

What Do NEETs Need?

The Effect of Combining Activation Policies and Cash Transfers

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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 providing 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'Haultfoeuille (2020a) to a setting where rolling over a third dimension is needed. While no significant effect arises during enrollment in the program, after completion takers report +26 percentage points in the probability of employment and +71 hours worked on a quarterly basis. No effect on wages is detected. 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

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1 Introduction

Youths Neither in Employment, Education or Training (NEETs) are a persisting problem in Europe. NEET rates in the last decade ranged for youths aged 15-24 between 15% and 22% in countries such as Spain, Italy and France. Higher levels were reported for women, less educated and foreign born individuals. Economists have long wondered about the possible causes. Given that more disadvantaged youths are more likely to become NEETs (Carcillo and Königs, 2015), an hypothesis is that NEETs face significantly higher job search frictions, lacking networks and soft-skills¹. Then, NEET spells can become a poverty trap. In fact, unemployment has proven “scarring” permanent consequences on employability (Oreopoulos et al., 2012; Schwandt and Von Wachter, 2019; Rothstein, 2019), as much as prematurely ending up out of education (Brunello and De Paola, 2014), keeping NEETs job finding rates low.

Can combining cash transfers and activation policies break the loop? “Passive” policies such as cash transfers risk decreasing labor supply and search effort (Moffitt, 1985). This could happen through pure moral hazard or liquidity effect (Card et al., 2007; Chetty, 2008), but also through distorsive implicit taxation implied by benefit reduction with job earnings (Le Barbanchon, 2020). A possible solution, often advocated by international institutions (OECD, 2013; Pignatti and Van Belle, 2018), is accompanying passive with active labor market policies. The literature has mostly evaluated the effect of activation measures conditional on cash transfers, and viceversa. Activating receivers of social protection is found to improve employability in the medium run (Card et al., 2018)². Offering cash transfers on top of activation measures might finance their opportunity cost (Heckman et al., 1999), although this doesn’t always translate in better 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 so far. There are reasons to believe that complementarities might arise when cash transfers and activation are jointly provided, for example if activation functions as a monitoring device (Boone et al., 2007), or if better search technology reduces search costs compensating disincentives to search caused by the cash transfer.

This paper fills the gap by evaluating an innovative and large French program targeting disadvantaged NEETs between 16 and 25 years old, *Garantie Jeunes*. The program combines a year of cash transfers equivalent to the French minimum income (€485 in 2018) with intensive activation measures, i.e. soft-skills training for a month, regular counseling and short 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 contract signed by youths in 2013-2017. This dataset has the advantage of being large in the number of observations and detailed in information on youths and events experienced while at the employment center.

The results highlight that, while no significant effect arises during enrollment in the program, a strong positive effect of *Garantie Jeunes* materializes 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. The effect is driven by the share of youths having completed the program, while no effect is associated to youths still receiving cash transfers and activation. Wages are instead never affected. Because the number of participants is small relative to a large population, ITT effects translate in high LATEs on takers: +26

¹The 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).

²The effectiveness of activation policies is shown to depend a lot on the mix of actions. Other factors such as target populations (Kluve et al., 2019; Babcock et al., 2012) and market conditions (Crépon et al., 2013a) are shown to matter.

points in employment (on a mean rate of 49%) and +71 in quarterly hours. To hedge the result, it should be noted that the effect comes vastly from fixed-term contracts and agency jobs. Also, large gains in employment came with comparable costs, as 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’Haultfoeuille (2020a) to roll over a third dimension. This necessity arises out of two characteristics of my setting. First, units enter the population of interest (register to youth employment centers, YECs) at a specific point in time, and are subsequently exposed to treatment at different times since registration. However, time since registration at YECs can be a source of selection into treatment, so that distinguishing between different times since registration becomes crucial. Second, my identification arises from staggered adoption of treatment by groups, but the possible dynamic of the effects arises over individuals’ exposure/enrollment to treatment. Because some individuals can enter treated group later than the time of adoption, treatment effect since *adoption* of the treatment in the group 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 – time since registration – allows me to identify first stage and reduced form effects (ITT) since exposure. A second step in my methodology allows me to recover LATEs since enrollment.

I disentangle the mechanisms behind my effects 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 keep receiving the transfer, the fall concentrates in jobs earning €300-1100, where transfers are only partially cumulative with job earnings.

I interpret this heterogeneity with the lenses 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, depending on cash transfer and implicit taxation implied by *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 find that, on the one hand, the probability to choosing not to work full-time is estimated to decrease by 9% when the cash transfer is available, while the probability of choosing no employment or part-time jobs increases by 15%. Moreover, the implicit taxation from the phase-out further reduces employment in the taxed brackets by 0.9% for each point of marginal implicit taxation. Finally, lock-in from training reduces the probability of finding a job by 36%. On the other hand, activation compensates these negative effects by doubling the probability of finding the chosen job.

The main contribution of this work is to the literature on unemployment policies, offering evidence on the joint effect of active and passive labor market policies. Papers until now mostly evaluated one conditional on

the other. An example is the smaller-scale experimental study by (Aeberhardt et al., 2020), on a program offering only additional cash transfers in the same context of this paper. They find a significant increase in attendance to compulsory counseling (not as intense as in *Garantie Jeunes*) but a non-significant effect on job-search and a negative effect on employment the first six months. Conversely, Card et al. (2018) meta-analyzes programs that increase activation measures, but not cash support. They find that programs with a work-first approach can have a positive impact in the 1-2 years after activation. The main estimates of this paper also signals a positive effect of activation, but the magnitude is large compared to programs in Card et al. (2018)³, and benefits materialize only after the end of cash transfers. Further 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 by activation. This suggests potential complementarities between cash transfers and activation. For instance, activation might provide a monitoring device for conditional cash benefits, as in Boone et al. (2007). Complementarities might also arise if individuals find it convenient to reduce less search effort than what they would with cash transfers only, since the cost of effort or the potential benefit from it increased with better search technology.

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. Concerning the reaction to cash benefits (Le Barbanchon, 2020; Saez et al., 2012), I highlight significant effects of implicit taxation from transfers phase-out. Back of envelope calculations point at 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). In turn, Gautier et al. (2018) model activation measures and suggest that they affect job search through the number of applications sent 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 job finding. An implication of large effect of activation is that job finding is estimated low for would-be takers in the control group. This supports different streams of the literature showing that lack of networks, geographical isolation and low soft skills can dramatically limit job search of disadvantaged youth⁴.

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 students in different grades, as in Martorell et al. (2016). Another one 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 time 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 entrance in the population. A second instance is if the treatment effect on units (i.e. students, workers, ...) within groups is somehow dynamic. Because units might enter the population (or enter treatment) after treatment adoption in their group, then 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 entrance in the population allows to flexibly estimate dynamic effects since exposure and since enrollment.

³Our 50% increase in employment of takers is in the top 5% of the effects considered in the meta-analysis

⁴See 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)

The results have remarkable policy relevance. First, the paper offers a success case of a public labor market program 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 on motivation. These aspects caution about the external validity of my results. Second, the analysis offers support to 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 distorsive 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 cash transfers phase-out, and the jump in employment subsequent to the end of cash transfers, policy makers might consider making cash transfers fully cumulable with job earnings, while keeping them limited in time. These insights are valid also for programs using different combinations of the same ingredients, such as minimum income schemes with activation measures, or combinations of unemployment insurance and job search assistance.

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 introduced in the context of the European Union hat program European Youth Initiative⁵. The French version of the program was launched in October 2013, co-financed by the French government, targeting NEETs aged 16-25. The program lasts one year, but it's renewable for 1-6 months only in exceptional cases (2% of participants eventually renew). At acceptance, the participant is required to sign a contract of engagement, including penalties for non-respect of the mandated activities. The early activation part consists in a 6 weeks period of collective courses held 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 to personal management and self confidence (learn to be timely, manage your health, plan your week, ...). It follows a 10 months period of regular counseling, with interviews held once every 21 days on average. This second part is characterized by a “work-first” approach, i.e. high frequency of proposals of internships and short work experiences of at most a month called “job immersions”⁶, during which the youth works on small tasks in a partner firm with the aim of discovering the working environment and the industry. Meanwhile, youths receive a monthly cash transfers equal to the level of the French minimum income scheme RSA, updated every year: for example, it was up to 484,82 €gross in April 2018. Importantly, the cash transfers are not digressive with job earnings earned while enrolled in *Garantie Jeunes* until 300 €, while after this

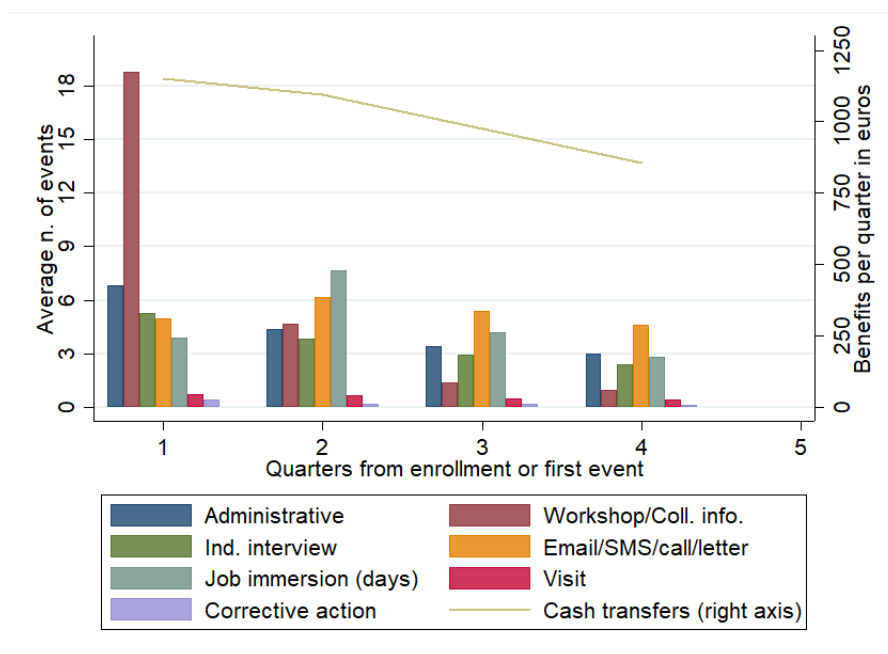
⁵The concept of Youth Guarantee derives actually from a Nordic tradition of establishing a right to employment or training for youth entering the labor market. The EU channeled part of the European Social Fund (6.6 billions) toward financing nationally-defined implementation programs aiming at supporting 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).

