What Do NEETs Need?
The Joint Effect of Active and Passive Labor Market Policies

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Abstract

Active and passive labor market policies are often used jointly, but the literature has only evaluated them one conditional on the other. This paper evaluates a flagship French program for disadvantaged youth Not in Employment Education or Training (NEETs) that provided a year of cash transfers and intensive activation measures. I exploit the staggered adoption of the program using a classical event study and a difference-in-differences methodology that extends De Chaisemartin and D'Haultfœuille (2020a) to a setting where individuals enter the population of interest in cohorts. The results highlight a strong positive joint effect of active and passive policies (+21 percentage points in employment, +63% with respect to control) after youths exit the program. During program enrollment, I show that part-time employment decreases in the first semester – when youths are busy in activation measures – while in the second semester the decrease is concentrated in income brackets where the cash transfer is phased-out with labor income. This suggests that cash transfers and lock-in from training reduce youth employment, but this is more than compensated by the positive effect of activation measures.

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 who are neither in employment, education or training (NEETs) are a persisting problem in Europe[1]. To rescue NEETs, governments often resort to social protection, for example cash transfers and income support. Economists have long argued that such "passive" policies alone risk creating welfare dependence, without structurally changing individual behavior, or even decreasing job search (Moffitt, 1985; Card et al., 2007; Britto et al., 2020). For this reason, the combination of active and passive labor market policies is often advocated by international institutions (OECD, 2013; Pignatti and Van Belle, 2018), and governments are increasingly following this advice. Yet, what is the *joint* effect of active and passive policies? It is not guaranteed that active labor market policies will improve employability enough to compensate for a negative effect of passive policies, especially in the case of NEETs.

The economic literature has so far only evaluated the effect of active policies conditional on passive ones, or vice versa. A large literature summarized by Card et al. (2018) studied the effect of active labor market policies, and some of these estimates concern programs offered to receivers of passive policies, obtaining the effect of active policies conditional on a given level of passive ones. Vice versa, other papers estimate the effect of passive policies given a particular level of active ones, for example Aeberhardt et al. (2020), which evaluates a cash transfer offered to individuals who wold have anyway undertook an activation program. Yet, to the best of my knowledge, no estimates exist of the effect of a program offering passive and active policies combined, making it difficult to directly understand how much active policies can balance-out the effect of passive ones. Moreover, there are reasons to believe that the joint effect of active an passive polices might be larger than the sum of the effects of active and passive policies alone, for example if active policies act as a monitoring device (Boone et al., 2007).

This paper fills the gap in the literature by evaluating the flagship French program for disadvantaged NEETs between 16 and 25 years old, *Garantie Jeunes*. The program combines a year of cash transfers equivalent to the French minimum income with intensive activation measures, namely soft-skills training for a month, regular counseling and short-term job experiences. My main results show that the combination of active and passive policies has a positive effect on employment and hours worked from the second year after exposure to the program, driven by youths who finished the program. In fact, I show that during enrollment in the program cash transfers and lock-in from training are associated to a reduction in youth employment, compensated by the positive effect of activation measures. When the program ends, only the positive effect of activation remains, driving the improvement in youth employability.

To identify the effects of the program, I exploit its staggered adoption by youth employment centers (YECs) in 2013-2017. Youth enter YECs in cohorts of registration with YEC, with their employment being low at time of registration and then increasing as time since registration increases. In other words, units are grouped in cohorts, and potential outcomes are conditional on cohorts and on a combination of cohort and time. For estimation of the Intention-to-Treat (ITT) effect of *exposure* to *Garantie Jeunes*, I firstly use a

[1] NEET rates in the last decade for youths aged 15-24 ranged between 12% and 22% in countries such as Spain or Italy, and were persistently above 10% in others, such as France. Higher levels were reported for women, less educated persons and foreign-born individuals. Economists have long wondered about the possible causes. Given that disadvantaged youths are more likely to become NEETs (Carcillo and Königs, 2015), some have posited that those who become NEETs face significantly higher job search frictions, lacking networks and soft-skills[2]. Moreover, NEET spells can become a poverty trap. In fact, unemployment has proven to be "scarring", in the sense that it can permanently harm one's employability (Oreopoulos et al., 2012; Schwandt and Von Wachter, 2019; Rothstein, 2019) as much as prematurely dropping out of formal education (Brunello and De Paola, 2014).

simple fixed-effects model. Then, I develop a new diff-in-diff methodology, which extends De Chaisemartin and D'Haultfœuille (2020a) to such a setting, where units are grouped in cohorts. Finally, leveraging my diff-in-diff methodology, I regress ITT effects for a specific wave-cohort-time since YEC registration cell on the share of youths at specific stages of program enrollment, recovering dynamic LATEs since actual *enrollment* in the program.

The ITT estimates show that employment and hours worked for treated youth increase from the second year after they were exposed to the program. Instead, no significant effect is observed on wages. Crucially, the effect on employment and hours worked is entirely driven by youth who have completed the program, to which a 21 percentage points increase in employment is associated (+63% with respect to control) as well as an increase of 49 hours worked on a quarterly basis (+81% with respect to control). During program enrollment, the effect is instead zero or slightly negative on employment.

To disentangle the role of cash transfer and activation measures, I exploit the timing of the activation measures and the phase-out of the cash transfer. Time-consuming activities such as intensive training and job immersions are concentrated in the first semester of enrollment *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. During the first semester of enrollment, when youths are involved in intensive training and receive the cash transfer, I find a decrease in the probability of having job earnings below €300 or between €300 and €1100. In the second semester, when youths are out of the training but continue receiving the transfer, the decrease is concentrated in jobs earning €300-1100, where transfers are only partially cumulative with job earnings. I interpret this heterogeneity through the lens of a simple model of labor supply with discrete hours choice and search frictions. Under the assumptions of the model, cash transfers reduce employment mostly through implicit taxation, lock-in from training dents the probability of finding a job by about 40%, while activation compensates these negative effects by doubling the probability of finding the chosen job thanks to improved search technology.

The main contribution of this work is to offer evidence on the joint effect of active and passive labor market policies, while prior work evaluated one component conditional on the other. For instance, Aeberhardt et al. (2020) evaluates a cash transfer of similar value and context as that of this paper, with no change in activation requirements, finding a non-significant effect on job search and a small negative effect on employment. In my program, where cash transfer are bundled with activation, I estimate a negative reaction of youth employment to cash transfers phase-out which is consistent with their finding. In turn, an extensive literature evaluates programs that increase activation measures, but not cash support, finding that in the medium term programs with a work-first approach improve employability, while more intensive forms of training risk a lock-in effect (Card et al., 2018). In the French context, some working papers indicate a large positive effect of job search assistance (Crépon et al., 2015) and of collective counseling (van den Berg et al., 2015) for youth. Compared to these papers, my results suggest a similar lock-in effect, but also that activation and cash transfers jointly generate a persisting improvement in employment after program completion. In the US context, a close setting to mine is the *Year-Up* sectoral training program, where youth didn't receive a cash transfer but were paid a stipend by partner employers to work after the training occurred. Katz et al. (2022) evaluate the program finding very similar lock-in from training and positive effects only after completion of the program.

Secondly, the results provide empirical insights on labor supply and job search of disadvantaged NEETs. Similarly to (Le Barbanchon, 2020; Saez et al., 2012), I highlight significant effects of implicit taxation, but the implied elasticity of earnings to net-of-tax rate which is very large, possibly due to larger reactions observed in sub-populations less attached to work (Card and Hyslop, 2005). I also empirically confirm the role of time constraints and search technology in activation policies (Gautier et al., 2018), finding that activation generates small lock-in but increases job finding. An implication of the large observed effect of activation search technology is that job finding is estimated low for untreated compliers. This speaks to several streams of the literature that show how lack of networks, geographical isolation and low soft skills can dramatically limit job search efforts on the part of disadvantaged youth[3].

My final contribution is methodological, as I extend De Chaisemartin and D'Haultfœuille (2020a) to a setting where individuals enter the population of interest in cohorts and are staggeredly exposed to treatment. An example of this circumstances can be staggered adoption of a restructuring program across schools, where students enter schools in cohorts corresponding to their class, as for example in Martorell et al. (2016). Similar instances can arise when programs affect different cohorts of workers entering firms, or cohorts of patients entering hospitals, with staggered exposure. In all these cases, the rolling diff-in-diffs approach I develop is useful when the researcher needs to apply heterogeneity-robust estimators.

Finally, in terms of policy implications the paper supports the importance of providing active and passive labor market policies jointly. In fact, although cash transfers are shown to reduce employment, especially if the transfer is sharply phased-out with job earnings, activation has a strong enough net effect to compensate for lock-in and for the negative effects of the cash transfers. While bearing in mind the limits in terms of external validity of my results, the insights I find are interesting also for policies using different combinations of the same ingredients, such as when active policies are combined with minimum income or unemployment benefits. Finally, the paper proves the effectiveness of an important French labor market policy, promoting employability of disadvantaged NEETs. However, the gain is concentrated in precarious jobs, the costs of the program are large, and the population was strongly selected on motivation, thus it is not guaranteed that the program will remain cost-effective if its scope is extended.

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. Section 5 disentangle the mechanisms, namely the effect of better search technology obtained through activation, lock-in, disincentives from cash-on-hand and implicit taxation. Section 6 discusses the results in comparison with related studies. Section 7 draws policy implications and concludes.

2 Institutional Background

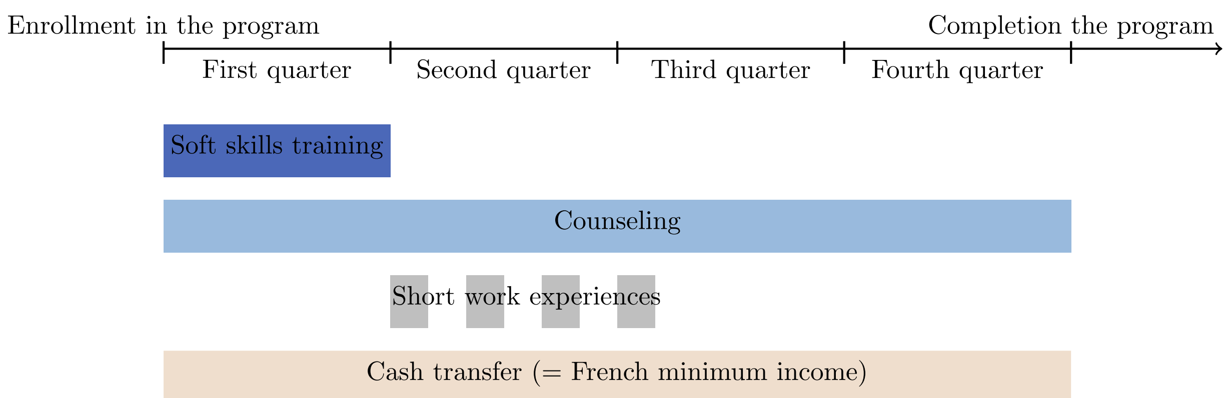
Garantie Jeunes was part of the European Union Youth Guarantee, which financed a number of national programs aimed at promoting youth employment, sharing the same name but having very different characteristics[4]. The French version of the program was launched in October 2013, co-financed by the French government, and targeted disadvantaged NEETs aged 16-25. The program lasts one year, and its outline

[3]See Ioannides and Datcher Loury (2004); Pellizzari (2010); Dustmann et al. (2016); Cingano and Rosolia (2012); Kramarz and Skans (2014); Marinescu and Rathelot (2018); Mendolia and Walker (2014); Schlosser and Shanan (2022)

[4]The concept of Youth Guarantee derives actually from a Nordic tradition of establishing a right to employment or training for youth entering the labor market. The EU channeled part of the European Social Fund toward financing nationally-defined implementation programs aiming at supporting employment of disadvantaged youth. There was quite some variability in focus and kind of the implementation programs at national level (Escudero and López, 2017; Escudero and Mourelo, 2018). Other counterfactual evaluations of country-specific initiatives include Bratti et al. (2017) and Pastore and Pompili (2019).

is reported in Figure 1[5]. Upon enrollment, the participant is required to sign a contract of engagement, risking exclusion from the program if not participating in the activities. The early activation part consists of a six-weeks period of collective courses provided by 2 counselors, with 10-20 participants per class. The training is centered on job search and search frictions covering soft skills linked to job search (presentation skills, job search strategies, applications, CVs, motivation letters) but also personal habits and self confidence (learn to be timely, manage your health, plan your week, ...). There follows a ten-month period of job search assistance, with a personal counselor following the youth by phone, emails and interviews held once every 21 days on average. This second part is characterized by a "work-first" approach, i.e. frequent proposals of internships and short work experiences of at most a month, during which the youth works on small tasks in a partner firm with the aim of learning about the working environment and the industry.

Figure 1: Outline of the program of *Garantie Jeunes*



During the year of the program, youths receive a monthly cash transfer equal to the amount provided by the French minimum income scheme, which is annually updated. For example, it was €484.82 gross in April 2018. Importantly, if youth find a job before the end of the program, the cash transfers is not reduced until €300 of labor earnings. Above €300, cash transfers decrease proportionally with earnings until they reach zero at 80% of the French gross monthly minimum wage (i.e. between €1,120 and €1,174 in the period considered). Most of the youths arrive until the end of the program, but 3% were expelled for not adhering to the terms of of the contract[6]. Such a combination of activation policies and generous cash transfers was considered quite innovative in the French context, and the design of *Garantie Jeunes* was done in light of evidence from previous experimental programs and evaluations of comparable policies (Gurgand and Wargon, 2013).

