# 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 only evaluated them one conditional on the other. This paper evaluates a large French program providing a year of cash transfers and intensive activation measures to disadvantaged youths Not in Employment Education of Training (NEETs). I develop a difference-in-difference methodology which extends De Chaisemartin and D'Haultfœuille (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 is instead detected on wages. I investigate the mechanisms using the timing of activation measures, the phase-out of the cash transfer, and a framework with discrete labor supply and search frictions. I find that the zero effect during enrollment arises from a negative reaction to implicit taxation from transfer phase-out, lock-in from training, and a counterbalancing positive effect of activation. This suggests that the elasticity of labor supply, time constraints, and search frictions play a significant role for disadvantaged NEETs. Finally, if disincentives to work generated by the cash transfer are assumed larger, the results imply larger effect of activation, which suggests potential complementarities.

**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-skills[1]. Then, NEET spells can become a poverty trap. In fact, unemployment has proven "scarring" permanent consequences on employablity (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).

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 often 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 points in employment

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

[2] The 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.

(on a mean rate of 49%) and +70 in quarterly hours. To hedge the result, it should be noted that the ITT 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 1.15.

To identify the effect of the program, I exploit its staggered adoption across different regions, between the last quarter of 2013 and the beginning of 2017. For estimation, I develop a new "rolling" diff-in-diff methodology, which extends De Chaisemartin and D'Haultfœuille (2020a) 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 time 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 can be a misleading proxy of the effect since exposure/enrollment. 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, employment, based on the monetary incentives (cash transfer and implicit taxation) provided by *Garantie Jeunes*. Then, they face a search friction that depends on their time availability to search and on their activation. I find that implicit taxation from the phase-out reduces employment in the taxed brackets by 1.5% for each point of marginal implicit taxation. Formal estimation of the lock-in effect and of the effect of activation requires an assumption either on the amount of search frictions faced by treated youths after the program or on the increase in total utility generated by the program. For a range of reasonable values, lock-in reduces the probability of finding a job by 18-21 percentage points. The improvement associated to activation varies more widely with the assumptions, between 31 and 46 percentage points, since it increases with the increase in total utility generated by the cash transfer. In fact, the larger the increase in total utility generated by the cash transfer, the larger the share of youths who will find it convenient to be unemployed to get the benefit. Given the zero effect while youths are receiving both cash and activation, only a larger positive activation effect can rationalize such results with larger disincentives to work generated by the cash transfer. This suggests that activation might function, at least in part, as a complement to cash transfers, providing larger monitoring and lower search costs, pushing youths to exert more search effort.

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. A smaller-scale experimental study by (Aeberhardt et al., 2020) on a program offering cash transfers to a similar population found 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. Crucially, the program they evaluate only topped up standard employment services with a conditional cash transfer, without providing additional activities[3]. In fact, meta-analysis as in Card et al. (2018) shows that activation measures with a work-first approach can have a 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)[4], and benefits materialize only after the end of cash transfers. I also show that cash transfer affect labor supply through implicit taxation and by increasing the relative utility of partial employment, a possible mechanism behind the estimates of Aeberhardt et al. (2020). Finally, analyzing my results with some structure I find that activation has larger effect the larger the assumed disincentives to work generated by cash transfers. This might suggest the presence of 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 incrased with better search technology.

Secondly, the results provide empirical insights for the literature on labor supply and job search behavior. The effect on partial employment of implicit taxation arising from benefits phase-out is studied by (Le Barbanchon, 2020), who finds that additional 10% taxation reduces labor earnings by 1-2%. In the case of disadvantaged NEETs, with a very peculiar and selected population of takers, I estimate a 75% decrease in employment in brackets where taxation decreases marginal earnings by 55%, a reaction in the upper range of estimates in the literature (Saez et al., 2012). 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 confirm the role of time empirically, finding that time-consuming activation measures generate lock-in. Activation is shown to increase search efficacy, although the magnitude of this boost depends from how much of the effect is due to monitoring. Possibly, as different streams of the literature have shown, the effect arises from the fact that *Garantie Jeunes* tackles lack of networks, geographical isolation and low soft skills which can dramatically limit job search in this population[5].

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

[3]The contest is exactly the same, but eligible youths are randomized (not selected). Also, compared to *Garantie Jeunes*, in the program evaluated by Aeberhardt et al. (2020) cash transfers are twice as long for half the monthly amount, and are never cumulative with job earnings.

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

[5]See Ioannides and Datcher Loury (2004); **?**); **?**); **?**); Kramarz and Skans (2014); Marinescu and Rathelot (2018); Mendolia and Walker (2014)

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.

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 search technology, lock-in, cash and implicit taxation on labor supply. 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 Initiative[6]. 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

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

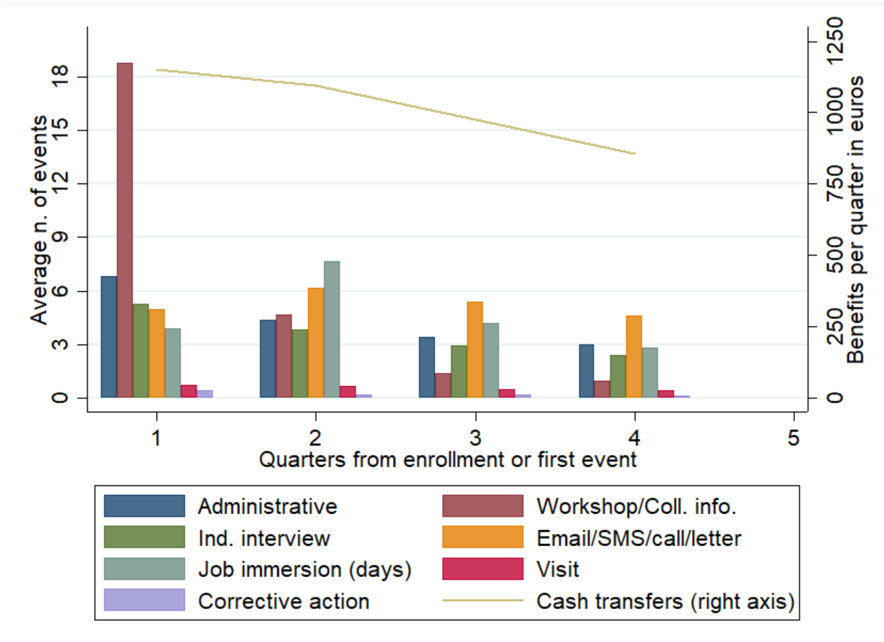
second part is characterized by a "work-first" approach, i.e. high frequency of proposals of internships and short work experiences of at most a month called "job immersions"[7], during which the youth works on small tasks in a partner firm with the aim of discovering the working environment and the 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 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)[8].

[7] Technically these contracts are called *période de mise en situation en milieu professionelle* (PMSMP)

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

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



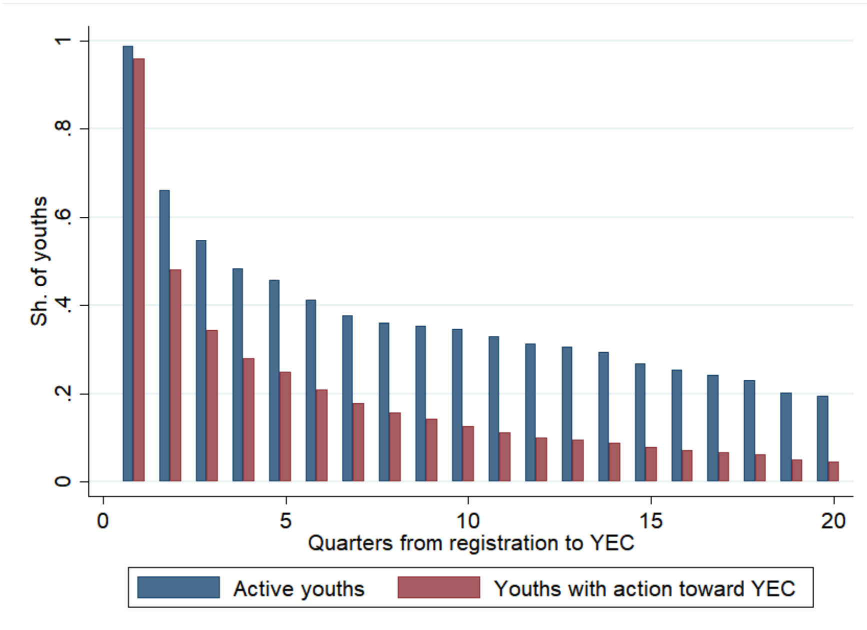

Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from enrollment in *Garantie Jeunes*. The cash transfers series plots instead the average amount of benefit to youths participating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

French local Youth Employment Centers (YECs)[9] 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 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 17 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"[10], e.g. an email sent by the youth, an interview, or another activity with participation by the youth.

[9] *Missions Locales* in French

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

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



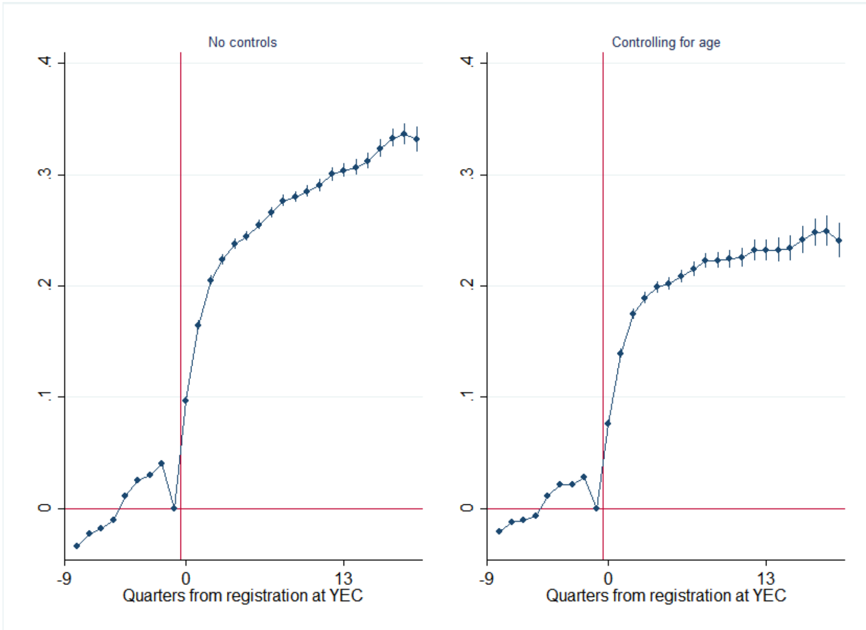

Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from registration in the YEC. "Active youths" are considered as those whose *dossier* records any kind of action in the quarter.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

YECs are organized into 459 centers[11] 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 point[12]. 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 YECs).