⁶Technically these contracts are called *période de mise en situation en milieu professionnelle* (PMSMP)

threshold they decrease proportionally to labor earnings to reach zero at 80% of the french minimum wage gross (i.e. between €1143 and €1174). Sanctions are possible if the engagement contract is not respected, up until suspension from the program. 23% of participants quits the program in advance, almost all in the last quarter. Among these youths, roughly one third quits for having found employment, one third for exogenous reasons (age, moving), the remaining for non-respect of the contract (3% of overall 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 by previous experimental programs (Aeberhardt et al., 2020) and supported by a working group of experts which summarized 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, and 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 stick quite well to what expected on-paper (Figure 1)⁷.

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.

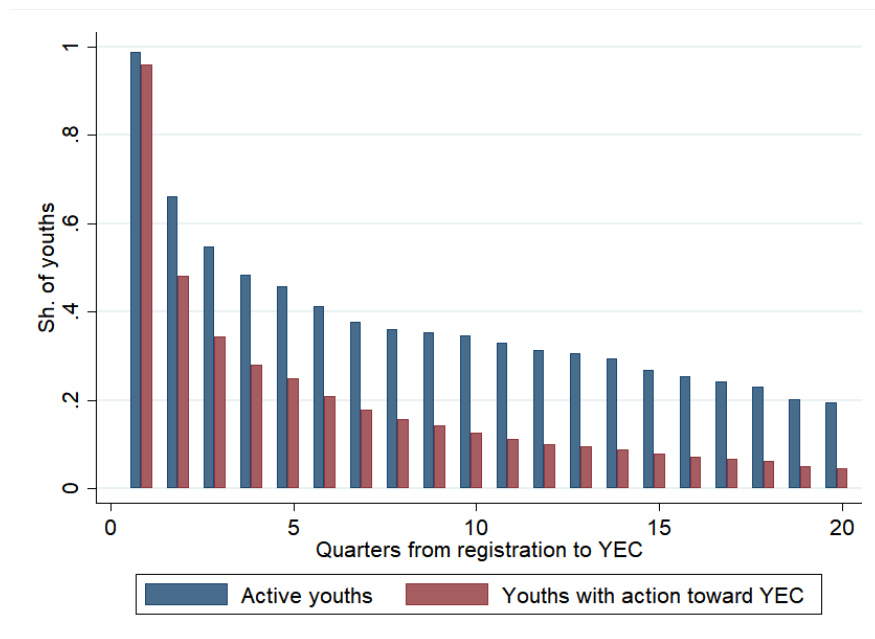
French local Youth Employment Centers (YECs)⁸ are in charge of the administration of the program. These employment centers were introduced in the 1990s, they have a decentralized structure (they are created and managed by groups of municipalities), and focus specifically on youths between 16 and 25. In the

⁷It 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

⁸*Missions Locales* in French

standard case, YECs offer to 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 amount of activities required (Figure 16 in Appendix). Once youths register to YECs there is no formal de-registration, so they can keep contact with YECs for a variable amount of time, depending on youth's and the YEC reliability, even without participating to any program. Figure 2 points out that the share of youth considered active in a specific cohort of registration - meaning youths for which the YEC records at least one action on their *dossier* during a quarter - after 3 years from registration is still 31.4%. However, after 3 years from 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 YEC over time from 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 in the YEC. "Active youths" are considered as those whose *dossier* records any kind of action in the quarter. The red series reports instead youths for which a "youth toward ML" action is recorded.

YECs are organized into 459 centers¹⁰ with more than 9000 local offices. The assignment to a YEC is based on your municipality of residency. There are about half a million youths registering at YECs every year, over a target population of about 9 million youths aged 16-25, suggesting that more than one third of French youth registers to YECs at some point¹¹. At the time of registration, youths are likely just out of education and starting entering the labor market. Figure 3 pictures the average employment trajectory of a youth when registering to YEC: it is evident that the time of registration to YEC corresponds to a rise in the probability of employment of the youth (of course, possibly independently from the activities underwent at

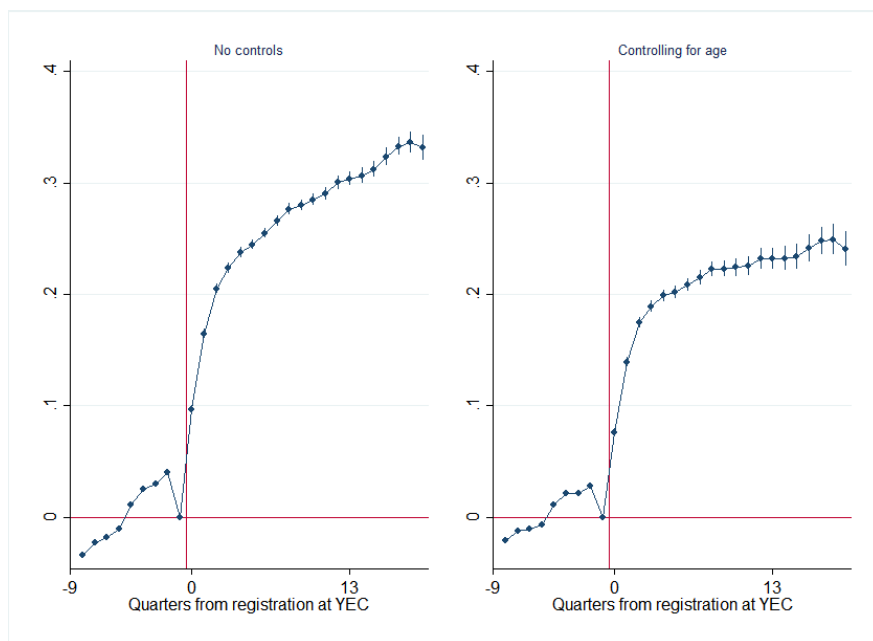
⁹Actions are categorized in the data as "youth toward YEC", "YEC towards youth", "internal" or "third-party information"

¹⁰These are total numbers in the data available, today 436 centers and 6800 offices are active, since some offices have been closed, and some are "artificial" in our data, i.e. correspond to special administrative categories

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

YECs).

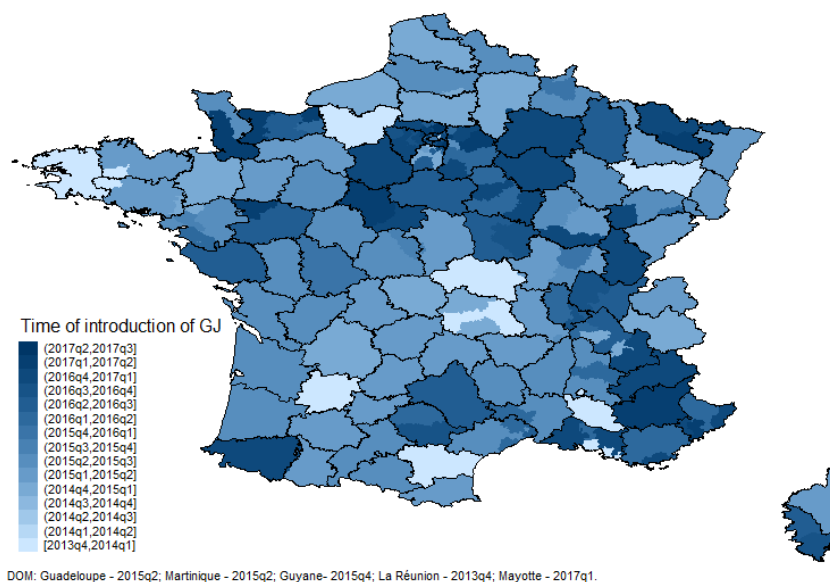
Figure 3: Average employment rates in the quarters precedent/following registration at YEC, controlling or not for age



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 first pilot 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 areas sometimes corresponded to an agreement between different YECs, sometimes to a whole department, sometimes only to a fraction of it. The program was extended progressively in six waves until reaching 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 some YECs started with delay the offer of the program, so that eventually between 2013q3 and 2017q2 each quarter except one there is some YECs adopting the program for the first time.

Figure 4: Progressive extension of *Garantie Jeunes*



Notes. French municipalities (black borders correspond to *départements*) by date of first case of enrollment in *Garantie Jeunes* in their corresponding YEC. Overseas territories (DOM) are reported at the *département* 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 finished 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 and selected for the program. On the one hand, in order to be eligible youths should either live in a household below the minimum income (RSA) threshold, or have quit their parents or receive no support from them, or having dropped out from school without a qualifying secondary school diploma, or be convicted¹². Note that in France minimum income is not available for youths younger than 25 with no kids. On the other hand, for enrolling in *Garantie Jeunes* youths are asked to demonstrate a condition of “fragility” and “motivation” through an application process. Qualitative reports describing such process argue that the first selection mechanism went through selective promotion by YECs, which often themselves organized information sessions and proposed to a selection of registered youths to apply to the program. After submission, the decision on the application is taken by local independent commissions¹³. There are thus two possible layer of selection between potentially eligible youths (estimated to be between 187,000-189,000 in 2016) and actually enrolled youths, who are roughly half of the eligibles according to Gaini et al. (2018).

Garantie Jeunes is nowadays a large program: since 2013 more than 350.000 youths participated to it, with

¹²Young 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.

¹³These 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*.

yearly costs estimated at 354 millions of Euros in 2018, when it reached the full extension. Until this paper, the program has undergone qualitative evaluation by Gautié (2018), a pilot one by Gaini et al. (2018). Being considered a success, *Garantie Jeunes* is currently at the center of political debate for being expanded with a new name, *Revenu d'Engagement*. According to press reports, this expansion should cover all individuals earning below minimum income, with no or milder selection on motivation upfront.

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. First, this dataset includes abundant information given by youths when registering at YECs. For most individuals we 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 reference YEC main office or peripheric office of reference. 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). Second, it reports details on 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 since late 2010, until nowadays.