French local Youth Employment Centers (YECs)[7] are in charge of the administration of the program. These employment centers were introduced in the 1990s, and focus specifically on youths between 16 and 25, who are assigned to a specific YEC based on municipality of residence. A large number of youths registers to YECs, about half a million youths every year, for reasons independent from *Garantie Jeunes*. YEC

[5] While implementation details may vary in different youth centers (Gautié, 2018), the timeline of activities and income benefits observed in the data aligns quite well with the national guidelines (Figure 9 in Appendix). 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.

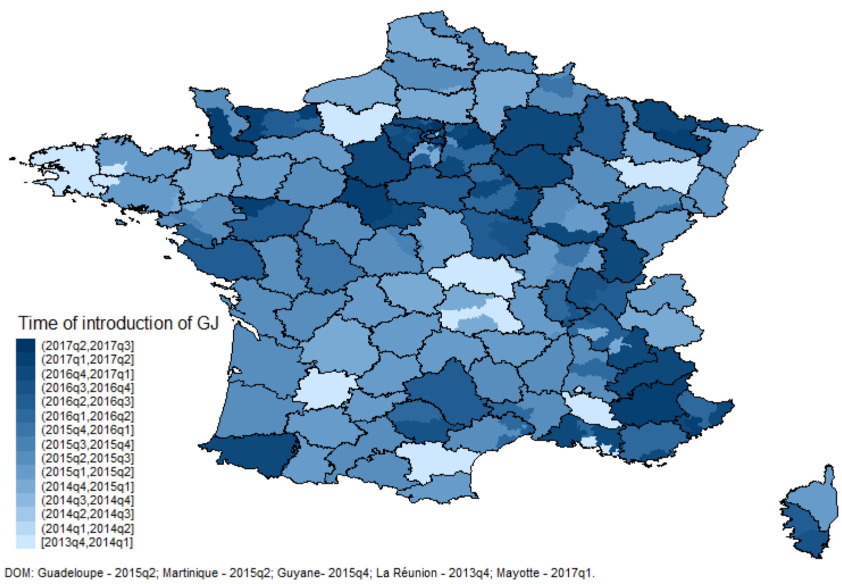
[6] Only 13% quits before the last quarter of the program. Of those who quit, roughly a third quits because they found a full-time job or training, one-third quit for exogenous reasons (age, relocation), and the remainder split between unmotivated voluntary quit and sanctioned youth.

[7] *Missions Locales* in French

registration is in fact often automatic for young unemployed and youth in the last year of professional schools, and it's required for several forms of subsidized training and employment, including the standard job search assistance program (*Contrat d'insertion dans la vie sociale*, CIVIS) which offers a modest number of required activities (Figure 10 in Appendix). Importantly, YEC registration coincides with the beginning of job search for most of the youths, who are likely just out of school and are beginning to enter the labor market, so that their employment rate tends to rise from registration with YECs onward (Figure 12 in the Appendix). Once youths register with a YEC, there is no formal de-registration, so youths can remain in contact with YECs for a variable amount of time and can come back if needed [8].

The introduction of *Garantie Jeunes* was staggered over time, which provides our source of identification. A pilot wave was launched in October 2013 in a number of areas selected as those with the highest reported NEETs rate among a set of volunteer territories. The program was then extended in six waves until it reached all volunteer territories in January 2016. Finally, after a preliminary evaluation, the program was extended to the whole French territory in January 2017. Figure 2 maps this process. Beside the seven official waves of extension, some YECs delayed the introduction of the program, so that between 2013q3 and 2017q2 in every quarter except one there were some YECs adopting the program for the first time. Finally, it is important to note that YECs receive additional funding for administering *Garantie Jeunes* conditional on the number of youths enrolled (70% of the funding), on the number of youths who complete the program successfully (20%), and 10% conditional on the provision of complete data in their information system and proof of their correctness (e.g. enrollment documentation).

Figure 2: Progressive extension of *Garantie Jeunes*.



Notes. French municipalities (black borders correspond to *départments*) by quarter of first case of enrollment in *Garantie Jeunes* in their corresponding YEC. Overseas departments (DOM) are reported in the note.

[8] Figure 11 in the Appendix indicates that 31.4% of youths are still considered active in a specific cohort of registration – meaning youths for whom the YEC records at least one action on their file during a quarter – 3 years from the time of registration. However, after 3 years since registration only 10.1% of the youth still records an action "youth toward YEC"[9], e.g. an email sent by the youth, an interview, or another activity with participation by the youth.

Crucially, among youths registered at YECs, not all youths are eligible to apply, and not all who apply are selected for the program. Firstly, in order to be eligible, youths must either live in a household below the minimum income threshold (minimum income is not available for youths below 25 years old in France), have quit their parents and receive no support from them, have dropped out of school without a qualifying secondary school diploma, or be convicted[10]. Second, to enroll in *Garantie Jeunes* youths must demonstrate a condition of "fragility" and "motivation" through an application process. Qualitative reports describing this process argue that the first selection mechanism involved selective targeting of youths by YECs, which often themselves organized information sessions and pitched the program to a selected group of registered youths. After an individual applies, the decision on the application is made by local independent commissions[11]. Eventually, youths who actually enrolled are roughly half of the eligible ones according to Gaini et al. (2018).

In the public debate, *Garantie Jeunes* is perceived as a successful program: since 2013 more than 500,000 youths have participated in the program, and the program got scaled-up as an answer to the Covid-19 pandemic, with the goal of doubling the number of enrolled youths by easing the up-front selection. In 2022, a new version of the program re-named *Contrat d'Engagement Jeunes* was hotly debated in the electoral campaign, and should cover all individuals earning below minimum income starting in March 2022.

3 Research Design

3.1 Data, Sample and Measurement

To evaluate *Garantie Jeunes*, I build a novel dataset using two administrative sources. The first source is the administrative system of YECs, called I-Milo. This dataset reports details of programs and activities undertaken by the youth at the YEC or with partner firms, including the dates and duration of the events attended. In addition, the dataset includes socio-demographics of youth and additional information provided by youths when registering at YECs. For most individuals, I have information on housing difficulties, access to child-care services, mean of transportation used, and financial resources. I can also calculate the distance between youths' declared residency and the local YEC main office or satellite office[12]. The dataset covers all YECs from late 2010 until the present.

To follow the employment path of youth also when they are not in contact with YEC, I use as second source an extraction of French social security records. The dataset, which was prepared by the French Agency for Social Security under an agreement with the French Labor Ministry, includes information on all contracts signed during the period 2013-2018 by all youths who registered in YECs between 2013 and 2017. The available information includes date of start and termination of the contract, type of contract, total earnings and hours worked.

I merge these two sources to obtain a dataset covering all youths who registered with YECs between January

[10]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.

[11]These commissions are composed by a president appointed by the local representative of central government (*Prefecture*), one representative of the government of the department, presidents of local YECs, and other members named by the *Prefecture*.

[12]The dataset also contains information on French or foreign language proficiency, skills, and hobbies, but only for smaller samples.

2013 and December 2016, approximately 2 Million individuals, following their employment history and YEC activities from the time of registration with YECs onwards. The percentage of youth in our sample who earned less than secondary vocational qualification is similar to that of the overall French population, but a larger share of youth in our sample has at most a secondary diploma (about 52%, against a national mean of 44%). With respect to all youths 16-25 in France, the population of YECs is not significantly different in terms of share of females and French nationals. However, the population is characterized by early experience of activities which are typical of adult life. On average, 35% of youth in YECs have experienced in the quarter preceding registration (national mean 30%), and 37% live independently (national mean 23%), 8% have children of their own (national mean 4%). Youths who got selected for *Garantie Jeunes* are instead not easily distinguishable from the pool of youth at YECs in terms of these observable characteristics, except that they have a much lower employment rate in the quarter before registration.

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

	All youth 16-25 (Census)	Youth in YECs	Youth in standard pr.	Youth in *Gar. Jeunes*
Number of youths (stock)	9327476	1967000	444659	118606
Number of youths (quarter inflow)		125689	41471	14899
Lower than secondary educ.	0.394	0.373	0.424	0.469
Upp. secondary edu. diploma	0.434	0.519	0.541	0.506
Avg. age	20.3	20.1	19.7	18.8
Female	0.491	0.491	0.511	0.461
French nat.	0.915	0.912	0.919	0.930
Empl. last quarter	0.297	0.349	0.335	0.211
Lives independently	0.230	0.365	0.369	0.352
Has children	0.0390	0.0838	0.0878	0.0498

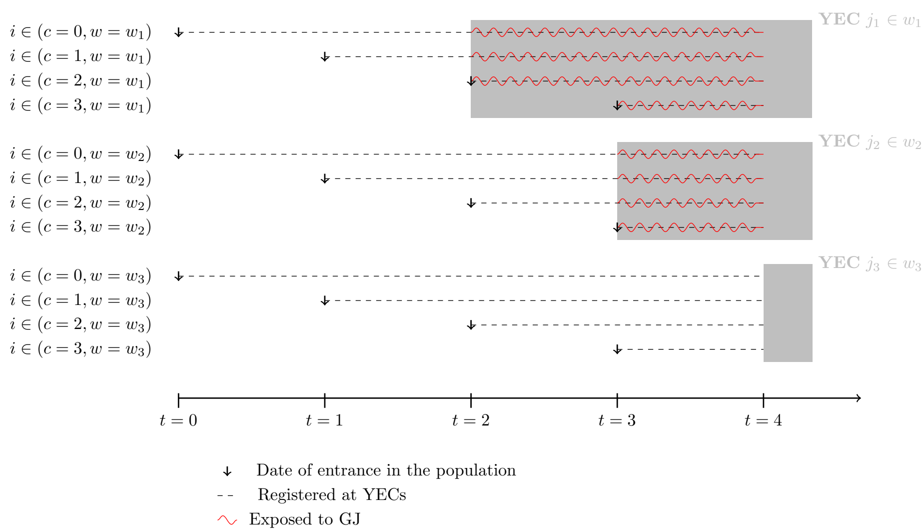
Notes. The table compares the characteristics of youths in different population. The first column concerns all youths aged 16-25 in France, as reported by the Census in years 2013-2016. The second column reports all youths in the sample, namely all youths who registered at YECs in the 2013-2016 period.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

For simplicity, I will aggregate time variables by quarters. Concerning the measurement of my outcomes, I calculate quarterly earnings and hours based on the duration of the contract, and trim values at 99%. For employment, I define a dummy equal to one if the youth has at least one hour of work reported in the quarter. Then, I define the cohort of registration at YEC as the quarter in which the youth first checks in at her YEC, and the wave of introduction of *Garantie Jeunes* as the quarter in which the first enrollment in *Garantie Jeunes* occurs in the YEC. Tables 10-12 in the Appendix provide some descriptive statistics of the cohorts entering our panel.

3.2 Illustration of the Setting

Figure 3 reports a simplified illustration of our setting, including only 12 youths, in 4 cohorts of registration with YECs, and 3 different YECs, j_1, j_2 and j_3. Each line in the exhibit represents a youth in the population, denoted by i, and grouped by their three YECs. Youths first register with YECs at different points in time, called "cohorts", $c \in \{2013q1, ..., 2016q4\}$, are then observed over the time they are registered with YECs, $h = t - c + 1$, where t is calendar time in quarters and $h \in \{1, ...\}$, with $h = 1$ at time of registration. Finally, YECs adopt the program staggeredly (the gray shaded area), according to their wave of introduction of *Garantie Jeunes*, $w \in \{2013q4, ..., 2017q1\}$. Hence, youth who are registered in different YECs and from different cohorts get "exposed" to the program (the red snaky line) at different times since their initial registration with YECs.

Figure 3: A simplified illustration of the setting.



3.3 Identification of ITT

Denote $Y_{w,c}^h := \mathbb{E}(Y_{i,j,c}^h | j \in w, c, h)$ as the conditional expectation for all youths in YECs j belonging to treatment wave w, in cohort c, and observed h quarters after registration, and $G_{w,c}^h$ will be the number of periods these youth are exposed to *Garantie Jeunes*. Note that $G_{w,c}^h$ is equal to either the time passed since adoption of the program or to the full time since registration with YEC, in case the youth registered with a YEC which was already offering the program: $G_{w,c}^h = \min(t - w, h)$. Note also that for given h, w, c, there is only one value of $G_{w,c}^h$ associated, so we will can denote as $Y_{w,c}^h(g)$ the expected outcome in the h, w, c cell, where $G_{w,c}^h = g$.

The first parameter of interest is the intention-to-treat (ITT) effect of exposure to *Garantie Jeunes*, i.e. the average causal change in employment of a cohort as a function of the number of periods of exposure to *Garantie Jeunes*. This estimand corresponds to the expected value of the difference in outcomes when treatment exposure is $G_{w,c}^h = g$ and when not exposed[13]:

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

[13]An alternative option is to estimate what happens to the average employment rate in treated YECs. Yet, in Appendix A, I show that in settings like ours, where youth enter the population of interest in cohorts, classical difference-in-differences estimates of the effect *since adoption* of the program (the gray area in Figure 3) will be a mix of youths at different stages of exposure and enrollment in the program. If the program has dynamic effects or if youths self-select over time since registration with YECs, then the effect *since adoption* will be a mix which is difficult to interpret. Note also that the parameter of interest should not be confused with a variation in a survival rate, since we are looking at the probability of *being* employed at a specific point (reversible) in time and not at the probability of *having found* an employment by a specific time (irreversible), without requiring assumptions on the shape of the hazard function.