[11] These 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 cathegories

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

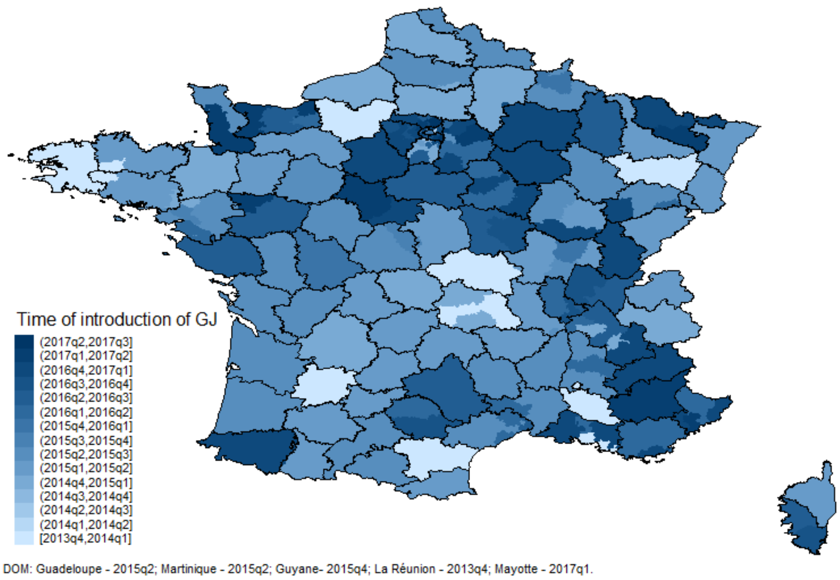
Figure 3: Average employment rates in the quarters precedent/following registration at YEC, controlling or not for age




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

The introduction of *Garantie Jeunes* was progressive, which provides our source of identification. A 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 *départments*) by date of first case of enrollment in *Garantie Jeunes* in their corresponding YEC. Overseas territories (DOM) are reported at the *départment* level.

*Garantie Jeunes* represented a challenge for YECs: the program was much more organizationally demanding than their standard activities, and political attention by the central government was high, with regular reporting requirements. YECs receive additional funding for administering *Garantie Jeunes* conditional on the number of youths enrolled (70% of the funding), on the number of youths who 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 convicted[13]. 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 commissions[14]. 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

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

[14]These commissions are composed by a president appointed by the local *Prefecture*, one representative of the government of the *départment*, 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 records[15] 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 8I will simply refer to this as "wage" later).

After cleaning[16], 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

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

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

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 *Garantie Jeunes* |
|---|---|---|---|---|
| Number of youth (total youth stock) | 9327476 | 1967000 | 444309 | 113085 |
| Number of youth (avg. quarter inflow) | | 125689 | 41471 | 14899 |
| Lower than secondary educ. | 0.394 | 0.373 | 0.424 | 0.467 |
| Upp. secondary edu. diploma | 0.434 | 0.519 | 0.541 | 0.507 |
| Avg. age | 20.3 | 20.1 | 19.7 | 18.8 |
| Female | 0.491 | 0.491 | 0.511 | 0.463 |
| French nat. | 0.915 | 0.912 | 0.919 | 0.929 |
| Empl. last quarter | 0.297 | 0.349 | 0.335 | 0.212 |
| Lives independently | 0.230 | 0.365 | 0.369 | 0.354 |
| Has 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 YEC[17].

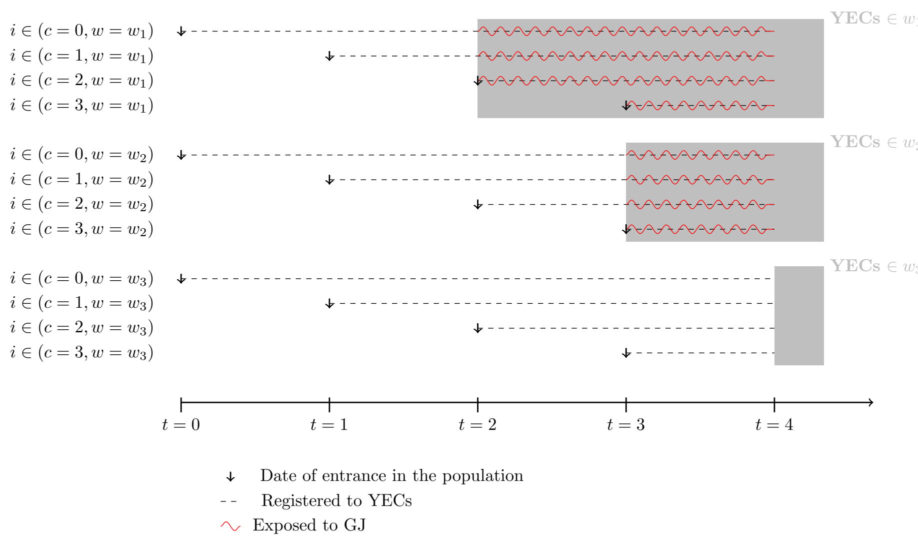
## 3.2 Identification

### 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, $c \in \{1, ..., \bar{c}\}$, based on the period when she enters the population. Each YEC belongs to a wave of introduction of *Garantie Jeunes*, $w \in \{1, ..., \overline{w}\}$. In my data, I am then able to follow each individual over cardinal time $t$, or equivalently over time since registration in the YECs $h = t - c + 1, h \in \{1, ...\overline{h}\}$, 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.

[17] Waves 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 10 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 11) and the number of youths registering to YECs (Table 12) is distributed across waves and cohorts.

Figure 5: A simplified illustration of the setting



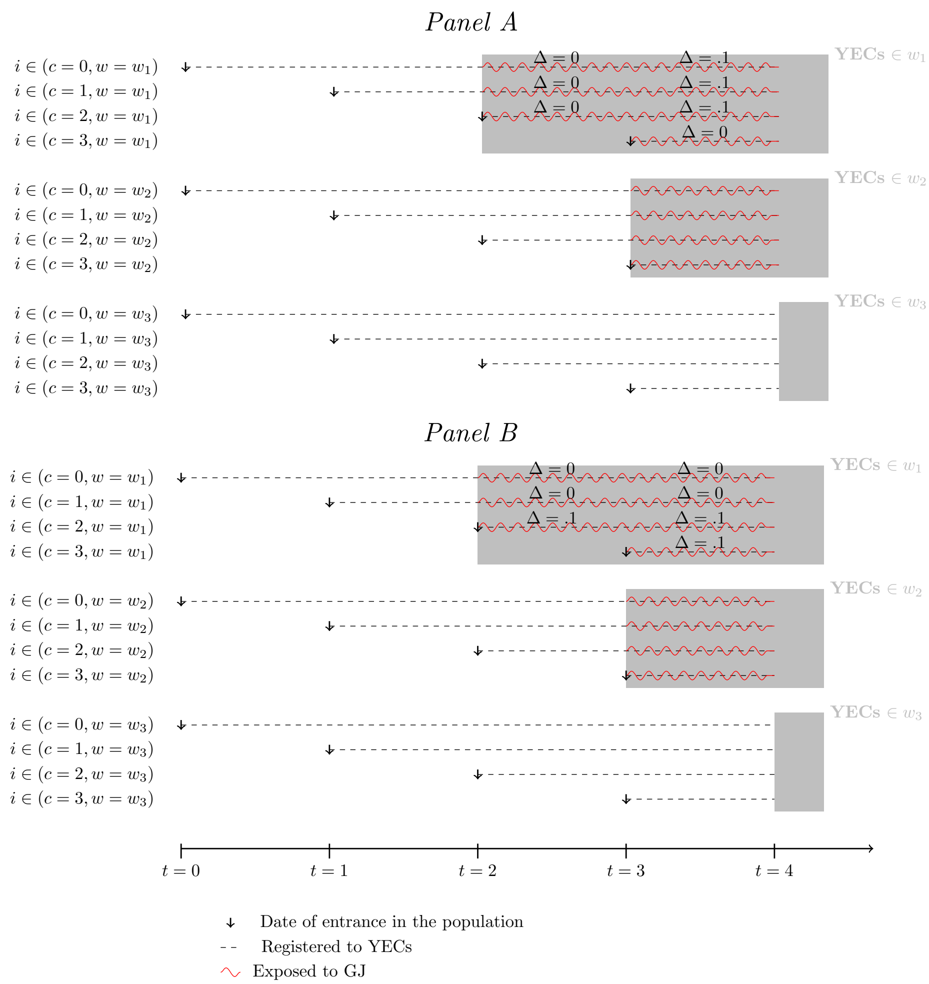

De Chaisemartin and D'Haultfœuille (2020b) show that in staggered adoption designs event studies using two-way fixed effects or first difference estimators heavily rely on homogeneous treatment effects, and are otherwise biased due to negative weighting of the effect in some groups. They propose a version 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 $G^h_{w,c}$ 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 $G^h_{w,c} = 1$ and $\Delta = .1$ when $G^h_{w,c} = 2$, so that the true program effect is increasing over exposure. The average effect since two periods of *exposure*, whenever $G^h_{w,c} = 2$, is then 0.1. However, the average effect since 2 periods of *adoption* is $DID^{DCDH}_{w_1,t=4} = 0.075$. 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 $G^h_{w,c} > 0$, $h > G$, i.e. for youths who registered before treatment introduction and are exposed later. Instead, $\Delta = .1$ if $G^h_{w,c} > 0$, $h = G$, i.e. for youths who registered at the moment of introduction of the program or later. The average effect when $G^h_{w,c} = 2$ is .03, but the effect two periods since adoption $DID^{dCDC}_{w_1,t=2} = 0.05$.

To assess these two concerns, I propose an estimator which extends the one by De Chaisemartin and D'Haultfœuille (2020a) but rolling over time since registration $h$, $DID^h_{w,c}$. 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 $(w, c|h) \rightarrow G^h_{w,c}$, $DID^h_{w,c}$ 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 interpretation[18].