To get information on youths also when they are not in contact with YEC, I merge the dataset with an extraction of French social security records¹⁴ The matching was operated by the French Agency for Social Security under a convention with the French Labor Ministry. The resulting dataset includes information on all contracts signed in the period 2013-2018 by all youths who registered in YECs in the period 2013-2017. The available information is date of start and termination of the contract, kind of contract, total earnings and hours worked. I can derive information on hourly earnings indirectly by dividing earnings by the number of hours. I will simply refer to this as "wage" later).

After cleaning¹⁵, the population which we observe in our dataset consists in all youths who register in YECs between January 2013 and December 2016, a panel of 1,967,000 individuals over 2013-2017. It includes a similar share of youth 16-25 with less than secondary vocational qualification with respect to the overall French population, but a larger share of youth with 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 largely different in terms of share of females and French nationals. However, the population is characterized by early experiencing of activities which are typical of adulthood. 35% on average of youth in YECs has experienced at least one employment episode in the quarter preceding registration (national mean 30%). 37% on average of youth in YECs lives independently from his parents (national mean, 23%). 8% on average of youth in

¹⁴The 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.

¹⁵I 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).

YECs has kids (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), and of youth registering in YECs standard program of YECs, and in *Garantie Jeunes*

	All youth 16-25 (Census)	Youth in YECs	Youth in standard pr.	Youth in <i>Garantie Jeunes</i>
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 kids	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 column reports respectively information on youth enrolling in the standard program offered at YECs, CIVIS, and 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 to YECs.

For simplicity, I collapse time variables by quarters. I define the cohort of registration to YEC as the quarter at which the youth first checks in at her YEC, and the wave of introduction of *Garantie Jeunes* as the quarter when the first enrollment in *Garantie Jeunes* occurs in the YEC¹⁶.

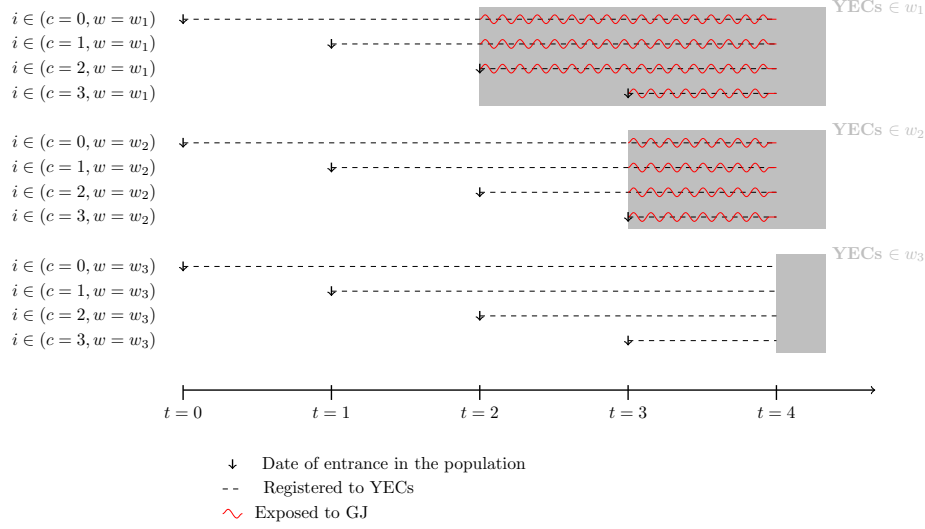
3.2 Identification

3.2.1 Intuition

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

¹⁶Waves programmed by authorities are actually less than what can be observed in the data: the pilot one started in October 2013 q4, a second in January 2015, third in April 2015, fourth in September 2013 q3, fifth in March 2016, sixth in September 2016, and finally a the whole territory in January 2017. Yet, some YECs experienced a delay, or preferred starting one or two months later. Table 8 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 9) and the number of youths registering to YECs (Table 10) is distributed across waves and cohorts.

Figure 5: A simplified illustration of the setting



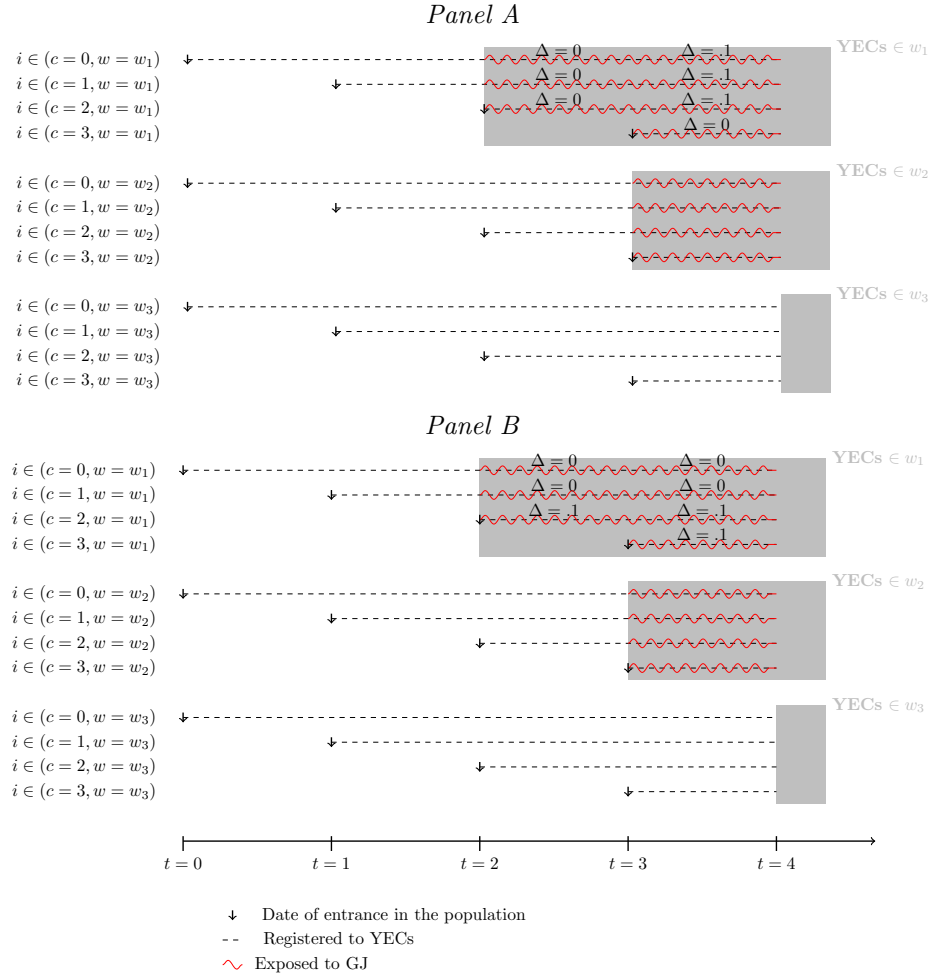
De Chaisemartin and D’Haultfœuille (2020b) show that in staggered adoption designs event studies using two-way fixed effects or first difference estimators heavily rely on homogeneous treatment effects, and are otherwise biased due to negative weighting of the effect in some groups. They propose a version 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 these 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 like

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

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 application 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, $DID_{w,t}$ 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 the program immediately after exposure, 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 since the same 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 over exposure. The average effect since two periods of *exposure*, whenever $G_{w,c}^h = 2$, is then 0.1. However, the average effect since 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 in the past 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 meanwhile, remaining 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 $G_{w,c}^h > 0$, $h > G$, i.e. for youths who registered before treatment introduction and are exposed later. Instead, $\Delta = .1$ if $G_{w,c}^h > 0$, $h = G$, i.e. for youths who registered at the moment of introduction of the program or later. The average effect when $G_{w,c}^h = 2$ is .03, but the effect two periods since adoption $DID_{w_1,t=2}^{dCDC} = 0.05$.

To assess these two concerns, I propose an estimator which extends the one by De Chaisemartin and D’Haultfoeulle (2020a) but rolling over time since registration h , $DID_{w,c}^h$. This introduces a third dimension beside treatment wave w and cohort of registration to 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 to YECs c is equivalent, given the same time since registration h . Because $(w, c|h) \rightarrow G_{w,c}^h, DID_{w,c}^h$ will deliver estimates of the effect after *exposure* to my program. However, this granularity comes with a cost. In fact, one needs to observe much more cohorts of youths registering before the adoption of the program in order to assess long-term dynamic effects. Moreover, coefficient further in exposure will be necessarily estimated using only early exposed, which implies large standard errors and requires caution in interpretation¹⁷.

3.2.2 Formal Identification of ITT

Let $G_{w,c}^h$ denote the treatment status – i.e. the number of periods exposed to *Garantie Jeunes* – and $\{Y_i^h(g)\}_{\forall g}$ be the set potential outcomes:

$$Y_i^h(G_{w,c}^h) = \begin{cases} Y_i^h(g) & \forall g > 0 \\ Y_i^h(0) & \end{cases}$$

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 $G_{w,c}^h = g$ of the difference in outcome when treatment exposure is g

¹⁷In 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 still 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 exposed¹⁸ :

$$\Delta^{ITT}(g) = \mathbb{E}(Y_{w,c}^h(g) - Y_{w,c}^h(0)) \quad (w, c, h) : G_{w,c}^h = g > 0$$

Where I denote $Y_{w,c}^h := \mathbb{E}(Y_i^h | w, c)$, the empirical mean in 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. $\mathbb{E}(Y_{w,c}^h(0) - Y_{w,c'}^h(0) | G_{w,c}^h, G_{w,c'}^h) = \mathbb{E}(Y_{w,c}^h(0) - Y_{w,c'}^h(0) | G_{w,c}^h)$ 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: $G_{w,c}^h \perp\!\!\!\perp \mathbb{E}(Y_{w,c}^h(0) - Y_{w,c'}^h(0)), \forall c, c'$, given YEC-tenure h ;
3. (*No anticipation*) Mean potential outcomes $Y_{w,c}^h$ in a cohort at a specific point in time are independent from treatment status in the next period $G_{w,c+1}^h = G_{w,c}^{h+1}$, so that outcomes when treated depend only on past exposure $Y_i^h = Y_i^h(G_{w,c}^h)$;
4. (*Common trends*) Expected variation in potential outcomes when non-treated doesn't vary across waves, given YEC-tenure h : $\mathbb{E}(Y_{w,c}^h(0) - Y_{w,c'}^h(0)) = \mathbb{E}(Y_{w',c}^h(0) - Y_{w',c'}^h(0))$.