3.3.1 Event-Study Approach

A common approach in the literature for identifying ITTs of this kind is the event-study approach. This approach uses multiple-ways fixed-effects regressions to estimate dynamic treatment effects. Consider:

$$y_{i,t} = \sum_{g \neq 0} \beta^g \mathbb{1}(G^h_{w,c} = g) + \gamma_{c,h} + \mu_{j,h} + \epsilon_{i,t} \tag{1}$$

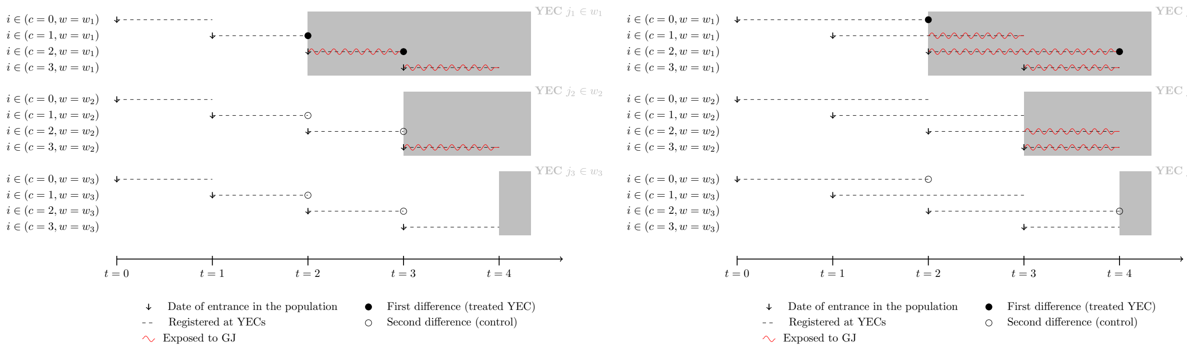
Where $\gamma_{c,h}$ is the cohort fixed effects, $\mu_{j,h}$ YEC fixed effects, all interacted with time-since-registration with YECs. By interacting all fixed effects with time-since-registration with YECs h, our model compares youths at the same time since registration with YECs. This allows to make sure that potential outcomes of youth are comparable, as youth will be at the same point in their job search. In other words, identification of β^g stems from comparing cohorts which have been exposed for g quarters to the program to cohorts not yet exposed. When running regression (1), standard errors are double-clustered at the YEC-time since registration level, following Cameron and Miller (2015).

3.3.2 Difference-in-Differences Approach

As an alternative to the Event-Study Approach, I propose a difference-in-differences estimator which has two advantages. First, it is robust to heterogeneous treatment effects over time and groups, unlike fixed effects estimators (De Chaisemartin and D'Haultfœuille, 2020a). Second, it allows us to flexibly obtain cell-specific ITTs and use them to study LATEs associated to individuals at a specific stage of enrollment in the program. Assumptions and propositions are detailed in Appendix B. In a nutshell, my estimator first estimates cell-specific $DID^h_{w,c}$, i.e. the effect for youth in wave w, cohort c, who have been registered to YECs for h quarters and exposed to *Garantie Jeunes* for g quarters. This is obtained by taking the difference between expected outcomes of youths in cell (h, w, c), minus the earliest cohort from the same YEC not-yet exposed (first difference), minus the difference in outcomes in the same cohorts but in YECs where both cohorts are not-yet-exposed (second difference).

To get the intuition, Figure 4 reports in the left panel the observations used to estimate $DID^{h=1}_{w_1,c=2}$, the effect for youth in cell $(h = 1, w_1, c = 2)$, who are exposed to the program for one period $(g = 1)$. This estimator compares the evolution of the outcome across cohort 2 and baseline, for individuals 1 period after registration with YECs, in YECs where cohort 2 is treated and baseline is not vs. YECs where both cohort 2 and baseline are untreated. Accordingly, in the right panel of Figure 4, I show which observations will be used to construct $DID^{h=2}_{w_1,c=2}$, the effect for youth in cell $(h = 2, w_1, c = 2)$, who are exposed to the program for two periods $(g = 2)$.

Figure 4: Examples of data used for estimation of the rolling difference-in-differences estimator. $DID^{h=1}_{w_1,c=2}$ (left panel) and $DID^{h=2}_{w_1,c=2}$ (right panel)



To obtain an estimator of $\Delta^{ITT}(g)$, I then aggregate all $DID^h_{w,c}$ where (h, w, c) is such that $G^h_{w,c} = c$, obtaining an estimator of the ITT effect of being exposed for g quarters, DID^g. From a different angle, this methodology simply adapts the one by De Chaisemartin and D'Haultfœuille (2020a) to our cohort setting by comparing individuals at the same time since registration h, therefore "rolling" over h. In this case, I will obtain standard errors by bootstrapping, accounting for clustering at the level of treatment variation (YEC and time-since registration level), following De Chaisemartin and D'Haultfœuille (2020b).

3.4 Identification of LATEs

While ITTs estimate the effect of exposure to *Garantie Jeunes*, I might be interested in understanding the magnitude of the effect associated to being actually enrolled in *Garantie Jeunes*. First, I can estimate LATE on all compliers exposed for g quarters to the program:

$$\Delta^{LATE}(g) \quad = \quad \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(g) | D^h_{i,j,c} > 0)$$

Where $D^h_{i,j,c}$ is the number of quarter elapsed since a youth enrolled in the program (by construction, in w, c, h cells where $g > 0$ there is always at least one youth for which $D^h_{i,j,c} > 0$). Yet, $\Delta^{LATE}(g)$ is only the average program effect associated to *any* complier, after g quarters that youth could have enrolled in the program. Instead, we might be more interested in obtaining an estimate of the effect associated to compliers at a specific stage of the program (i.e. by *enrollment* status in the program). For example, it would be useful to disentangle the program effect on compliers who are in the early vs. the later part of the program, or have completed the program. Such estimand will be a LATE depending on the number of periods since actual enrollment in *Garantie Jeunes*, and can be written as:

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

My difference-in-difference methodology is particularly handy for recovering both $\Delta^{LATE}(g)$ and $\Delta^{LATE}(d)$. Proposition 3 in Appendix B points out that $\Delta^{LATE}(g)$ can be estimated by simple rescaling of ITT estimates by the share of compliers. This is not a novelty in IV estimation, but it is worth pointing out that the caveats highlighted by De Chaisemartin and d'Haultfoeuille (2018) don't apply because we always have at least one fully untreated wave and no defiers/always takers in the control group. Under more restrictive assumptions, and leveraging the definition of expectations, Proposition 4 in Appendix B shows that we can recover

$\Delta^{LATE}(d)$ using a Minimum Distance regression of cell-specific ITTs on the share of youths at different stages since enrollment in the program in that specific cell. Namely, I will recover LATE effects since actual enrollment in the program as the $\hat{\delta(.)}$ estimated from the regression:

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

Where, to gain more power, I aggregated d into three classes: $0 < d \leq 2$, $2 < d \leq 4$ and $d > 4$, respectively the first semester of enrollment in the program, the second, and more than one year after enrollment. Regression 2 also clarifies the intuition behind this last step of my methodology: the $\hat{\delta(.)}$s are coefficients estimating how much the cell-specific ITT $DID^h_{w,c}$ changes following a change in the share of youths at a particular stage of the program in that cell.

4 Results

4.1 Balance Checks

An implication of the strong exogeneity assumption underlying our identification strategy is that cohorts of youth entering YECs before and after the introduction of *Garantie Jeunes* should be comparable. That is, the composition of youths registering to YECs must not change with the introduction of *Garantie Jeunes*. In this section I exploit the wide range of information available in YECs administrative data to run a set of balance checks that test this hypothesis on a wide range of observable characteristics in YEC data. Table 2 reports a set of regressions of average characteristics of a cohort on a dummy for *Garantie Jeunes* adoption (Check 1), on a linear trend by quarter after adoption (Check 2), and on both the dummy and the linear trend together (Check 3). The results are reassuring: of the many variables evaluated, the only relevant concern is an increase in youths registering with housing problems, which increases by 0.6 percentage points over a mean of 10.5% before *Garantie Jeunes* introduction. It also appears that there was a mildly significant increase in the share of youth registering who have children, but the magnitude is again very small. All other characteristics of youths registering with YECs don't significantly change with *Garantie Jeunes* introduction, supporting the assumption that treatment status doesn't affect individuals' potential outcomes.

Table 2: Balance checks.

	(Check 1)	(Check 2)		(Check 3)	(Mean)
	GJ adopt.	GJ adopt.*quart. adopt.	GJ adopt.	GJ adopt.*quart. adopt.	
Share of female	-0.00111	-0.00031	-0.00145	-0.000371	0.491
	(0.00179)	(0.000392)	(0.00177)	(0.000388)	
Age at registration	0.0138	-0.000236	0.0135	0.000533	20.1
	(0.0121)	(0.00324)	(0.0127)	(0.00333)	
Lower secondary	0.00156	0.0000216	0.00157	0.000108	0.0744
	(0.00160)	(0.000375)	(0.00151)	(0.000354)	
Upper secondary education (CAP/BAC)	0.0000462	0.000184	0.000258	0.000186	0.817
	(0.00239)	(0.000614)	(0.00236)	(0.000605)	
French nationality	-0.00212	0.000487	-0.00157	0.000368	0.912
	(0.00217)	(0.000511)	(0.00230)	(0.000538)	
Housing problems	0.00588***	0.000388	0.00633***	0.000716	0.0500
	(0.00157)	(0.000431)	(0.00175)	(0.00046)	
Resident in Urban Sensitive Area	0.000656	0.00298	0.0041	0.00301	0.105
	(0.00355)	(0.00211)	(0.0052)	(0.00220)	
Distance residency-YEC	-4.63	1.00	-3.44	0.752	715
	(3.48)	(1.43)	(3.75)	(1.43)	
Resources declared	1.11	0.400	1.56	0.461	155
	(2.26)	(0.780)	(2.59)	(0.815)	
Has a motor vehicle	-0.00386*	0.000128	-0.00371	-0.0000836	0.410
	(0.00233)	(0.000499)	(0.00239)	(0.000516)	
Lives alone	0.000523	0.000252	0.000814	0.000281	0.899
	(0.00217)	(0.000473)	(0.00223)	(0.000486)	
Has children	0.00154	0.000652*	0.00230*	0.000738*	0.0837
	(0.00119)	(0.000383)	(0.00125)	(0.000381)	
Problems with childcare	0.00621	-0.00122	0.00479	-0.000865	0.348
	(0.00620)	(0.00145)	(0.00609)	(0.00140)	

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

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

I then proceed to estimate the effect of being exposed g quarters to the program (ITT effect). Figure 5 reports the results obtained both using a fixed effect regression as in (1) and using our DID methodology. The first stage indicates that in each additional quarter of exposure about 1% of youth enters the program, quite linearly over the first two years since exposure. This linear increase in first stage coefficients shows that compliers of a cohort are not entering the program all together as soon as they are exposed, but quite staggeredly over time of exposure, with some youth entering the program much later, even 8 quarters after they have been exposed the first time. The coefficients before the introduction of the program are all omitted because nobody participates in *Garantie Jeunes* in YECs which are not yet treated (no defiers and no always takers). Turning to our outcomes of interest, coefficients on employment, hours worked and wages display a clear and long parallel trend in all three outcome variables, with all coefficients close to zero before exposure, which reassures us on the validity of our identification strategy. After youth starts being exposed to *Garantie Jeunes*, there is still no significant differences in outcomes in the first 4 quarters of exposure. However a positive effect arises in employment and hours worked starting at the beginning of the second year after exposure. Because the fifth quarter of exposure coincides with the time when the first youths who entered *Garantie Jeunes* in the first quarters of exposure complete the program, this dynamic of the ITT effect might be driven by youth who complete the program. In fact, the effect increases in the subsequent quarters, as more and more youth complete the program. Finally, results using Event-Study with fixed-effects and Difference-in-Differences methodologies are extremely similar, suggesting that heterogeneous effects are not a concern and reassuring us about the validity of our methodology.

Figure 5: Intent to treat (ITT) estimates using the rolling diff-in-diff approach.

Notes. The figure reports results of the rolling diff-in-diff approach. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable is a dummy for exposure to *Garantie Jeunes*. The other three panels report the reduced-form coefficients: the dependent variables are employment, hours and wages (earnings per hour), while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 7. Cell-specific effects were obtained as in Equation 6. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.

In addition, Table 3 reports the aggregated effects of the first semester, second semester and from second year of exposure. The average effect in the second year of exposure is +1.15 percentage points in employment probability, while hours worked increases by +3 hours on a quarterly basis. Wages (measured as average earnings per hour) are instead not significantly affected, remaining at a mean close to the minimum wage and with small standard errors.

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

	Enrollment in GJ		Employment		Hours		Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FE	DID	FE	DID	FE	DID	FE	DID
ITT 1st semester of exposure	0.000502*	0.0158***	0.00269**	-0.000452	1.239***	0.334	-0.0430	0.0292
	(0.000285)	(0.000634)	(0.00136)	(0.00131)	(0.356)	(0.529)	(0.0278)	(0.0797)
Total n.obs	4003538		4003538		3957848		1529554	
ITT 2nd semester of exposure	0.0171***	0.0401***	0.00126	-0.00328	0.643	-0.248	-0.0610	0.0162
	(0.000523)	(0.000699)	(0.00203)	(0.00217)	(0.537)	(0.604)	(0.0381)	(0.0564)
Total n.obs	3890678		3890678		3833155		1587769	
ITT 2nd year of exposure	0.0367***	0.0631***	0.00849***	0.0115**	2.365***	3**	-0.0179	0.0957
	(0.000694)	(0.000911)	(0.00277)	(0.00524)	(0.700)	(1.5)	(0.0484)	(0.0666)
Total n.obs	5574885		5574885		5472754		2373426	
Control mean 1st sem. in YEC			0.386		63.7		12.02	
Control mean 2nd sem. in YEC			0.468		99.3		11.85	
Control mean 2nd year in YEC			0.486		124.6		11.85	

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. Columns (1), (3), (5) and (7) use a specification regressing the outcome on an indicator of exposure, YECs interacted with time-since-registration and cohort interacted with time-since-registration fixed effects. Standard errors are clustered at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level. Columns (2), (4), (6) and (6) use the rolling diff-in-diff approach outlined in Appendix B, where I estimate a full set of $DID^h_{w,c}$, for every $(w, c|h)$ cell, and then aggregate $DID^h_{w,c}$ corresponding to same levels of g. Standard errors are in parenthesis and obtained by bootstrap sampling with clustering at the YEC-time since registration level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Subsequently, in Table 4 I can estimate LATEs on all compliers, conditional on time of exposure to *Garantie Jeunes*, which allows to have a better sense of the magnitude of the effect. Namely, the coefficients suggest that compliers in the second year of exposure increase their probability of employment by 18 percentage points, and quarterly hours worked by 48.