### 3.2.2 Formal Identification of ITT

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

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

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

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

and when not exposed[19] :

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

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

**Assumptions 1-4.**

1. *(Independent groups)* Treatment (i.e. *Garantie Jeunes* introduction) of one wave doesn't influence the evolution of potential outcomes of others, i.e. $\mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)|G^h_{w,c}, G^h_{w',c}) = \mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)|G^h_{w,c})$ for each wave $w$, given YEC-tenure $h$;
2. *(Strong exogeneity)* Treatment (i.e. *Garantie Jeunes* introduction) is independent from the evolution of mean potential outcomes when non-treated: $G^h_{w,c} \perp\!\!\!\perp \mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)), \forall c, c'$, given YEC-tenure $h$;
3. *(No anticipation)* Mean potential outcomes $Y_w, c^h$ in a cohort at a specific point in time are independent from treatment status in the next period $G^h_{w,c+1} = G^{h+1}_{w,c}$, so that outcomes when treated depend only on past exposure $Y^h_i = Y^h_i(G^h_{w,c})$;
4. *(Common trends)* Expected variation in potential outcomes when non-treated doesn't vary across waves, given YEC-tenure $h$: $\mathbb{E}(Y^h_{w,c}(0) - Y^h_{w,c'}(0)) = \mathbb{E}(Y^h_{w',c}(0) - Y^h_{w',c'}(0))$.

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

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

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

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

Where $G^h_{w,c'} = 0$ but $G^h_{w,c'+1} = 1$, and $\Omega_{w,c}$ is the set of waves such that $G^h_{w',c} = G^h_{w',c'} = 0$, for each $w' \neq w$ and $c' \neq c$. $n_{w'}$ is the number of individuals of cohort $c$ in wave $w'$ while $N_{\Omega_{w,c}}$ is the total number of individuals of cohort $c$ in all waves $w' \in \Omega_{w,c}$. Proposition 1, proven in the Appendix, yields cell-specific unbiased estimators $DID^h_{w,c}$ for $\Delta^{ITT}(h, w, c)$.[20]

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

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

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

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

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

It can be shown that:

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

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

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

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

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

#### 3.2.3 Getting LATE

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

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

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

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

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

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

The first point of proposition 3 just points out at the possibility to recover a LATE on all takers, conditional on being in cells $(w, c|h) : G^h_{w,c} = g$ from simple rescaling of ITT estimates. To obtain that, one can divide estimates of the ITT effect by the take-up probability. This is not a novelty in IV estimation, but it is worth pointing out that the caveats highlighted by De Chaisemartin and d'Haultfoeuille (2018) don't apply 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 $d$: $\mathbb{E}(\delta(d, h, w, c)) = \mathbb{E}(\delta(d))$[21]. For gaining more power[22], 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 $\delta s$ applying Equally-Weighted Minimum Distance (Altonji and Segal, 1996; Card and Lemieux, 2001) to the regression:

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

# 4 Results

## 4.1 Balance checks

An implication of Assumption 2 (strong exogeneity) is that youth entering YECs before and after the introduction of *Garantie Jeunes* are comparable. In fact, youth belonging to cohorts before and after the introduction of *Garantie Jeunes* might differ, for example if the YEC becomes congested due to the program, if it tries to recruit more fragile youth to fill *Garantie Jeunes* spots (based on which YECs receive financing), or if a different selection of youth is attracted to the YEC due to the presence of *Garantie Jeunes*. Thus, in this section I exploit the wide range of information available in YECs administrative data to run a set of balance checks, making sure that 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.

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

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

Notes. The table reports the coefficients of a separate regression of each characteristic of youths registering to YECs (listed in the first column) on a dummy for GJ introduction (Check 1), on a linear trend (Check 2), and on both (Check 3). The last column reports the mean of the variable before GJ introduction. The dependent variables used are cohort size (number of youths registering), share of females, average age of youths registering, share of registering youth with lower than vocational-secondary education, with at most vocational secondary, and with at most secondary education, share with french nationality, residency in disadvantaged zones, housing difficulties, average resources declared, and distance between residency and 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

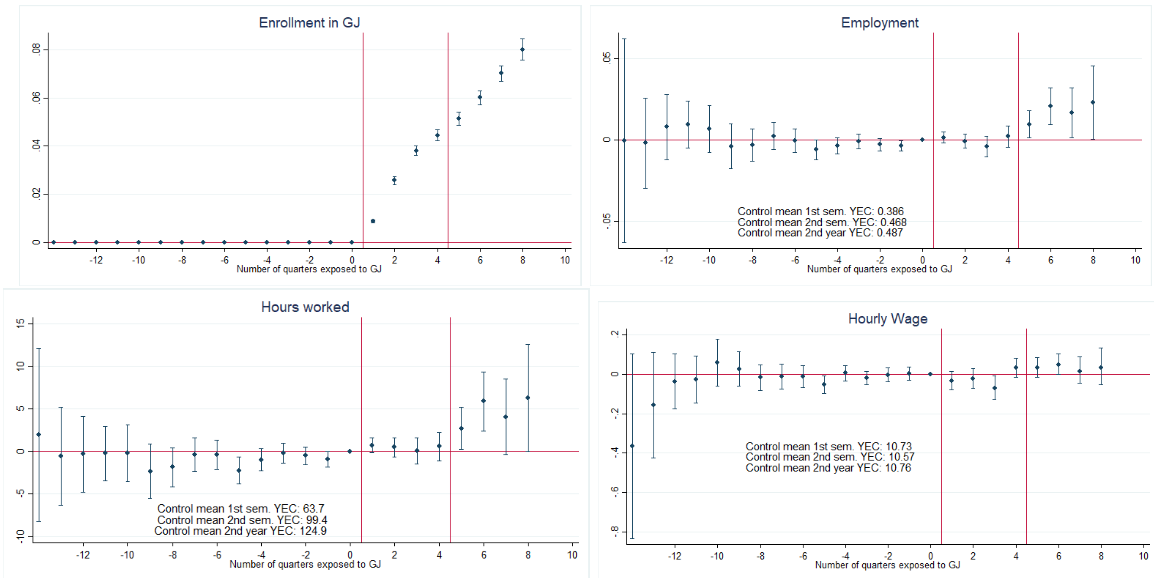
Following the rolling diff-in-diff approach outlined in Section 3, I estimate a full set of $DID^h_{w,c}$, for every $(w, c|h)$ cell, using equation (1)[23]. I group $DID^h_{w,c}$ corresponding to same levels of $g$ using Equation (2), and obtain the estimates of the ITT effect after being exposed $g$ quarters to the program, $DID^g$, plotted in Figure 7. Standard errors are obtained by bootstrapping, accounting for clustering at the YEC and cohort level, following the same algorithm of De Chaisemartin and D'Haultfœuille (2020b) and basing on Cameron and Miller (2015).

[23]For example, for $h = 4$ we obtain a wide set of coefficients such as in Table 13 in the Appendix. The distributions of $DID^h_{w,c}$ by $g$ are also reported in Figure 13 in the Appendix.

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

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

Subsequently, I can follow Proposition 3 to obtain estimates of the LATEs. First, I can use point a) to obtain

LATEs on all takers, conditional on quarters of exposure to *Garantie Jeunes*[24]. The results are shown in the upper panel of Table 4. This is symply 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<br>(1) | Hours<br>(2) | Wages<br>(3) |
|---|---|---|---|
| LATE 1st semester of exposure | 0.0246 | 35.1 | -1.76 |
| | (0.104) | (27.1) | (1.14) |
| LATE 2nd semester of exposure | -0.0322 | 6.63 | -0.695 |
| | (0.0680) | (17) | (0.573) |
| LATE 2nd year of exposure | 0.259*** | 70.7*** | 0.550 |
| | (0.0837) | (24.5) | (0.340) |
| LATE 1st semester of enrollm. | -0.0504 | 15.1 | -0.193 |
| | (0.0566) | (14.6) | (0.635) |
| LATE 2nd semester of enrollm. | -0.00801 | 14.1 | -0.0241 |
| | (0.0758) | (24.3) | (0.707) |
| LATE after completion | 0.326*** | 72.0** | 1.00 |
| | (0.104) | (34.2) | (0.659) |

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

However, LATEs since exposure are a mix of 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 program[25]. 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

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

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

disadvantaged population, selected already at ITT level, so their estimate is pushed down by higher average earnings in the control group[26].

## 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 14 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 14-16 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

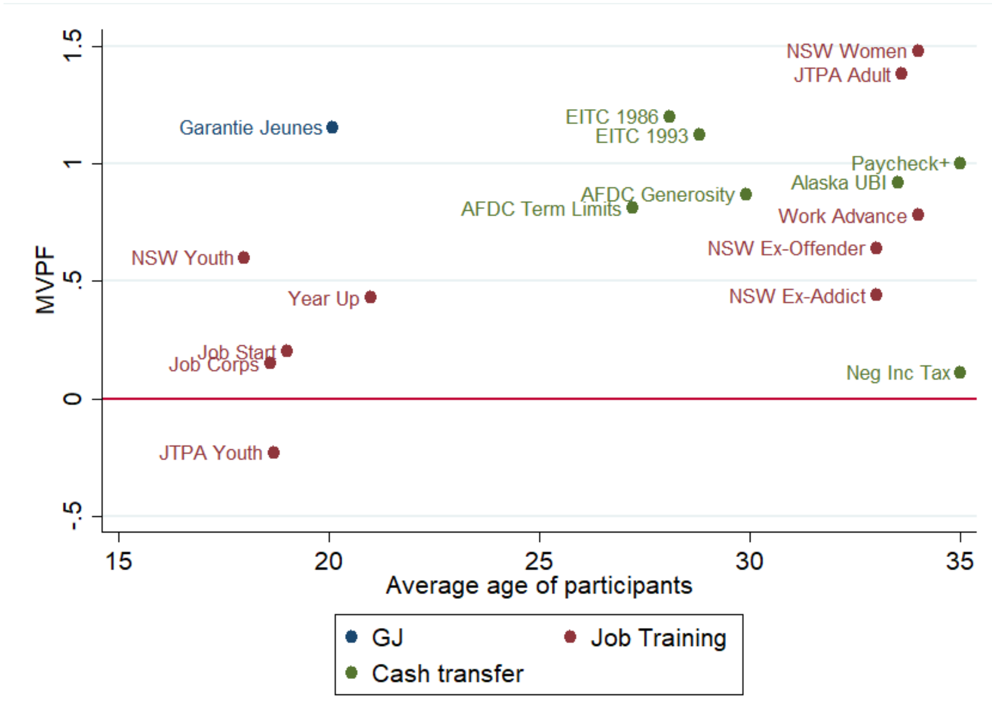
[26] The program is a sectoral training program, it includes a selection process based on motivation and skills at the beginning, before random assignment, and also includes a specific training component

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 assumptions[27], 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).