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 $G_{w,c}^h = g > 0$, define

$$\Delta^{ITT}(h, w, c) = Y_{w,c}^h(g) - Y_{w,c}^h(0) \quad \forall \text{ given } (w, c, h) : G_{w,c}^h = g > 0$$

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

$$DID_{w,c}^h := Y_{w,c}^h - Y_{w,c'}^h - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_{w,c}}} (Y_{w',c}^h - Y_{w',c'}^h) \quad \forall \text{ given } (w, c, h) : G_{w,c}^h = g > 0 \quad (1)$$

Where $G_{w,c'}^h = 0$ but $G_{w,c'+1}^h = 1$, and $\Omega_{w,c}$ is the set of waves such that $G_{w',c}^h = G_{w',c'}^h = 0$, 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 $DID_{w,c}^h$ for $\Delta^{ITT}(h, w, c)$.¹⁹

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

¹⁸Note 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 don't need to apply the tools of duration models, which would require assumptions on the shape of the hazard function.

¹⁹An 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 $DID_{w,c}^h$. 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 $DID_{w,c}^h$, corresponding to levels $G_{w,c}^h = g$ of exposure to treatment. I am then then interested in meaningfully aggregate $DID_{w,c}^h$ into unbiased estimators of $\Delta^{ITT}(g)$. Consider:

$$DID^g := \sum_{(w,c|h): G_{w,c}^h = g} \frac{n_{w,c}}{\sum_{(w,c|h): G_{w,c}^h = g} n_{w,c}} DID_{w,c}^h \quad (2)$$

It can be shown that:

Proposition 2. *Given a set of $DID_{w,c}^h$, for all $(w,c|h) : G_{w,c}^h = g$, 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:

$$Y_{w,c}^h - Y_{w,c'}^h - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_w,c}} [Y_{w',c}^h - Y_{w',c'}^h] = 0 \quad \forall \text{ given } (w,c,h) : G_{w,c}^h = 0 \quad (3)$$

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:

$$\Delta^{LATE}(d) = \mathbb{E}(Y_i^h(d) - Y_i^h(0) | D_{w,c}^h = d)$$

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

Proposition 3. *Consider a set of $DID_{w,c}^h$, 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_i^h the (random) variable describing it. Then, if $Pr(D_{w,c}^h > 0)$ whenever $G_{w,c} = 0$ (no defiers):*

- (a) $\sum_{(w,c|h): G_{w,c}^h = g} \frac{n_{w,c}}{\sum_{(w,c|h): G_{w,c}^h = g} n_{w,c}} [DID_{w,c}^h / Pr(D_{w,c}^h > 0)]$ is an unbiased estimator of $\Delta^{LATE}(g) = \mathbb{E}(Y_i(d > 0) - Y_i(0) | G_{w,c}^h)$
- (b) $DID_{w,c}^h = \sum_{d=1}^g \delta(d, h, w, c) Pr(D_{w,c}^h = d)$ 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 just points out at the possibility to recover a LATE on all takers, conditional on being in cells $(w,c|h) : G_{w,c}^h = g$ 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 since we always have at least a wave fully untreated 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 to infer 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 : $\mathbb{E}(\delta(d, h, w, c)) = \mathbb{E}(\delta(d))$ ²⁰. For gaining more power²¹, 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 one, and more than one year after enrollment (that is, after completion). Under these assumptions, one can recover structural δ s applying Equally-Weighted Minimum Distance (Altonji and Segal, 1996; Card and Lemieux, 2001) to the regression:

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

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 of the introduction *Garantie Jeunes* doesn't significantly modify the characteristics of cohorts at the time of registration to YEC. Table 2 reports a set of regressions of average characteristics of a cohort on a dummy for *Garantie Jeunes* introduction (named "instrument"), on a linear trend (named "l.trend"), and on both. 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 has kids, but the magnitude is again very small. All other characteristics of youths registering to YECs don't significantly change with *Garantie Jeunes* introduction, supporting the assumption that treatment doesn't affect potential outcomes.

²⁰This 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.

²¹The fully dynamic specification is instead more noisy, and is reported in the Appendix

Table 2: Balance checks

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

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 closer 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 it lives independently, has kids, and if so if it 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 $DID_{w,c}^h$, for every $(w,c|h)$ cell, using equation (1)²². I group $DID_{w,c}^h$ 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 basing on Cameron

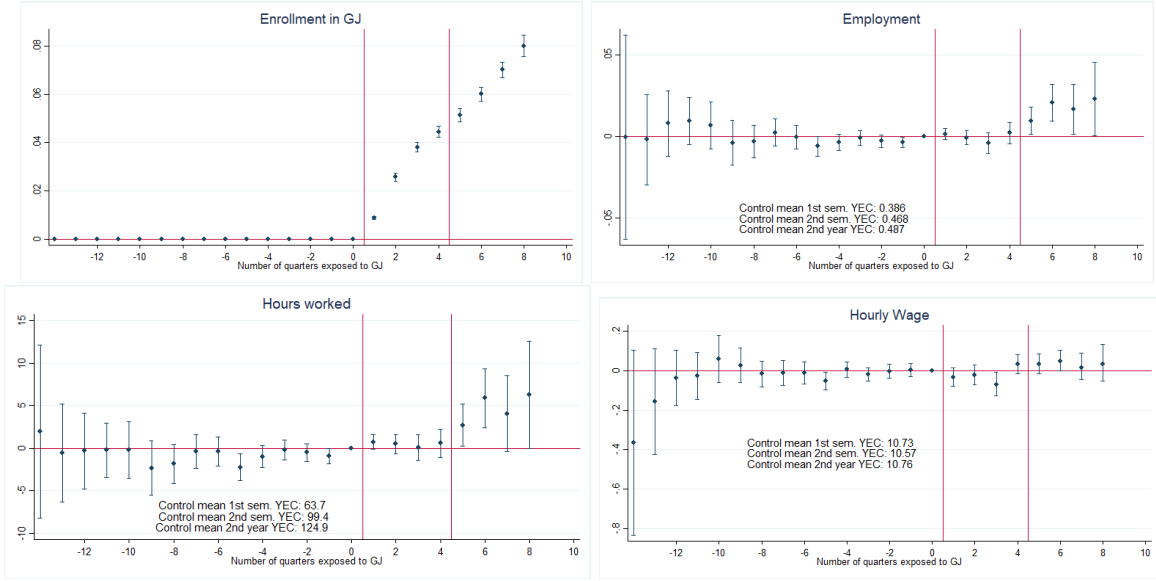
²²For example, for $h = 4$ we obtain a wide set of coefficients such as in Table 11 in the Appendix. The distributions of $DID_{w,c}^h$ by g are also reported in Figure 11 in the Appendix. Finally, Figure 12 shows that the effect seem relatively uniform for youth "waiting" differet times for the treatment.

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 got exposed. The coefficients before the introduction of the program are all omitted since nobody participates to *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. No significant differences in outcomes are found also 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. Such effect is between +0.9 and +2.3 percentage points in terms of employment and between +3 and +6 hours 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. This suggests that the new jobs obtained by participants are mostly minimum wage jobs.

We can further aggregate the treatment effect coefficients in yearly averages in the upper panel of Table 3. The first line of the table reports the average ITT effect in the first year after the program is introduced, while the fourth reports the ITT effect for the second year of exposure. 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 ITT effect on our outcomes of interest. For the first year 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 that youths are exposed to *Garantie Jeunes*, their employment probability is significantly higher, +1.6 percentage points. Accordingly, hours worked increase 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 a dummy for exposure to *Garantie Jeunes*. The other three panel 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 (1)	Employment (2)	Hours (3)	Wages (4)
ITT 1st semester of exposure	0.0168*** (0.000559)	0.000417 (0.00174)	0.592 (0.455)	-0.0298 (0.0197)
Total n.obs	4003538	4003538	3957848	1518029
ITT 2nd semester of exposure	0.0406*** (0.000807)	-0.00131 (0.00278)	0.270 (0.693)	-0.0282 (0.0233)
Total n.obs	3890678	3890678	3833155	1576052
ITT 2nd year of exposure	0.0609*** (0.00101)	0.0157*** (0.00517)	4.31*** (1.50)	0.0337 (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 $DID_{w,c}^h$ 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 quarters of exposure to *Garantie Jeunes*²³. 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 over for the early takers.

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

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

Notes. The upper panel 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 takers at different stages of the program. Using point b of Proposition 3 and Equation 4 I estimate dynamic LATEs on takers who are actually at different stages of program enrollment. The results of such approach are reported in the lower panel of Table 4. The estimates indicate that in the second year LATE on takers is driven by a high LATE effect for youths in the second year of enrollment in the program. This confirms the intuition that the program affects employment after completion. Interestingly, in LATE terms the effect on wages relative to the mean in the control group becomes higher, even compared to the one on employment. This suggests that not only *Garantie Jeunes* increase employability, but that the quality of employment increases as well.

As a general remark, the LATE effects we find when youths finish the program are very high, yet they are not so far from the one found by the pilot evaluation by Gaini et al. (2018), who find a LATE of +22.2 in employment (over a control mean of 25%) on the fifth quarter after enrollment in the program²⁴. Gaini et al. (2018) don't report results on hours and wages, so comparison with them is not possible. The effects in the fifth and sixth quarter are comparable to the effects found by Fein and Hamadyk (2018) for a year-long youth program in the US, called "Year-Up", which is similar to *Garantie Jeunes*. Their estimated effects on earnings is +60% when adjusting for take-up. This is not so much lower than the effect in Table ?? in the Appendix, where I report the effect on earnings of *Garantie Jeunes*. Yet, their effect is estimated over a less

²³This 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

²⁴For 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.

disadvantaged population, selected already at ITT level, so their estimate is pushed down by higher average earnings in the control group²⁵.

4.3 Heterogeneity

In this section I analyze the heterogeneity of the impact of *Garantie Jeunes* both on different kind of employment contracts and by characteristics of the youth. Table 12 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 could possibly be transformed later in employment contracts (the maximum duration ranges between 2 and 3 years).