To better understand if the effect in the second year of exposure is driven by youth who have completed the program, I can use Equation 2 to estimate how much of the effect is associated to compliers at different stages of program enrollment, obtaining LATEs since actual enrollment in the program. The estimates on employment and hours worked indicate that indeed the positive effect in the second year of exposure is driven by the share of youth who has completed *Garantie Jeunes*. The LATE estimated on compliers in the second year after enrollment (LATE after completion) is +21 percentage points in employment and +49 hours worked. We can compare the estimated LATEs to average employment of compliers in the treatment group, and see that estimates imply roughly a 50% increase of employment probabilities and a 80% increase in hours worked after completing the program[14]. Finally, also in terms of LATE there is no significant effect on wages.

[14] The counterfactual outcomes for compliers were-they-not treated can be obtained by subtracting the estimated LATE from the observed average outcome of compliers in the treatment group.

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

	Employment	Hours	Wage
	(1)	(2)	(3)
	DID	DID	DID
LATE 1st semester of exposure	-0.0282	20.8	1.83
	(0.0854)	(33.7)	(5.19)
LATE 2nd semester of exposure	-0.0819	-6.15	0.402
	(0.0538)	(14.9)	(1.41)
LATE 2nd year of exposure	0.183**	47.6**	1.51
	(0.0826)	(23.7)	(1.05)
LATE 1st semester of enrollm.	-0.0958*	5.44	-0.312
	(0.0559)	(14.4)	(2.73)
LATE 2nd semester of enrollm.	-0.0310	-1.66	0.315
	(0.0680)	(22.7)	(2.5)
LATE after completion	0.209**	48.6*	3.74
	(0.0962)	(26.7)	(2.32)
Compliers mean 1st semester in GJ	0.327	33.84	11.72
Compliers mean 2nd semester in GJ	0.408	59.75	11.85
Compliers mean after completing GJ	0.541	108.7	12.02

Notes. The upper panel reports reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and wages for compliers, obtained according to Proposition 3 a). The middle panel reports the LATE effect of being at different stages of *Garantie Jeunes*, obtained according to Equation 2. The lower panel reports average employment rates for compliers in the treatment group. Standard errors are bootstrapped and reported in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Although the estimated LATE effects after completion of *Garantie Jeunes* are very high, some aspects reassure us on the credibility of the estimates. First, our results are extremely similar to the one found by the pilot evaluation of *Garantie Jeunes* by Gaini et al. (2018), who focused on the first wave and used a matched survey to estimate LATE of +22.2 in the probability of employment (over a control mean of 25%) on the fifth quarter after enrollment in the program[15]. Second, the results are driven by very precarious forms of contracts, which can be more volatile. Table 13 in the Appendix reports the ITT and LATE effect on employment in open-ended contracts, temporary contracts, agency jobs (quite frequent in this population) and apprenticeship. The effect on open-ended employment is very close to zero, while the overall employment effect mostly comes from temporary contracts (+.5 percentage points in ITT) and agency jobs (+.4 percentage points ITT). Apprenticeships also increase significantly, but they do so since the beginning of enrollment, suggesting that many youths are channeled into this type of contract also as a form of activation measure. Third, the program has large benefits but also large costs. Appendix Section C runs a cost-benefit analysis by estimating the Marginal Value of Public Funds invested in *Garantie Jeunes* (Hendren and Sprung-Keyser, 2020), and finds that benefits of the program are only 19% larger than its costs. Eventually, the Marginal Value of Public Funds of *Garantie Jeunes* is comparable to similar cash transfers and job training programs (Figure 8 in the Appendix).

[15]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. Gaini et al. (2018) do not report results for hours of work and wages, so comparison with them is not possible.

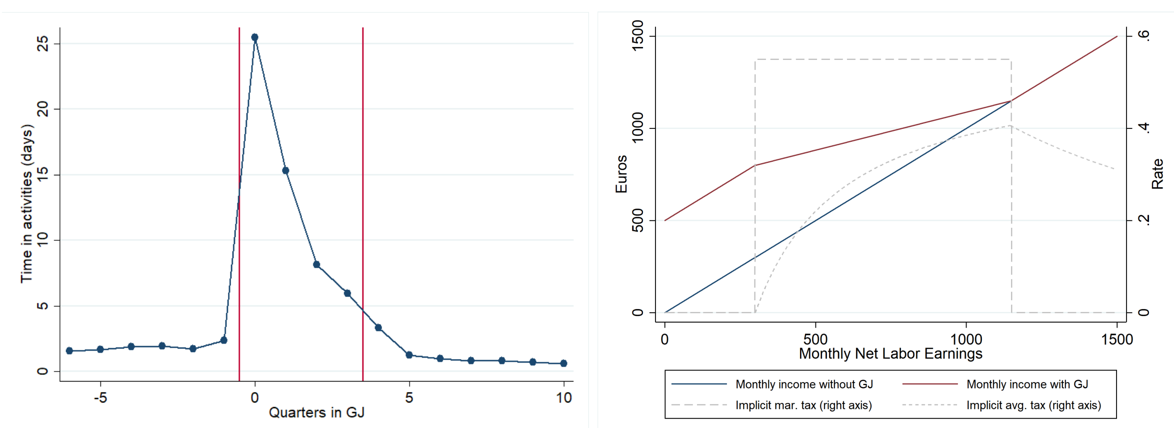
To conclude, I run heterogeneity by youth characteristics (Figure 13-15 in the Appendix). The effect in ITT terms does not vary by gender, it's stronger for youth aged over 19 years-old, and it appears to be fully driven by youth with upper secondary education, as the ones with less than secondary education are likely channeled toward formal training rather than employment.

5 Disentangling the Role of Cash Transfers and Activation

5.1 Earnings at Different Stages of the Program

In this section, I interpret the mechanisms behind our reduced-form estimates of program effect from Section 4 by exploiting two dimensions of treatment variation: the schedule of activation measures and the cash transfer phase-out with job earnings, summarized in Figure 6. The left panel reports the number of working days with a scheduled training, interview or job immersion for participants in *Garantie Jeunes*, before and after enrollment in the program. In the first two quarters of the program, youths are busy 25 and 15 days in a quarter respectively, possibly lacking the time to actually look for a job ("lock-in" effect). This is due to the intensive collective training sessions held in the first quarter, and to job immersions that peak in the second quarter. The right panel reports instead the evolution of income with and without *Garantie Jeunes*. The cash transfer of *Garantie Jeunes* can be fully cumulated with job earnings up until €300 of net earnings. The transfer is then reduced quite steeply for every additional Euro of job earnings, until it disappears at 80% of the gross minimum wage (€1120 in 2013, €1159 on average in 2013-2016), where income with *Garantie Jeunes* equals income without. Hence, the phase-out of the cash transfer significantly flattens the schedule of monthly income with *Garantie Jeunes*: for every additional Euro earned the cash transfer is reduced by about 55 cents, implying 55% marginal tax rate and up to 40% average rate.

Figure 6: Working days with a scheduled activity as a function of time since enrollment in *Garantie Jeunes* (left panel) and cash transfer phase-out (right panel).



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

Given these variations in the treatment, we aim at studying how the front-loading of time-consuming activation measures and the discontinuities in cash transfers cumulability are reflected in labor earnings of participants in *Garantie Jeunes*. Namely, I can use Proposition 3 to recover the LATE for individuals in the 1st semester, 2nd semester after enrollment, and after completion of *Garantie Jeunes*, using as outcome the probability of earning a monthly amount below €300, between €300 and €1100, or above €1100. Note that because €1100 roughly corresponds to monthly net earnings at a full-time minimum wage, earning a monthly amount below €300 or between €300 and €1100 corresponds respectively to very short part-time or agency jobs and to more consistent part-time jobs[16].

Table 5 reports the results. In the first semester after enrollment, when youths are busy in soft-skill training and activation policies, I find a significant decrease in the probabilities of part-time jobs, while no significant effect is found for the probability of earning over €1100. This could be interpreted interpreted as youths being as busy in activation measure as to reduce search effort and availability for less remunerative jobs, while still remaining open or targeting their search on full-time minimum-wage jobs. Then, once youths completed the most time-consuming part of the program, but are still eligible for the cash transfer, the estimated LATEs suggest an increase in the probability of earning below €300 and in the probability of earning above €1100, but also a strong decrease in the number of youths earning €300-€1100. This could be rationalized by a general increase in youth employability, and a negative reaction of youth to implicit marginal taxation on earnings in the €300-€1100 range. Finally, in the second year after enrollment, when youths completed the program and stop being eligible for the cash transfers, both the probability of earning in the €300-€1100 range and of earning above €1100 increase substantially. This corresponds to a generally positive effect of the program on employability and job quality after completion.

[16]For separating the second and third category, I will use a threshold of €1100 instead of €1159 (the precise average of 20% gross minimum wage in the period) since I want to avoid including in the previous class individuals bunching around the net minimum wage (which is slightly lower, especially at the beginning of the period). Note that an alternative option would be to look for bunching at €300. However, it is possibly difficult for youths to bunch sharply in terms of net earnings. Moreover, the resources are self-declared, so there might be a wedge between the actual earnings reported in our administrative data and those declared.

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

Local Average Treatment Effect

	Monthly income €1-€300	Monthly income €300-€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	0.188***	0.197**
	(0.0618)	(0.0700)	(0.0793)

Average outcomes of takers in treatment group

	Monthly income €1-€300	Monthly income €300-€1100	Monthly income over €1100
1st semester of enrollm.	.068	.042	.119
2nd semester of enrollm.	.091	.085	.211
After completion	.101	.153	.336

Notes. The table reports estimates of LATE effects obtained using Proposition 3b and Equation 2, using as outcome the probability of earning in different income brackets. Standard error are reported in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The lower panel reports the average outcomes estimated for the compliers of the treatment group. Estimates are obtained using Equally Weighted Minimum Distance.

5.2 A Framework for Formally Disentangling the Mechanisms

Yet, to causally interpret the coefficients of Table 5 as the result of variations in cash transfers and activation measures in Figure 6, one needs a more formal set of restrictions on the channels through which cash transfers and activation measures can have an effect on labor earnings. In the literature, activation measures are mostly considered as affecting job search. Gautier et al. (2018) model the impact of activation measures on search effort, assuming that participation in activation programs improves the matching technology (i.e. increases the number of applications sent per unit of time) but requires limited time and effort and hence risks reducing the amount of job search. In turn, passive policies typically influence the amount of earnings through the elasticity of labor supply, by changing the relative utility of employment/unemployment (Card et al., 2007; Chetty, 2008; Le Barbanchon, 2020). Saez et al. (2012) reviews the literature on the topic and points out that compensated earning elasticities of labor supply are generally small (0.1-0.5), but larger effects are observed for workers less attached to labor force, including low income earners in welfare programs (Card and Hyslop, 2005).

In light of this, let us first model how cash transfers affect labor supply in the context of *Garantie Jeunes*. Suppose that wages are given and equal for all individuals so that, for each period, youth maximize utility from choosing gross working earnings $z^t \in \{z^0, z^1, z^2, z^3\}$. The last three brackets correspond to those of Table 5, i.e. working earning €1-300, €300-1100, >€1100 per month, while z^0 corresponds to unemployment. Since €1100 is roughly the minimum wage, and wages are assumed equal for all individuals, z^1 includes those who work by the hour for short time, discontinuous jobs or low-intensity part-time (e.g 5-10 hours per week),

z^2 corresponds to less discontinuous jobs or normal part-time, and z^3 corresponds to full-time employment. The assumption of fixed wages is strong but plausible, because takers of *Garantie Jeunes* mostly work at the minimum wage, given that the estimated effect of the program on wages is non-significant (Table 4), and because participants are few with respect to the overall population of minimum-wage earners (hence general equilibrium effects are unlikely).