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

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



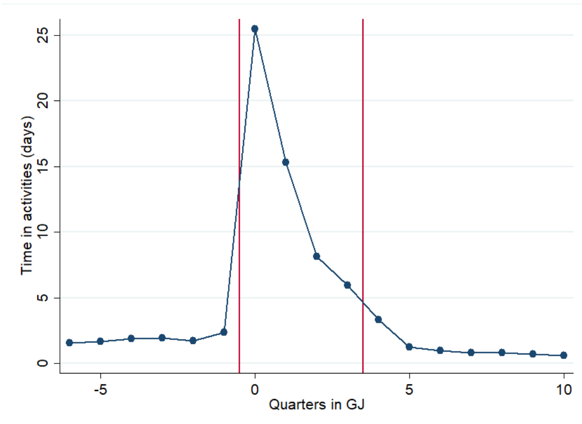

Notes. The figure reports the Marginal Value of Public Funds (MVPF) *Garantie Jeunes* and for programs in the"Job Training" and "Cash Transfer" categories analyzed by Hendren and Sprung-Keyser (2020) in the US context, plotted over average age of the participants in the program.

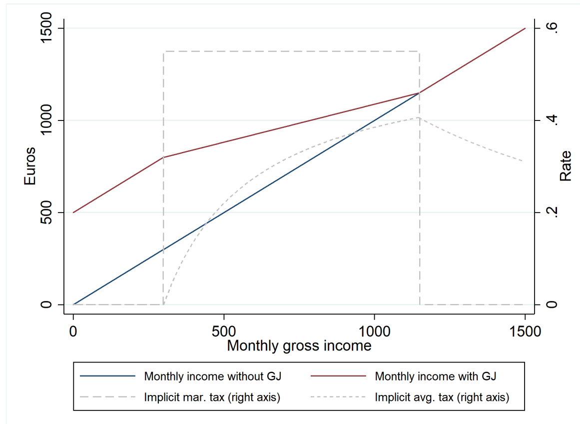
# 5 Disentangling the mechanisms

## 5.1 Identifying Variations: Timing of the Program and Cash-Transfer Phase-Out

To investigate the mechanisms behind the effect of *Garantie Jeunes*, 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*. 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)



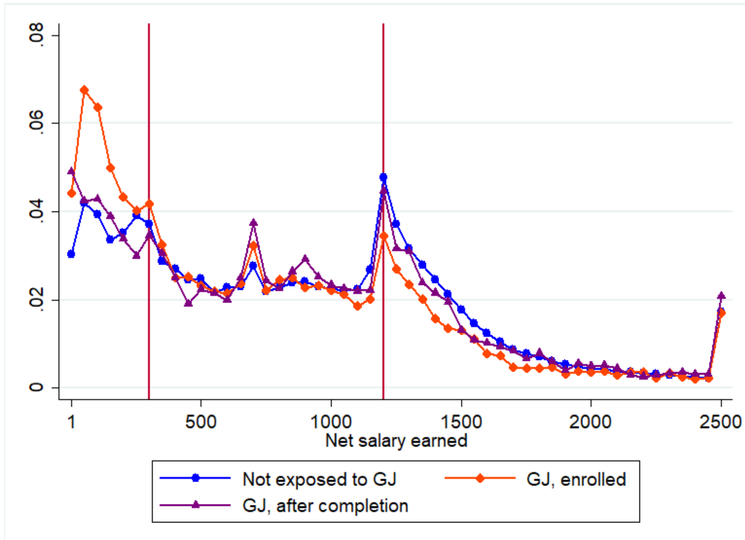




Notes. The left panel reports the estimated average working days with a scheduled activity over time since enrollment in *Garantie Jeunes*. Source: I-Milo. The right panel shows the implicit marginal and average tax rate and the effect on the difference between monthly gross and net income. The figure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

## 5.2 Results: Heterogeneous Impact by Program Stage and for Different Job Earnings

I now turn to my data to estimate possible reactions of participants in *Garantie Jeunes* to treatment variations in terms of lock-in and marginal implicit tax from cash phase-out. It is useful to start with an overview of the distribution of monthly earnings for youths in different treatment status. Figure 10 reports the distribution of net earnings (only when net earnings are bigger than zero) for youths who take-up the program, when enrolled and after completion, and for youths who are not exposed. While the number of full time jobs above the minimum wage of €1159 increases after program completion, during the program the number of youths earning below €300 is larger than for non-exposed youths, while the share of youths earning above €300 is lower.

Figure 10: 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 in *Garantie Jeunes*, for all youths in the sample in the period they are observed. Bins are of €50. Vertical lines correspond to the bins of €300 and €1159 (80% of French minimum wage). Monthly net earnings of each youth are estimated from adminitrative data on earnings of each contract gross of social contributions, assuming constant daily earnings.

The descriptives above highlight some variation in youths earning distribution according to the treatment stage, but do not constitute evidence. In fact, youths in the early phase of *Garantie Jeunes* might differ also in terms of tenure or any unobservable from the whole set of control group individuals. To obtain identified estimates of treatment heterogeneity, one option would be bunching at €300. Although a little spike is observed at €300, the magnitude is relatively small, as it's probably quite difficult for youths to obtain contracts so that their net earnings are exactly €300. Alternatively, I can 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 a the beginning of the period).

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

| | Monthly income 1-300 | Monthly income 350-1100 | Monthly income over 1100 |
|---|---|---|---|
| | (1) | (2) | (3) |
| LATE 1st semester of enrollm. | -0.0674* | -0.0482* | 0.0221 |
| | (0.0359) | (0.0290) | (0.0361) |
| LATE 2nd semester of enrollm. | 0.0846** | -0.146*** | 0.129** |
| | (0.0431) | (0.0544) | (0.0577) |
| LATE after completion | -0.0863 | .188*** | 0.197** |
| | (0.0618) | (0.0700) | (0.0793) |

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.

## 5.3 A Simple Theoretical Framework for Interpretation

To interpret mechanisms more formally, the literature points out two ways through which *Garantie Jeunes* can impact the probability of being employed in a specific earning bracket. First – if the environment is characterized by search frictions – *Garantie Jeunes* can affect the efficacy of search effort and the probability of actually finding the optimal job (one can see this as an "extensive margin"). Gautier et al. (2018) is one of the first attempts to model the impact of activation measures on search effort. In their model, program participation improves the matching technology (i.e.increases the number of applications sent) but costs time.

Second, the program affects the optimal amount of hours an individual is willing to supply (one can see this as an "intensive margin"). Le Barbanchon (2020) shows that individuals react to implicit taxation generated by unemployment benefit phase-out by reducing their labor supply in order to bunch their earnings in part-time jobs at the kink of the unemployment insurance benefit-withdrawal schedule, just before marginal taxation increases. Cesarini et al. (2017) find that lottery winnings – which are wealth shocks comparable to the present value of cash transfers in *Garantie Jeunes* – impact labor supply negatively, although the magnitude of the effect is modest. Finally, Card et al. (2007); Chetty (2008) find that guaranteeing more cash in the context of unemployment insurance – either through benefit increase, extension of benefit duration or through lump-sum severance payments – reduces labor supply and job search effort.

Suppose wages are given and normalized to one so that, for each period, youth maximize utility from choosing gross working earnings $z^t \in \{z^0, z^1, z^2, z^3\}$[28]. These brackets correspond to those of Table 5, i.e. unemployment, working earning €1-300, €300-1100, >€1100. Since wages are at the minimum wage and normalized, $z^0$ 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. Suppose the probability of being employed in a bracket $j$ is equal to the product of the share

[28]This could be plausible since takers of *Garantie Jeunes* are few with respect to the overall population, and since they mostly work at minimum wage jobs

of youth who chooses that bracket times a probability of not remaining unemployed $P(.)$, due to search frictions.

$$Pr(Y_{ji} = 1) = Pr(z_{j*} = z_j) \cdot P(active, time) \tag{5}$$

I assume that the magnitude of the search frictions depends from if the youth has received activation measures (*active*) and on time spent searching (*time*). I rule out that search frictions depend also from the cash transfer and from implicit taxation. 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*. The relationship between *active* and $P(.)$ is ambiguous *ex-ante*: although I might expect that the knowledge derived from activities provided by *Garantie Jeunes* improves search efficacy, it could also disorient the youth (choice overload), or make him overconfident, or represent a stigma, decreasing the probabilities to find an employment[29]. Search 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 mostly the case in the first two quarters of the program.

Assume that utility for individual $i$ and choice $j$ is linear $U_{ji} = u_j + \eta_i = a_1((1-\tau)z_j + b) + a_2 z_j/w + \eta_i$. 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*, $\tau$ is implicit taxation, and $\eta_i$ is individual heterogeneity. Denote $\alpha_j = a_1 z_j$, $\beta = a_1 b$, $\gamma_j = a_2 z_j/w$. Let consumers maximize the utility of their desired employment so that $U_{j*i} > U_{ji} \quad \forall j \neq j*$. This implies the assumption, done for simplicity, that consumers are naive and don't incorporate the fact that search friction might prevent them from working in the bracket corresponding to their labor supply decision, possibly because the magnitude of the friction is unclear to them. Given such kind of naive consumers, if $\eta_i$ 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:

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

Where *enrolled* is a dummy for being enrolled in treatment or not, $\hat{\alpha_j} = \alpha_j + \gamma_j$, $K_0 = e^{\alpha_0} + e^{\alpha_1+\gamma_1} + e^{\alpha_2+\gamma_2} + e^{\alpha_3+\gamma_3}$ and $K_1 = e^{\alpha_0+\beta} + e^{\alpha_1+\gamma_1+\beta} + e^{\alpha_2*(1-\tau)+\gamma_2+\beta} + e^{\alpha_3+\gamma_3}$. At this point, we can plug Equation 6 into Equation 5, 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).

[29] One option is that *Garantie Jeunes* increases $\eta_i$, or that the youth was underestimating its $\eta_i$ and now bids for higher $z^*$. I tend to exclude the hypothesis that *Garantie Jeunes* leads to shocks to $\eta_i$ since I find no effect on wage per-hour worked.