Turning to heterogeneity by characteristics of youths (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 upper secondary diploma is null. This can be both 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, or to an effective difficulty of *Garantie Jeunes* in engaging less educated youths. 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 upper secondary diploma in France, while the 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. In 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 of one year. Conservatively, I assume no effect from *Garantie Jeunes* at an horizon longer than one year after completion, since the literature 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 to *Garantie Jeunes*, one should sum the direct cost of

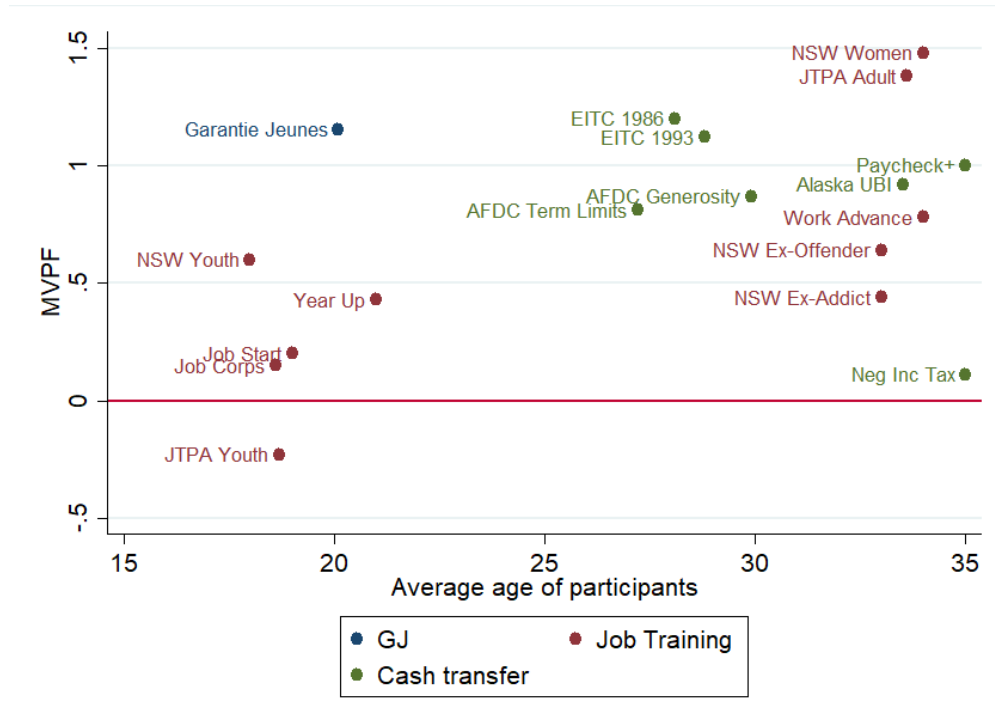
²⁵The program is a sectoral training program, it includes a selection process based on motivation and skills at the beginning, before random assignment, and also includes a specific training component

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 20% of the total reimbursement). The additional funding allocated to each youth employment center is of €1120 per youth enrolling in the program, plus €320 after completion of the program or finding of employment or formal training by the youth and €160 for data reporting, hence a total of €1600 per youth. Given that only 17% of participants quits the program before the end not due 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's added both to WTP and to net costs.

Under these assumptions²⁶, 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*, their effect is also smaller than the one of *Garantie Jeunes* (Katz et al., 2020) and of low magnitude relatively to the cash transfer (which squeezes MVPF toward 1). In fact, 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 MVPF of policies supporting college attendance (which tend to have MVPF between 2 and infinite), while slightly outperforms most unemployment insurance and income support schemes (which tend to have MVPF around 1).

²⁶First, 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.

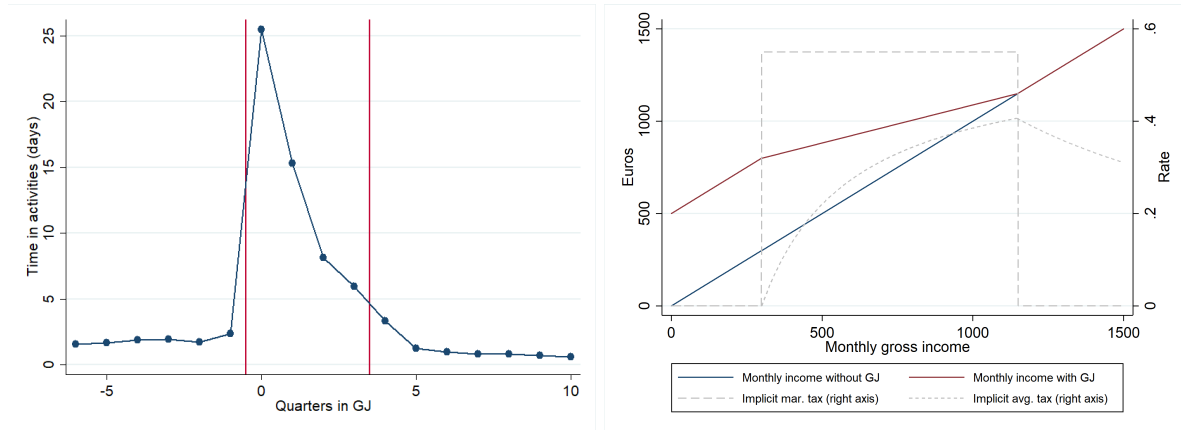
5 Disentangling the Role of Cash Transfers and Activation

In this section I aim at disentangling the roles of cash transfers and activation measures. This is interesting not only to open the black box behind the aggregate effects, but also to better relate to the literature. In fact, until nowadays scholars have mostly studied cash transfers and activation measures one conditional on the other. On the one hand, if the environment is characterized by search frictions, activation measures of *Garantie Jeunes* can affect the efficacy of search effort and the probability of actually finding the job individuals are looking for. 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) but costs time. On the other hand, studies on the effect of cash transfer to job seekers focus on its effect on the amount of work an individual is willing to supply, which reacts to implicit taxation derived from cash transfers phase out, and to disincentives to work due to 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 to jobseekers, through benefit increase (moral hazard) or wealth shocks (liquidity effect), reduces labor supply and search effort.

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

I will exploit two dimensions of treatment variation: the timing of the activation program and the 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 time for actually looking for a job (“lock-in” effect). The right panel of Figure 9 reports instead the evolution of income with and without *Garantie Jeunes*. Second, as mentioned in Section 2, the cash transfer of *Garantie Jeunes* can be fully cumulated with job earnings up until €300. The transfer is then reduced quite steeply for every additional Euro of job earnings, until disappearing 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 appears clearly how 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 quite a high marginal tax rate.

Figure 9: Working days with a scheduled activity over 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 over 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.

I now turn to my data to estimate possible reactions of participants in *Garantie Jeunes* to time-consuming activation measures and the phase-out of the cash transfer. To obtain identified estimates of treatment heterogeneity, one possibility is to use Proposition 3 to recover the LATE for individuals in the 1st semester, 2nd semester, or after *Garantie Jeunes*. I can do this separately for the probability of earning a monthly amount below €300, between €300 and €1159, or above €1159 as outcome. For separating the second and third category, I will use 1100 threshold 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 at the net minimum wage (which is slightly lower, especially at the beginning of the period)²⁷.

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

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, not remunerative jobs. Then, once youths completed the most time-consuming part of the program, but still receive 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, 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, as already clear from Section 3.

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 into 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* (0.0359)	-0.0482* (0.0290)	0.0221 (0.0361)
LATE 2nd semester of enrollm.	0.0846** (0.0431)	-0.146*** (0.0544)	0.129** (0.0577)
LATE after completion	-0.0863 (0.0618)	0.188*** (0.0700)	0.197** (0.0793)

Average outcomes of takers in treatment group			
	Monthly income 1-300	Monthly income 350-1100	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 brackets. The lower panel reports the average outcomes estimated for the takers of the treatment group.

5.2 A Simple Theoretical Framework for Interpretation

I now aim at disentangling more formally the effects of activation and cash transfers. the cash transfer (implicit taxation and disincentives to labor supply), from activation measures (lock-in from lack of time and increase in job search through activation), and from potential complementarities between the two. To do so, it is necessary to impose some structure. 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,

Although a little spike is observed at €300, the magnitude is relatively small.

>€1100 per month. Since €1100 is 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 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, since takers of *Garantie Jeunes* mostly work at the minimum wage, are few with respect to the overall population, and the estimated effect of the program on wages is non-significant (Table 4).

Assume that utility for individual i and choice j is linear, $U_{ji} = u_j + \eta_i = a_1((1-\tau)z_j + b) + a_2z_j/w + \eta_i$. 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 taxation, and η_i is individual heterogeneity. Denote $\alpha_j = a_1z_j$, $\beta = a_1b$, $\gamma_j = a_2z_j/w$. 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:

$$Pr(z^{j^*} = z^j) = \Phi_j(cash)$$

$$\text{where } \begin{cases} \Phi_1(1) = \frac{e^{\alpha_1 + \beta + \gamma_1}}{e^{\alpha_0 + \beta} + e^{\alpha_1 + \beta + \gamma_1} + e^{\alpha_2 * (1-\tau) + \gamma_2 + \beta} + e^{\alpha_3 + \gamma_3}} = \frac{e^{\hat{\alpha}_1}}{K_1} e^{\beta} \\ \Phi_1(0) = \frac{e^{\alpha_1 + \gamma_1}}{e^{\alpha_0} + e^{\alpha_1 + \gamma_1} + e^{\alpha_2 + \gamma_2} + e^{\alpha_3 + \gamma_3}} = \frac{e^{\hat{\alpha}_1}}{K_0} \\ \Phi_2(1) = \frac{e^{\alpha_2 * (1-\tau) + \gamma_2 + \beta}}{e^{\alpha_0 + \beta} + e^{\alpha_1 + \gamma_1 + \beta} + e^{\alpha_2 * (1-\tau) + \gamma_2 + \beta} + e^{\alpha_3 + \gamma_3}} = \frac{e^{\hat{\alpha}_2}}{K_1} e^{\beta - \alpha_2 \tau} \\ \Phi_2(0) = \frac{e^{\alpha_2 + \gamma_2}}{e^{\alpha_0} + e^{\alpha_1 + \gamma_1} + e^{\alpha_2 + \gamma_2} + e^{\alpha_3 + \gamma_3}} = \frac{e^{\hat{\alpha}_2}}{K_0} \\ \Phi_3(1) = \frac{e^{\alpha_3 + \gamma_3}}{e^{\alpha_0 + \beta} + e^{\alpha_1 + \beta + \gamma_1} + e^{\alpha_2 * (1-\tau) + \gamma_2 + \beta} + e^{\alpha_3 + \gamma_3}} = \frac{e^{\hat{\alpha}_3}}{K_1} \\ \Phi_3(0) = \frac{e^{\alpha_3 + \gamma_3}}{e^{\alpha_0} + e^{\alpha_1 + \gamma_1} + e^{\alpha_2 + \gamma_2} + e^{\alpha_3 + \gamma_3}} = \frac{e^{\hat{\alpha}_3}}{K_0} \end{cases} \quad (5)$$

Where *cash* is a dummy for being enrolled in treatment or not, hence having the right to receive the cash transfer, while $\hat{\alpha}_j = \alpha_j + \gamma_j$, $K_0 = e^{\alpha_0} + e^{\alpha_1 + \gamma_1} + e^{\alpha_2 + \gamma_2} + e^{\alpha_3 + \gamma_3}$ and $K_1 = e^{\alpha_0 + \beta} + e^{\alpha_1 + \gamma_1 + \beta} + e^{\alpha_2 * (1-\tau) + \gamma_2 + \beta} + e^{\alpha_3 + \gamma_3}$.