Let *cash* be a dummy for being enrolled in the year-long *Garantie Jeunes* program, thus having the right to receive the cash transfer, which is equal to one for compliers when they are enrolled in the program, and equal to zero otherwise. Assume that utility for individual i and choice j is linear:

$$\begin{aligned} &U_{ij} = u_j(cash) + \eta_i \\ &\text{where} \quad u_j(cash) = a_1(z_j + cash \cdot (b - \min[b, \max[0, (z_j - 300)\tau]]) + a_2 z_j / w \end{aligned} \tag{3}$$

In such expression a_1 is the marginal utility of consumption, a_2 is the marginal utility of leisure, b is the amount of the cash transfer from *Garantie Jeunes* (€484.82 gross in April 2018), τ is implicit marginal taxation due to the phase-out (55%), and η_i is individual heterogeneity. The dummy variable *cash* indicates if the youth is enrolled in Garantie Jeunes, hence receiving the cash transfer if she earns less than the minimum wage each month. Denote $\alpha_j = a_1 z_j$, $\beta = a_1 b$, $\gamma_j = a_2 z_j / w$. Let consumers maximize the utility of their desired employment so that $U_{j^*i} > U_{ji} \quad \forall j \neq j^*$. If η_i is distributed as extreme values, then McFadden et al. (1973) shows that $Pr(z^{j*} = z^j) = \frac{e^{u_j}}{\sum_j e^{u_j}}$. Hence:

$$\begin{aligned} &Pr(z^{j*} = z^j) = \Phi_j(cash) \\ &\text{where} \quad \begin{cases} \Phi_1(1) = \frac{e^{\alpha_1+\beta+\gamma_1}}{e^{\alpha_0+\beta}+e^{\alpha_1+\beta+\gamma_1}+e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_1}}}{K_1}e^{\beta} \\ \Phi_1(0) = \frac{e^{\alpha_1+\gamma_1}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_1}}}{K_0} \\ \Phi_2(1) = \frac{e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}}{e^{\alpha_0+\beta}+e^{\alpha_1+\gamma_1+\beta}+e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_2}}}{K_1}e^{\beta-(\alpha_2-300a_1)\tau} \\ \Phi_2(0) = \frac{e^{\alpha_2+\gamma_2}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_2}}}{K_0} \\ \Phi_3(1) = \frac{e^{\alpha_3+\gamma_3}}{e^{\alpha_0+\beta}+e^{\alpha_1+\beta+\gamma_1}+e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_3}}}{K_1} \\ \Phi_3(0) = \frac{e^{\alpha_3+\gamma_3}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_3}}}{K_0} \end{cases} \end{aligned} \tag{4}$$

Where $\hat{\alpha_j} = \alpha_j + \gamma_j$ is the net value of choice j when there are no cash transfers, $K_0 = e^{\alpha_0} + e^{\alpha_1+\gamma_1} + e^{\alpha_2+\gamma_2} + e^{\alpha_3+\gamma_3}$ and $K_1 = e^{\alpha_0+\beta} + e^{\alpha_1+\gamma_1+\beta} + e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta} + e^{\alpha_3+\gamma_3}$.

Then, I introduce job search with activation measures. Suppose that the probability of being employed in a bracket j is equal to the product of the share of youth who supply labor in that bracket times the probability of obtaining a job instead of remaining unemployed $P(.)$. Following Gautier et al. (2018), I impose $P(.)$ to depend on whether the youth has improved his job search technology thanks to activation measures (*tech*) and on time spent searching (*time*). I call the activation term "search technology" as it will be sort of a residual of the effect on employment when youths are activated, so that *tech* is equal to zero in the control group, and equal to one for treated youth, who receive soft-skills training, counseling and network opportunities with *Garantie Jeunes*[17]. Finally, the dummy for time availability *time* is equal to one as a default and equal to zero in the first semester of enrollment, when the youth must attend activities

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

offered at the YECs risking the so-called lock-in effect. As Figure 6 suggests, this is the case in the first semester of enrollment in *Garantie Jeunes*[18].

$$Pr(Y_{ji} = 1) = Pr(z^{j*} = z^j) \cdot P(tech, time) \tag{5}$$

At this point, we can plug Equation 4 into Equation 5, obtaining $Pr(Y_{ji} = 1)$ for compliers of *Garantie Jeunes*, as a function of different labor supply factors and of search frictions $P(tech, time)$. This expression will vary for every income bracket j, at different stages of the program, and according to individuals being in treatment or control group, as summarized in Table 6. For understanding our interpretation, let us start from the control group (lower panel of Table 6). In the control, compliers are not exposed to *Garantie Jeunes* and cannot enroll, hence $cash = 0$ for all youth and brackets. They also don't receive activation measures, so $tech = 0$, and they don't risk to not have enough time to look for a job due to lock-in, so $time = 1$. Thus, the $P(tech, time)$ term representing job search is $P(0, 1)$ in the control group.

Turning to treated compliers, in the upper panel of Table 6, during the first semester of enrollment $cash = 1$, as youths are receiving the cash transfer. Note also that $\Phi_1(1) = \Phi_1(0)\frac{K_0}{K_1}e^{\beta}$ in the upper left cell of the upper panel of Table 6: intuitively, labor supply for the €1-300 bracket with cash transfers is equal to labor supply without cash transfers times the effect of cash transfers e^{β} rescaled by $\frac{K_0}{K_1}$. Turning to the probability of finding a job $P(tech, time)$, in the first semester of enrollment youths are receiving activation measures, hence $tech = 1$, and risk lock-in as they might be too busy to effectively look for a job, so $time = 0$. Now, moving to the second column in the first line of Table 6, labor supply in the €300-€1100 income bracket reports an additional term, $e^{-(\alpha_2 - 300a_1)\tau}$, which is the effect of implicit taxation on earnings arising from the phase-out of the cash transfer. Finally, in the third column, labor supply differs with respect to control group only due to the term $\frac{K_0}{K_1}$. This term can be interpreted as the option value or the spillover effect of cash transfers, as it multiplies labor supply in all brackets, independently from whether youth are actually receiving cash transfers or not. In fact, cash transfers are zero for jobs above €1100, but youths might still reduce labor supply in this bracket as the other options become relatively more attractive.

Turning to the second semester of enrollment, in the second line of the upper panel, all terms remain the same as in the first line except that $time = 1$, because youth in the second semester of enrollment have completed activation measures and have time to dedicate to job search. Finally, after completion of *Garantie Jeunes* youth stop receiving cash transfers, and labor supply of treated compliers is the same as in the control group. The term representing the probability of finding a job $P(.)$, however, has still $active = 1$, reflecting the fact that youth have now a better search technology thanks to activation measures.

[18] Note that Equation 4 derives from consumers maximizing their utility as-if search frictions did not exist. That is, they choose the optimal employment they will look for only as a function of *cash* and η_i, without considering that they could have more/less probabilities of obtaining the job. This corresponds to fully separate the channel of the cash transfer (labor supply) and of activation measures (search frictions). Although this structure might appear simplistic, it is useful as an extreme case. Also, in the context of inexperienced youth this hypothesis might be realistic that youth only care about their direct incentives to supply labor, failing to incorporate the risk of not being hired

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

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

	Monthly income €1-€300	Monthly income€300-€1100	Monthly income over €1100
1st semester of enrollm.	$\Phi_1(0)\frac{K_0}{K_1}e^{\beta}\cdot P(1,0)$	$\Phi_2(0)\frac{K_0}{K_1}e^{\beta-(\alpha_2-300a_1)\tau}\cdot P(1,0)$	$\Phi_3(0)\frac{K_0}{K_1}\cdot P(1,0)$
2nd semester of enrollm.	$\Phi_1(0)\frac{K_0}{K_1}e^{\beta}\cdot P(1,1)$	$\Phi_2(0)\frac{K_0}{K_1}e^{\beta-(\alpha_2-300a_1)\tau}\cdot P(1,1)$	$\Phi_3(0)\frac{K_0}{K_1}\cdot P(1,1)$
After completion	$\Phi_1(0)\cdot P(1,1)$	$\Phi_2(0)\cdot P(1,1)$	$\Phi_3(0)\cdot P(1,1)$

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

	Monthly income €1-€300	Monthly income €300-€1100	Monthly income over €1100
No program	$\Phi_1(0)\cdot P(0,1)$	$\Phi_2(0)\cdot P(0,1)$	$\Phi_3(0)\cdot P(0,1)$

Notes. The table reports structural interpretation of $Pr(Y_{ji}=1)$ the probability of being actually employed in bracket j conditional on enrollment status in *Garantie Jeunes*. It is obtained from Equation 4 and Equation 5.

Now, the goal is to fit the structural interpretation to our results and estimate the role of each component. Notice in fact that our estimates in the lowest panel of Table 5 provide an empirical counterpart to every moment in the upper panel of Table 6. Moreover, I can subtract estimates of the LATEs to average outcomes for compliers in the treatment group in Table 5 to recover estimates of average outcomes for control group compliers (Imbens and Rubin, 1997), corresponding to moments in the lower panel of Table 6. For example, having an estimate of $\mathbb{E}(Y_{ji}(D^h_{i,j,c})|0<D^h_{i,j,c}\leq 2)$ and of $\mathbb{E}(Y_{ji}(D^h_{i,j,c})|0<D^h_{i,j,c}\leq 2)-\mathbb{E}(Y_{ji}(0)|0<D^h_{i,j,c}\leq 2)$, I can recover $\mathbb{E}(Y_{ji}(0)|0<D^h_{i,j,c}\leq 2)$. In sum, by equating each of the estimated average outcomes for compliers in treatment and control to their structural interpretation in Table 6, I obtain a system of 18 equations, which I can use to solve for our effects of interest $P(1,0)/P(1,1)$,$P(1,1)/P(0,1)$, β, $(\alpha_2-300a_1)\tau$,and K_0/K_1. $P(1,0)/P(1,1)$ and $P(1,1)/P(0,1)$ are respectively the lock-in effect (having $time=0$ w.r.t. $time=1$, keeping $tech$ constant) and the effect of activation (having $tech=1$). Then, e^{β} represents the effect of receiving cash-on-hand (moral hazard/liquidity effect, as we cannot distinguish the two), while $e^{-(\alpha_2-300a_1)\tau}$ represents the effect of implicit taxation, and K_0/K_1 captures the spillovers of cash transfers. Note that all these estimates are to be interpreted as multiplicative factors of the probability of employment.

Results are reported Table 7. Because the system is over-identified, I either aggregate the different estimates of the parameters by averaging them (the detailed procedure is reported in the Appendix), or I estimate the results by nonlinear least squares. Column (1) shows the results when different estimates of the parameters are simply averaged. Alternatively, one might want to take into consideration the different levels of significance of the underlying LATEs, so Column (2) of the table reports the estimates using a weighted average, weighting by the average of the inverse of the standard errors squared of the LATEs used to derive the components of the effect. Finally, the estimates obtained with weighted Nonlinear Least Squares are reported in Column (3).

Table 7: Estimated net effects of cash (implicit tax, cash-on-hand, and spillovers) and activation measures (lock-in and search tech.) – multiplicative effect on $\mathbb{E}(Y_{ji})$.

Effect (interpretation)	(1)	(2)	(3)
$e^{-(\alpha_2 - 300a_1)\tau}$ (implicit tax)	.226	.146	.523
e^{β} (cash-on-hand)	.967	.989	1.100
$\frac{K_0}{K_1}$ (cash tr. spillovers)	.628	.627	.992
$\frac{P(1,0)}{P(1,1)}$ (lock-in)	.601	.600	.565
$\frac{P(1,1)}{P(0,1)}$ (search tech.)	2.053	2.162	2.125
Method	Avg. of estimates	Weighted avg. of estim.	Solve system by wNLS

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

The results concerning the effect of implicit taxation show that implicit taxation drives away enrolled youths from the implicitly-taxed brackets, reducing employment by 48%-85% depending on the estimation method. In fact, the first row of Table 7 suggests that the presence of implicit taxation multiplies expected employment by a factor ranging between .146 and .523. Concerning instead the effect of cash-on-hand, the estimated multiplicative effect is very close to one, signaling an insignificant role of this aspect. The large effect of implicit taxation and the relatively smaller reaction to cash transfers availability can be seen as suggestive evidence that cash-on-hand effects are contained in *Garantie Jeunes*.

Turning to the effect of activation measure on search technology, the results point at a negative lock-in effect, reducing expected employment by about 40%. This shows that youths participating in the program face significant time constraints. Finally, the positive effect of activation on youths search technology is highly positive, corresponding, on average, to more than doubling employment in cells where youths have been activated. An implication of this large effect is that the probability of finding a job for compliers were-they-not treated is very low. This points out that disadvantaged NEETs who are the target of *Garantie Jeunes* have very low matching probability without the program, either because they face high search frictions, or because they would exert low effort in absence of the program.

6 Discussion

Section 4 estimated the reduced form effect of jointly providing a year of cash transfers and activation measures in the context of *Garantie Jeunes*. The estimated coefficients suggest a null effect during enrollment in the program and a significantly positive effect after completion. Subsequently, Section 5 disentangled the role of cash transfers and activation measures by exploiting variations in the timing of activities and in the phase-out of cash transfers with job earnings. Under the assumptions of a discrete-choice model, I estimated a negative effect of cash transfers and a compensating positive effect of activation. In this section I discuss these results in comparison with closely related studies in the same context of *Garantie Jeunes*, i.e. French YECs and disadvantaged youth.

On the one hand, a work closely related to mine is Aeberhardt et al. (2020). This working paper studies the effect of an increase in cash transfers but keeps activation measures constant, in the same context as that of this paper. The authors consider an experimental program introduced in a small set of French YECs in 2011, which offered a similar cash transfer to youths in the standard YECs program, but no extra activities. The cash transfer was equivalent to that of *Garantie Jeunes* in terms of cumulative amount, but was spread over two years rather than one. Moreover, the transfer was reduced proportionally to job earnings since the first euro earned, not since 300 Euros as in *Garantie Jeunes*. Hence, the monthly amount of the transfer and the rate of implicit taxation were roughly half than in *Garantie Jeunes*. Crucially, in the setting of Aeberhardt et al. (2020) youths are only required to attend the standard program at YECs, that was available also for control group youths[19] Aeberhardt et al. (2020) find that the program they evaluated increased the amount of time youth stayed at YECs, and increased attendance at compulsory activities. Yet, the effect on search effort was null, while employment decreased between 7% and 13% in the first year of the program. Interestingly, their estimated effect on employment can be very closely replicated using the model I estimated in Section 5.3, as proved in the Appendix. This result corroborates the validity of my simple model, and the estimated negative effect of cash transfers that results from it.