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 | $\Phi_1(0) \cdot P(0,1)$ | $\Phi_2(0) \cdot P(0,1)$ | $\Phi_3(0) \cdot P(0,1)$ |

To estimate the empirical counterparts of Table 6, I can use estimates of the average outcomes in treatment group compliers and recover estimates of average outcomes for control group compliers, which are otherwise non observed, subtracting LATEs in Table 5 to average outcomes in treatment 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, $P(1,1)$,$P(1,0)$,$P(0,1)$,$\hat{\alpha_1}$,$\hat{\alpha_2}$,$\hat{\alpha_3}$, $K_0$, $K_1$, $\beta$, $\alpha_2\tau$, with $\tau$ known. However, as shown in the Appendix, the system is not identified since only 8 equations are linearly independent. The solution can be estimated if an assumption on $K_1/K_0$ or on $P(1,1)$ is made, and if one renounces to recover $K_1$ and $K_0$ separately[30]. The results are reported in Table 7, where the fixed variable is $K_1/K_0$, and Table 8 where $P(1,1)$ is fixed.

We can exclude some of the assumed values of $K_1/K_0$ and $P(1,1)$ since they lead to not plausible results. First, all parameters representing probabilities should be between 0 and 1. Hence, the last line in Table 7 can be excluded. Second, note that $K_1$ and $K_0$ are respectively the total utility of takers when the program is offered vs. when it's not. Hence, $\frac{K_1}{K_0}$ is certainly bigger than 1 for takers, since otherwise they would prefer not to apply to *Garantie Jeunes* at all. This suggests that the first two lines in Table 8 should be excluded. I am left with an interval of plausible assumptions which is $XX$. Given this, one can conjecture that time availability $P(1,1) - P(1,0)$ increases the probability of finding the desired job by 18-21 percentage points, causing lock-in when youths are busy with activation measures. For the same range, the effect of activation $P(1,1) - P(0,1)$ corresponds an increase between 31 and 46 percentage points.

The effect of activation is steeply increasing in $\frac{K_1}{K_0}$. Note that $K_1/K_0$ exactly corresponds to the ratio between the share of takers willing to work full time without the program over the share of those willing to work full-time with the program. The larger $\frac{K_1}{K_0}$ the smaller the number of youths who – while enrolled – will find it convenient not to work full-time because their utility in the lower brackets increased. This can

[30]Alternatively, one needs to fix two of the other parameters, but this clearly complicates interpretation and it's less parsimonious.

Table 7: Estimated structural parameters based on $\frac{K_1}{K_0}$

| $\frac{K_1}{K_0}$ | $\Phi_1(0)$ | $\Phi_2(0)$ | $\Phi_3(0)$ | $\Phi_1(1)$ | $\Phi_2(1)$ | $\Phi_3(1)$ | $P(1,1)$ | $P(1,0)$ | $P(0,1)$ | $P(1,1)-P(1,0)$ | $P(1,1)-P(0,1)$ | $e^{\beta}$ | $e^{-\alpha_2\tau}$ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | .111 | .197 | .197 | .15 | .067 | .197 | .854 | .672 | .536 | .181 | .318 | 1.355 | .251 |
| 1.05 | .111 | .197 | .197 | .143 | .064 | .188 | .896 | .706 | .536 | .19 | .361 | 1.355 | .251 |
| 1.1 | .111 | .197 | .197 | .137 | .061 | .179 | .939 | .739 | .536 | .2 | .403 | 1.355 | .251 |
| 1.15 | .111 | .197 | .197 | .131 | .058 | .171 | .982 | .773 | .536 | .209 | .446 | 1.355 | .251 |
| 1.2 | .111 | .197 | .197 | .125 | .056 | .164 | 1.024 | .807 | .536 | .218 | .489 | 1.355 | .251 |

The table reports the estimated structural parameters as a function of $\frac{K_1}{K_0}$. The estimates are 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). This provides 8 linearly independent equations and 10 unknowns. Fixing $\frac{K_1}{K_0}$ and avoiding to solve for $K_1, K_0$ separately the system can be estimated with Equally Weighted Minimum Distance.

Table 8: Estimated structural parameters based on $P(1,1)$

| $\frac{K_1}{K_0}$ | $\Phi_1(0)$ | $\Phi_2(0)$ | $\Phi_3(0)$ | $\Phi_1(1)$ | $\Phi_2(1)$ | $\Phi_3(1)$ | $P(1,1)$ | $P(1,0)$ | $P(0,1)$ | $P(1,1)-P(1,0)$ | $P(1,1)-P(0,1)$ | $e^{\beta}$ | $e^{-\alpha_2\tau}$ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| .937 | .111 | .197 | .197 | .161 | .071 | .21 | .8 | .63 | .536 | .17 | .264 | 1.355 | .251 |
| .996 | .111 | .197 | .197 | .151 | .067 | .198 | .85 | .669 | .536 | .181 | .314 | 1.355 | .251 |
| 1.054 | .111 | .197 | .197 | .143 | .064 | .187 | .9 | .709 | .536 | .191 | .364 | 1.355 | .251 |
| 1.113 | .111 | .197 | .197 | .135 | .06 | .177 | .95 | .748 | .536 | .202 | .414 | 1.355 | .251 |
| 1.172 | .111 | .197 | .197 | .128 | .057 | .168 | 1 | .787 | .536 | .213 | .464 | 1.355 | .251 |

The table reports the estimated structural parameters as a function of $P(1,1)$. The estimates are 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). This provides 8 linearly independent equations and 10 unknowns. Fixing $P(1,1)$ and avoiding to solve for $K_1, K_0$ separately the system can be estimated with Equally Weighted Minimum Distance.

be seen as a sort of moral hazard (or liquidity effect, Chetty, 2008). The larger such effect of cash transfers, the larger the effect of activation needed to rationalize the results. Intuitively, with large $\frac{K_1}{K_0}$ and large moral hazard/liquidity effect, rationalizing the zero effect while enrolled and the jump in employment at the end of the program requires a large effect of activation.

# 6 Discussion

The empirical results found in this paper show that *Garantie Jeunes* increases employment after completion, with lock-in and implicit taxation compensated by a positive effect of activation. On top of this, the model-based interpretation suggests that if potential disincentives from having the right to cash transfers are present, the positive effect of activation should be even larger to compensate these effects and rationalize the results. Although they cannot be estimated, it is likely that total utility of having access to cash transfers is positive ($K_1 > K_0$), at least for takers. Disincentives by cash transfers are then present and reduce labor supply. However, the sources of the implied additional increase in the effect of activation can be multiple.

The simplest hypothesis is that activation causes a large improvement in search efficacy $P(.)$ (i.e. the success rate of applications) which is orthogonal to the disincentives provided by the cash transfer. This additional effect should exactly balance the reduction in labor supply so as to yield constant employment while youths are enrolled even if the number of applicants is reduced. An alternative hypothesis is that the effect of activation increases with utility from cash transfers because job search actually depends from activation and cash transfers jointly – let us call this phenomenon "complementarities" between cash transfer and activation. This happens, for example, if youths fear the risk of being sanctioned (suspension of the cash transfer) and activities in *Garantie Jeunes* function as a monitoring device, pushing youths to exert search effort (Boone et al., 2007). Or, more optimistically perhaps, the cash transfer allows youths to attend the activities offered by *Garantie Jeunes*, which they otherwise wouldn't be able to due to financial constraints. They might also attend activities with better motivation.

Unfortunately, my setting is not equipped to further identify if complementarities are actually at play, i.e. 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 also modelling the disincentives of cash (do they go through labor supply, as modeled in this paper? do they also go through job search?) in a way that identifies them. Eventually, clear evidence would require to measure the search process precisely (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 counseling[31]. 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 appears only after the end of cash transfers. Again, this might be suggestive of interactions between cash and activation, with cash reducing labor supply but adding an extra effect to activation measures of *Garantie Jeunes*, augmenting 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 negative effect during enrollment in the program.

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 large sole of search frictions for this population. Secondly, the results provide empirical insights for the literature on labor supply and job search behavior. I estimate a 75% reduction in employment as a reaction to a 55% increase in implicit taxation from benefits phase-out, implying significant elasticity of labor supply 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

[31] It should be noted that there are additional sources of difference with their study. A first one might be selection of the takers, since in *Garantie Jeunes* eligible youths are selected on motivation and fragility, requiring a sunk cost of application, while in the 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).

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 (Neilson and Zimmerman, 2014).

I suggest three main avenues for future research. First, this paper is not able to disentangle the exact magnitude of the disincentives from the cash transfer of *Garantie Jeunes*, nor the exact channel through which activation compensates them. Future studies should look for shocks and direct measures of the monitoring, motivation, and job search technology components of activation measures. The question is extremely relevant for understanding the mechanisms of job search and labor supply, and the interactions between key drivers of them. 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 on the side of YECs, since counselors will not be able anymore to focus on few youths in their flagship program. Third, the cost-benefit analysis relies on the assumption that *Garantie Jeunes* generates no externalities, whether positive or negative. These represent a challenge for policy evaluators in the future, for example if the program is extended, and displacement effects on other disadvantaged job seekers 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 Jjeunes* grown up in high-delinquency environments.

Finally, this work is of crucial importance for policy. First, it proves that that the mix of services and cash transfers provided by *Garantie Jeunes* is effective. This is in line with pilot evidence by Gaini et al. (2018) and qualitative results by Gautié (2018). Hence, it offers a solid base for programs trying to promote employability of disadvantaged NEETs. However, external validity should be handled with care, for example for the projected extension of the program to a larger pool of youth. *Garantie Jeunes* concerned until now a very selected population, with an application based on motivation and fragility run at the local level. also, the gain is coming in large part from agency jobs and fixed-term contracts, so that longer-term results cannot be taken for guaranteed. Finally, the large costs estimated implied that the willingness to pay for the program is only 15 percentage points higher than its net costs. If returns on the marginally eligible youth are decreasing, it might not be easy to scale the program up maintaining cost-effectiveness.

More generally, the analysis gives ground to policy institutions recommending active and passive labor market policies OECD (2020). The effect of activation is estimated strong enough to compensate for lock-in and distorsive effects of the cash transfers. Although I could not identify complementarities, these are shown to be likely in light of comparison with the literature and of how the effect of implied activation increases the larger the disincentives by cash transfers. Third, the estimated negative effect of implicit taxation and possible disincentives from cash transfers suggest that cash transfers should be fully cumulable with job earnings but as limited in time as possible. My insights can be considered more generally valid for policies that combine cash transfers and activation policies, like in some minimum income schemes or in unemployment insurance with activation requirements.

