,c}$ for $\Delta^{ITT}(h,w,c)$.[21]

Proposition 1. *Under Assumptions 1-4, $DID^h_{w,c}$ is an unbiased estimator of $\Delta^{ITT}(h,w,c)$.*

[20] Note that the parameter of interest should not be confused with a variation in a survival rate, since we are not studying irreversible events but reversible outcomes. In other words, we are looking at the probability of *being* employed at a specific point in time and not at the probability of *having found* an employment by a specific time. For this reason, our data are not censored, and we do not need to apply the tools of duration models, which would require assumptions on the shape of the hazard function.

[21] An additional comment concerns the use of $\sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_{w,c}}}$ as weights for aggregating control group effects in the definition of $DID^h_{w,c}$. This choice is done for efficiency reasons and consistently with De Chaisemartin and D'Haultfœuille (2020a), but unbiased alternatives include not weighting or using different weights. Note in fact that the proof of Proposition 1 works well with any definition of weights.

Proposition 1 delivers a large number of $DID^h_{w,c}$, corresponding to levels $G^h_{w,c} = g$ of exposure to treatment. I am then then interested in meaningfully aggregate $DID^h_{w,c}$ into unbiased estimators of $\Delta^{ITT}(g)$. Consider:

$$DID^g := \sum_{(w,c|h):G^h_{w,c}=g} \frac{n_{w,c}}{\sum_{(w,c|h):G^h_{w,c}=g} n_{w,c}} DID^h_{w,c} \tag{2}$$

It can be shown that:

Proposition 2. *Given a set of $DID^h_{w,c}$, for all $(w,c|h) : G^h_{w,c} = g$, unbiased estimators of $\Delta^{ITT}(h,w,c)$, DID^g is an unbiased estimator of $\Delta^{ITT}(g)$.*

Intuitively, Proposition 2 aggregates cell-specific ITT into a weighted average of effects from different waves, cohorts and tenures, sharing the same level g of treatment exposure.

Finally, I can also define a placebo test for predictions implied by strong exogeneity and common trends:

$$Y^h_{w,c} - Y^h_{w,c'} - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_{w,c}}}[Y^h_{w',c} - Y^h_{w',c'}] = 0 \qquad \forall \, given \, (w,c,h) : G^h_{w,c} = 0 \tag{3}$$

And aggregate placebos sharing the same distance from treatment introduction $w - c$.

3.2.3 Getting LATE

At this point, I have obtained an unbiased estimator for $\Delta^{ITT}(g)$, my first parameter of interest. A second target is the local average treatment effect (LATE) of having *actually* enrolled in *Garantie Jeunes* d periods before:

$$\Delta^{LATE}(d) \quad = \quad \mathbb{E}(Y^h_i(d) - Y^h_i(0)|D^h_{w,c} = d)$$

What can we say about $\Delta^{LATE}(d)$? Proposition 3 suggests a starting point.

Proposition 3. *Consider a set of $DID^h_{w,c}$, unbiased estimators of $\Delta^{ITT}(h,w,c)$, the cell-specific ITT treatment effect. Let d be the number of periods since enrollment and D^h_i the variable describing it. Then, if $Pr(D^h_{w,c} > 0)$ whenever $G_{w,c}$=0 (no defiers):*

(a) $\sum_{(w,c|h):G^h_{w,c}=g} \frac{n_{w,c}}{\sum_{(w,c|h):G^h_{w,c}=g} n_{w,c}}[DID^h_{w,c}/Pr(D^h_{w,c} > 0)]$ *is an unbiased estimator of* $\Delta^{LATE}(g) = \mathbb{E}(Y_i(d > 0) - Y_i(0)|G^h_{w,c})$

(b) $DID^h_{w,c} = \sum_{d=1}^{g} \delta(d,h,w,c) Pr(D^h_{w,c} = d)$ *for all w, c, h, where $\delta(d,h,w,c)$ is an unbiased estimator of* $\Delta^{LATE}(d,h,w,c) = \mathbb{E}(Y_i(d) - Y_i(0)|w = w, C = c, H = h)$

The first point of proposition 3 simply points out the possibility of recovering a LATE on all takers, conditional on being in cells $(w,c|h)$ such that $G^h_{w,c} = g$ from simple rescaling of ITT estimates. To obtain that, one can divide estimates of the ITT effect by the take-up probability. This is not a novelty in IV estimation, but it is worth pointing out that the caveats highlighted by De Chaisemartin and d'Haultfoeuille (2018) don't apply because we always have at least one fully untreated wave and no defiers in the control group. The second point instead shows that ITT can be seen as a function of dynamic LATEs. In fact, we can express the $|h| \cdot |w| \cdot |c|$ ITTs in terms of $|d| \cdot |h| \cdot |w| \cdot |c|$ unknown probabilities of having actually taken up treatment since d periods.

Because $|h| \cdot |w| \cdot |c| < |d| \cdot |h| \cdot |w| \cdot |c|$, the result is useful for inferring information on structural parameters $\{\delta(d, h, w, c)\}$ only if one is willing to impose restrictions. In my setting, a convenient restriction is assuming LATEs to be mean independent from cohorts, waves, and tenure, conditional on d: $\mathbb{E}(\delta(d, h, w, c)) = \mathbb{E}(\delta(d))$[22]. To gain more power, I also aggregate the dynamic of d into three classes $0 < d \leq 2$ $2 < d \leq 4$ and $d > 4$, respectively the first semester of enrollment in the program, the second, and more than one year after enrollment (that is, after completion). Under these assumptions, one can recover structural δs applying Equally-Weighted Minimum Distance (Altonji and Segal, 1996; Card and Lemieux, 2001) to the regression:

$$\begin{aligned} DID^h_{w,c} = {} & \delta(0 < d \leq 2) Pr(0 < D^h_{w,c} \leq 2) \\ & + \delta(2 < d \leq 4) Pr(2 < D^h_{w,c} \leq 4) \\ & + \delta(d > 4) Pr(D^h_{w,c} > 4) + \varepsilon_{h,w,c} \end{aligned} \tag{4}$$

4 Results

4.1 Balance checks

An implication of Assumption 2 (strong exogeneity) is that youth entering YECs before and after the introduction of *Garantie Jeunes* are comparable. In fact, youth belonging to cohorts before and after the introduction of *Garantie Jeunes* might differ, for example if the YEC becomes congested due to the program, if it tries to recruit more fragile youth to fill *Garantie Jeunes* spots (based on which YECs receive financing), or if a different selection of youth is attracted to the YEC due to the presence of *Garantie Jeunes*. Thus, in this section I exploit the wide range of information available in YECs administrative data to run a set of balance checks, making sure that the introduction *Garantie Jeunes* doesn't significantly modify the characteristics of cohorts at the time of registration with YEC. Table 2 reports a set of regressions of average characteristics of a cohort on a dummy for *Garantie Jeunes* adoption, on a linear trend by quarter after adoption, and on both the dummy and the linear trend together. The results are reassuring: of the many variables evaluated, the only relevant concern is an increase in youths registering with housing problems, which increases by 0,6 percentage points over a mean of 10.5% before *Garantie Jeunes* introduction. It also appears that there was a mildly significant increase in the share of youth registering who have children, but the magnitude is again very small. All other characteristics of youths registering with YECs don't significantly change with *Garantie Jeunes* introduction, supporting the assumption that treatment status doesn't affect individuals' potential outcomes.

[22] This assumption is strong, given the emphasis has been given in the literature to potential heterogeneous treatment effects. In this paper itself I highlighted threats arising from heterogeneity depending on tenure h. However, it turns out that heterogeneity by h is not so important in my case. Also, the fact that the effect estimated with classical event-study design is quite similar to the one estimated with my methodology – which is robust to heterogeneity in w and c – suggests that such heterogeneity might not be so strong. In any case, different assumptions are possible and different aggregations are left to further research.

Table 2: Balance checks.

	(Check 1)	(Check 2)	(Check 3)		(Mean)
	GJ adopt.	GJ adopt.*quart. adopt.	GJ adopt.	GJ adopt.*quart. adopt.	
Share of female	-0.00115	-0.00148	-0.000295	-0.000358	0.491
	(0.00179)	(0.00177)	(0.000391)	(0.000388)	
Age at registration	0.0135	0.0133	-0.000154	0.000599	20.1
	(0.0121)	(0.0127)	(0.00322)	(0.00333)	
No diploma	0.00376	0.00337	-0.000326	-0.000118	0.373
	(0.00244)	(0.00236)	(0.000489)	(0.000478)	
CAP or BAC	-0.00212	-0.00153	0.000521	0.000403	0.519
	(0.00227)	(0.00230)	(0.00056)	(0.000566)	
French nationality	-0.00208	-0.00154	0.000473	0.000357	0.912
	(0.00217)	(0.00230)	(0.00051)	(0.000538)	
Housing problems	0.00591***	0.00634***	0.000376	0.000704	0.0500
	(0.00157)	(0.00175)	(0.000431)	(0.00046)	
Resident in Urban Sensitive Area	0.000625	0.00407	0.003	0.00303	0.105
	(0.00355)	(0.0052)	(0.00211)	(0.00220)	
Distance residency-YEC	-4.67	-3.47	1.01	0.759	715
	(3.47)	(3.74)	(1.43)	(1.43)	
Resources declared	1.07	1.54	0.411	0.470	155
	(2.26)	(2.59)	(0.779)	(0.814)	
Owns a motor vehicle	-0.00389*	-0.00373	0.000135	-0.0000778	0.410
	(0.00233)	(0.00239)	(0.000499)	(0.000516)	
Lives alone	0.000507	0.000805	0.000259	0.000287	0.899
	(0.00217)	(0.00223)	(0.000472)	(0.000485)	
Has children	0.00154	0.00230*	0.000652*	0.000738*	0.0837
	(0.00119)	(0.00125)	(0.000382)	(0.000381)	
Problems with childcare	0.00614	0.00474	-0.00119	-0.000841	0.348
	(0.00620)	(0.00609)	(0.00145)	(0.00140)	

Notes. The table reports the coefficients of a separate regression of each characteristic of youths registering to YECs (listed in the first column) on a dummy for GJ introduction (Check 1), on a linear trend (Check 2), and on both (Check 3). The last column reports the mean of the variable before GJ introduction. The dependent variables used are cohort size (number of youths registering), share of females, average age of youths registering, share of registering youth with lower than vocational-secondary education, with at most vocational secondary, and with at most secondary education, share with French nationality, residency in disadvantaged zones, housing difficulties, average resources declared, and distance between residency and closest YEC office. I also exploit the abundant information in the administrative data of YECs to check balance for a dummy of whether the youth owns a motor vehicle, whether she lives independently, has kids, and, if so, if she has problems with childcare.

4.2 Main Results: ITT and LATE on Employment, Hours Worked and Earnings per Hour

Following the rolling diff-in-diff approach outlined in Section 3, I estimate a full set of $DID^h_{w,c}$, for every $(w, c|h)$ cell, using equation (1)[23]. Then, I group $DID^h_{w,c}$ corresponding to same levels of g using Equation (2), and obtain the estimates of the ITT effect after being exposed g quarters to the program, DID^g, plotted in Figure 7. Standard errors are obtained by bootstrapping, accounting for clustering at the YEC and cohort level, following the same algorithm of De Chaisemartin and D'Haultfœuille (2020b) and based on Cameron

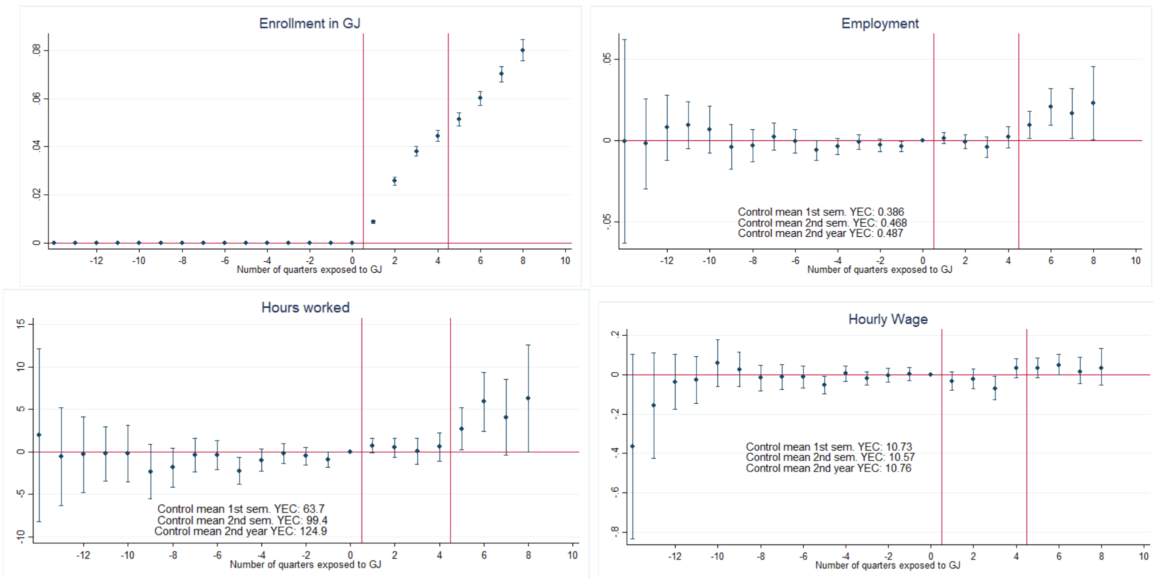
[23]For example, for $h = 4$ we obtain a wide set of coefficients such as in Table 12 in the Appendix. The distributions of $DID^h_{w,c}$ by g are also reported in Figure 11 in the Appendix. Finally, Figure 12 shows that the effect seem relatively uniform for youth "waiting" differet times for the treatment.

and Miller (2015).