On the other hand, there exist two working papers which study a shock to activation measures but not to cash transfers in the French context. First, Crépon et al. (2015) study the effect of job search assistance targeting disadvantaged youth aiming at entering apprenticeship. They exploit an experimental increase in the number of invitations that the youth would receive from YECs to counseling meetings, generating an increase of about three times of the number of meetings (up to one 1.25 every month). Counselors at YECs are also instructed to re-focus job search assistance on apprenticeship contracts. The result of this activation program, which is relatively modest compared to activation measures in *Garantie Jeunes*, is an increases in the probability of signing a contract of around 20%, almost totally driven by apprenticeships contracts. The effect is caused by an increase in the returns from applications (i.e. an increase in the search technology), since the number of applications is actually unchanged. Second, (van den Berg et al., 2015) study a switch from individual intensive counseling to collective job clubs, similar to the ones occurring in the early phase of *Garantie Jeunes*. They show that, in the case of disadvantaged young jobseekers in France, switching from individual to collective counseling further increases employment by 10% (and permanent employment by 28%).

The result of a negative effect of cash transfers alone found by Aeberhardt et al. (2020) is consistent with the results of this paper. In their setting, the model estimated in Section 5 would predict a mild negative effect

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

of cash-on-hand on employment, and no positive effect of activation and lock-in. An additional negative effect from implicit taxation (which is less than half in their case) could be occurring, but it's hard to detect it in their setting where no kink is present. The effect of activation measures at YECs without additional cash transfers, such as job search assistance or job clubs, estimated by Crépon et al. (2015) and van den Berg et al. (2015), is also consistent with our results, since both studies signal positive and large effects of collective and individual activation measures. However, the magnitude of the effect we find is larger, concentrated after completion, and increased further when netting-out the effect of cash transfers and lock-in in Section 5. Thus, there seems to be an additional wedge between the joint effect of activation measures and cash transfers, estimated in this paper, and the sum of the two conditional effects of either an increase in activation or in cash transfers or in activation measures, estimated in the literature. This wedge could arise from potential complementarities between active and passive labor market policies, arising only when the two are jointly provided. For example, Boone et al. (2007) suggest that activation can function as a monitoring device, and conditional cash transfers represent the potential loss in case of sanctions for not respecting conditionality. Alternatively, complementarities might arise from sources other than monitoring, for instance if cash transfers enable youths to exert effort in activities, e.g. if they are credit constrained and need to work when not attending the training sessions.

7 Conclusions

In this paper I studied the effects of a labor market policy combining an intense activation program and generous monthly cash transfers to young disadvantaged NEETs. The results point in the direction of a strong positive effect of the program on employment and hours worked of participants, starting the year after completion, and no effect during enrollment in the program. The increase in employment is however driven by precarious contracts such as fixed-term contracts and agency jobs. I show that the results can be explained by a negative effect of cash transfers, particularly through implicit taxation, and a positive one of activation. The positive effect of activation compensates for lock-in and for the negative effect of cash transfers during enrollment in the program, and drives the positive effect after completion.

This work speaks chiefly to the literature on employment policies. Prior research has mostly evaluated active policies, such as activation measures, conditional on a given level of passive policies, such as cash transfers, and vice versa. This paper provides the first evidence of the joint effect of cash transfers and activation measures. The results suggest a large positive joint effect of active and passive policies after completion of the program. The effect is determined by activation compensating for a negative effect of cash transfers. The large magnitude of the effect of activation signals a significant role of search frictions for this population, with control youths facing very low matching probabilities. Secondly, the results provide empirical insights for the literature on labor supply and job search behavior. I estimate a 52% reduction in employment as a reaction to a 55% increase in implicit taxation from benefits phase-out, implying an elasticity to net-of-tax rate between .4 and .8 for this very specific population. I also confirm the role of time and activation in determining job search efficacy, as in Gautier et al. (2018). As a final methodological contribution, my rolling diff-in-diff methodology is relevant for studies where units enter the population of interest in group-cohort cells, and are exposed to treatment at different tenures. When a treatment is adopted by these groups in a staggered fashion, so that units are exposed to treatment at different tenures, the diff-in-diff methodology proposed is flexible for estimating dynamic ITT and LATE, is robust to selection into treatment over tenure, as well as to heterogeneous treatment effects. Although tailored for our setting, this setting is not uncommon in applied work. For example, we can imagine a similar setting for a school restructuring program, where cohorts are age cohorts, tenure is their school grade, and the program is staggeredly adopted by schools (Martorell et al., 2016).

I suggest three main avenues for future research. First, this paper is not able to disentangle the exact magnitude and nature of complementarities between cash transfers and activation measures. Future studies should look for shocks and direct measures of the possible sources of complementarities, namely monitoring, motivation, and job search technology components of activation measures. The question is extremely relevant for understanding the extent to which social policy should worry about moral hazard vs. abating obstacles to employment and break poverty traps. Second, externalities represent a challenge for policy evaluators, whether positive or negative. In the period of this evaluation, *Garantie Jeunes* concerned a very small population, but displacement effects on other disadvantaged job seekers will become more likely when the program is extended (Crépon et al., 2013). Alternatively, given the extremely disadvantaged population targeted by *Garantie Jeunes*, positive externalities might arise from a reduction in the crime rates of participants (Britto et al., 2020). Qualitative research by Loison-Leruste et al. (2016) reports abundant anecdotal evidence of youths in *Garantie Jeunes* grown up in high-delinquency environments.

Nonetheless, this work already offers relevant policy implications. The simplest one is that a combination of active and passive policies seems indeed desirable to improve employability of disadvantaged NEETs, as argued by comparative analysis such as OECD (2020); Pignatti and Van Belle (2018). The mix of services and cash transfers provided by *Garantie Jeunes* is effective, in line with pilot evidence by Gaini et al. (2018) and qualitative results by Gautié (2018). Second, my estimates show that youths reduce employment due to welfare benefits, so that their elasticity of labor supply is large. A possible solution would be allowing youths to fully cumulate benefits and job earning, but this could clearly be costly. Activation is shown to be a viable alternative, as its effect is estimated strong enough to compensate for lock-in and distortive effects of the cash transfers. Finally, my insights can be used to study other policies that combine cash transfers and activation policies, like many minimum income schemes or unemployment insurance with activation requirements. However, external validity should be handled with care. *Garantie Jeunes* concerned only a very selected population, and the costs of the program are only 19 points lower than total benefits after 2 years. In the ongoing extension of the program, it might not be easy to maintain cost-effectiveness, as marginal returns from the program can be decreasing the broader the target population.

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Appendices

A Why I Need a Rolling Diff-in-Diff?

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[20]. The building block for this kind of diff-in-diffs is basically a cell-specific estimator of the effect for youths in treatment wave w at time t, which in the notation of my setting would be

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

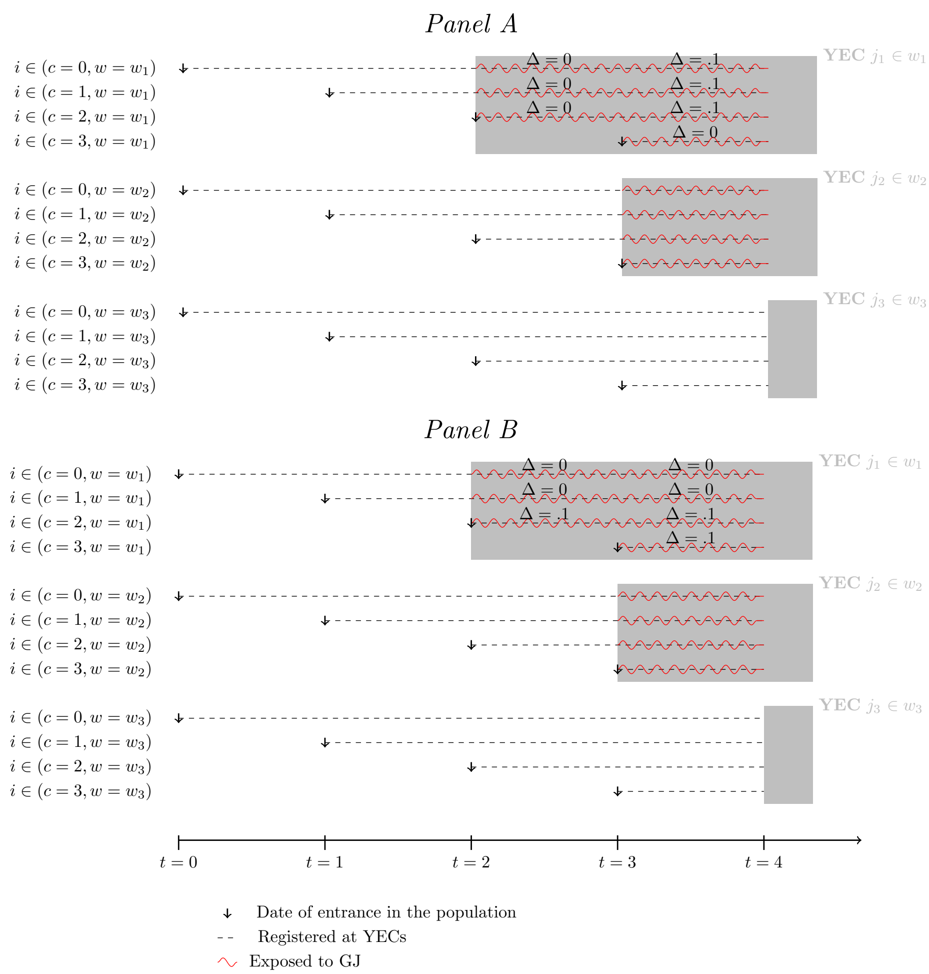
Where $Y_{w,t}$ is the empirical average of the outcome of interest in cell w,t, t' is the period before w gets treated, $n_{w',t}$ is the number of units in cell w',t and $N_{\Omega_{w,t}}$ is the number of youths in all cells w,t such that treatment at t is still zero. If the program has been adopted at time T_w, units in cell w,t are $t - T_w$ periods away from adoption of the program, so that $DID_{w,t}$ identifies the treatment effect *since adoption*. In the context of *Garantie Jeunes*, such estimator would yield e.g. how much average employment improved in treated YECs after adoption of the program.

The need to roll over time since registration with YECs, and to estimate the effect since cohort *exposure* or individual *enrollment* arises first of all if there are dynamic effects of the program. Define G_i as the number of quarters a youth has been *exposed* to the program, meaning that he was registered at YEC who was offering *Garantie Jeunes*, and as D_i the time since actual enrollment in the program. For simplicity, assume by now that all youths who can enroll in *Garantie Jeunes* do so as soon as it is offered in their YEC (full and immediate take-up), so that $G_i = D_i$ and the dynamic over *exposure* of a cohort coincides with the actual dynamic effect over *enrollment*. Panel A of Figure 7 exemplifies this case. Now, suppose the program has dynamic effects, for example if $\Delta = 0$ when $G_{w,c}^h = 1$ and $\Delta = .1$ when $G_{w,c}^h = 2$, i.e. program effect is increasing with exposure/enrollment, and the average effect after two periods of *exposure*, when $G_{w,c}^h = 2$, is 0.1. Because some cohorts of youths register with YECs after that YEC has already adopted the program, $DID_{w,t}^{DCDH}$ will be an average of cohorts with different exposure $G_{w,c}^h$. For example, the average effect after 2 periods of *adoption* is estimated as $DID_{w_1,t=4}^{DCDH} = 0.075$, which is not informative about the relevant dynamic of treatment.

A second problematic case arises if time since registration is a source of selection into treatment, hence of potential heterogeneity. In my setting, this cannot be excluded *a priori*. 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 long time before will be able to take-up the program. These two groups might not be comparable, since the latter will be composed only of those youths who have not found a job or a formal training during the time they have been in contact with the YEC. Hence, treatment effect might be heterogeneous across these groups. For instance, in Panel B of Figure 7 the true treatment effect is $\Delta = 0$ if $G_{w,c}^h > 0$, $h > G$, i.e. there is no treatment effect for youths who registered before treatment introduction and are exposed later. Instead, $\Delta = .1$ if $G_{w,c}^h > 0$, $h = G$, i.e. there is a positive 0.1 treatment effect for youths who registered at the moment of introduction of the program or

[20] A similar estimator is suggested by Callaway and Sant'Anna (2018)

Figure 7: When the effect since adoption is different than the average effect since exposure.



later. The average effect when $G^h_{w,c} = 2$ is 0.03, but the effect two periods since adoption $DID^{dCDC}_{w_1,t=2} = 0.05$. My methodology solves this problem by comparing youths at the same time after registration with YECs, as explained in Section 3.

B Assumptions, Propositions and Proof of Identification of ITT and LATEs

B.1 ITT

Denote $Y^h_{w,c} := \mathbb{E}(Y^h_{i,j,c}|j \in w, c, h)$, the conditional expectation for all youths in YECs j belonging to treatment wave w, in cohort c, and observed h quarters after registration. Let $G^h_{w,c}$ denote the number of periods that youths are exposed to *Garantie Jeunes* in that cell. Note that each h, w, c cell is associated to only one value of exoposure, $h, w, c \to G^h_{w,c} = g$.

Consider a set of assumptions typical of diff-in-diff settings.

Assumptions 1-4.

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

The first two assumptions exclude that mean potential outcomes depend from treatment in other waves, in the next cohort or in the next period, but only depends from current cumulated treatment status, conditional on h, w, c. This allows us to write $Y^h_{w,c}(g)$ to represent the expected outcome in cell h, w, c when being treated for g quarters.