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# Proofs

## Proof of Proposition 1

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

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

## Proof of Proposition 2

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

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

## Proof of Proposition 3

It follows straightforwardly from the definition of expectations that

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

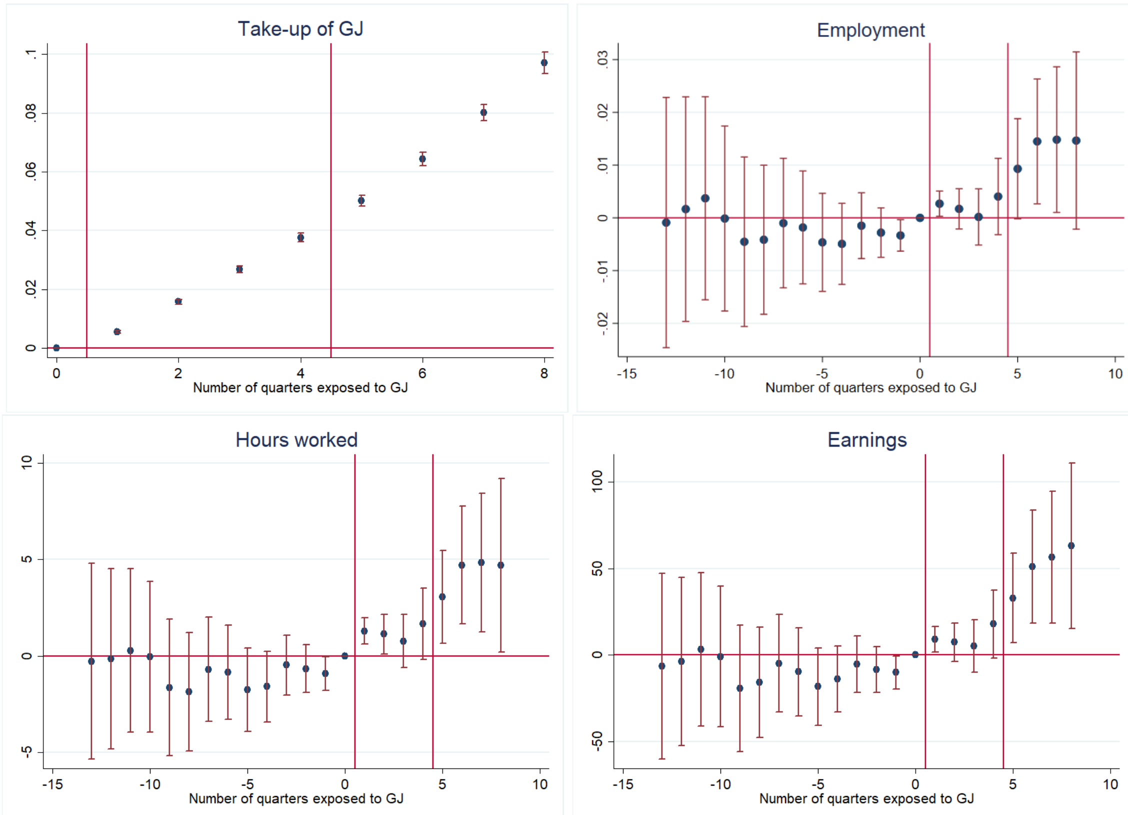
# Appendices

## A ITT Through an Event-Study Approach

Figure 11 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 $g$, $\beta^g$ 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 11: 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 Dynamic LATE under Different Assumptions

Similarly to what done for $d$, also the dynamic of $h$ is aggregated into two classes $0 < h \leq 4$ and $h > 4$, respectively the first year of registration at YEC and more than one year after enrollment. Under these restrictions, the regressions we run is:

$$\begin{aligned} DID^h_{w,c} =& \delta(0 < d \leq 4, 0 < h \leq 4) Pr(0 < D^h_{w,c} \leq 4)\mathbb{1}(0 < h \leq 4) \\ &+ \delta(0 < d \leq 4, h > 4) Pr(0 < D^h_{w,c} \leq 4) + \delta(d > 4, h > 4) Pr(D^h_{w,c} > 4)\mathbb{1}(h > 4) + \epsilon_{h,w,c} \end{aligned} \tag{7}$$

Fully dynamic LATE:

$$DID^h_{w,c} = \sum_{d=1}^{g} \delta(d) Pr(D^h_{w,c} = d)$$

Table 9: Local average treatment effects (LATE) estimates using Proposition 3.b and fully dynamic LATE

| VARIABLES | (1) Employment | (2) Employment | (3) Hours | (4) Hours | (5) Earnings | (6) Earnings |
|---|---|---|---|---|---|---|
| $Pr(D^h_{i,w,c} = 1)$ | -0.152 | -0.0918 | -16.19 | -17.73 | -203.2 | -332.2* |
| | (0.0993) | (0.0848) | (23.34) | (17.92) | (281.6) | (193.8) |
| $Pr(D^h_{i,w,c} = 2)$ | 0.0592 | -0.233** | 37.00 | 17.32 | 273.9 | -141.4 |
| | (0.118) | (0.109) | (31.18) | (25.84) | (321.7) | (278.3) |
| $Pr(D^h_{i,w,c} = 3)$ | 0.0198 | 0.135 | 58.28* | -0.136 | 336.2 | 296.3 |
| | (0.112) | (0.104) | (33.49) | (31.36) | (289.2) | (282.1) |
| $Pr(D^h_{i,w,c} = 4)$ | -0.127 | 0.241 | -44.55 | 51.05 | -319.2 | 882.6* |
| | (0.157) | (0.162) | (52.99) | (47.25) | (509.9) | (485.0) |
| $Pr(D^h_{i,w,c} = 5)$ | 0.477** | 0.540*** | 17.11 | 147.0** | 563.7 | 1,618** |
| | (0.186) | (0.203) | (63.92) | (65.42) | (644.7) | (706.2) |
| $Pr(D^h_{i,w,c} = 6)$ | 0.364 | 0.366 | 324.8*** | 83.47 | 3,760*** | 1,703** |
| | (0.510) | (0.246) | (119.5) | (72.86) | (1,353) | (813.1) |
| $Pr(D^h_{i,w,c} = 7)$ | -0.578 | 0.748** | -216.6 | 160.8 | -2,566 | 1,797 |
| | (0.590) | (0.372) | (151.9) | (123.7) | (1,744) | (1,332) |
| $Pr(D^h_{i,w,c} = 8)$ | 2.000** | -0.278 | 482.1** | 8.508 | 6,543** | 476.9 |
| | (0.995) | (0.578) | (225.7) | (176.8) | (2,776) | (2,172) |
| Weights | No | Yes | No | Yes | No | Yes |

Notes. The table reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and wages obtained according to Proposition 3. Standard errors are are obtained by bootstrapping.

If one wants to allow instead for LATE to be heterogeneous across cohorts and waves, but additively separable $\delta(d,h,w,c) = \delta(d,h) + \delta(d,w) + \delta(d,c)$. Again, the dynamic of $d$ is aggregated into two classes $0 < d \leq 4$ and $d > 4$, the dynamic of $h$ is aggregated into $0 < h \leq 4$ and $h > 4$. Under these restrictions, the regression we run is:

$$\begin{aligned} DID^h_{w,c} = &\delta(0 < d \leq 4, 0 < h \leq 4) Pr(0 < D^h_{w,c} \leq 4) \cdot \mathbb{1}(0 < h \leq 4) \\ &+ [\delta(0 < d \leq 4, h > 4) Pr(0 < D^h_{w,c} \leq 4) + \delta(d > 4, h > 4) Pr(D^h_{w,c} > 4)] \cdot \mathbb{1}(h > 4) \\ &+ \sum_{w_o \in W} [\delta(0 < d \leq 4, w = w_o) Pr(0 < D^h_{w,c} \leq 4) + \delta(d < 4, w = w_o) Pr(D^h_{w,c} > 4)] \cdot \mathbb{1}(w = w_o) \\ &+ \sum_{c_o \in C} [\delta(0 < d \leq 4, c = o) Pr(0 < D^h_{w,c} \leq 4) + \delta(d > 4, c = o) Pr(D^h_{w,c} > 4)] \cdot \mathbb{1}(c = o) \end{aligned} \tag{8}$$

## C Estimation of structural parameters

By equating each of the estimated average outcomes in treatment and control to their structural interpretation and using Equation 6 I obtain a system of 18 equations in 14 unknowns.

$$\begin{cases} \mathbb{E}(Y_{1i}(D_i)|0 < D_i \leq 2) & = \Phi_1(1) \cdot P(1,0) \\ \mathbb{E}(Y_{2i}(D_i)|0 < D_i \leq 2) & = \Phi_2(1) \cdot P(1,0) \\ \mathbb{E}(Y_{3i}(D_i)|0 < D_i \leq 2) & = \Phi_3(1) \cdot P(1,0) \\ \mathbb{E}(Y_{1i}(D_i)|2 < D_i \leq 4) & = \Phi_1(1) \cdot P(1,1) \\ \mathbb{E}(Y_{2i}(D_i)|2 < D_i \leq 4) & = \Phi_2(1) \cdot P(1,1) \\ \mathbb{E}(Y_{3i}(D_i)|2 < D_i \leq 4) & = \Phi_3(1) \cdot P(1,1) \\ \mathbb{E}(Y_{1i}(D_i)|D_i > 4) & = \Phi_1(0) \cdot P(1,1) \\ \mathbb{E}(Y_{2i}(D_i)|D_i > 4) & = \Phi_2(0) \cdot P(1,1) \\ \mathbb{E}(Y_{3i}(D_i)|D_i > 4) & = \Phi_3(0) \cdot P(1,1) \\ \mathbb{E}(Y_{1i}(0)|0 < D_i \leq 2) & = \Phi_1(0) \cdot P(0,1) \\ \mathbb{E}(Y_{2i}(0)|0 < D_i \leq 2) & = \Phi_2(0) \cdot P(0,1) \\ \mathbb{E}(Y_{3i}(0)|0 < D_i \leq 2) & = \Phi_3(0) \cdot P(0,1) \\ \mathbb{E}(Y_{1i}(0)|2 < D_i \leq 4) & = \Phi_1(0) \cdot P(0,1) \\ \mathbb{E}(Y_{2i}(0)|2 < D_i \leq 4) & = \Phi_2(0) \cdot P(0,1) \\ \mathbb{E}(Y_{3i}(0)|2 < D_i \leq 4) & = \Phi_3(0) \cdot P(0,1) \\ \mathbb{E}(Y_{1i}(0)|D_i > 4) & = \Phi_1(0) \cdot P(0,1) \\ \mathbb{E}(Y_{2i}(0)|D_i > 4) & = \Phi_2(0) \cdot P(0,1) \\ \mathbb{E}(Y_{3i}(0)|D_i > 4) & = \Phi_3(0) \cdot P(0,1) \end{cases} \tag{9}$$