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 achieving that job, and alternatively remaining unemployed, $P(\cdot)$, due to search frictions.

$$Pr(Y_{ji} = 1) = Pr(z_{j^*} = z_j) \cdot P(\text{active}, \text{time}) \quad (6)$$

where the magnitude of the search frictions depends from if 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 while equal to zero if 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 5 derives from consumers maximizing only as a function of *cash*, without considering that the labor supply decision depends on search frictions, hence on activation measures. This corresponds to fully separate the channel of the cash transfer (labor supply) and of activation measures (search frictions).

²⁸The relationship between *active* and $P(\cdot)$ 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 probabilities to find an employment. One option is that *Garantie Jeunes* increases η_i , or that the youth was underestimating its 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.

At this point, we can plug Equation 5 into Equation 6, 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 employment in different income brackets, for compliers in treatment and control groups, at different stages of the program

Treatment group			
	Monthly income 1-300	Monthly income 350-1100	Monthly income over 1100
LATE 1st semester of enrollm.	$\Phi_1(0) \frac{K_0}{K_1} e^\beta \cdot P(1, 0)$	$\Phi_2(0) \frac{K_0}{K_1} e^{\beta - \alpha_2 \tau} \cdot P(1, 0)$	$\Phi_3(0) \frac{K_0}{K_1} \cdot P(1, 0)$
LATE 2nd semester of enrollm.	$\Phi_1(0) \frac{K_0}{K_1} e^\beta \cdot P(1, 1)$	$\Phi_2(0) \frac{K_0}{K_1} e^{\beta - \alpha_2 \tau} \cdot P(1, 1)$	$\Phi_3(0) \frac{K_0}{K_1} \cdot P(1, 1)$
LATE after completion	$\Phi_1(0) \cdot P(1, 1)$	$\Phi_2(0) \cdot P(1, 1)$	$\Phi_3(0) \cdot P(1, 1)$
Control group			
	Monthly income 1-300	Monthly income 350-1100	Monthly income over 1100
LATE 1st semester of enrollm.	$\Phi_1(0) \cdot P(0, 1)$	$\Phi_2(0) \cdot P(0, 1)$	$\Phi_3(0) \cdot P(0, 1)$
LATE 2nd semester of enrollm.	$\Phi_1(0) \cdot P(0, 1)$	$\Phi_2(0) \cdot P(0, 1)$	$\Phi_3(0) \cdot P(0, 1)$
LATE 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 $Pr(Y_{ji} = 1|enroll)$, the probability of being actually employed in bracket j given enrollment status in *Garantie Jeunes*. It is obtained from Equation 5 and Equation 6.

To estimate the empirical counterparts of Table 6, I need to recover estimates of average outcomes for control group takers, which are otherwise non observed. 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 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 in treatment and control to their structural interpretation I obtain a system of 18 equations with 10 unknowns of interest, $P(1, 1), P(1, 0), P(0, 1), \hat{\alpha}_0, \hat{\alpha}_1, \hat{\alpha}_2, \hat{\alpha}_3, \beta, \alpha_2 \tau$. However, as shown in the Appendix, the system is not identified since only 8 equations are linearly independent. One can normalize $\hat{\alpha}_0 = 0$, the utility of being unemployed, and obtain the estimates in Table 7.

The results show that the 55% implicit tax rate drives away enrolled youths from the implicitly-taxed brackets, reducing employment of 52% in them. This corresponds to a 0.9% reduction in employment for each point of marginal implicit taxation. The implied elasticity to net-of-tax earnings clearly depends from the average earnings of youths who react to the cash transfer, which in difference-in-difference context with 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. In addition, cash transfers attract youths in unemployment or part-time jobs, where the transfer is guaranteed, increasing the probability of being part of those brackets by 16% on average. As a consequence, note that $\frac{K_0}{K_1}$ corresponds to $\frac{\phi_3(1)}{\phi_3(0)}$, i.e. the reduction in full-time employment due to the cash transfers attracting youths to lower brackets, estimated at -5%.

Turning to the effect of activation measure, the results point at a negative lock-in effect (effect of time availability is +64%). This shows that youths participating in the program, especially the ones who chose to work part-time, face significant time constraints. Finally, the positive effect of activation of the youth is

Table 7: 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	$e^{-\alpha_2\tau}$ (implicit tax)	.483
$\hat{\alpha}_1$	-1.652	$\phi_2(0)$.130		
$\hat{\alpha}_2$	-1.436	$\phi_3(0)$.215	e^β (cash transfer)	1.163
$\hat{\alpha}_3$	-.935	$\phi_1(1)$.116		
β	.151	$\phi_2(1)$.069	$\frac{K_0}{K_1}$ (cash tr. option)	.952
$\alpha_2\tau$.726	$\phi_3(1)$.205		
$P(1, 0)$.608			$\frac{P(1,0)}{P(1,1)}$ (lock-in)	1.642
$P(1, 1)$	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 effects of different components of *Garantie Jeunes*. The effects in the last column are multiplicative.

extremely high, corresponding to more than doubling employment in cells where youths have been activated, on average (note that these estimates deliver a corner solution, $P(1) = 1$). The mirror image of this large effect is a probability of finding the optimal employment for youths in the control group which is very low, only 48% in a semester. This points out that at baseline youths have low matching probability, either because they face high search frictions, or because they exert low effort.

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

The analysis in the previous section necessarily assumed that cash transfers and activation measures affected employment through independent channels (Φ and P respectively). However, cash and activation can interact among each other. For instance, the joint effect of cash transfers and activation measures can be larger than the sum of the two conditional effects. Given the large magnitude of the effect of activation I find, it seems possible that it is at least in part due to “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 further disentangle which channel is actually at play, and whether higher activation *causes* a reduction in the disincentives implied by cash transfers, or just compensates them. This would require not only an exogenous shock to cash transfers or activation measures in *Garantie Jeunes*, but probably also modelling the channels of the complementarities and measure 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 tightly related to mine is the one by Aeberhardt et al. (2020). Such working paper studies the effect of an increase in cash transfers but keeping activation measures constant, in the same context of this paper. The authors consider an experimental program introduced in a small set of French YECs in 2011 before *Garantie Jeunes* and offering a similar cash transfer to youths in the standard YECs program, but no extra activities. The cash transfer was equivalent to the one of *Garantie Jeunes* in terms of cumulative amount, but was spread over two rather than one year, and never cumulative with job earnings. Hence, the monthly amount of the transfer and the rate of implicit taxation were roughly half than in *Garantie Jeunes*. The authors find that such a program determined only an increase in time staying at YECs and in attendance to compulsory activities. The effect on search effort is null, while a -2 p.p. effect on employment appears in the first six months of the program.

Firstly, part of the difference in the estimated effects on employment in this paper and in Aeberhardt et al. (2020) could be explained by the long duration and ubiquitous implicit marginal taxation of their cash transfer. Yet, it seems natural to attribute a part of the wedge in results to the significant additional activation requirements introduced by *Garantie Jeunes*. While in Aeberhardt et al. (2020) youths are required to attend only monthly counseling sessions, the standard program at YECs, *Garantie Jeunes* requires a month of initial intensive training, several job immersions, and twice as much counseling²⁹. This can suggest the presence of complementarities, for example if guaranteeing extra cash only augments benefits/sanctions, but generating a larger increase in job search requires an increase in monitoring.

An opposite setting to the one 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*, but some working papers signal 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. More generally, Card et al. (2018) run a meta-analysis on 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. This paper also finds a positive effect of activation, but the magnitude is large compared to programs in Card et al. (2018); Crépon et al. (2013b); van den Berg et al. (2015), and crucially appears only after the end of cash transfers. Again, this might be suggestive of interactions between cash and activation, with cash transfers reducing labor supply but adding an extra channel to the effect of activation, through an increase in monitoring or participation.

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.

²⁹It 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 context 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).

This work speaks chiefly to the literature of policies for employment. Papers until now only evaluated active policies, such as activation measures, conditional on receiving passive policies, such as cash transfers, and viceversa. 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 staggeredly adopted by these groups, so that units are exposed to treatment at different tenures, the diff-in-diff methodology proposed is flexible to estimate dynamic ITT and LATE, is robust to selection into treatment over tenure, as well as to heterogeneous treatment effects. Although 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, 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 would they change, is an open question hiding insights for different fields. The effect might in fact decrease not only for youths motivation, but also for organizational changes on the side of YECs. In this early phase, 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). Or, given the extremely disadvantaged population targeted by *Garantie Jeunes*, positive externalities might arise from a reduction in crime rates of participants (Britto et al., 2020). Sociological evaluation by Loison-Leruste et al. (2016) reports numerous stories of youths in *Garantie Jeunes* grown up in high-delinquency environments.

This work has relevant policy implications. The simplest one is that it proves that 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 Gauthié (2018). This offers counterfactual evidence in support of comparative analysis recommending active and passive labor market policies OECD (2020). My estimates show that disincentives from cash benefits are present and that program phase-out with job earnings is costly due to implicit taxation. A possible solution is allowing youths to fully cumulate benefits and job earning, but this is clearly costly. Activation is shown to be a viable alternative, as its effect is estimated strong enough to compensate for lock-in and distorsive effects of the cash transfers. Finally, my insights can be applied to 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 based on motivation and fragility,

run at a decentralized level by well-established YECs. Also, 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. Concerning potential extensions and scale-up, it might not be easy to maintain cost-effectiveness if returns on the marginally eligible youth are decreasing.