The first stage indicates that about 1% of youth enters the program each quarter of exposure, quite linearly over the first two years since exposure. This is already not trivial, since it shows that youths might enter the program much later than when they first were exposed. The coefficients before the introduction of the program are all omitted because nobody participates in *Garantie Jeunes* in YECs which are not yet treated (no defiers). Coefficients on employment, hours worked and wages (defined as total earnings divided by total hours) display a clear and long parallel trend in outcome variables between different waves before the introduction of the program. Nor are significant differences in outcomes found in the first 4 quarters of exposure to *Garantie Jeunes*. Conversely, a positive effect arises in the second year after exposure, when youths who entered *Garantie Jeunes* in the first quarters of exposure start completing the program. This effect is between +0.9 and +2.3 percentage points in terms of employment and between +3 and +6 hours worked on a quarterly basis. The dynamic is similar for the two outcomes: a weaker effect in the fifth quarter of exposure (possibly due to the fact that few youths have completed the program) and a stronger one from the sixth quarter of exposure, as more and more youths complete the program. Wages (earnings per hour) are instead unaffected, remaining at a mean close to the minimum wage and with small standard errors. This suggests that the new jobs obtained by participants are mostly minimum-wage jobs.

We can further aggregate the treatment effect coefficients in averages for the first and second semester of exposure, and in the second year of exposure, in Table 3. The first column reports the effect of exposure to *Garantie Jeunes* on its take-up – our first stage – and shows that an average of 3% of the youth in the first year and 6% in the second year take-up the program when it becomes available. Columns 2-4 report the ITT effect on our outcomes of interest. For the two semesters of exposure, the estimated effect is insignificant and close to zero. This suggests that when the first youth start receiving substantial cash transfers and activation policies thanks to *Garantie Jeunes* this doesn't significantly affect employment. Instead, after more than a year during which youths are exposed to *Garantie Jeunes*, their employment probability is significantly higher, +1.6 percentage points. Accordingly, hours worked increases by + 4.3 hours on a quarterly basis.

Figure 7: Intent to treat (ITT) estimates using the rolling diff-in-diff approach.



Notes. The figure reports results of the rolling diff-in-diff approach. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable is a dummy for exposure to *Garantie Jeunes*. The other three panels report the reduced-form coefficients: the dependent variables are employment, hours and wages (earnings per hour), while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 2. Cell-specific effects were obtained as in Equation 1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-level, corrected for multiple testing, and confidence intervals are reported at 95% confidence level.

Table 3: Intent to treat (ITT) estimates aggregated.

	Enrollment in GJ	Employment	Hours	Wages
	(1)	(2)	(3)	(4)
ITT 1st semester of exposure	0.0168***	0.000417	0.592	-0.0298
	(0.000559)	(0.00174)	(0.455)	(0.0197)
Total n.obs	4003538	4003538	3957848	1518029
ITT 2nd semester of exposure	0.0406***	-0.00131	0.270	-0.0282
	(0.000807)	(0.00278)	(0.693)	(0.0233)
Total n.obs	3890678	3890678	3833155	1576052
ITT 2nd year of exposure	0.0609***	0.0157***	4.31***	0.0337
	(0.00101)	(0.00517)	(1.50)	(0.0206)
Total n.obs	5574885	5574885	5472754	2358279
Control mean 1st semester in YEC		0.386	63.7	10.73
Control mean 2nd semester in YEC		0.468	99.4	10.57
Control mean 2nd year in YEC		0.487	124.9	10.76

Notes. The table reports the weighted averages of the $DID^{h}_{w,c}$ coefficients where exposure is between 1 and 2 quarters, between 2 and 4 quarters, or above 4 quarters.

Subsequently, I can follow Proposition 3 to obtain estimates of the LATEs. First, I can use point a) to obtain LATEs on all takers, conditional on time of exposure to *Garantie Jeunes*[24]. The results are shown in the upper panel of Table 4. This is simply a rescaling of the ITT effects, and the figures suggest a positive effect on outcome variables when the program is finished for the early takers, in the second year of exposure. The effect on the probability of employment for takers in the second year of exposure is estimated at +27 percentage points. Compared to the average probability of employment for youths in untreated cells in the second year after registering with YECs, estimated at 49%, this corresponds to a 55% increase in the probability of employment. Instead, the effect on hours is an increase of 72 hours worked quarterly, which is smaller relatively to what one would expect if the increase in employment was driven by full-time employment. Still, it is equivalent to a 58% increase relatively to the control mean for youths in the second year of registration with YECs and yet untreated.

Table 4: Local average treatment effects (LATEs) on all takers at a particular point of exposure and by level of enrollment.

	Employment	Hours	Wages
	(1)	(2)	(3)
LATE 1st semester of exposure	0.0246	35.1	-1.76
	(0.104)	(27.1)	(1.14)
LATE 2nd semester of exposure	-0.0322	6.63	-0.695
	(0.0680)	(17)	(0.573)
LATE 2nd year of exposure	0.259***	70.7***	0.550
	(0.0837)	(24.5)	(0.340)
LATE 1st semester of enrollm.	-0.0504	15.1	-0.193
	(0.0566)	(14.6)	(0.635)
LATE 2nd semester of enrollm.	-0.00801	14.1	-0.0241
	(0.0758)	(24.3)	(0.707)
LATE after completion	0.326***	72.0**	1.00
	(0.104)	(34.2)	(0.659)

Notes. The upper panel reports reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and wages for takers, obtained according to Proposition 3 a). The lower panel reports the LATE effect of being at different stages of *Garantie Jeunes*, obtained according to Equation 4. Standard errors are bootstrapped and reported in parenthesis.

However, LATEs since exposure are a mix of program effects from takers at different stages of the program. Using point b) of Proposition 3 and Equation 4 I can estimate dynamic LATEs on takers who are actually at different stages of program enrollment. The results of such an approach are reported in the lower panel of Table 4. The estimates indicate that positive ITT effects are mostly found where the share of youths having completed the program is larger. This points out that youth increase their probability of employment only after they have completed the program, and finished receiving the cash transfer. Concerning hours worked, the magnitude of the LATE after completion is similar to the LATE for all takers in the second year of exposure (in the upper panel). Conversely, for employment, the LATE after completion is quite larger than for all takers in the second year of exposure, compensating for slightly negative but non-significant effects during the first semester enrollment. This may suggest a decrease in part-time jobs associated to youths in the first semester of enrollment, an hypothesis that will be confirmed in Section 5. Finally, also in terms of LATE there is no significant effect on wages.

As a general remark, the LATE effects we find when youths finish the program are very high, yet they are

[24]This is consistent with De Chaisemartin and d'Haultfoeuille (2018), where both in the pre-period for the treatment group, and in both pre and post for the control group, the treatment probability is zero

not so far from the one found by the pilot evaluation by Gaini et al. (2018), who found a LATE of +22.2 in the probability of employment (over a control mean of 25%) on the fifth quarter after enrollment in the program[25]. Gaini et al. (2018) do not report results for hours of work and wages, so comparison with them is not possible.

4.3 Heterogeneity

In the Appendix I analyze the heterogeneity of the impact of *Garantie Jeunes* both on different kinds of employment contracts and by characteristics of the youth. Table 13 in the Appendix reports the ITT and LATE effect on employment in open-ended contracts, temporary contracts, agency jobs (quite frequent in this population) and apprenticeship. Interestingly, the effect on open-ended employment is not significantly different from zero, while the overall employment effect mostly comes from temporary contracts (+.7 percentage points) and agency jobs (+.4 percentage points). This is important to give credibility to our large effect, since these kinds of contracts are perhaps more volatile. On the other hand, this result implies that the gains generated by *Garantie Jeunes* might be short-lived. Finally, apprenticeships increase significantly immediately after exposure to *Garantie Jeunes*, suggesting that many youths are channeled into this type of contract, which may later turn into full-time employment contracts.

Turning to heterogeneity by youth characteristics (Figure 13-15 in the Appendix), effects in ITT terms do not vary dramatically by gender. Conversely, it appears that all the effect arises from youths with at least upper secondary education, while the effect on youths without an upper secondary diploma is null. This can be due to the fact that youths without a secondary diploma are mostly channeled by *Garantie Jeunes* counselors toward undertaking more formal training or education rather than employment. Heterogeneity by age offers an additional insight: the effects are indeed concentrated in youths in the 19-21 years-old bracket when registering at YECs, the age at which most students obtain an upper secondary diploma in France, while the effects are null for younger and noisy for older youths.

4.4 Cost-benefit Analysis

In this section, I compare the benefits to the costs, by calculating the Marginal Value of Public Funds (Hendren and Sprung-Keyser, 2020) for *Garantie Jeunes*

$$MVPF = \frac{WTP}{NetCost}$$

Where WTP represents the aggregate willingness to pay for the program. By analogy with the work done by the same authors for estimating the MVPF for programs similar to *Garantie Jeunes*, such as the Job Corps program, I estimate WTP as the present value of the impact of the policy on after-tax income. This is given by the significant LATE effect on gross labor earnings the second year after enrollment in *Garantie Jeunes*, €1126 each quarter, net of taxes and social contributions, discounted by one year. Conservatively, I assume no effect from *Garantie Jeunes* at an horizon longer than one year after completion, since the literature

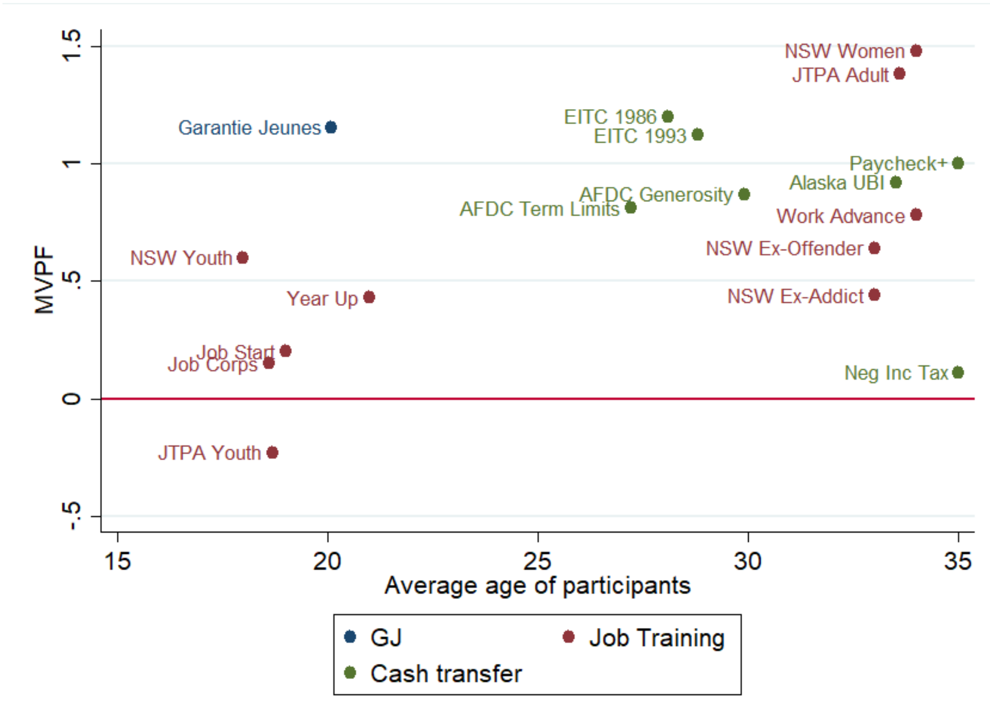
[25] For the first quarter of exposure, our estimates are similar but not significant compared to Gaini et al. (2018). This can be linked to the fact that their design is different, and that I might lack power for estimating significant effects in the first quarter. Differently from them, I find estimates close to zero in the second and third quarter. This might be due to the fact that they use a survey question asking for "having worked at least one hour in the quarter", while short work immersions (PMSMP) usually proposed to youths in the second and third quarter of *Garantie Jeunes* are not reported in our administrative data.

suggests that job-search assistance has effects mostly in the short run (Card et al., 2018; Crépon et al., 2013a), and our heterogeneity analysis highlights the precarious nature of employment contracts obtained thanks to *Garantie Jeunes*. Concerning the costs associated with *Garantie Jeunes*, one should sum the direct cost of implementing the program for youths employment centers and the opportunity cost of using YECs assets such as classrooms and offices (estimated by Arambourou et al., 2016, at an extra 20% of the total reimbursement). The additional funding allocated to each youth employment center is €1120 per youth enrolling in the program, plus €320 after the youth completes the program or secures employment or formal training and €160 for data reporting, hence a total of €1600 per youth. Given that only 17% of participants quit the program before the end for reasons not related to having found an employment or formal training (Gautié, 2018), I can estimate the net cost at €1546. The cumulated cash transfer received while in the program, calculated from the data at €4039 on average, is a simple transfer so it is added both to WTP and to net costs.

Under these assumptions[26], the MVPF of *Garantie Jeunes* is estimated at 1.15. In order to better benchmark this result, Figure 8 reports MVPF for all programs in the job training and cash transfer category analyzed in Hendren and Sprung-Keyser (2020) in the US. Compared to job training programs, the MVPF of *Garantie Jeunes* is relatively large, especially with respect to programs targeting youth. This arises from the fact that, although job training programs are in general cheaper than *Garantie Jeunes*. The MVPF of *Garantie Jeunes* appears to be in line with the upper tercile of MVPF estimates for cash transfer programs, although these programs usually have an older target population (some are targeted to wage-earners only). Further comparison with other kind of programs, less comparable to *Garantie Jeunes* and therefore not reported in the plot, shows that *Garantie Jeunes* vastly underperforms relative to the MVPF of policies supporting college attendance (which tend to have MVPF between 2 and infinity), while it slightly outperforms most unemployment insurance and income support schemes (which tend to have a MVPF around 1).