Take logs of both sides of system 6, and indicate $\phi_j(.) = \ln(\Phi_j(.))$, $k_0 = \ln(K_0)$ and $k_1 = \ln(K_1)$.

$$\begin{cases} \phi_1(1) & = \hat{\alpha_1} - k_1 + \beta \\ \phi_2(1) & = \hat{\alpha_2} - k_1 + \beta - \alpha_2\tau \\ \phi_3(1) & = \hat{\alpha_3} - k_1 \\ \phi_1(0) & = \hat{\alpha_1} - k_0 \\ \phi_2(0) & = \hat{\alpha_2} - k_0 \\ \phi_3(0) & = \hat{\alpha_3} - k_0 \end{cases} \tag{10}$$

Now, with slightly sloppy but simpler notation denote $\ln(\mathbb{E}(Y_{ji}(treated)|D_i \in semester)) = y_{line}$, with *line* corresponding to each of the lines in (3). For example, $\ln[\mathbb{E}(Y_{1i}(D_i)|0 < D_i \leq 2)] = y_1$. Take logs of both sides of equations in (3), denoting $p = \ln(P(.))$, and plug (5) into the result. Then:

$$
\begin{cases}
y_1 & = \hat{\alpha}_1 - k_1 + \beta + p(1,0) \\
y_2 & = \hat{\alpha}_2 - k_1 + \beta - \alpha_2 \tau + p(1,0) \\
y_3 & = \hat{\alpha}_3 - k_1 + p(1,0) \\
y_4 & = \hat{\alpha}_1 - k_1 + \beta + p(1,1) \\
y_5 & = \hat{\alpha}_2 - k_1 + \beta - \alpha_2 \tau + p(1,1) \\
y_6 & = \hat{\alpha}_3 - k_1 + p(1,1) \\
y_7 & = \hat{\alpha}_1 - k_0 + p(1,1) \\
y_8 & = \hat{\alpha}_2 - k_0 + p(1,1) \\
y_9 & = \hat{\alpha}_3 - k_0 + p(1,1) \\
y_{10} & = \hat{\alpha}_1 - k_0 + p(0,1) \\
y_{11} & = \hat{\alpha}_2 - k_0 + p(0,1) \\
y_{12} & = \hat{\alpha}_3 - k_0 + p(0,1) \\
y_{13} & = \hat{\alpha}_1 - k_0 + p(0,1) \\
y_{14} & = \hat{\alpha}_2 - k_0 + p(0,1) \\
y_{15} & = \hat{\alpha}_3 - k_0 + p(0,1) \\
y_{16} & = \hat{\alpha}_1 - k_0 + p(0,1) \\
y_{17} & = \hat{\alpha}_2 - k_0 + p(0,1) \\
y_{18} & = \hat{\alpha}_3 - k_0 + p(0,1)
\end{cases}
$$

The identification problem can be seen easily by denoting $\bar{k} = k_1 - k_0$, and $\hat{\alpha}_j - k_0 = \tilde{\alpha}_j$. With quick manipulation:

$$
\begin{cases}
y_1 - y_4 & = p(1,0) - p(1,1) \\
y_2 - y_5 & = p(1,0) - p(1,1) \\
y_3 - y_6 & = p(1,0) - p(1,1) \\
y_4 & = \tilde{\alpha_1} + \beta + p(1,1) - \bar{k} \\
y_5 & = \tilde{\alpha_2} + \beta - \alpha_2 \tau + p(1,1) - \bar{k} \\
y_6 & = \tilde{\alpha_3} + p(1,1) - \bar{k} \\
y_7 - y_{10} & = p(1,1) - p(0,1) \\
y_8 - y_{11} & = p(1,1) - p(0,1) \\
y_9 - y_{12} & = p(1,1) - p(0,1) \\
y_{10} & = \tilde{\alpha_1} + p(0,1) \\
y_{11} & = \tilde{\alpha_2} + p(0,1) \\
y_{12} & = \tilde{\alpha_3} + p(0,1) \\
y_{13} & = \tilde{\alpha_1} + p(0,1) \\
y_{14} & = \tilde{\alpha_2} + p(0,1) \\
y_{15} & = \tilde{\alpha_3} + p(0,1) \\
y_{16} & = \tilde{\alpha_1} + p(0,1) \\
y_{17} & = \tilde{\alpha_2} + p(0,1) \\
y_{18} & = \tilde{\alpha_3} + p(0,1)
\end{cases}
$$

This configuration shows clearly that the interpretation imposed to the estimated treatment and control outcomes delivers only 8 linearly independent equations, while we have 9 unknowns. The system becomes solvable only if one is willing to do an assumption either on $\bar{k}$ or on $p(1,1)$, because dropping any other variable would make another equation in the system become linearly dependent. Also, one renounces to recover both $k_1$ and $k_0$, which is linked to the fact that it's not possible to recover the three $\hat{\alpha_j}$.

Once the assumption is made, one can estimate the system using Equally Weighted Minimum Distance applied to the regression:

Finally, how well estimated theoretical values (i.e. $y_{\hat{line}}$), using any line in Tables 2-3, fit the true values $y_{line}$? Figure 12 below reports a comparison. The dimension of the bubbles is inversely proportional to the square of the standard errors of that effect.

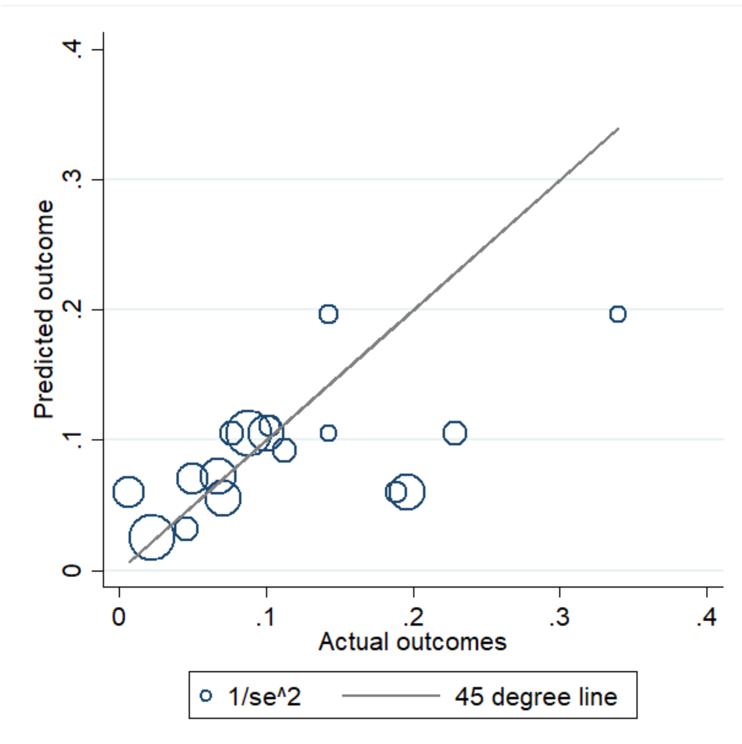
# D Additional Tables and Figures

Table 10: 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.

Figure 12



Predicted outcome
0
.1
.2
.3
.4
0
.1
.2
.3
.4
Actual outcomes
1/se^2
45 degree line

Table 11: 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 12: 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 |

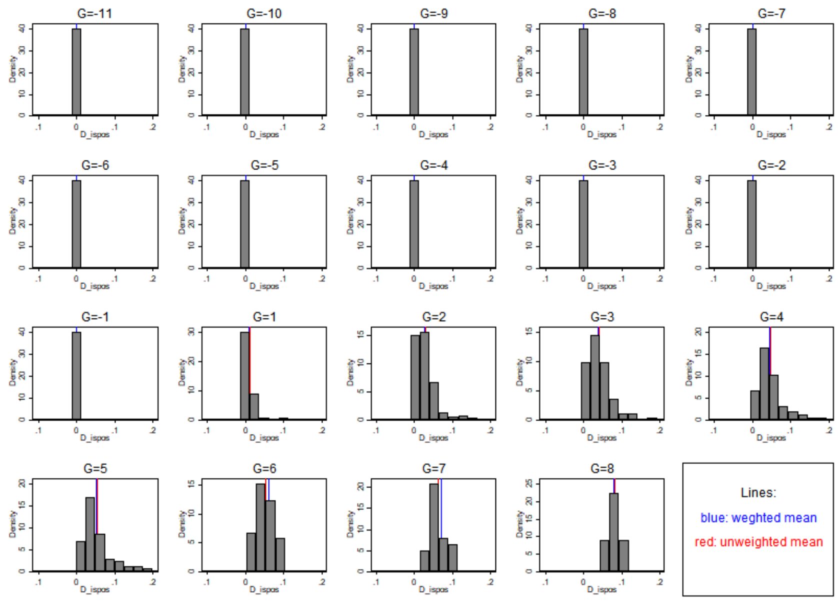
Table 13: Coefficients of reduced form and first stage for every wave (each line corresponds to one wave) and cohort (each column corresponds to one cohort of registration). YEC tenure is 4 quarters after registration. Colors represent the scale of the value in the cell relative to the table, red for positive green for negative.