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Proofs

Proof of Proposition 1

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

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

Proof of Proposition 2

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

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

$$\begin{aligned}
\mathbb{E}(DID_{w,c}^h) &= \mathbb{E}(Y_{w,c}^h(g) - Y_{w,c}^h(0)) \\
&= \mathbb{E} \left[\sum_{i \in w,c|h} Y_{i,w,c}^h(d > 0) - Y_{i,w,c}^h(0) \middle| D_{i,w,c}^h > 0 \right] Pr(D_{i,w,c}^h > 0) \\
&= \sum_{d=1}^g \mathbb{E} \left[\sum_{i \in w,c|h} Y_{i,w,c}^h(d) - Y_{i,w,c}^h(0) \middle| D_{i,w,c}^h = d \right] Pr(D_{i,w,c}^h > 0) \quad \forall w, c, h
\end{aligned}$$

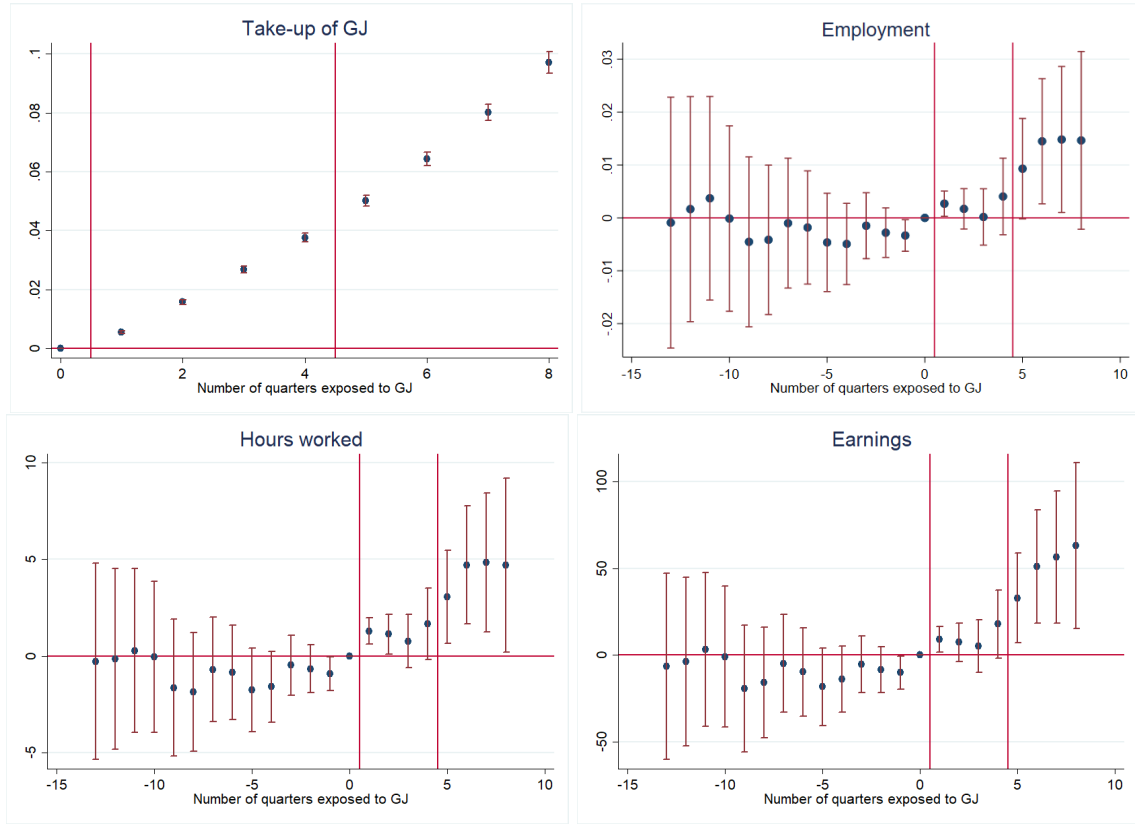
Appendices

A ITT Through an Event-Study Approach

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 ???. 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 Additional Tables and Figures

Table 8: Characteristics of youth at time of registration to 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 9: 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

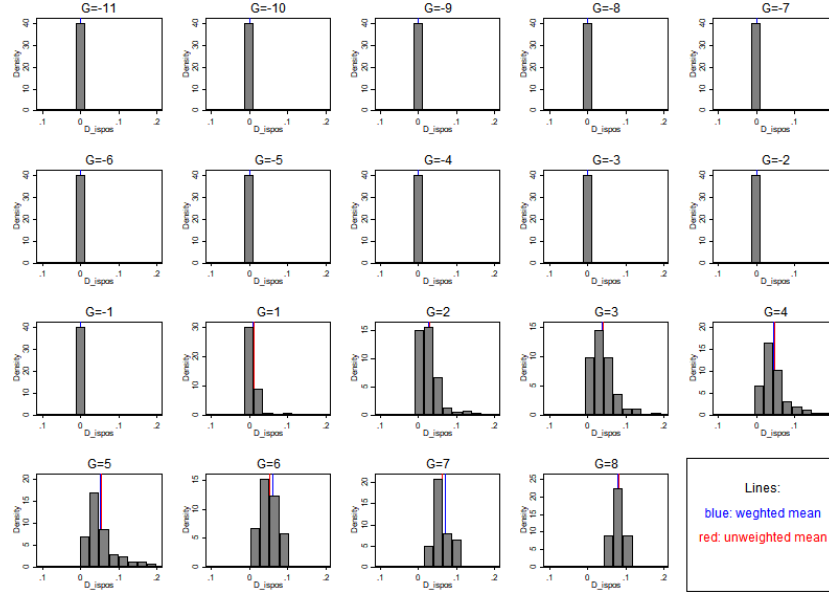
Table 10: Number of youths registering to YEC by quarter and wave

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 11: 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.

$DD_{w,c}^4$	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	0.0001	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
$Pr(D_{w,c}^4 > 1)$	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	0.0027	0.0076	0.0165	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
$n_{w,c}^4$	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 $DID_{w,c}^h \quad \forall w, c, h : G_{w,c}^h = g$ 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

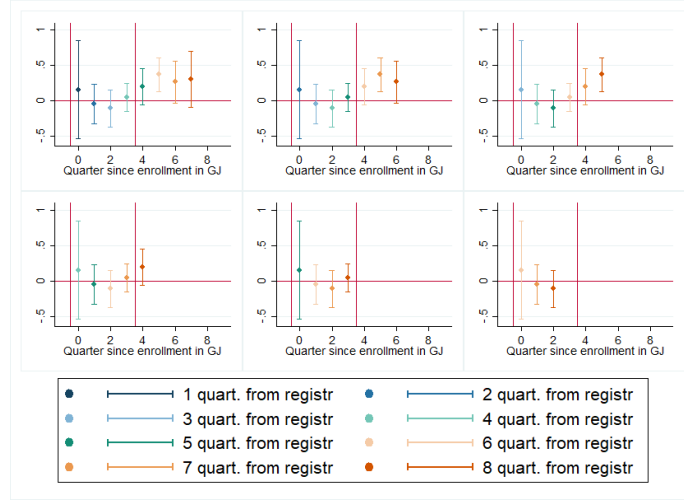
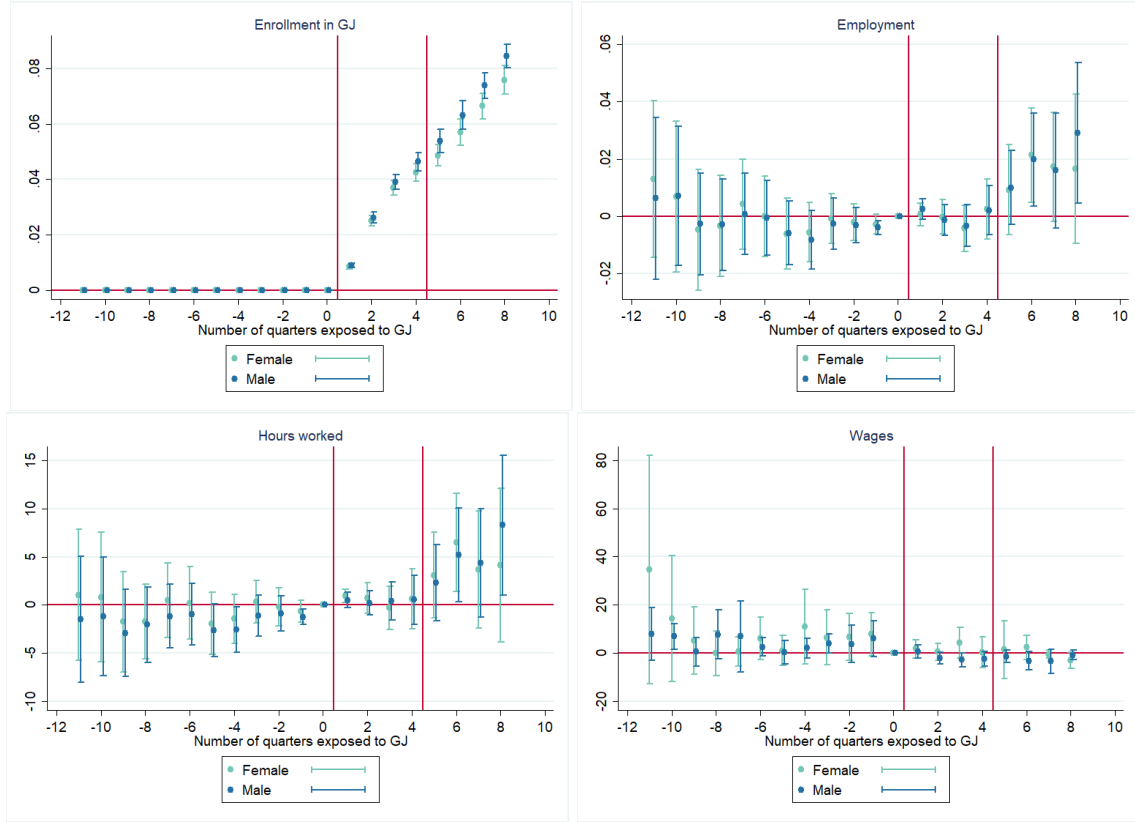