[26]First, to address potential substitution between programs (Kline and Walters, 2016) I assume that both the opportunity cost of the infrastructure and the cost-saving arising from substitution away from alternative programs is included in the extra funding guaranteed for each youth in *Garantie Jeunes*. Second, the estimated MVPF doesn't consider externalities. These can be both negative and positive. As an example of potential negative externalities, Crépon et al. (2013a) highlighted significant displacement effects in the French context for a population of young, educated, job-seekers. Positive externalities may instead arise from potential effects on social capital, health, or crime rates of target youth. Finally, time discounting is assumed exponential in the calculation of the present value of net earnings, with a discount rate of 3%, as in Hendren and Sprung-Keyser (2020). The MVPF falls to 1.13 when using a discount rate of 5% and to 1.09 when using a discount rate of 10%.

Figure 8: Marginal Value of Public Funds (MVPF Hendren and Sprung-Keyser, 2020) for *Garantie Jeunes* and for comparable programs, by average age of participants.



Notes. The figure reports the Marginal Value of Public Funds (MVPF) *Garantie Jeunes* and for programs in the"Job Training" and "Cash Transfer" categories analyzed by Hendren and Sprung-Keyser (2020) in the US context, plotted over average age of the participants in the program.

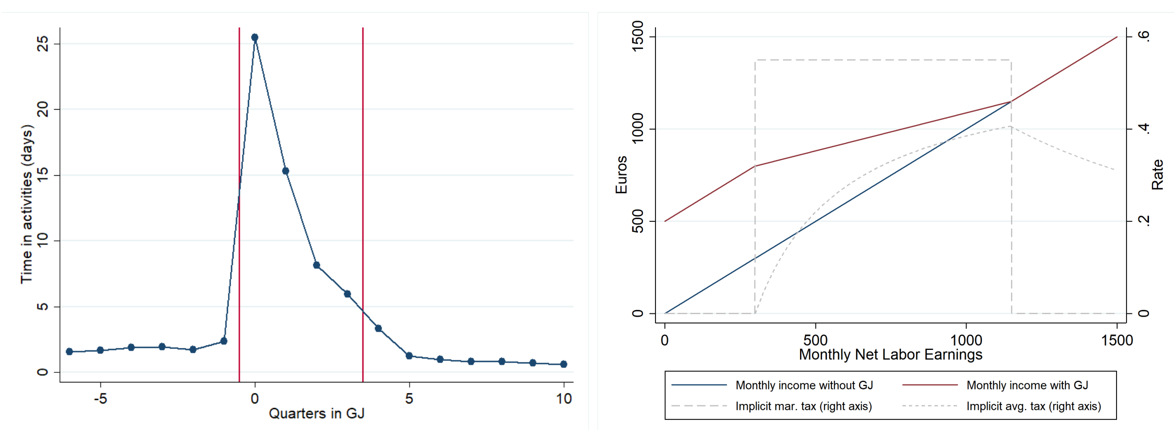
5 Disentangling the Role of Cash Transfers and Activation

In this section I aim at opening the black box of the the mechanisms behind my results, disentangling the role of cash transfers and activation measures. The literature has so far highlighted different channels through which activation measures and cash transfers can affect employment of participants. Activation measures are mostly considered as affecting employment through the channel of job search. Gautier et al. (2018) model the impact of activation measures on search effort, assuming that participation in activation programs improves the matching technology (i.e. increases the number of applications sent per unit of time) but costs time. In turn, studies on the effect of cash transfers to job seekers focus on their effect on the amount of work an individual is willing to supply, which is related to the level of implicit taxation from cash transfers phase out, and to disincentives to work due to the improved value of unemployment. The literature studying the reaction of labor supply to taxation finds that compensated earning elasticities of labor supply are relatively small (0.1-0.5), but larger effects are observed for workers less attached to labor force, including low income earners in welfare programs (Card and Hyslop, 2005; Le Barbanchon, 2020). In the context of unemployment insurance, Card et al. (2007); Chetty (2008) find that guaranteeing more cash-on-hand to job-seekers, through benefit increase or wealth shocks, reduces labor supply and search effort, through so-called moral hazard and/or liquidity effect.

5.1 Effect of Time Constraints and Phase-Out of the Cash Transfer

Time constraints from activation measures and implicit taxation from cash transfers phase out are two possible dimensions through which *Garantie Jeunes* can affect labor supply and job search. To get insights on the role of these two channels, I will exploit two dimensions of treatment variation: the the change in the amount of time required to participate in *Garantie Jeunes* and the change cash transfer phase-out with job earnings. The left panel of Figure 9 reports the number of working days with a scheduled training, interview or job immersion for participants in *Garantie Jeunes*, before and after enrollment in the program. In the first two quarters of the program, youths are busy 25 and 15 days in a quarter respectively, possibly lacking the time to actually look for a job ("lock-in" effect). This is due to the intensive collective training sessions held in the first quarter, and to job immersions that peak in the second quarter. The right panel of Figure 9 reports instead the evolution of income with and without *Garantie Jeunes*. As already mentioned, the cash transfer of *Garantie Jeunes* can be fully cumulated with job earnings up until €300 of net earnings. The transfer is then reduced quite steeply for every additional Euro of job earnings, until it disappears at 80% of the gross minimum wage (€1159 on average in 2013-2016), where income with *Garantie Jeunes* equals income without. In the figure, it is quite evident how much the phase-out of the cash transfer flattens the schedule of monthly income with *Garantie Jeunes*, since for every additional Euro earned the cash transfer is reduced by about 55 cents, implying 55% marginal tax rate and up to 40% average rate.

Figure 9: Working days with a scheduled activity as a function of time since enrollment in *Garantie Jeunes* (left panel) and cash transfer phase-out (right panel).



Notes. The left panel reports the estimated average working days with a scheduled activity as a function of time since enrollment in *Garantie Jeunes*. Source: I-Milo. The right panel shows the implicit marginal and average tax rate and the effect on the difference between monthly gross and net income. The figure is estimated from interpretation of the legislation.

To obtain identified estimates of treatment heterogeneity, I can use Proposition 3 to recover the LATE for individuals in the 1st semester or 2nd semester after enrollment, or after completion of *Garantie Jeunes*. I can do this repeatedly using as outcome the probability of earning a monthly amount below €300, between €300 and €1159, or above €1159. For separating the second and third category, I will use a threshold of €1100 instead of €1159 (the precise average of 20% gross minimum wage in the period) since I want to avoid including in the previous class individuals bunching around the net minimum wage (which is slightly lower, especially at the beginning of the period)[27].

[27] An alternative option would be to look for bunching at €300. However, it is possibly difficult for youths to bunch sharply

Table 5 reports the results. In the first semester after enrollment, when youths are involved in soft-skill training and activation policies, I find a significant decrease in the probabilities of part-time or discontinuous jobs, while no significant effect is found for the probability of earning over €1100. I interpret this result as youths reducing search effort for small, part-time, or less remunerative jobs. Then, once youths completed the most time-consuming part of the program, but are still eligible for the cash transfer, I find an increase in the probability of earning below €300 and in the probability of earning above €1100, but also a strong decrease in the number of youths earning €300-€1100. This could be rationalized by a general increase in youth employability, and a negative reaction of youth to implicit marginal taxation on earnings in the €300-€1100 range. Finally, in the second year after enrollment, when youths completed the program and stop being eligible for the cash transfers, both the probability of earning in the €300-€1100 range and of earning above €1100 increase substantially. This corresponds to a generally positive effect of the program on employability and job quality after completion, a result that reinforces the insights from Section 4.

Table 5: Diff-in-diff estimates of the impact of *Garantie Jeunes* on the probability of declaring at least once in the quarter monthly job earnings in different income brackets.

Local Average Treatment Effect

	Monthly income €1-€300 (1)	Monthly income €300-€1100 (2)	Monthly income over €1100 (3)
LATE 1st semester of enrollm.	-0.0674*	-0.0482*	0.0221
	(0.0359)	(0.0290)	(0.0361)
LATE 2nd semester of enrollm.	0.0846**	-0.146***	0.129**
	(0.0431)	(0.0544)	(0.0577)
LATE after completion	-0.0863	0.188***	0.197**
	(0.0618)	(0.0700)	(0.0793)

Average outcomes of takers in treatment group

	Monthly income €1-€300	Monthly income €300-€1100	Monthly income over €1100
1st semester of enrollm.	.079	.04	.122
2nd semester of enrollm.	.091	.083	.206
After completion	.103	.143	.34

Notes. The table reports estimates of LATE effects obtained using Proposition 3b and Equation 4, using as outcome the probability of earning in different income brackets. The lower panel reports the average outcomes estimated for the takers of the treatment group. Estimates are obtained using Equally Weighted Minimum Distance.

5.2 A Simple Theoretical Framework for Comprehensive Interpretation

Besides time constraints and implicit taxation, other aspects of *Garantie Jeunes* might affect employment. For example, cash transfers might generate moral hazard/liquidity effect reductions in labor supply, even if no implicit taxation is involved. Importantly, activation measures can mitigate the negative effects of

in terms of net earnings. Moreover, the resources are self-declared, so there might be a wedge between the actual earnings reported in our administrative data and those declared. Figure 17 in the Appendix reports the distribution of net earnings (only when net earnings are bigger than zero) for youths who take-up the program, at different stages of enrollment and after completion. Although a little spike is observed at €300, the magnitude is relatively small.

cash transfers and increase the effectiveness of youths' job search. Finally, all these effects might co-exist at some stages of the program. Hence, in this section I propose a simple structure to consistently interpret the reduced form estimates of Table 5, separating the different channels. This will allow to comprehensively asses and compare the magnitude of the effects generated by the cash transfer – the one of implicit taxation and the moral hazard/liquidity effect – and activation measures – lock-in effect due to time-consuming training activities, and potential change in job search due to activation.

Suppose wages are given and equal for all individuals so that, for each period, youth maximize utility from choosing gross working earnings $z^t \in \{z^0, z^1, z^2, z^3\}$. These brackets correspond to those of Table 5, i.e. unemployment, working earning €1-300, €300-1100, >€1100 per month. Since €1100 is roughly the minimum wage, and wages are assumed equal for all individuals, z^0 corresponds to not working, z^1 corresponds to work by the hour for short time, discontinuous jobs or low-intensity part-time (e.g 5-10 hours per week), z^2 corresponds to normal part-time and z^3 corresponds to full-time. The assumption of fixed wages is strong but plausible, because takers of *Garantie Jeunes* mostly work at the minimum wage, given that the estimated effect of the program on wages is non-significant (Table 4), and because participants are few with respect to the overall population of minimum-wage earners (so general equilibrium effects are unlikely).

Let *cash* be a dummy for being enrolled in the year of *Garantie Jeunes*, hence equal to one when youth have the right to receive the cash transfer, zero for takers before the program and after completion of the program or for non-takers. Assume that utility for individual i and choice j is linear:

$$U_j(cash, \eta_i) = u_j + \eta_i = a_1(z_j + \mathbb{1}(cash = 1) \cdot (b - \min[b, \max[0, (z_j - 300)\tau]]) + a_2 z_j / w + \eta_i \tag{5}$$

In such expression a_1 is the marginal utility of consumption, a_2 is the marginal utility of leisure, b is the cash transfer from *Garantie Jeunes*, τ is implicit marginal taxation due to the phase-out, and η_i is individual heterogeneity. Denote $\alpha_j = a_1 z_j$, $\beta = a_1 b$, $\gamma_j = a_2 z_j / w$. Let consumers maximize the utility of their desired employment so that $U_{j^*i} > U_{ji} \quad \forall j \neq j^*$. If η_i is distributed as extreme values, then McFadden et al. (1973) shows that $Pr(z^{j*} = z^j) = \frac{e^{u_j}}{\sum_j e^{u_j}}$. Hence:

$$Pr(z^{j*} = z^j) = \Phi_j(cash)$$
$$\text{where} \quad \begin{cases} \Phi_1(1) = \frac{e^{\alpha_1+\beta+\gamma_1}}{e^{\alpha_0+\beta}+e^{\alpha_1+\beta+\gamma_1}+e^{\alpha_2-(\alpha_2-300)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_1}}}{K_1} e^{\beta} \\ \Phi_1(0) = \frac{e^{\alpha_1+\gamma_1}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_1}}}{K_0} \\ \Phi_2(1) = \frac{e^{\alpha_2-(\alpha_2-300)\tau+\gamma_2+\beta}}{e^{\alpha_0+\beta}+e^{\alpha_1+\gamma_1+\beta}+e^{\alpha_2-(\alpha_2-300)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_2}}}{K_1} e^{\beta-\alpha_2\tau} \\ \Phi_2(0) = \frac{e^{\alpha_2+\gamma_2}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_2}}}{K_0} \\ \Phi_3(1) = \frac{e^{\alpha_3+\gamma_3}}{e^{\alpha_0+\beta}+e^{\alpha_1+\beta+\gamma_1}+e^{\alpha_2-(\alpha_2-300)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_3}}}{K_1} \\ \Phi_3(0) = \frac{e^{\alpha_3+\gamma_3}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_3}}}{K_0} \end{cases} \tag{6}$$

Where $\hat{\alpha_j} = \alpha_j + \gamma_j$ is the net value of choice j when there are no cash transfers, $K_0 = e^{\alpha_0} + e^{\alpha_1+\gamma_1} + e^{\alpha_2+\gamma_2} + e^{\alpha_3+\gamma_3}$ and $K_1 = e^{\alpha_0+\beta} + e^{\alpha_1+\gamma_1+\beta} + e^{\alpha_2-(\alpha_2-300)\tau+\gamma_2+\beta} + e^{\alpha_3+\gamma_3}$.

Suppose further that the probability of being employed in a bracket j is equal to the product of the share of youth who chooses that bracket times a probability of obtaining that job, and alternatively remaining unemployed, $P(.)$, due to search frictions.

$$Pr(Y_{ji} = 1) = Pr(z_{j*} = z_j) \cdot P(active, time) \tag{7}$$

where the magnitude of the search frictions depends on whether the youth has received activation measures (*active*) and on time spent searching (*time*). The activation term *active* is equal to zero in the control group, and equal to one once participants receive soft-skills training, counseling and network opportunities with *Garantie Jeunes*[28]. Finally, the dummy for time availability *time* is equal to one as a default and equal to zero in the first semester of enrollment, when the youth must attend activities offered at the YECs risking the so-called lock-in effect. As Figure 1 suggests, this is the case in the first semester of enrollment in *Garantie Jeunes*.