Analogously to 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. Denote $Y^h_{w,c} := \mathbb{E}(Y^h_{i,j,c}|j \in w, c, h)$, the conditional expectation for all youths in YECs j belonging to treatment wave w, in cohort c, and observed h quarters after registration. For each h, w, c such that $G^h_{w,c} = g > 0$, define the cell-specific ITT estimand:

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

Consider:

$$DID^h_{w,c} := Y^h_{w,c} - Y^h_{w,c'} - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_w,c}} (Y^h_{w',c} - Y^h_{w',c'}) \qquad \forall\, given\, (w, c, h) : G^h_{w,c} = g > 0 \tag{6}$$

Where $G^h_{w,c'} = 0$ but $G^h_{w,c'+1} = 1$, and Ω_w 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 $w' \in \Omega_w$.

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

Proof.

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

The first equality applies the definition in (6), the second follows from no anticipation and independent groups, the third is obtained by adding and subtracting $Y^h_{w,c}(0)$ plus strong exogeneity to get rid of conditional expectation, 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.

I am then then interested in meaningfully aggregate cell-specific ITT estimator $DID^h_{w,c}$ into an unbiased estimators of $\Delta^{ITT}(g)$, the average effect of being exposed for g quarters to the program. Consider:

$$
DID^g := \sum_{(w,c|h):G^h_{w,c}=g} \frac{n_{w,c}}{\sum_{(w,c|h):G^h_{w,c}=g} n_{w,c}} DID^h_{w,c} \tag{7}
$$

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 $\Delta^{ITT}(g)$.*

Proof.

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

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.

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.

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

$$Y^h_{w,c} - Y^h_{w,c'} - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_w},c}[Y^h_{w',c} - Y^h_{w',c'}] = 0 \qquad \forall\, given\,(w,c,h) : G^h_{w,c} = 0 \tag{8}$$

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

B.2 LATE

As explained in the institutional context, once youth are exposed to *Garantie Jeunes* it's not guaranteed that they actually enroll in the program. In fact, only some youths are eligible, and only some of the eligibles eventually applies and gets selected for the program. Once they start being exposed, youths from a specific cohort can apply and be selected to enroll in *Garantie Jeunes* immediately, later, or never enroll.

We then study what can be said about the effect of the program since enrollment, which requires studying potential outcomes at the individual level. Let potential outcomes for youths i, registering in cohort c to YEC j, h quarters after registration, be $Y^h_{i,j,c}(\mathbf{D_{i,j}})$, where $\mathbf{D_{i,j}} = \{d^p_{i,j,c}\}^\infty_{p=1}$ is a vector of dummies representing treatment status of individual i in YEC j and cohort c, from his registration with YECs onward. Define as $D^h_{i,j,c} = \sum^h_1 d^p_{i,j,c}$ the cumulated treatment of individuals in a cell. Note that potential outcome in YEC j and cohort c is independent from enrollment status in other cohorts and YECs following assumptions 1 and 2.

A first parameter of interests is then the LATE on compliers:

$$\Delta^{LATE}(g) \quad = \quad \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0) | D^h_{i,j,c} > 0)$$

Consider

Assumption 5. *(No spillovers on non-compliers)* $\mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0) \Big| D^h_{i,j,c} = 0) = 0$

Then

Proposition 3. *Consider a set of* $DID^h_{w,c}$*, unbiased estimators of* $\Delta^{ITT}(h,w,c)$*, the cell-specific ITT treatment effect. Under assumptions 1-5, if* $Pr(D^h_{i,j,c} > 0|h,w,c) = 0$ *whenever* $G^h_{w,c}$*=0 (no defiers and no always-takers), then* $\sum_{(w,c|h):G^h_{w,c}=g} \frac{n_{w,c}}{\sum_{(w,c|h):G^h_{w,c}=g} n_{w,c}} [DID^h_{w,c}/Pr(D^h_{i,j,c} > 0|h,w,c)]$ *is an unbiased estimator of* $\Delta^{LATE}(g)$

Proof. It follows straightforwardly from the definition of expectations that

$$\begin{aligned}
\mathbb{E}(DID^h_{w,c}) =& \mathbb{E}(Y^h_{w,c}(g) - Y^h_{w,c}(0)) \\
=& \mathbb{E}\left[\mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0) \Big| D^h_{i,j,c} > 0, h, w, c) \cdot Pr(D^h_{i,j,c} > 0 | h, w, c) \right. \\
& \left. + \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0) \Big| D^h_{i,j,c} = 0, h, w, c) \cdot Pr(D^h_{i,j,c} = 0 | h, w, c) \right] \\
\mathbb{E}(DID^h_{w,c}/Pr(D^h_{i,j,c} > 0|h,w,c)) =& \mathbb{E}\left[\mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0) \Big| D^h_{i,j,c} > 0) | h, w, c \right] \\
=& \Delta^{LATE}(g)
\end{aligned}$$

Where the third passage holds since the second term is zero due to the assumption of no-spillovers on non-compliers, and the final equality is based on the law of iterated expectations. Yet, we might be interested in obtaining an estimate of LATE for some specific values of $D^h_{i,j,c}$, not only for $D^h_{i,j,c} > 0$.

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

Consider:

Assumption 6. *(Exogeneity of enrollment).* Potential outcomes when actually treated, conditional on h, j, c, depend only on cumulated past treatment take-up, so that $Y^h_{i,j,c}(\mathbf{D_{i,j}}) = Y^h_{i,j,c}(D^h_{i,j,c})$, and the expected effect of being enrolled since d quarters in the program is homogeneous across cohorts, waves, and time since registration: $\mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)|D^h_{i,j,c} = d, h, w, c) = \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)|D^h_{i,j,c} = d)$

Assumption 6 is strong, given that it imposes for example an equal treatment effect on youth who take up the program early in exposure and youth who take up the program later (as $h, w, c \to g$), but allows us to express the cell-specific ITTs in terms of unknown LATE effect of being treated for d quarters, $\delta(d)$, and the known share of youths at different stages since enrollment in the program, i.e. $DID^h_{w,c} = \sum_{d=1}^{g} \delta(d) Pr(D^h_{i,j,c} = d|h, w, c)$.

Proposition 4. *Under assumptions 1-6, $\delta(0 < d \leq 2), \delta(2 < d \leq 4), \delta(d > 4)$ in regression 2 are estimators of LATEs in the first semester of program enrollment, in the second semester, and after completion.*

Proof.

$$\begin{aligned}
\mathbb{E}(DID^h_{w,c}) =& \mathbb{E}(Y^h_{w,c}(g) - Y^h_{w,c}(0)) \\
=& \mathbb{E}\Big[\mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|0 < D^h_{i,j,c} \leq 2, h, w, c) \cdot Pr(0 < D^h_{i,j,c} \leq 2|h, w, c) \\
&+ \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|2 < D^h_{i,j,c} \leq 4, h, w, c) \cdot Pr(2 < D^h_{i,j,c} \leq 4|h, w, c) \\
&+ \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} > 4, h, w, c) \cdot Pr(D^h_{i,j,c} > 4|h, w, c) \\
&+ \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} = 0, h, w, c) \cdot Pr(D^h_{i,j,c} = 0|h, w, c)\Big] \\
=& \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|0 < D^h_{i,j,c} \leq 2) \cdot Pr(0 < D^h_{i,j,c} \leq 2|h, w, c) \\
&+ \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|2 < D^h_{i,j,c} \leq 4) \cdot Pr(2 < D^h_{i,j,c} \leq 4|h, w, c) \\
&+ \mathbb{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} > 4) \cdot Pr(D^h_{i,j,c} > 4|h, w, c)
\end{aligned}$$

The first equality uses the definition of expectations and Assumption 5, while the last equality is obtained using Assumption 6 and the Law of Iterated Exectations. Which can be estimated using Equally-Weighted Minimum Distance (Altonji and Segal, 1996; Card and Lemieux, 2001).

C Cost-benefit Analysis

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

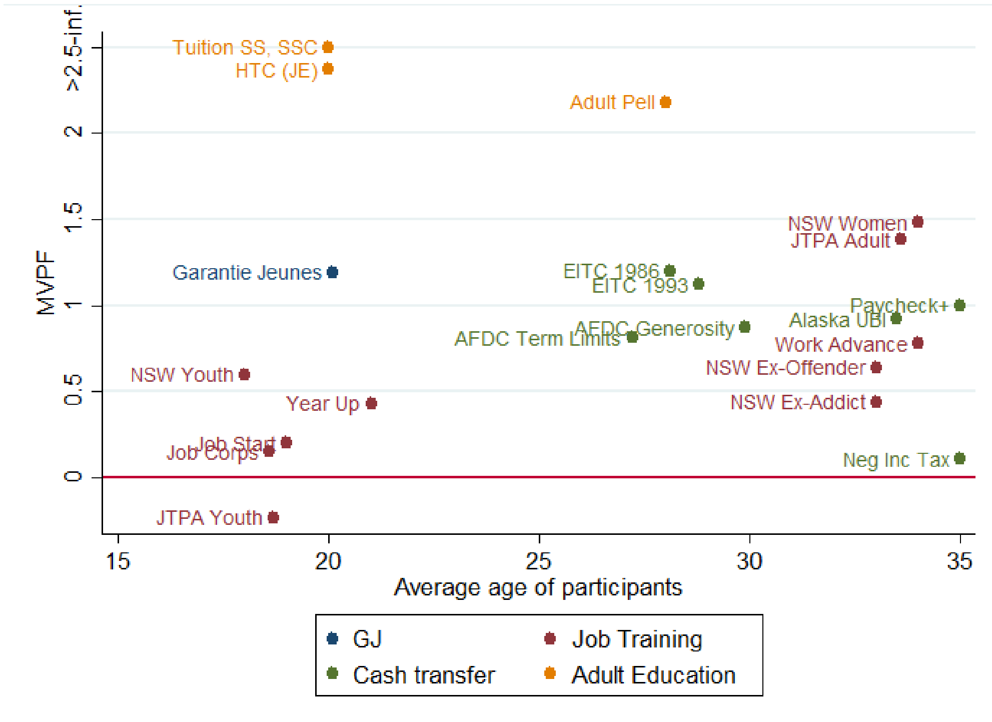
$$MVPF = \frac{WTP}{NetCost}$$

Where WTP represents the aggregate willingness to pay for the program. By analogy with the work done by the same authors for estimating the MVPF for programs similar to *Garantie Jeunes*, such as the Job Corps program, I estimate WTP as the present value of the impact of the policy on after-tax income. This is given by the significant LATE effect on gross labor earnings the second year after enrollment in *Garantie Jeunes*, €828 quarterly in the year after completion, discounted by 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., 2013), and our heterogeneity analysis highlights the precarious nature of employment contracts obtained thanks to *Garantie Jeunes*. Concerning the costs associated with *Garantie Jeunes*, one should sum the direct cost of implementing the program for each youth and the opportunity cost of using YECs pre-existing infrastructure (classrooms, fixed admin personnel). I estimate the latter by calculating the average per-youth cost before *Garantie Jeunes* (Arambourou et al., 2016). The per-youth direct cost is instead covered by additional funding allocated to each YEC, consisting in €1120 per youth enrolling in the program, plus €320 after the youth completes the program or secures employment or formal training and €160 for data reporting, hence a total of €1600 per youth. Given that only 17% of participants quit the program before the end for reasons not related to having found an employment or formal training (Gautié, 2018), I can estimate the net cost at €1964. The cumulated cash transfer received while in the program, calculated from the data at €4039 on average, is a simple transfer so it is added both to WTP and to net costs.

Under these assumptions[21], the MVPF of *Garantie Jeunes* is estimated at 1.19. 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* appears to be larger than the one for programs targeting youth, but quite in line with similar programs targeting the whole working-age population in financial difficulties (such as JTPA Adult). The MVPF of *Garantie Jeunes* appears also very similar to the one for cash transfer programs, although these programs usually target the whole working-age population and thus report an higher average age. Further comparison with other kind of programs such as adult education, which are less comparable to *Garantie Jeunes*, shows that *Garantie Jeunes* underperforms relative to the MVPF of policies supporting college attendance, which tend to have MVPF between 2 and infinity.

[21]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. (2013) 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.

D Estimation of structural parameters

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

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

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

$$
\begin{cases}
y_{1,1}(1) & = \hat{\alpha_1} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta + p(1,0) \\
y_{2,1}(1) & = \hat{\alpha_2} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta - (\alpha_2 - 300a_1)\tau + p(1,0) \\
y_{3,1}(1) & = \hat{\alpha_3} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + p(1,0) \\
y_{1,2}(1) & = \hat{\alpha_1} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta + p(1,1) \\
y_{2,2}(1) & = \hat{\alpha_2} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta - (\alpha_2 - 300a_1)\tau + p(1,1) \\
y_{3,2}(1) & = \hat{\alpha_3} - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{1,3}(1) & = \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{2,3}(1) & = \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{3,3}(1) & = \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{1,1}(0) & = \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{2,1}(0) & = \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{3,1}(0) & = \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{1,2}(0) & = \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{2,2}(0) & = \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{3,2}(0) & = \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{1,3}(0) & = \hat{\alpha_1} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{2,3}(0) & = \hat{\alpha_2} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{3,3}(0) & = \hat{\alpha_3} - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1)
\end{cases}
\tag{9}
$$

Note that it is not possible to recover all parameters separately. For example, suppose the left-hand side of the system is a noisy estimate, there are actually only 8 different equations on the right-hand side and 9 unknowns, as showed below.