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

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

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

Figure 13: Distribution of $DID^h_{w,c} \quad \forall w,c,h : G^h_{w,c} = g$ for employment



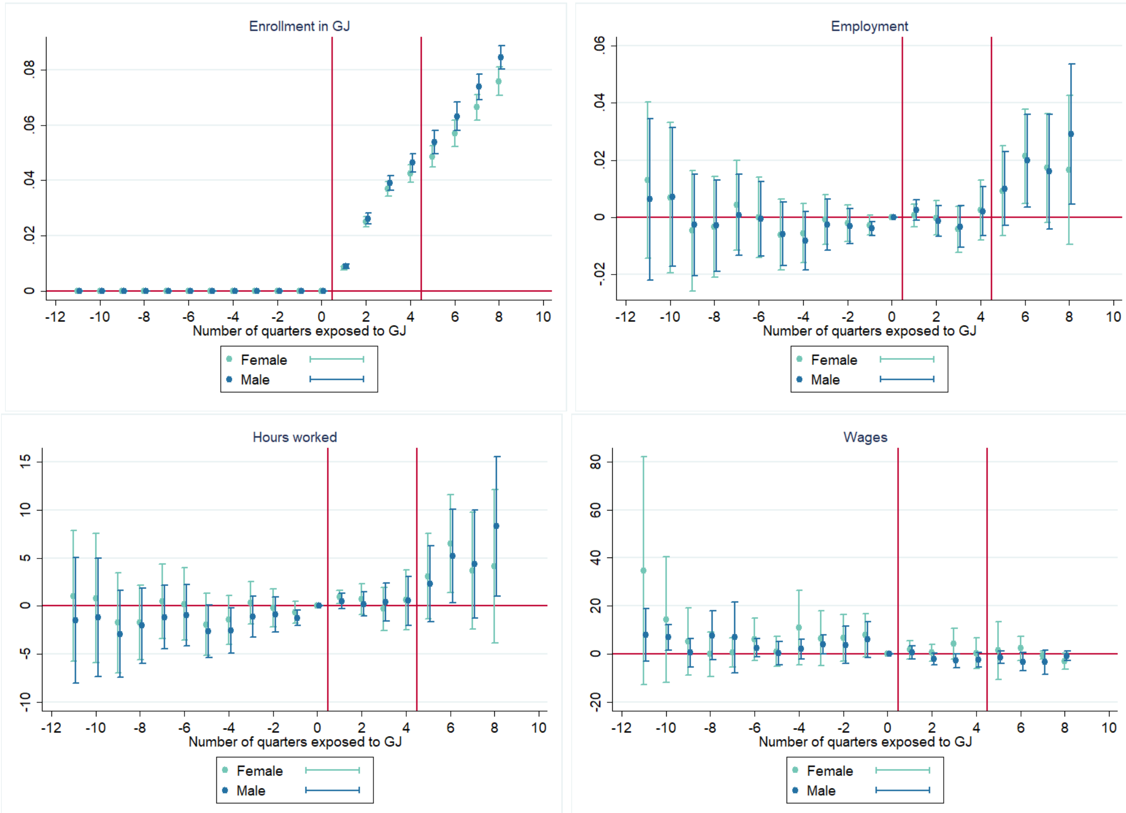

Notes. The Figure reports the distribution of the DID coefficients for every wave-cohort-tenure cell, corresponding to each level treatment exposure $g$, the number of quarters exposed to *Garantie Jeunes*. The blue line is the mean weighted by the number of observation used to estimate each DID, while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

Table 14: Heterogeneity by employment contract

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

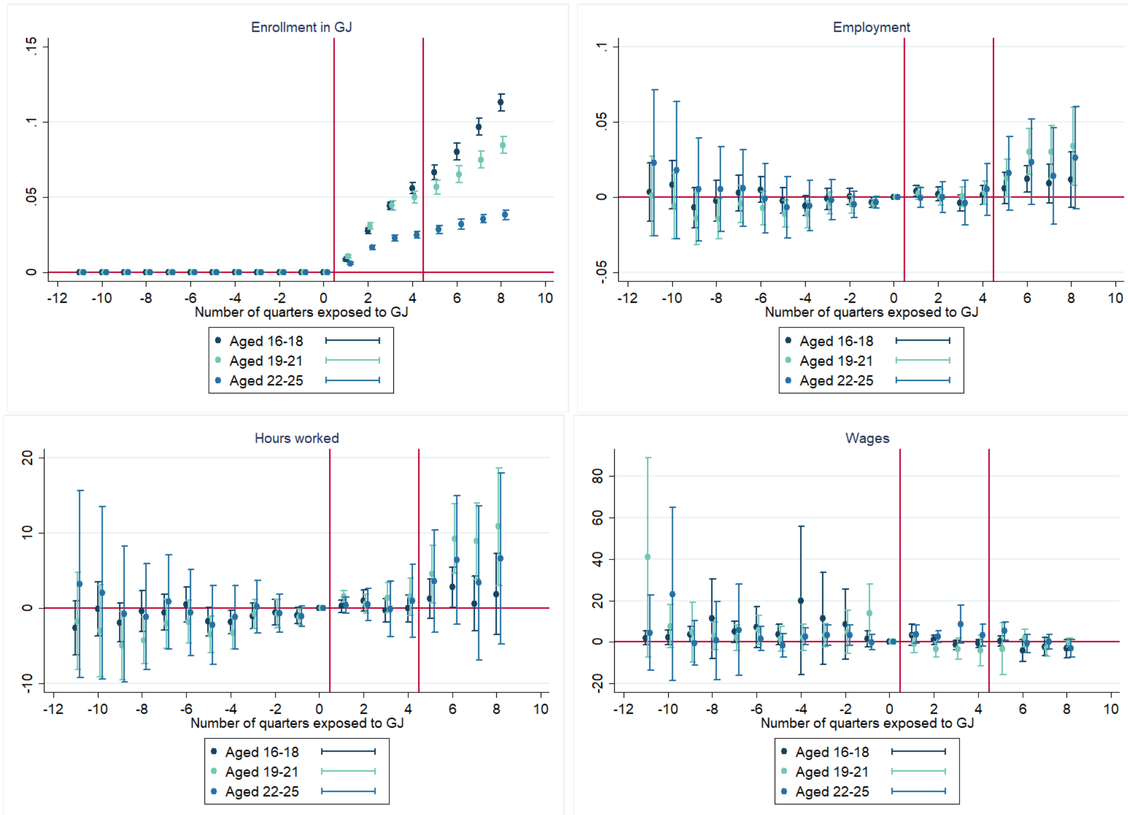
Notes. The table reports the main results obtained following the rolling diff-in-diff methodology developed in Section 3. The upper panel reports weighted averages of the $DID^h_{w,c}$ coefficients where exposure is between 1 and 4 quarters or above 4 quarters. The lower panel reports the estimates of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

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



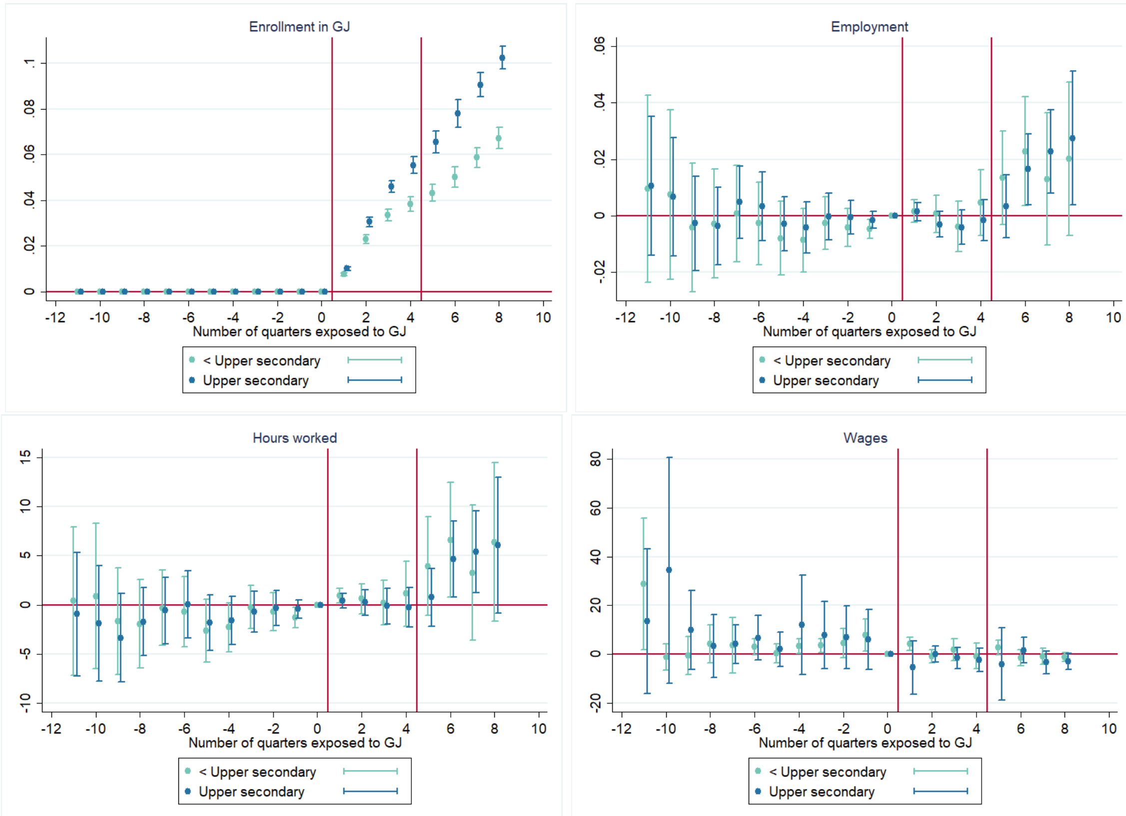

Notes. The figure reports results of the rolling diff-in-diff approach for different gender sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a 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 15: Intent to treat (ITT) estimates using the rolling diff-in-diff approach by age



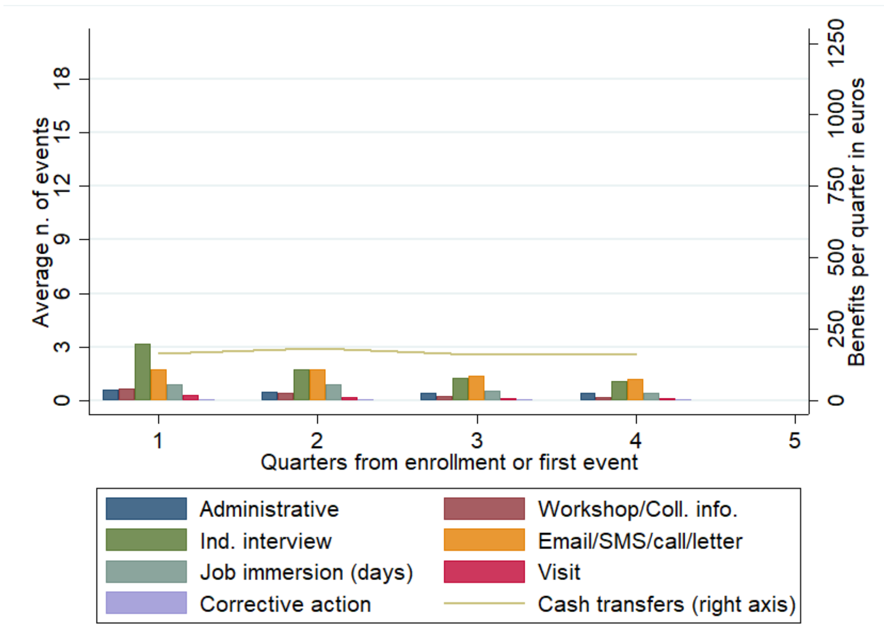

Notes. The figure reports results of the rolling diff-in-diff approach for different age sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy 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 16: 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 17: 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.