Note that Equation 6 derives from consumers maximizing their utility as-if search frictions did not exist. That is, they choose the optimal employment they will look for only as a function of *cash* and η_i, without considering that they could have more/less probabilities of obtaining the job. This corresponds to fully separate the channel of the cash transfer (labor supply) and of activation measures (search frictions)[29]. At this point, we can plug Equation 6 into Equation 7, obtaining $Pr(Y_{ji} = 1)$ for every income bracket j, conditional on being at different stages of the program and being in treatment or control (Table 6).

Table 6: Structural interpretation of the probability of employment in different income brackets, $Pr(Y_{ji} = 1)$, for compliers in treatment and control groups, at different stages of the program.

$Pr(Y_{ji} = 1)$ in treatment group

	Monthly income €1-€300	Monthly income€300-€1100	Monthly income over €1100
1st semester of enrollm.	$\Phi_1(0)\frac{K_0}{K_1}e^{\beta} \cdot P(1,0)$	$\Phi_2(0)\frac{K_0}{K_1}e^{\beta-(\alpha_2-300)\tau} \cdot P(1,0)$	$\Phi_3(0)\frac{K_0}{K_1} \cdot P(1,0)$
2nd semester of enrollm.	$\Phi_1(0)\frac{K_0}{K_1}e^{\beta} \cdot P(1,1)$	$\Phi_2(0)\frac{K_0}{K_1}e^{\beta-(\alpha_2-300)\tau} \cdot P(1,1)$	$\Phi_3(0)\frac{K_0}{K_1} \cdot P(1,1)$
After completion	$\Phi_1(0) \cdot P(1,1)$	$\Phi_2(0) \cdot P(1,1)$	$\Phi_3(0) \cdot P(1,1)$

$Pr(Y_{ji} = 1)$ in control group

	Monthly income €1-€300	Monthly income €300-€1100	Monthly income over €1100
1st semester of enrollm.	$\Phi_1(0) \cdot P(0,1)$	$\Phi_2(0) \cdot P(0,1)$	$\Phi_3(0) \cdot P(0,1)$
2nd semester of enrollm.	$\Phi_1(0) \cdot P(0,1)$	$\Phi_2(0) \cdot P(0,1)$	$\Phi_3(0) \cdot P(0,1)$
After completion	$\Phi_1(0) \cdot P(0,1)$	$\Phi_2(0) \cdot P(0,1)$	$\Phi_3(0) \cdot P(0,1)$

Notes. The table reports structural interpretation of $Pr(Y_{ji} = 1)$ the probability of being actually employed in bracket j conditional on enrollment status in *Garantie Jeunes*. It is obtained from Equation 6 and Equation 7.

To estimate the different parameters, I need to equate the model-based interpretations of $Pr(Y_{ji} = 1)$ of Table 6 to their empirical counterpart. To do so, I need to recover estimates of average outcomes for control group takers, who cannot otherwise be distinguished from non-takers, at different stages of enrollment in *Garantie Jeunes*. For this purpose, I can subtract estimates of the LATEs to average outcomes for takers in the treatment group in Table 5 to obtain average outcomes in control group compliers (Imbens and

[28] The relationship between *active* and $P(.)$ is ambiguous *ex-ante*: although I might expect that the knowledge derived from activities provided by *Garantie Jeunes* improves search efficacy, it could also disorient the youth (choice overload), or make him overconfident, or represent a stigma, decreasing the probability of finding employment. One option is that *Garantie Jeunes* increases η_i, or that the youth was underestimating her η_i and now bids for higher z^*. I tend to exclude the hypothesis that *Garantie Jeunes* leads to shocks to η_i since I find no effect on wage per-hour worked.

[29] Although this structure might appear simplistic, it is useful as an extreme case. Also, in the context of inexperienced youth this hypothesis might be realistic that youth only care about their direct incentives to supply labor, failing to incorporate the risk of not being hired

Rubin, 1997). For example, having an estimate of $\mathbb{E}(Y_{ji}(D_i)|0 < D_i \leq 2)$ and of $\mathbb{E}(Y_{ji}(D_i)|0 < D_i \leq 2) - \mathbb{E}(Y_{ji}(0)|0 < D_i \leq 2)$, I can recover $\mathbb{E}(Y_{ji}(0)|0 < D_i \leq 2)$.

By equating each of the estimated average outcomes for takers in treatment and control to their structural interpretation in Table 6, I obtain a system of 18 equations with 9 unknowns of interest, $P(1,1)$,$P(1,0)$,$P(0,1)$,$\hat{\alpha_0}$,$\hat{\alpha_1}$,$\hat{\alpha_2}$,$\hat{\alpha_3}$, β, $\alpha_2\tau$. However, the system is not identified since only 8 equations are linearly independent, once the left-hand side is considered equal to the structural interpretation plus noise. A first solution solution is to focus instead on recovering $P(1,0)/P(1,1)$,$P(1,1)/P(0,1)$, β, $\alpha_2\tau$, which correspond to the four meaningful effects of interest. In addition, one can estimate K_0/K_1, which captures the spillovers of β and $\alpha_2\tau$. In this case, the system becomes over-identified, with multiple estimates of some parameters. Hence, I average the different estimates (the detailed procedure is reported in the Appendix).

Results are reported Table 7. In the Table, $e^{\alpha_2\tau}$ corresponds to the decrease in employment due to implicit taxation from cash transfer phase-out. instead, e^{β} estimates how much cash transfers attract youths in unemployment or part-time jobs, increasing the probability of youths being in these brackets where the transfer is guaranteed, which can be interpreted as moral hazard/liquidity effect. Turning to $P(1,0)/P(1,1)$ and $P(1,1)/P(0,1)$, these are respectively the effect of lock-in from training and of activation improving job search. Finally, $\frac{K_0}{K_1}$ reports the change in all brackets due to the introduction of cash transfers and implicit taxation in others. Note that all these estimates are to be interpreted as multiplicative factors of the probability of employment.

Column (1) shows the results when different estimates of the parameters are simply averaged. Alternatively, one might want to take into consideration the different levels of significance of the underlying LATEs, so Column (2) of the table reports the estimates using a weighted average, weighting by the average of the inverse of the standard errors squared of the LATEs used to derive the components of the effect. Finally, one can note that when normalizing one parameter, the system equating Table 5 and Table 6 (Equation 9 in the Appendix) becomes a just identified system. Then, estimates of the effects of interest can be recovered by solving the system with weighted Nonlinear Least Squares. The estimates obtained with this method are reported in Column (3).

Table 7: Estimated structural effects and interpretation (multiplicative effect on $\mathbb{E}(Y_{ji})$).

Effect (interpretation)	(1)	(2)	(3)
$e^{-(\alpha_2-300)\tau}$ (implicit tax)	.219	.136	.483
e^{β} (moral h./liquidity)	1.008	1.055	1.163
$\frac{K_0}{K_1}$ (cash tr. spillovers)	.606	.605	.952
$\frac{P(1,0)}{P(1,1)}$ (lock-in)	.648	.645	.608
$\frac{P(1,1)}{P(0,1)}$ (activation)	2.042	2.125	2.096
Method	Avg. of estimates	Weighted avg. of estim.	Solve system by wNLS

Notes. The table reports the estimated structural parameters obtained by equating the structural interpretation in Table 6 to the average outcomes of compliers in treatment (estimated from the data) and of compliers in the control group (obtained by subtracting the effect in Table 5 to average outcomes of compliers in treatment). In column (1) and (2) the effects are obtained by solving for the effects and averaging the different estimates, with or without weights for inverse standard errors of LATE terms involved, as detailed in the Appendix. In column (3) normalizing α_0 provides 8 linearly independent equations and 8 unknowns (leftmost column) which can be estimated and used to recovered the distribution of $Pr(z_{j*} = z_j)$ and effets of different components of *Garantie Jeunes*. The effects in the last column are multiplicative.

The results concerning the effect of implicit taxation show that implicit taxation drives away enrolled youths from the implicitly-taxed brackets, reducing employment by 52%-86% depending on the estimation method. In fact, the first row of Table 7 suggests that the presence of implicit taxation multiplies expected employment by a factor ranging between .136 and .483. In the most conservative case, given the 55% implicit tax rate, this corresponds to a 0.9 percentage points reduction in employment for each point of marginal implicit taxation. The implied elasticity to net-of-tax earnings depends on the average earnings of youths who react to the cash transfer, which in this discrete-choice interpretation cannot be determined precisely. However, if one assumes that youths react on the intensive margin (i.e. they move to the lower income bracket), then back of envelope calculations show that the implied elasticity of earnings to the net-of-tax rate is between .4 and .8, hence quite high compared to the literature. Turning to the moral hazard/liquidity effect, this is corresponds to a 1-16% increase in the probability of working in brackets where the transfer is guaranteed. The large effect of implicit taxation and the relatively smaller reaction to cash transfers availability can be seen as suggestive evidence that moral hazard/liquidity effects are contained in *Garantie Jeunes*.

Turning to the effect of activation measure, the results point at a negative lock-in effect, reducing expected employment by 35-40%. This shows that youths participating in the program face significant time constraints. Finally, the positive effect of activation of youths is extremely high, corresponding, on average, to more than doubling employment in cells where youths have been activated. As a consequence of this large effect, the probability of finding for youths to find a job in the control is very low, estimated at only 48% in a semester. This points out that at baseline takers of *Garantie Jeunes* have very low matching probability, either because they face high search frictions, or because they would exert low effort in absence of the program.

6 Discussion: Are There Complementarities between Cash Transfers and Activation Measures?

Cash transfers and activation measures can interact among each other. In fact, the large magnitude of the effect of activation I find, and the relatively small moral hazard/liquidity effect estimated, could arise from "complementarities" between cash transfers and activation measures. For example, youths may fear the risk of being sanctioned (suspension of the cash transfer) and activities in *Garantie Jeunes* could represent monitoring device, pushing youths to exert search effort (Boone et al., 2007). Or, more optimistically perhaps, the cash transfer allows youths to put time and effort in the activities offered by *Garantie Jeunes*, which they otherwise wouldn't be able to do due to financial constraints. My setting is not equipped to identify complementarities precisely. This would require not only an exogenous shock to cash transfers or activation measures in *Garantie Jeunes*, but probably also modelling the sources of complementarities and measuring them (e.g. number of applications sent, to which jobs). By now, a comparison with the literature might be suggestive of some conclusions.

A work closely related to mine is Aeberhardt et al. (2020). This working paper studies the effect of an increase in cash transfers but keeps activation measures constant, in the same context as that of this paper. The authors consider an experimental program introduced in a small set of French YECs in 2011, which offered a similar cash transfer to youths in the standard YECs program, but no extra activities. The cash transfer was equivalent to that of *Garantie Jeunes* in terms of cumulative amount, but was spread over two years rather than one, and was not cumulative with job earnings since the first euro earned. Hence, the monthly amount of the transfer and the rate of implicit taxation were roughly half than in *Garantie Jeunes*. Crucially, in the setting of Aeberhardt et al. (2020) youths are only required to attend monthly counseling sessions, the standard program at YECs that was available also for control group youths, while *Garantie Jeunes* requires a month of initial intensive training, several job immersions, and twice as much counseling[30] Aeberhardt et al. (2020) find that the program they evaluated increased the amount of time youth stayed at YECs, and increased attendance at compulsory activities. Yet, the effect on search effort was null, while employment decreased slightly in the first six months of the program.

Firstly, part of the dissimilarity between the estimated effects on employment in this paper and in Aeberhardt et al. (2020) could be explained by the longer duration of the cash transfer and by the fact that implicit marginal taxation is imposed on the first euro earned in their case. Yet, it seems natural to attribute some of the difference in results to the significant additional activation requirements introduced by *Garantie Jeunes*. Boone et al. (2007) suggest that, with significant cash benefits, activation can function as a monitoring device. In the setting of Aeberhardt et al. (2020), cash disincentivizes search effort, and augments the potential loss in case of sanctions for not respecting conditionality. Yet, the risk of sanctions can be scarcely credible without extra monitoring (and vice versa, monitoring can have a larger effect if benefits are larger). It is important to note that complementarities might arise from different sources ahan monitoring, for instance

[30]It should be noted that there are additional sources of difference with their study. A first one might be selection of the takers, since in *Garantie Jeunes* eligible youths are selected on motivation and fragility, requiring a sunk cost of application, while in the setting of Aeberhardt et al. (2020) all youths in randomly selected YECs and cohorts are offered the cash transfer with no anticipation by them. Or, the commitment by YECs in implementing *Garantie Jeunes*, which was for them a structural change and a political spotlight, might have played a role, while for the experiment of Aeberhardt et al. (2020) YECs were mostly running business as usual. For instance, Aeberhardt et al. (2020) report a large drop in take-up after the first year of enrollment, when youth employment centers have to actively renovate the contract with the youth, checking the respect of activation conditions. For comparison, in *Garantie Jeunes* counselors are required to check monthly, and to provide detailed proof to central government (e.g. work contracts of the youth, proof of attendance).

if youths are not able to exert effort in activities offered due to credit constraints (e.g. if they need to work when not attending the training sessions).

An opposite setting to that of Aeberhardt et al. (2020) would be a shock to activation measures but not to cash transfers. This is not available with the exact mix of activities of *Garantie Jeunes*. Some working papers indicate a large positive effect of job search assistance in French YECs (Crépon et al., 2013b) and of collective counseling sessions (van den Berg et al., 2015) to disadvantaged youth in France. In the US context, Fein and Hamadyk (2018) evaluate a year-long youth program in the US, called "Year-Up", with similar activities to *Garantie Jeunes*, finding substantial positive effects[31]. More generally, Card et al. (2018) run a meta-analysis of the extensive literature on active labor market policies. They show that "work-first" programs tend to have positive impact in the 1-2 years after activation, an effect which is stronger for disadvantaged youth[32]. The present paper also finds a positive effect of activation, but the magnitude is large compared to programs reviewed in Card et al. (2018), and even slightly larger than the effects found by Crépon et al. (2013b); van den Berg et al. (2015) and Fein and Hamadyk (2018). Crucially, the effect of *Garantie Jeunes* appears to be driven by the share of youths who has completed the program and is not receiving anymore the cash transfers. Again, this might suggest that the extra chunk of positive effet from activation arises from the interaction with cash transfers. In this perspective, cash transfers reduce labor supply but add an extra channel to the effect of activation, through an increase in monitoring.