Table 12: Heterogeneity by employment contract

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

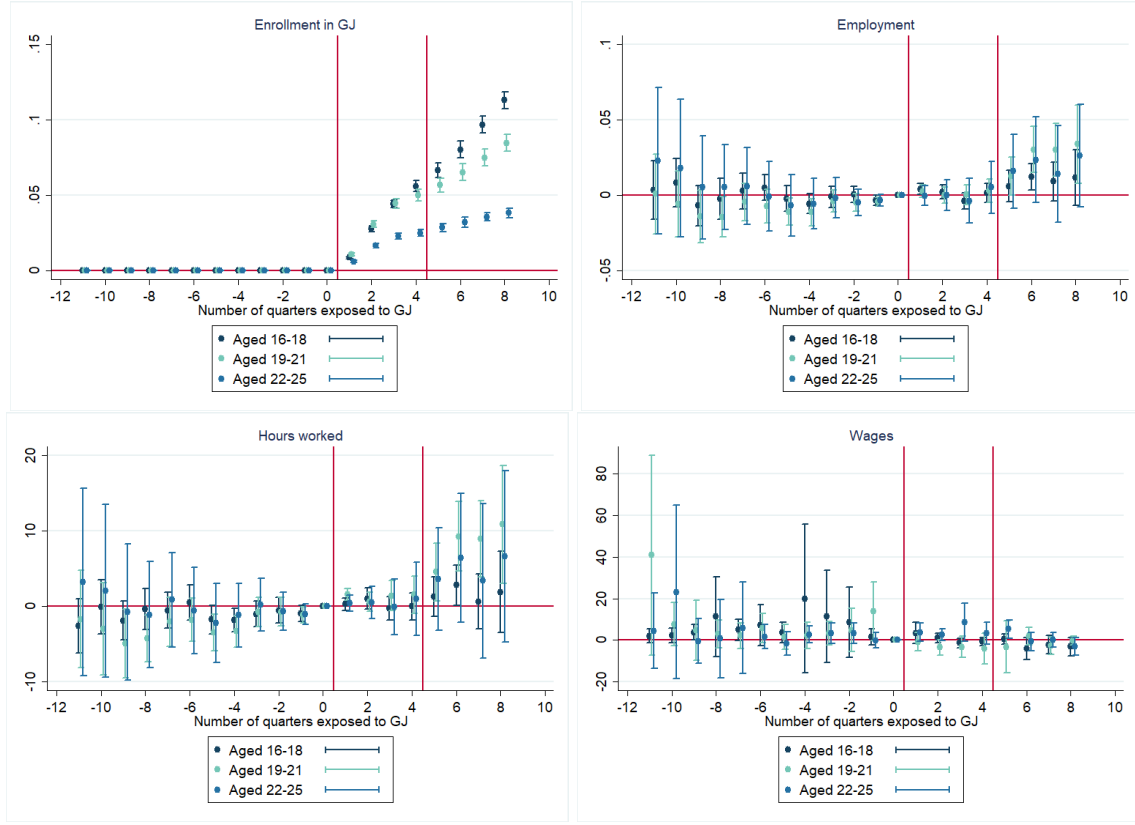
Notes. The table reports the main results obtained following the rolling diff-in-diff methodology developed in Section 3. The upper panel reports weighted averages of the $DID_{w,c}^h$ coefficients where exposure is between 1 and 4 quarters or above 4 quarters. The lower panel reports the estimates of 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



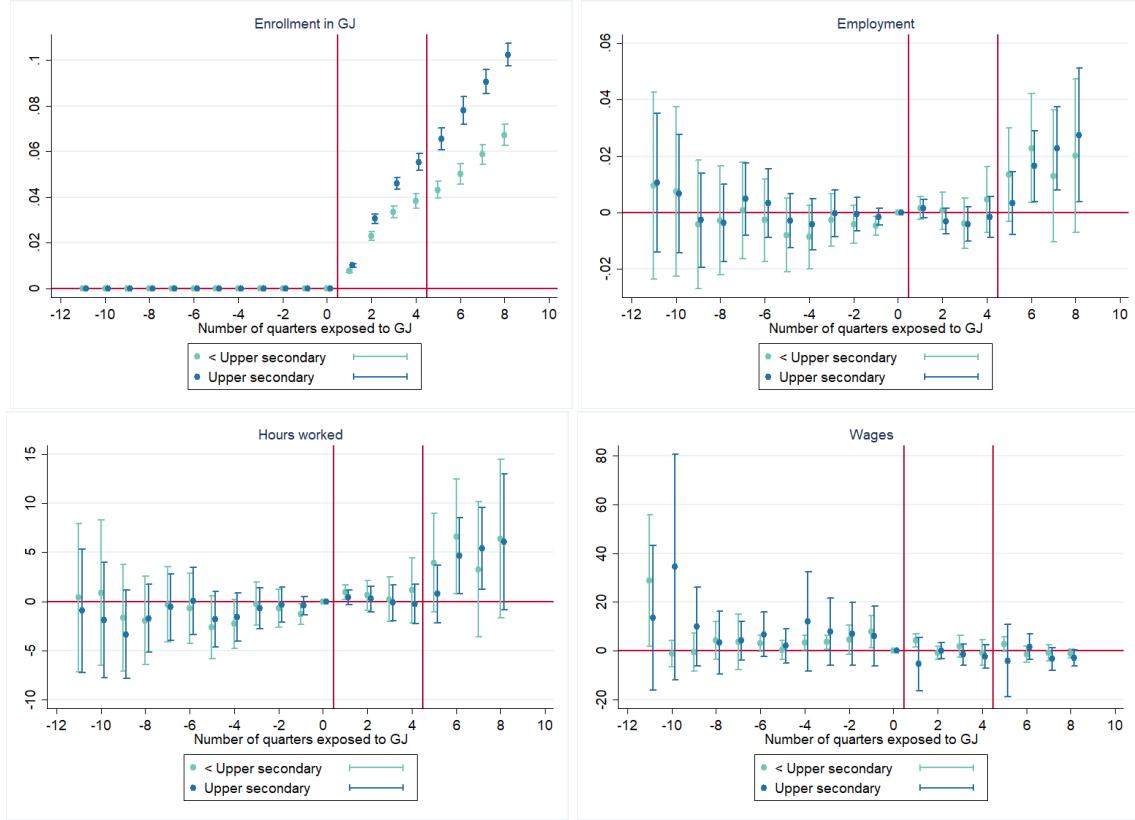
Notes. The figure reports results of the rolling diff-in-diff approach for different gender sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a 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



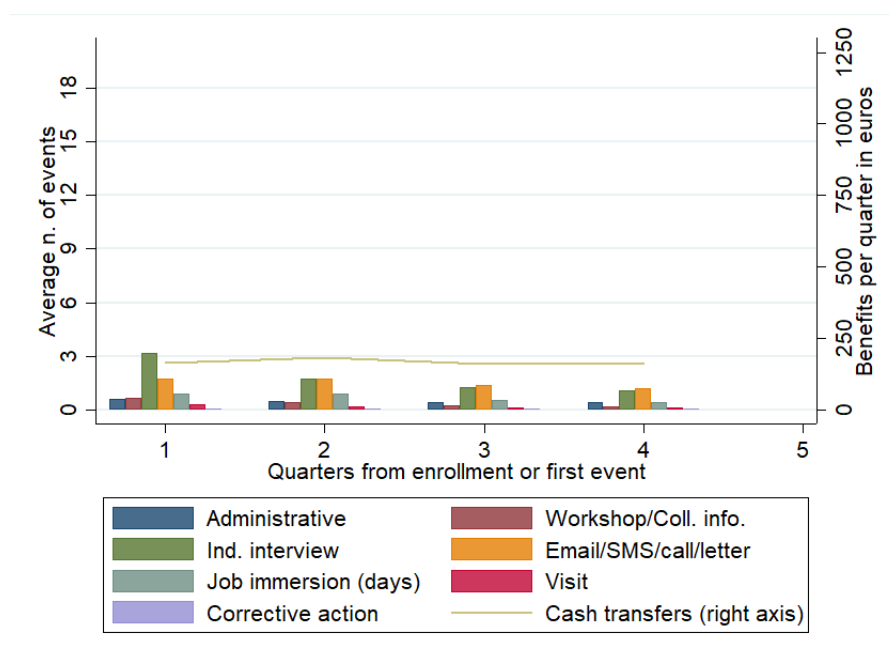
Notes. The figure reports results of the rolling diff-in-diff approach for different age sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy 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



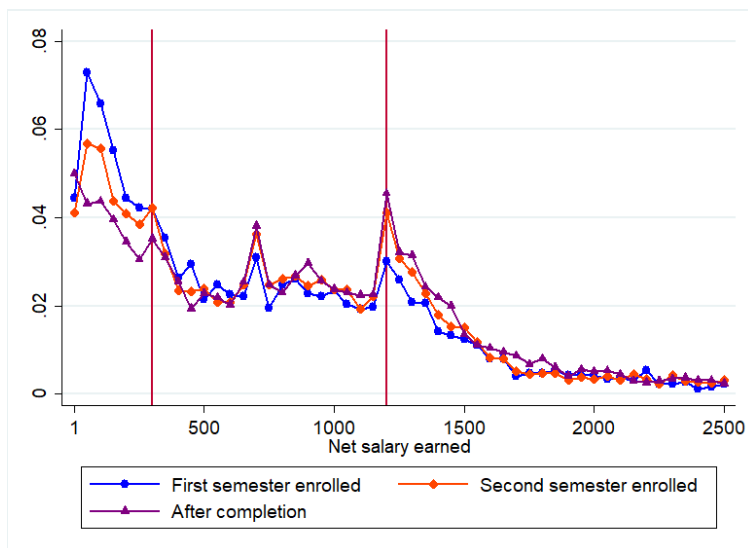
Notes. The figure reports results of the rolling diff-in-diff approach for different sub-samples defined by higher education degree attained. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* 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 administrative data on earnings of each contract gross of social contributions, assuming constant daily earnings.