7 Conclusions

In this paper I studied a case of a labor market policy offering an intense activation program and generous monthly cash transfers to young NEETs. The results point in the direction of a strong positive effect of the program in the year after completion, and no effect during enrollment in the program.

This work speaks chiefly to the literature on employment policies.Prior research has mostlry evaluated active policies, such as activation measures, conditional on a given level of passive policies, such as cash transfers, and vice versa. This paper provides the first evidence of the joint effect of cash transfers and activation measures. The results suggest large positive effects after completion, driven by activation. This signals a significant role of search frictions for this population, with control youths facing very low matching probabilities. Secondly, the results provide empirical insights for the literature on labor supply and job search behavior. I estimate a 52% reduction in employment as a reaction to a 55% increase in implicit taxation from benefits phase-out, implying an elasticity to net-of-tax rate between .4 and .8 for this very specific population. I also confirm the role of time and activation in determining job search efficacy, as in Gautier et al. (2018). Methodologically, my rolling diff-in-diff methodology is relevant for studies where units enter the population of interest in group-cohort cells, and are exposed to treatment at different tenures. When a treatment is adopted by these groups in a staggered fashion, so that units are exposed to treatment at different tenures, the diff-in-diff methodology proposed is flexible for estimating dynamic ITT and LATE, is robust to selection into treatment over tenure, as well as to heterogeneous treatment effects. Although

[31]The program is a sectoral training program, it includes a selection process based on motivation and skills at the beginning, before random assignment, and also includes a specific training component. Their effect on earnings is +60% when adjusting for take-up, which is similar to what we find for *Garantie Jeunes*. Yet, it is estimated over a less disadvantaged population, selected already at ITT level, so their estimate is pushed down by higher average earnings in the control group.

[32]The effectiveness of activation policies is shown to depend a lot on the mix of actions. Yet, it should be kept in mind that other factors such as target populations (Kluve et al., 2019; Babcock et al., 2012) and market conditions (Crépon et al., 2013a) matter.

tailored for our setting, this setting is not uncommon in applied work. For example, we can imagine a similar setting for a school restructuring program, where cohorts are age cohorts, tenure is their school grade, and the program is staggeredly adopted by schools (Martorell et al., 2016; Neilson and Zimmerman, 2014).

I suggest three main avenues for future research. First, this paper is not able to disentangle the exact magnitude and nature of complementarities between cash transfers and activation measures. Future studies should look for shocks and direct measures of the possible sources of complementarities, namely monitoring, motivation, and job search technology components of activation measures. The question is extremely relevant for understanding if providing social insurance to disadvantaged populations generates a story of moral hazard, or an escape from a poverty trap. Second, the external validity of the study is limited, due to the very peculiar selection process of youths in *Garantie Jeunes*. Whether results will be confirmed for broader populations, and why they might differ, remains an open question. The effect might decrease, in fact, not only because unselected youths can be less motivated, but also for organizational changes on the side of YECs. In the early deployment of *Garantie Jeunes*, studied in this paper, YECs concentrated a lot of effort on few youths in a new, politically salient program, which might not be possible with an expansion. Third, externalities represent a challenge for policy evaluators, whether positive or negative. If the program is extended, displacement effects on other disadvantaged job seekers will become more likely (Naegele et al., 2015). Alternatively, given the extremely disadvantaged population targeted by *Garantie Jeunes*, positive externalities might arise from a reduction in the crime rates of participants (Britto et al., 2020). Sociological evaluation by Loison-Leruste et al. (2016) reports abundant anecdotal evidence of youths in *Garantie Jeunes* grown up in high-delinquency environments.

This work has relevant policy implications. The simplest one is that it proves the mix of services and cash transfers provided by *Garantie Jeunes* is effective, in line with pilot evidence by Gaini et al. (2018) and qualitative results by Gautié (2018). This offers counterfactual evidence in support of comparative analysis recommending to combine active and passive labor market policies OECD (2020); Pignatti and Van Belle (2018). In fact, my estimates show that passive policies such as cash transfers can disincentivize labor supply, and that program phase-out is costly due to implicit taxation. A possible solution would be allowing youths to fully cumulate benefits and job earning, but this could clearly be costly. Activation is shown to be a viable alternative, as its effect is estimated strong enough to compensate for lock-in and distortive effects of the cash transfers. Finally, my insights can be used to study other policies that combine cash transfers and activation policies, like many minimum income schemes or unemployment insurance with activation requirements. However, external validity should be handled with care. *Garantie Jeunes* concerned only a very selected population, with an application process based on motivation and fragility, run at a decentralized level by well-established YECs. The estimated benefits are coming in large part from agency jobs and fixed-term contracts, while the costs are only 15 points lower than total benefits after 2 years. In fact, even in the forthcoming extension of the program, it might not be easy to maintain cost-effectiveness, as returns on the marginally eligible youth can be decreasing.

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Proofs

Proof of Proposition 1

$$
\begin{aligned}
&\mathbb{E}[DID^h_{w,c}|G^h_{w,c}] = \\
&= \mathbb{E}\left[Y^h_{w,c} - Y^h_{w,c'} - \sum_{w'\in\Omega_w} \frac{n_{w',c}}{N_{\Omega_{w,c}}}(Y^h_{i,w',c} - Y^h_{i,w',c'})\,\middle|\, G^h_{w,c}\right] \\
&= \mathbb{E}\left[Y^h_{w,c}(g) - Y^h_{w,c'}(0) - \sum_{w'\in\Omega_w} \frac{n_{w',c}}{N_{\Omega_{w,c}}}(Y^h_{i,w',c}(0) - Y^h_{i,w',c'}(0))\,\middle|\, G^h_{w,c}\right] \\
&= \mathbb{E}[Y^h_{w,c}(g) - Y^h_{w,c}(0)|G^h_{w,c}] + \mathbb{E}[Y^h_{w,c}(0) - Y^h_{w,c'}(0)] - \sum_{w'\in\Omega_w} \frac{n_{w',c}}{N_{\Omega_{w,c}}}\mathbb{E}[Y^h_{i,w'c}(0) - Y^h_{i,w'c'}(0)] \\
&= \mathbb{E}[\Delta^{ITT}(h,w,c)|G^h_{w,c}]
\end{aligned}
$$

The first equality applies the definition in (1), the second explicits realized outcomes, the third is obtained by adding and subtracting $Y^h_{w,c'}(0)$ plus strong exogeneity, while the last follows from common trends. $E[DID^h_{w,c}] = \mathbb{E}[\Delta^{ITT}(h,w,c)]$ follows by the law of iterated expectations.

Proof of Proposition 2

$$
\begin{aligned}
\mathbb{E}[DID^g] &= \sum_{(w,c|h):G=g} \frac{n_{w,c}}{\sum_{(w,c|h):G=g} n_{w,c}} \mathbb{E}[DID^h_{w,c}] \\
&= \sum_{(w,c|h):G=g} \frac{n_{w,c}}{\sum_{(w,c|h):G=g} n_{w,c}} \mathbb{E}(\Delta^{ITT}(h,w,c)) \\
&= \sum_{(w,c|h):G=g} \frac{n_{w,c}}{\sum_{(w,c|h):G=g} n_{w,c}} \mathbb{E}\left[Y^h_{w,c}(g) - Y^h_{w,c}(0)\right] \\
&= \mathbb{E}\{\mathbb{E}[Y^h_{w,c}(g)|G^h_{w,c}=g] - \mathbb{E}[Y^h_{w,c}(0)|G^h_{w,c}=g]\} \\
&= \mathbb{E}[\Delta^{ITT}(g)]
\end{aligned}
$$

Where the first equality is the definition of DID^g in Proposition 2, the second relies on the proof of Proposition 1, the third is the definition of $\Delta^{ITT}(h,w,c)$, the fourth uses the definition of expectation and the last relies on the Law of Iterated Expectations.

Proof of Proposition 3

It follows straightforwardly from the definition of expectations that

$$
\begin{aligned}
\mathbb{E}(DID^h_{w,c}) =& \,\mathbb{E}(Y^h_{w,c}(g) - Y^h_{w,c}(0)) \\
=& \,\mathbb{E}\left[\sum_{i \in w,c|h} Y^h_{i,w,c}(d > 0) - Y^h_{i,w,c}(0) \middle| D^h_{i,w,c} > 0\right] Pr(D^h_{i,w,c} > 0) \\
=& \sum_{d=1}^{g} \mathbb{E}\left[\sum_{i \in w,c|h} Y^h_{i,w,c}(d) - Y^h_{i,w,c}(0) \middle| D^h_{i,w,c} = d\right] Pr(D^h_{i,w,c} > 0) \qquad\qquad \forall\, w, c, h
\end{aligned}
$$

Appendices

A ITT Through an Event-Study Approach

A more common approach in the literature is the event-study approach. This approach uses multiple-ways fixed-effects regressions to estimate dynamic treatment effects. The approach it is proven non-robust to heterogeneous effects in groups and time by De Chaisemartin and D'Haultfœuille (2020b).

The regression-based equivalent to our setting is a diff-in-diff specific to each tenure level. Denoting γ_c our cohort fixed effects, μ_m YEC fixed effects (with each YEC belonging to one wave), and η_h the tenure fixed effects the regression to run for each tenure level is:

$$y_i^h = \sum_{g>0} \beta^{h,g} \mathbb{1}(G_{w,c}^h = g) + \gamma_c^h + \mu_m^h + \epsilon_i^h \quad \forall\, h$$

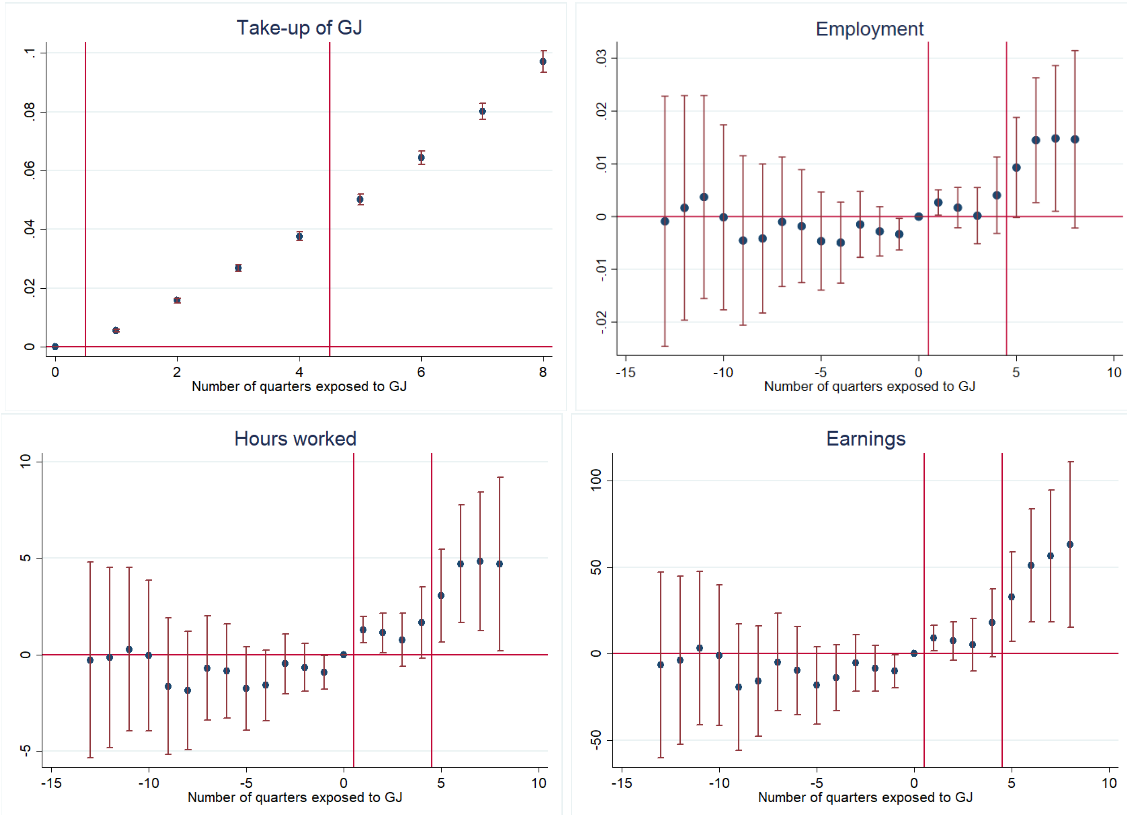
Which stacking all h in one regression, and with uniform β across tenure level h, is equivalent to :

$$y_{i,h} = \sum_{g} \beta^g \mathbb{1}(G_{w,c}^h = g) + \gamma_{c,h} + \mu_{m,h} + \epsilon_{i,h} \tag{8}$$

Figure 10 reports in the upper-left panel the results of the first-stage regression, where the outcome is a dummy for having been actually enrolled in *Garantie Jeunes*, regressed on exposure to *Garantie Jeunes*. The other three panels report three different reduced-form regressions, where the outcome y is respectively an employment dummy, the total earnings in the quarter, and the number of hours worked in the quarter. Standard errors are double-clustered at the YEC and YEC-tenure class level, following Cameron and Miller (2015). Borusyak and Jaravel (2017) point out that if one wants to include all leads and lags of treatment g, β^g is unidentified in the fully dynamic specification, since infinite sets of program effects $\{\beta^g\}$ can be obtained with linear combinations of $\gamma_{c,h}$ and $\mu_{m,h}$. Following solutions suggested in the same paper, we use a semi-dynamic specification for identifying program effects, with only leads and no lags of treatment. We report fully dynamic specifications in the Appendix to confirm parallel trends.

The results are similar to the ones obtained with our ad-hoc methodology, though not identical. In fact, the results tend to be more positive and more significant, especially in the first year of exposure to *Garantie Jeunes*, while effects in the second year of exposure are more stable. This differences suggest the presence of heterogeneity in the dynamic ITT effects in different waves of *Garantie Jeunes* (i.e. in different YECs), but reassures on the level of bias that would have actually emerged using an event-study methodology. It also reassures on the soundness of our rolling diff-in-diff.

Figure 10: Intent to treat (ITT) estimates using the event-study approach.



Notes. The upper right panel of the figure reports coefficients and 95% confidence intervals for the first stage regression, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward, and the independent variable is a dummy for exposure to *Garantie Jeunes*, as in Regression 8. The other three panels report reduced-form regressions where the outcomes are a dummy equal to one if the individual has been employed at least once in the quarter, the total amount of earnings, and the total amount of hours.

B Estimation of structural parameters

By equating each of the estimated average outcomes in treatment and control to their structural interpretation I obtain the following system:

$$
\begin{cases}
\mathbb{E}(Y_{1i}(D_i)|0 < D_i \leq 2) & = \Phi_1(1) \cdot P(1,0) \\
\mathbb{E}(Y_{2i}(D_i)|0 < D_i \leq 2) & = \Phi_2(1) \cdot P(1,0) \\
\mathbb{E}(Y_{3i}(D_i)|0 < D_i \leq 2) & = \Phi_3(1) \cdot P(1,0) \\
\mathbb{E}(Y_{1i}(D_i)|2 < D_i \leq 4) & = \Phi_1(1) \cdot P(1,1) \\
\mathbb{E}(Y_{2i}(D_i)|2 < D_i \leq 4) & = \Phi_2(1) \cdot P(1,1) \\
\mathbb{E}(Y_{3i}(D_i)|2 < D_i \leq 4) & = \Phi_3(1) \cdot P(1,1) \\
\mathbb{E}(Y_{1i}(D_i)|D_i > 4) & = \Phi_1(0) \cdot P(1,1) \\
\mathbb{E}(Y_{2i}(D_i)|D_i > 4) & = \Phi_2(0) \cdot P(1,1) \\
\mathbb{E}(Y_{3i}(D_i)|D_i > 4) & = \Phi_3(0) \cdot P(1,1) \\
\mathbb{E}(Y_{1i}(0)|0 < D_i \leq 2) & = \Phi_1(0) \cdot P(0,1) \\
\mathbb{E}(Y_{2i}(0)|0 < D_i \leq 2) & = \Phi_2(0) \cdot P(0,1) \\
\mathbb{E}(Y_{3i}(0)|0 < D_i \leq 2) & = \Phi_3(0) \cdot P(0,1) \\
\mathbb{E}(Y_{1i}(0)|2 < D_i \leq 4) & = \Phi_1(0) \cdot P(0,1) \\
\mathbb{E}(Y_{2i}(0)|2 < D_i \leq 4) & = \Phi_2(0) \cdot P(0,1) \\
\mathbb{E}(Y_{3i}(0)|2 < D_i \leq 4) & = \Phi_3(0) \cdot P(0,1) \\
\mathbb{E}(Y_{1i}(0)|D_i > 4) & = \Phi_1(0) \cdot P(0,1) \\
\mathbb{E}(Y_{2i}(0)|D_i > 4) & = \Phi_2(0) \cdot P(0,1) \\
\mathbb{E}(Y_{3i}(0)|D_i > 4) & = \Phi_3(0) \cdot P(0,1)
\end{cases}
$$

For simpler notation, denote $\ln(\mathbb{E}(Y_{ji}(treated)|D_i) = y_{j,\bar{d}}(treated)$, where $\bar{d} = 1$ if $0 < D_i \leq 2$, $\bar{d} = 2$ if $2 < D_i \leq 4$, $\bar{d} = 3$ if $D_i > 4$. Then taking logs from both sides:

$$\begin{cases} y_{1,1}(1) &= \hat{\alpha_1} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-\alpha_2\tau+\beta} + e^{\hat{\alpha_3}}) + \beta + p(1,0) \\ y_{2,1}(1) &= \hat{\alpha_2} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-\alpha_2\tau+\beta} + e^{\hat{\alpha_3}}) + \beta - (\alpha_2 - 300)\tau + p(1,0) \\ y_{3,1}(1) &= \hat{\alpha_3} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-\alpha_2\tau+\beta} + e^{\hat{\alpha_3}}) + p(1,0) \\ y_{1,2}(1) &= \hat{\alpha_1} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-\alpha_2\tau+\beta} + e^{\hat{\alpha_3}}) + \beta + p(1,1) \\ y_{2,2}(1) &= \hat{\alpha_2} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-\alpha_2\tau+\beta} + e^{\hat{\alpha_3}}) + \beta - (\alpha_2 - 300)\tau + p(1,1) \\ y_{3,2}(1) &= \hat{\alpha_3} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-\alpha_2\tau+\beta} + e^{\hat{\alpha_3}}) + p(1,1) \\ y_{1,3}(1) &= \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\ y_{2,3}(1) &= \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\ y_{3,3}(1) &= \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\ y_{1,1}(0) &= \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{2,1}(0) &= \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{3,1}(0) &= \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{1,2}(0) &= \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{2,2}(0) &= \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{3,2}(0) &= \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{1,3}(0) &= \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{2,3}(0) &= \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\ y_{3,3}(0) &= \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \end{cases} \tag{9}$$

Let us try to recover $p(1,1) - p(0,1)$, $p(1,1) - p(1,0)$,$\alpha_2\tau$, β. Note that there are multiple configurations of the system, including different combinations of different lines, that one can use to recover each parameter (see Appendix). This alternative configurations deliver different estimates of the parameters. To avoid cherry picking, I will estimate each parameter as an average of all possible ways to recover it. This means:

$$\begin{cases} y_{1,1}(0) - y_{2,1}(0) &= \hat{\alpha_1} - \hat{\alpha_2} \\ y_{1,2}(0) - y_{2,2}(0) &= \hat{\alpha_1} - \hat{\alpha_2} \\ y_{1,3}(0) - y_{2,3}(0) &= \hat{\alpha_1} - \hat{\alpha_2} \end{cases} \Rightarrow \widehat{\alpha_1 - \alpha_2} = \frac{y_{1,1}(0) - y_{2,1}(0) + y_{1,2}(0) - y_{2,2}(0) + y_{1,3}(0) - y_{2,3}(0)}{3}$$

$$\begin{cases} y_{1,1}(0) - y_{3,1}(0) &= \hat{\alpha_1} - \hat{\alpha_3} \\ y_{1,2}(0) - y_{3,2}(0) &= \hat{\alpha_1} - \hat{\alpha_3} \\ y_{1,3}(0) - y_{3,3}(0) &= \hat{\alpha_1} - \hat{\alpha_3} \end{cases} \Rightarrow \widehat{\alpha_1 - \alpha_3} = \frac{y_{1,1}(0) - y_{3,1}(0) + y_{1,2}(0) - y_{3,2}(0) + y_{1,3}(0) - y_{3,3}(0)}{3}$$

$$\begin{cases} y_{1,1}(1) - y_{2,1}(1) - \widehat{\alpha_1 - \alpha_2} &= \alpha_2\tau \\ y_{1,2}(1) - y_{2,2}(1) - \widehat{\alpha_1 - \alpha_2} &= \alpha_2\tau \end{cases} \Rightarrow \widehat{\alpha_2\tau} = \frac{y_{1,1}(1) - y_{2,1}(1) - \widehat{\alpha_1 - \alpha_3} + y_{1,2}(1) - y_{2,2}(1) - \widehat{\alpha_1 - \alpha_2}}{2}$$

$$\begin{cases} y_{1,1}(1) - y_{3,1}(1) - \widehat{\alpha_1 - \alpha_3} &= \beta \\ y_{1,2}(1) - y_{3,2}(1) - \widehat{\alpha_1 - \alpha_3} &= \beta \end{cases} \Rightarrow \widehat{\beta} = \frac{y_{1,1}(1) - y_{3,1}(1) - \widehat{\alpha_1 - \alpha_3} + y_{1,2}(1) - y_{3,2}(1) - \widehat{\alpha_1 - \alpha_3}}{2}$$

$$\begin{cases} y_{1,2}(1) - y_{1,1}(1) & = p(1,1) - p(1,0) \\ y_{2,2}(1) - y_{2,1}(1) & = p(1,1) - p(1,0) \\ y_{3,2}(1) - y_{3,1}(1) & = p(1,1) - p(1,0) \end{cases} \Rightarrow p(1,\widehat{1) - p}(1,0) = \frac{y_{1,2}(1) - y_{1,1}(1) + y_{2,2}(1) - y_{2,1}(1) + y_{3,2}(1) - y_{3,1}(1)}{3}$$

$$\begin{cases} y_{1,3}(1) - y_{1,1}(0) & = p(1,1) - p(0,1) \\ y_{2,3}(1) - y_{2,1}(0) & = p(1,1) - p(0,1) \\ y_{3,3}(1) - y_{3,1}(0) & = p(1,1) - p(0,1) \\ y_{1,3}(1) - y_{1,2}(0) & = p(1,1) - p(0,1) \\ y_{2,3}(1) - y_{2,2}(0) & = p(1,1) - p(0,1) \\ y_{3,3}(1) - y_{3,2}(0) & = p(1,1) - p(0,1) \\ y_{1,3}(1) - y_{1,3}(0) & = p(1,1) - p(0,1) \\ y_{2,3}(1) - y_{2,3}(0) & = p(1,1) - p(0,1) \\ y_{3,3}(1) - y_{3,3}(0) & = p(1,1) - p(0,1) \end{cases}$$

$$\Rightarrow p(1,\widehat{1) - p}(0,1) = [y_{1,3}(1) - y_{1,1}(0) + y_{2,3}(1) - y_{2,1}(0) + y_{3,3}(1) - y_{3,1}(0) +$$
$$y_{1,3}(1) - y_{1,2}(0) + y_{2,3}(1) - y_{2,2}(0) + y_{3,3}(1) - y_{3,2}(0) +$$
$$y_{1,3}(1) - y_{1,3}(0) + y_{2,3}(1) - y_{2,3}(0) + y_{3,3}(1) - y_{3,3}(0)] \cdot 1/9$$

$$\widehat{k_0 - k_1} = y_{3,2}(1) - y_{3,3}(1)$$

Alternatively, one can directly estimate the following regression:

$$y_{j,\bar{d}}(treated) = F(\hat{\alpha}, \beta, \alpha_2\tau, p(0,1), p(1,1), p(1,0)) \tag{10}$$

Where F is defined by 9. The results are reported below.

Table 8: Estimated structural parameter, effect, and interpretation as multiplicative effect on $\mathbb{E}(Y_{ji})$.

Parameter		$Pr(z^{j*} = z_j) = \phi_j(treat)$		Effect (interpretation)	
$\hat{\alpha_0}$	norm. to 0	$\phi_1(0)$	.105	$e^{-(\alpha_2 - 300)\tau}$ (implicit tax)	.483
$\hat{\alpha_1}$	-1.652	$\phi_2(0)$	.130		
$\hat{\alpha_2}$	-1.436	$\phi_3(0)$	.215	e^{β} (moral h./liquidity)	1.163
$\hat{\alpha_3}$	-.935	$\phi_1(1)$	.116		
β	.151	$\phi_2(1)$	.069	$\frac{K_0}{K_1}$ (cash tr. spillovers)	.952
$\alpha_2\tau$	.726	$\phi_3(1)$	.205		
$P(1,0)$	.608			$\frac{P(1,0)}{P(1,1)}$ (lock-in)	.608
$P(1,1)$	1				
$P(0,1)$	.476			$\frac{P(1,1)}{P(0,1)}$ (activation)	2.096

Notes. The table reports the estimated structural parameters obtained by equating the structural interpretation in Table 6 to the average outcomes of compliers in treatment (estimated from the data) and of compliers in the control group (obtained by subtracting the effect in Table 5 to average outcomes of compliers in treatment). Normalizing α_0, this provides 8 linearly independent equations and 8 unknowns (leftmost column) which can be estimated and used to recovered the distribution of $Pr(z_{j*} = z_j)$ and effets of different components of *Garantie Jeunes*. The effects in the last column are multiplicative.

C Additional Tables and Figures

Table 9: Characteristics of youth at time of registration at YEC.

Quarter of registration	Number of registrations	N. ever in GJ every 1000	N. with less than vocat. secondary qualification	Mean age at registration	Share of males
	(1)	(2)	(3)	(4)	(5)
2013q1	120,251	0.00	0.22	20.28	0.52
2013q2	106,620	0.00	0.23	20.26	0.50
2013q3	150,618	0.00	0.17	19.95	0.49
2013q4	149,523	0.37	0.19	20.31	0.52
2014q1	125,791	0.79	0.22	20.46	0.53
2014q2	105,165	0.92	0.22	20.32	0.50
2014q3	153,138	0.98	0.17	19.85	0.48
2014q4	145,520	2.16	0.19	20.22	0.52
2015q1	117,903	2.13	0.22	20.34	0.52
2015q2	101,984	3.87	0.22	20.21	0.50
2015q3	144,077	4.34	0.16	19.78	0.50
2015q4	132,399	10.36	0.18	20.17	0.52
2016q1	108,002	8.36	0.21	20.26	0.53
2016q2	96,003	9.27	0.22	20.08	0.50
2016q3	133,726	7.25	0.16	19.69	0.50
2016q4	114,930	16.62	0.18	20.05	0.53

Notes. The table reports summary statistics for each cohort of youths registering to YECs. Vocational secondary qualifications are defined as less than CAP/BEP diploma, obtainable after 2-years of professional vocational studies.

Table 10: Number of youth enrolling in *Garantie Jeunes* by quarter and wave.

quarter	w13q4	w14q1	w14q2	w14q4	w15q1	w15q2	w15q3	w15q4	w16q1	w16q2	w16q3	w16q4	w17q1	w17q2	w17q3
2013q4	154														
2014q1	496	164													
2014q2	563	193	15												
2014q3	568	297	48												
2014q4	938	628	161	2											
2015q1	832	380	41	37	1185										
2015q2	1103	484	118	27	1416	1681									
2015q3	988	387	24	18	1230	1444	1184								
2015q4	1597	902	184	21	2410	2994	3082	188							
2016q1	1237	578	101	18	1915	2120	3372	111	80						
2016q2	1387	659	86	35	2053	2558	3558	160	211	670					
2016q3	1056	422	58	28	1536	1706	2564	111	200	454	393				
2016q4	1568	640	164	31	2673	3377	4498	216	261	794	770	532			
2017q1	1343	489	62	27	2089	2423	3976	142	292	731	986	523	851		
2017q2	1205	441	40	24	1880	1900	3026	97	265	585	706	320	1111	400	
2017q3	743	283	34	30	1081	1063	1649	73	191	324	379	146	660	202	27
2017q4	748	308	56	13	1443	1415	2345	114	238	462	490	267	709	289	31

Table 11: Number of youths registering to YEC by quarter and wave.

yq	w13q4	w14q1	w14q2	w14q4	w15q1	w15q2	w15q3	w15q4	w16q1	w16q2	w16q3	w16q4	w17q1	w17q2	w17q3
2013q1	8118	5121	485	378	14276	18945	27608	1721	3571	6928	8848	4383	13587	5436	846
2013q2	7460	4459	389	743	12725	16766	24025	1493	3191	6001	8175	3848	12192	4531	622
2013q3	11558	7066	453	394	18498	23951	32568	2112	4296	8609	11056	5266	17334	6457	1000
2013q4	10186	6382	592	443	17615	23885	33580	2356	4344	8622	11400	5734	16531	6777	1076
2014q1	8196	5361	415	373	14777	20054	28218	1809	3726	7274	9617	4739	14621	5762	849
2014q2	7247	4589	364	707	12128	16778	23320	1525	3071	6195	8063	4074	11943	4531	630
2014q3	11793	7209	507	372	18655	24478	32848	2442	4413	8989	11102	5619	17096	6585	1030
2014q4	10026	6268	585	361	17175	22666	32470	2187	4419	8445	11045	6170	16081	6457	1165
2015q1	8066	5081	468	341	13779	18700	26701	1738	3366	6896	9005	4766	13060	5145	791
2015q2	7402	4523	338	441	12588	16242	22087	1399	2902	6079	7751	3857	11554	4190	631
2015q3	11942	6760	417	381	17658	23143	30636	2039	3995	8175	10473	5392	15987	6175	904
2015q4	9487	5685	664	378	15679	20885	29727	1657	3902	7473	10047	5548	14565	5731	971
2016q1	7489	4524	431	297	12903	17156	24730	1467	3398	6172	8186	4320	11584	4690	655
2016q2	6926	4064	308	474	11379	15607	21497	1145	3058	5854	7132	3489	10642	3906	522
2016q3	11047	6210	451	379	15805	21627	29398	1691	4013	7942	9645	4801	14562	5502	653
2016q4	7956	4845	555	419	13527	18021	26211	1402	3662	6703	8724	4620	12620	5035	630

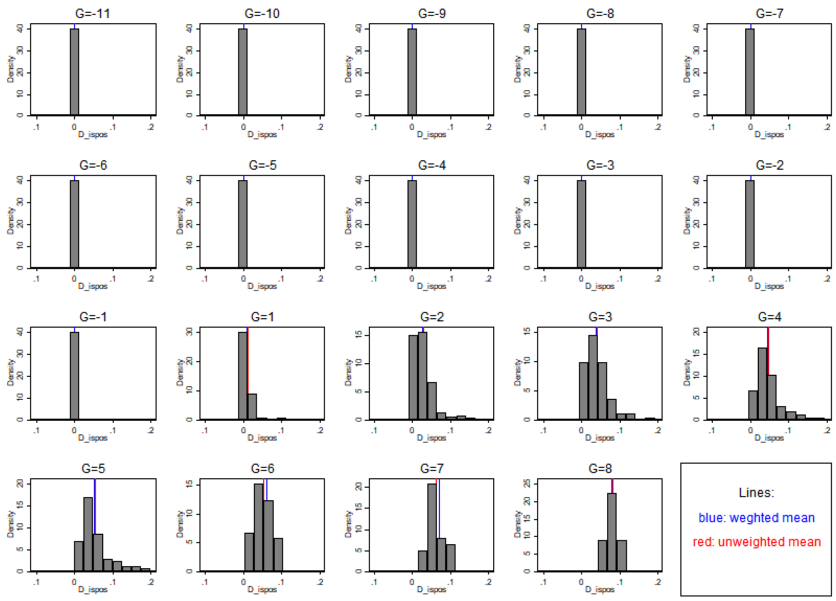
Table 12: Coefficients of reduced form and first stage for every wave (each line corresponds to one wave) and cohort (each column corresponds to one cohort of registration). YEC tenure is 4 quarters after registration. Colors represent the scale of the value in the cell relative to the table, red for positive green for negative.

$DID^4_{w,c}$	2013q1	2013q2	2013q3	2013q4	2014q1	2014q2	2014q3	2014q4	2015q1	2015q2	2015q3	2015q4	2016q1	2016q2
2014q2	0.0000	-0.0135	-0.0390	-0.0328	-0.0093	-0.0245	-0.0446	-0.0091	0.0516	-0.0270	-0.0003	0.0243	0.0273	0.0476
2014q4	0.0735	-0.1579	0.0000	-0.0004	0.0262	-0.1778	-0.0210	-0.0547	-0.0406	-0.1604	-0.0950	-0.0645	-0.0807	-0.1722
2015q1	-0.0440	-0.0193	-0.0096	0.0000	0.0023	0.0064	0.0144	0.0177	0.0073	-0.0101	0.0155	0.0097	-0.0063	-0.0016
2015q2	-0.0171	-0.0162	-0.0096	-0.0299	0.0000	0.0138	0.0185	0.0073	0.0071	-0.0008	0.0273	0.0163	0.0046	0.0082
2015q3	-0.0113	-0.0026	0.0098	0.0077	-0.0040	0.0000	-0.0052	-0.0081	-0.0055	-0.0055	0.0187	0.0066	-0.0072	-0.0028
2015q4	0.0711	0.0548	0.0613	0.0556	0.0593	0.0099	0.0000	0.0261	0.0269	0.0060	-0.0141	0.0428	0.0780	0.0460
2016q1	-0.0788	-0.1054	-0.1128	-0.1053	-0.0567	0.0001	-0.0033	0.0000	0.0102	-0.0183	-0.0193	-0.0242	-0.0181	-0.0509
2016q2	0.0279	0.0021	0.0249	0.0074	0.0044	-0.0045	0.0096	0.0018	0.0000	-0.0029	-0.0015	-0.0202	-0.0308	-0.0366
2016q3	-0.0380	-0.0188	0.0017	0.0102	-0.0134	-0.0036	0.0093	0.0054	-0.0024	0.0000	-0.0014	-0.0146	-0.0274	-0.0290
2016q4	-0.0027	-0.0257	-0.0156	-0.0418	-0.0099	-0.0046	0.0161	-0.0121	-0.0230	0.0011	0.0000	-0.0110	-0.0234	-0.0184

$Pr(D^4_{w,c} > 1)$	2013q1	2013q2	2013q3	2013q4	2014q1	2014q2	2014q3	2014q4	2015q1	2015q2	2015q3	2015q4	2016q1	2016q2
2014q2	0.0000	0.0055	0.0256	0.0510	0.0529	0.1232	0.1186	0.1641	0.1368	0.1095	0.1559	0.1976	0.1346	0.1234
2014q4	0.0000	0.0000	0.0000	0.0000	0.0056	0.0114	0.0351	0.0388	0.0528	0.0295	0.0474	0.0370	0.0572	0.0316
2015q1	0.0000	0.0000	0.0000	0.0000	0.0061	0.0163	0.0312	0.0421	0.0539	0.0620	0.0796	0.0775	0.0830	0.0935
2015q2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0064	0.0174	0.0276	0.0409	0.0574	0.0702	0.0740	0.0710	0.0783
2015q3	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0049	0.0123	0.0217	0.0388	0.0595	0.0649	0.0658	0.0741
2015q4	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0064	0.0127	0.0272	0.0383	0.0549	0.0546	0.0550
2016q1	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0027	0.0076	0.0165	0.0177	0.0265	0.0482
2016q2	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0072	0.0135	0.0208	0.0362	0.0489
2016q3	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0048	0.0096	0.0169	0.0352
2016q4	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0094	0.0215	0.0275

$n^4_{w,c}$	2013q1	2013q2	2013q3	2013q4	2014q1	2014q2	2014q3	2014q4	2015q1	2015q2	2015q3	2015q4	2016q1	2016q2
2014q2	452	363	430	569	397	357	506	585	468	338	417	663	431	308
2014q4	354	715	369	419	357	703	370	361	341	441	380	378	297	474
2015q1	13423	12015	17831	17003	14335	11912	18571	17106	13759	12571	17632	15659	12875	11361
2015q2	17701	15797	23058	22965	19450	16471	24314	22569	18653	16197	23054	20801	17081	15541
2015q3	25680	22528	31255	32295	27282	22789	32574	32289	26590	21985	30497	29523	24561	21390
2015q4	1591	1402	2028	2261	1735	1502	2428	2184	1738	1399	2038	1657	1466	1145
2016q1	3255	2981	4134	4138	3561	2991	4383	4411	3364	2901	3992	3901	3394	3052
2016q2	6467	5669	8273	8283	7062	6099	8935	8435	6886	6073	8170	7465	6162	5850
2016q3	8248	7679	10590	10901	9289	7868	10935	10911	8900	7649	10329	9896	8065	7038
2016q4	4053	3589	5042	5497	4566	4007	5589	6168	4765	3855	5391	5548	4320	3488

Figure 11: Distribution of $DID^h_{w,c} \quad \forall w, c, h : G^h_{w,c} = g$ for employment.



Notes. The Figure reports the distribution of the DID coefficients for every wave-cohort-tenure cell, corresponding to each level treatment exposure g, the number of quarters exposed to *Garantie Jeunes*. The blue line is the mean weighted by the number of observation used to estimate each DID, while the red line is the unweighted mean.

Figure 12: LATE on employment, grouping together cells containing the same individuals.

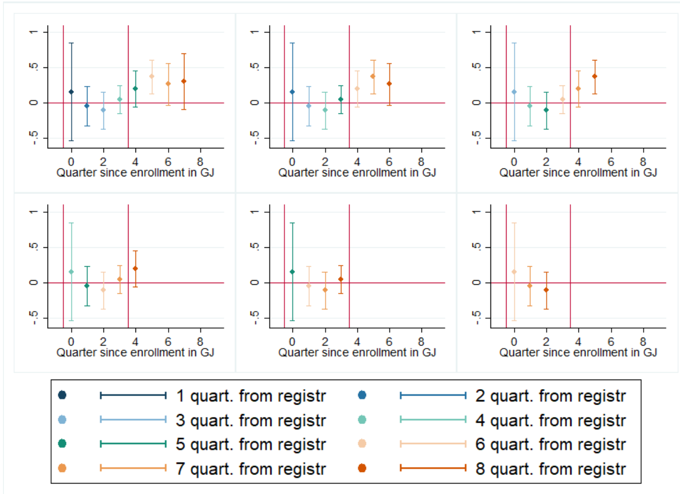
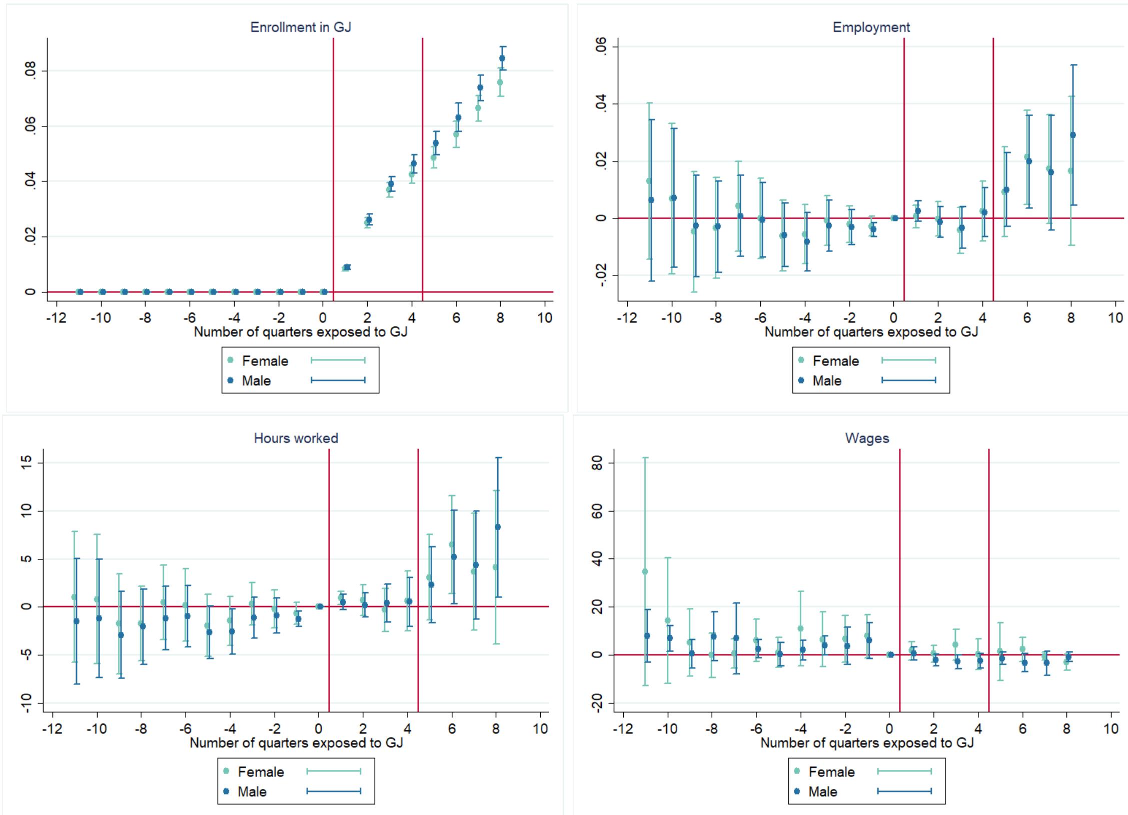


Table 13: Heterogeneity by employment contract.

	Open-ended (1)	Temporary (2)	Agency jobs (3)	Apprenticeships (3)
ITT effect 1st semester of exposure	0.000224	0.000858	0.00147	0.000971
	(0.00133)	(0.00205)	(0.00136)	(0.00113)
Total n.obs	3194961	3194961	3194961	3194961
ITT effect 2nd semester of exposure	0.000224	0.000858	0.00147	0.000971
	(0.00208)	(0.00258)	(0.00217)	(0.00115)
Total n.obs	2379924	2379924	2379924	2379924
ITT effect 2nd year of exposure	0.00218	0.00674	0.00389	0.00115
	(0.00437)	(0.00438)	(0.00246)	(0.00189)
Total n.obs	2665714	2665714	2665714	2665714
Mean for control 1st semester of registration in ML	0.084	0.155	0.078	0.031
Mean for control 2nd semester of registration in ML	0.109	0.184	0.081	0.034
Mean for control 2nd year of registration in ML	0.138	0.191	0.086	0.037
LATE 1st semester of exposure	0.00947	0.0363	0.0623*	0.0412
	(0.0348)	(0.0550)	(0.0362)	(0.0296)
LATE 2nd semester of exposure	0.00947	0.0363	0.0623***	0.0412***
	(0.0225)	(0.0278)	(0.0234)	(0.0126)
LATE 2nd year of exposure	0.0403	0.124***	0.0718***	0.0211
	(0.0326)	(0.0328)	(0.0179)	(0.0142)
LATE 1st semester after enrollm.	0.0264	0.0107	-0.00615	-0.00492
	(0.0192)	(0.0193)	(0.0137)	(0.0109)
LATE 2nd semester after enrollm.	0.0601	0.0405	0.0954**	-0.0144
	(0.0819)	(0.0640)	(0.0423)	(0.0630)
LATE 2nd year after enrollm.	0.0403	0.124***	0.0718***	0.0211
	(0.0326)	(0.0328)	(0.0179)	(0.0142)

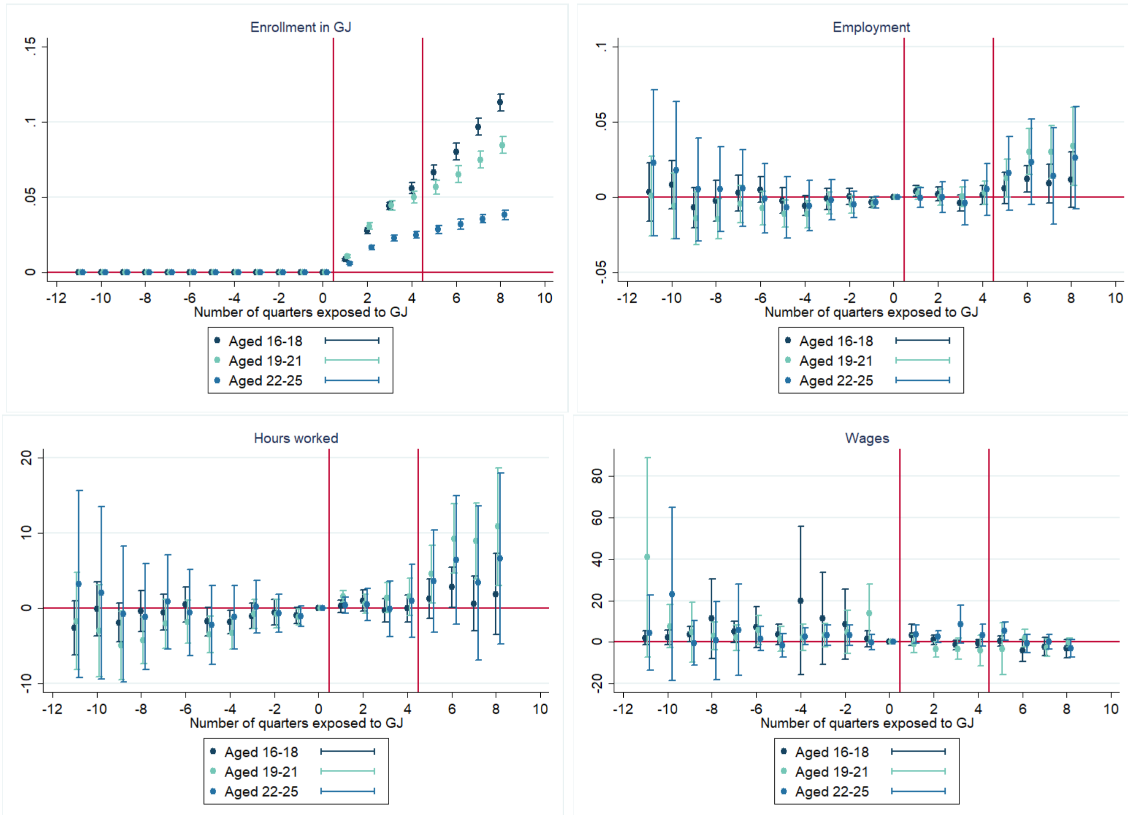
Notes. The table reports the main results obtained following the rolling diff-in-diff methodology developed in Section 3. The upper panel reports weighted averages of the $DID^h_{w,c}$ coefficients where exposure is between 1 and 4 quarters or above 4 quarters. The lower panel reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and wages (earnings per hour) obtained according to Equation 4. Standard errors are bootstrapped and reported in parenthesis.

Figure 13: Intent to treat (ITT) estimates using the rolling diff-in-diff approach by gender.



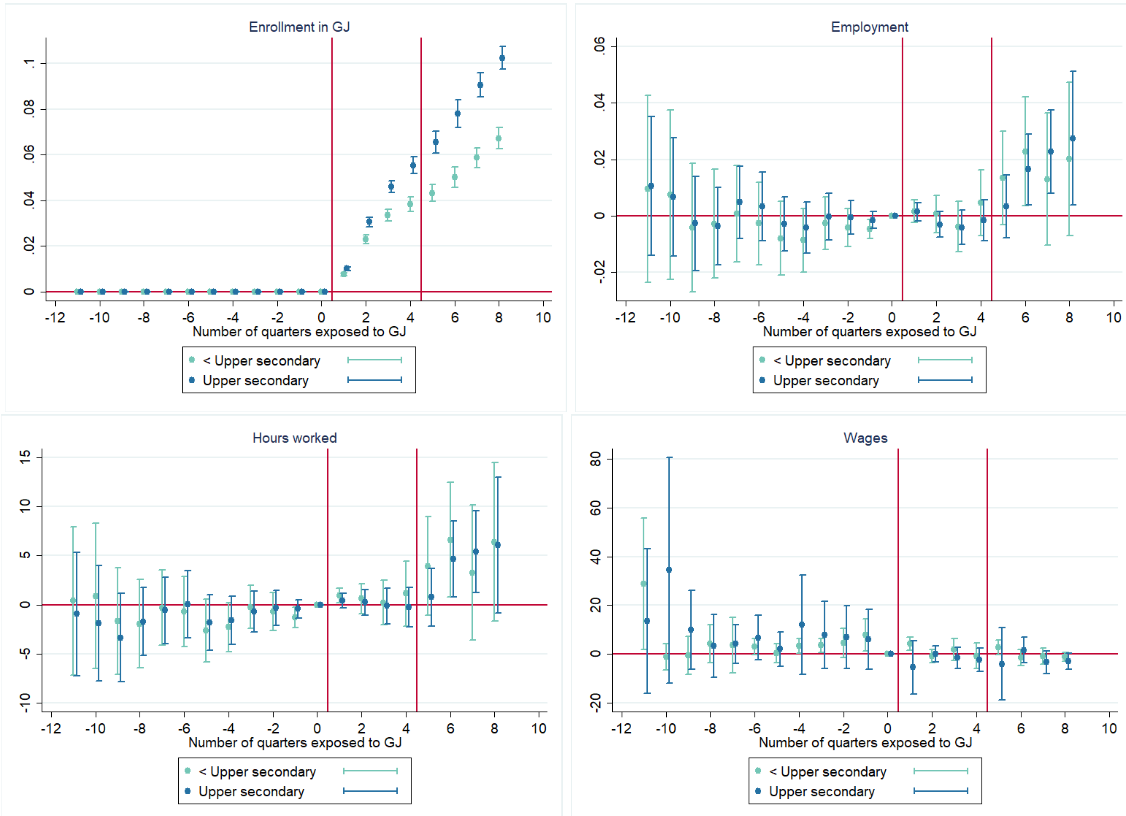
Notes. The figure reports results of the rolling diff-in-diff approach for different gender sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 2. Cell-specific effects were obtained as in Equation 1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-level, corrected for multiple testing, and confidence intervals are reported at 95% confidence level.

Figure 14: Intent to treat (ITT) estimates using the rolling diff-in-diff approach by age.



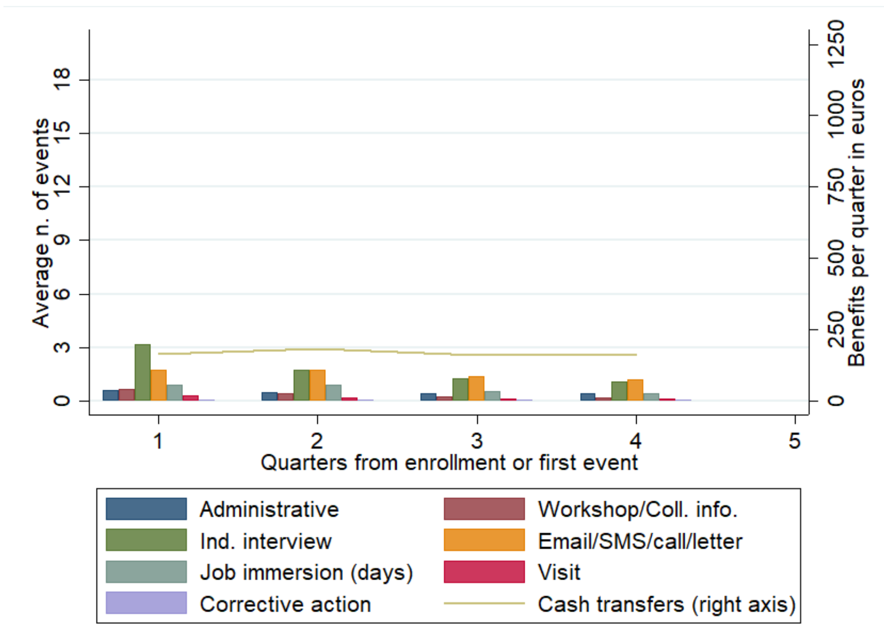
Notes. The figure reports results of the rolling diff-in-diff approach for different age sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 2. Cell-specific effects were obtained as in Equation 1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-level, corrected for multiple testing, and confidence intervals are reported at 95% confidence level.

Figure 15: Intent to treat (ITT) estimates using the rolling diff-in-diff approach by higher education degree attained.



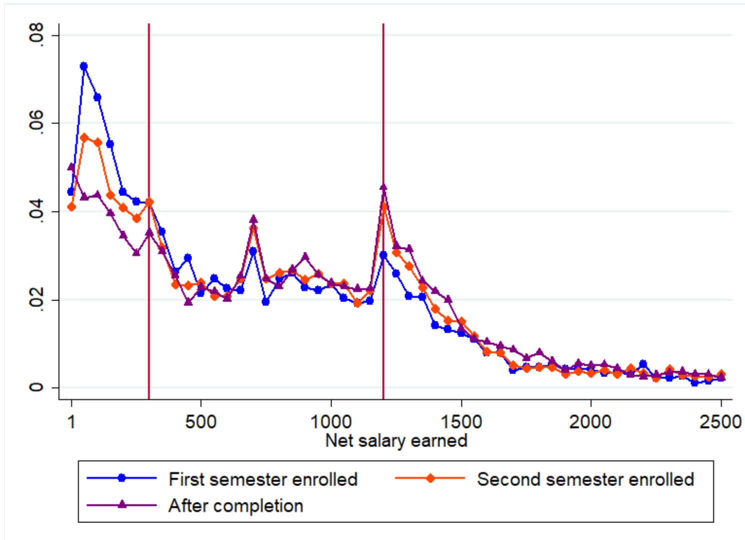
Notes. The figure reports results of the rolling diff-in-diff approach for different sub-samples defined by higher education degree attained. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 2. Cell-specific effects were obtained as in Equation 1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-level, corrected for multiple testing, and confidence intervals are reported at 95% confidence level.

Figure 16: Average number of events, by kind of event, and average benefits for participants in standard program available at YECs, *CIVIS*.



Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from enrollment in *CIVIS*. The cash transfers series plots instead the average amount of benefit to youths participating in *CIVIS*, basing on when the actual transfer of money is recorded in the information system I-Milo.

Figure 17: Distribution of net earnings for takers while enrolled, takers after completion, and non-exposed youths, given positive earnings.



Notes. The Figure reports histograms of monthly earnings for youth in contact with YECs, by enrollment stage in *Garantie Jeunes*, for all youths in the sample in the period they are observed. Bins are of €50. Vertical lines correspond to the bins of €300 and €1159 (80% of French minimum wage). Monthly net earnings of each youth are estimated from adminitrative data on earnings of each contract gross of social contributions, assuming constant daily earnings.