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

Let us instead try to recover $p(1,1) - p(0,1)$, $p(1,1) - p(1,0)$,$(\alpha_2 - 300a_1)\tau$, β. Note that there are multiple configurations of the system, including different combinations of different lines, that one can use to recover each parameter. These alternative configurations deliver different estimates of the parameters. To avoid cherry picking, I will estimate each parameter as an average of all possible ways to recover it. This means:

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

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

$$
\begin{cases}
y_{1,1}(1) - y_{2,1}(1) - \widehat{\hat{\alpha_1} - \hat{\alpha_2}} & = (\alpha_2 - 300a_1)\tau \\
y_{1,2}(1) - y_{2,2}(1) - \widehat{\hat{\alpha_1} - \hat{\alpha_2}} & = (\alpha_2 - 300a_1)\tau
\end{cases}
\Rightarrow (\alpha_2 \widehat{- 300}a_1)\tau = \frac{y_{1,1}(1) - y_{2,1}(1) - \widehat{\hat{\alpha_1} - \hat{\alpha_3}} + y_{1,2}(1) - y_{2,2}(1) - \widehat{\hat{\alpha_1} - \hat{\alpha_2}}}{2}
$$

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

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

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

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

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

Alternatively, one can normalize one parameter and directly estimate the following regression:

$$y_{j,\bar{d}}(treated) = F(\hat{\alpha}, \beta, (\alpha_2 - 300a_1)\tau, p(0,1), p(1,1), p(1,0)) \tag{10}$$

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

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

Parameter		$Pr(z^{j*} = z_j)$		Effect (interpretation)	
$\hat{\alpha_0}$	norm. to 0	$\Phi_1(0)$	.102	$e^{-(\alpha_2 - 300)\tau}$ (implicit tax)	.523
$\hat{\alpha_1}$	-1.670	$\Phi_2(0)$	.134		
$\hat{\alpha_2}$	-1.399	$\Phi_3(0)$	.215	e^{β} (moral h./liquidity)	1.100
$\hat{\alpha_3}$	-.928	$\Phi_1(1)$	.112		
β	.095	$\Phi_2(1)$	.076	$\frac{K_0}{K_1}$ (cash tr. spillovers)	.992
$-(\alpha_2 - 300a_1)\tau$	.648	$\Phi_3(1)$	.214		
$P(1,0)$	.565			$\frac{P(1,0)}{P(1,1)}$ (lock-in)	.565
$P(1,1)$	1				
$P(0,1)$	.470			$\frac{P(1,1)}{P(0,1)}$ (activation)	2.125

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

E Model's Predicted Outcomes in the Case of Aeberhardt et al. (2020)

In this section I use the model estimated in Section 5.2 to obtain the predicted impact in the case of Aeberhardt et al. (2020) and comparing the obtained prediction with the actual effect they estimate. In the setting of Aeberhardt et al. (2020), the cash transfer is smaller, $b' \simeq 250$, and the phase-out of the cash transfer starts from the first euro earned, with $\tau' = 24\%$. Hence, the predicted probabilities of employment in different income brackets in treatment and control are reported in Table 9.

Table 9: Predicted probabilities of employment in the case of Aeberhardt et al. (2020)

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

	Monthly income €1-€1100	Monthly income over €1100
1st year of enrollment	$(\Phi_1(0) + \Phi_2(0))\frac{K_0}{K_1'}e^{a_1(b'-(z_1+z_2)\tau')} \cdot P(0,1)$	$\Phi_3(0)\frac{K_0}{K_1'} \cdot P(0,1)$

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

	Monthly income €1-€1100	Monthly income over €1100
1st year of enrollment	$(\Phi_1(0) + \Phi_2(0)) \cdot P(0,1)$	$\Phi_3(0) \cdot P(0,1)$

Where $K_1' = e^{\hat{\alpha_0}+a_1b'} + e^{\hat{\alpha_1}+a_1(b'-z_1\tau')} + e^{\hat{\alpha_2}+a_1(b'-z_2\tau')} + e^{\hat{\alpha_3}}$, and the other parameters are identical to Section 5.3. Then, I can use estimates of $e^{-(\alpha_2-300a_1)\tau} = e^{-(a_1z_2-300a_1)\tau}$ and $e^{\beta} = e^{a_1b}$ from Table 7 (reported also in the third column of Table 8)[22], knowing that $b \simeq 480$ and $\tau = 55\%$ in the case of Garantie Jeunes, and recover a_1 and z_2. Finally, I can calculate the predicted % increase in employment in Aeberhardt et al. (2020) for different values of z_1:

$$\Delta^{Aeberh.} \log \mathbb{E}(Y_i) = \frac{(\Phi_1(0) + \Phi_2(0))\frac{K_0}{K_1'}e^{a_1(b'-(z_1+z_2)\tau')} + \Phi_3(0))\frac{K_0}{K_1}}{\Phi_1(0) + \Phi_2(0) + \Phi_3(0)} \simeq .93 \quad \forall z_1$$

Which is very close to 7-13% negative effect found in Aeberhardt et al. (2020).

[22] I use only estimated obtained through wNLS as I will also need estimates of $\hat{\alpha_j}$

F Additional Tables and Figures

Table 10: Characteristics of youth at time of registration at YEC.

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

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

Table 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.

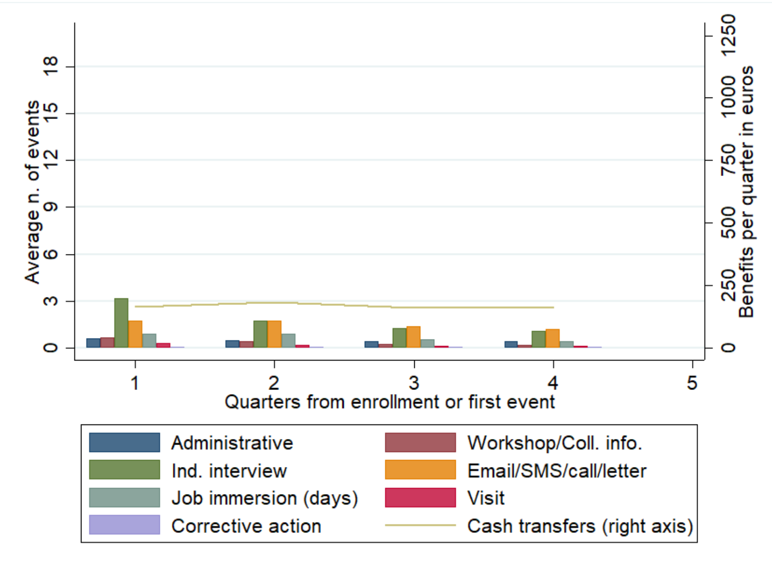
yq	w13q4	w14q1	w14q2	w14q4	w15q1	w15q2	w15q3	w15q4	w16q1	w16q2	w16q3	w16q4	w17q1	w17q2	w17q3
2013q1	8118	5121	485	378	14276	18945	27608	1721	3571	6928	8848	4383	13587	5436	846
2013q2	7460	4459	389	743	12725	16766	24025	1493	3191	6001	8175	3848	12192	4531	622
2013q3	11558	7066	453	394	18498	23951	32568	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: Heterogeneity by employment contract.

	Open-ended (1)	Temporary (2)	Agency jobs (3)	Apprenticeships (3)
ITT effect 1st semester of exposure	-0.000303	0.000686	0.00107	0.00125
	(0.00133)	(0.00165)	(0.00144)	(0.00137)
Total n.obs	3194961	3194961	3194961	3194961
ITT effect 2nd semester of exposure	0.000169	-0.00039	0.00174	0.000578
	(0.00236)	(0.00174)	(0.00154)	(0.00125)
Total n.obs	2379924	2379924	2379924	2379924
ITT effect 2nd year of exposure	0.000869	0.00503	0.00374	0.00118
	(0.00298)	(0.00358)	(0.00235)	(0.00174)
Total n.obs	2665714	2665714	2665714	2665714
Mean for control 1st semester of registration in YEC	0.084	0.155	0.078	0.031
Mean for control 2nd semester of registration in YEC	0.109	0.184	0.081	0.034
Mean for control 2nd year of registration in YEC	0.138	0.191	0.086	0.037
LATE 1st semester of exposure	-0.0197	0.0444	0.0691*	0.0808**
	(0.0351)	(0.0438)	(0.0390)	(0.0366)
LATE 2nd semester of exposure	0.00454	-0.0104	0.0467***	0.0155
	(0.0257)	(0.0190)	(0.0164)	(0.0137)
LATE 2nd year of exposure	0.0159	0.0927***	0.0689***	0.0218*
	(0.0219)	(0.0259)	(0.0169)	(0.0131)
LATE 1st semester after enrollm.	0.0178	-0.00661	-0.00730	-0.00607
	(0.0193)	(0.0195)	(0.0137)	(0.0109)
LATE 2nd semester after enrollm.	0.0262	-0.00221	0.0833**	-0.0153
	(0.0828)	(0.0663)	(0.0421)	(0.0630)
LATE 2nd year after enrollm.	0.0159	0.0927***	0.0689***	0.0218*
	(0.0219)	(0.0259)	(0.0169)	(0.0131)

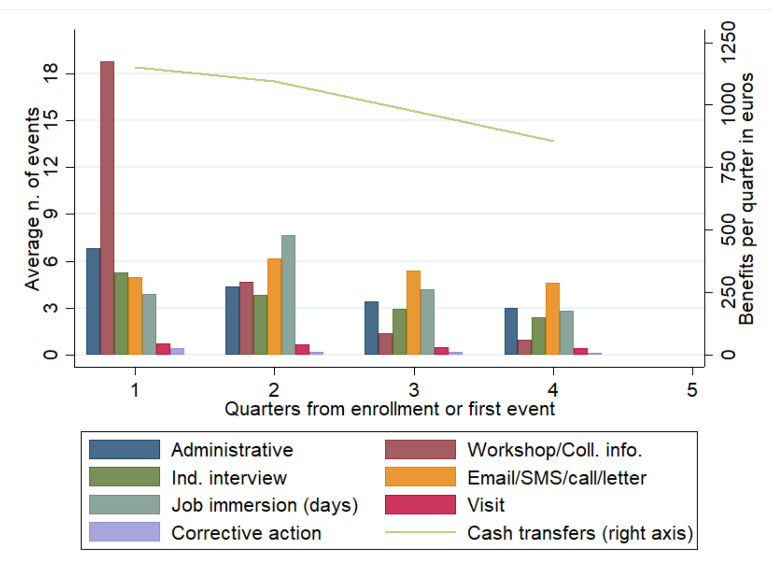
Notes. The table reports the main results obtained following the rolling diff-in-diff methodology developed in Section 3. The upper panel reports weighted averages of the $DID^h_{w,c}$ coefficients where exposure is between 1 and 4 quarters or above 4 quarters. The lower panel reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and wages (earnings per hour) obtained according to Equation 2. Standard errors are bootstrapped and reported in parenthesis.

Figure 10: Average number of events, by kind of event, and average benefits for participants in standard program available at YECs, *CIVIS*.



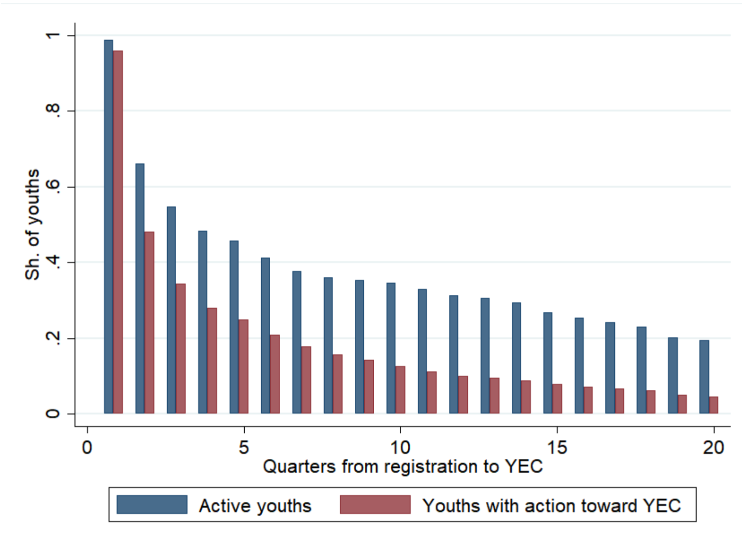
Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from enrollment in *CIVIS*. The cash transfers series plots instead the average amount of benefit to youths participating in *CIVIS*,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

Figure 9: Average number of events, by kind of event, and average benefits for participants in *Garantie Jeunes*.



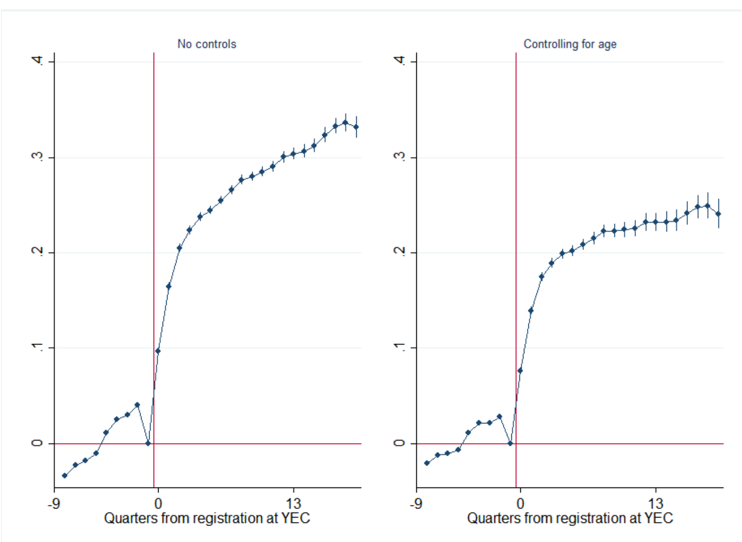
Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from enrollment in *Garantie Jeunes*. The cash transfers series plots instead the average amount of benefit to youths participating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

Figure 11: Share of youth considered active at the YEC and youths who actually undertake action toward a YEC from time of registration.



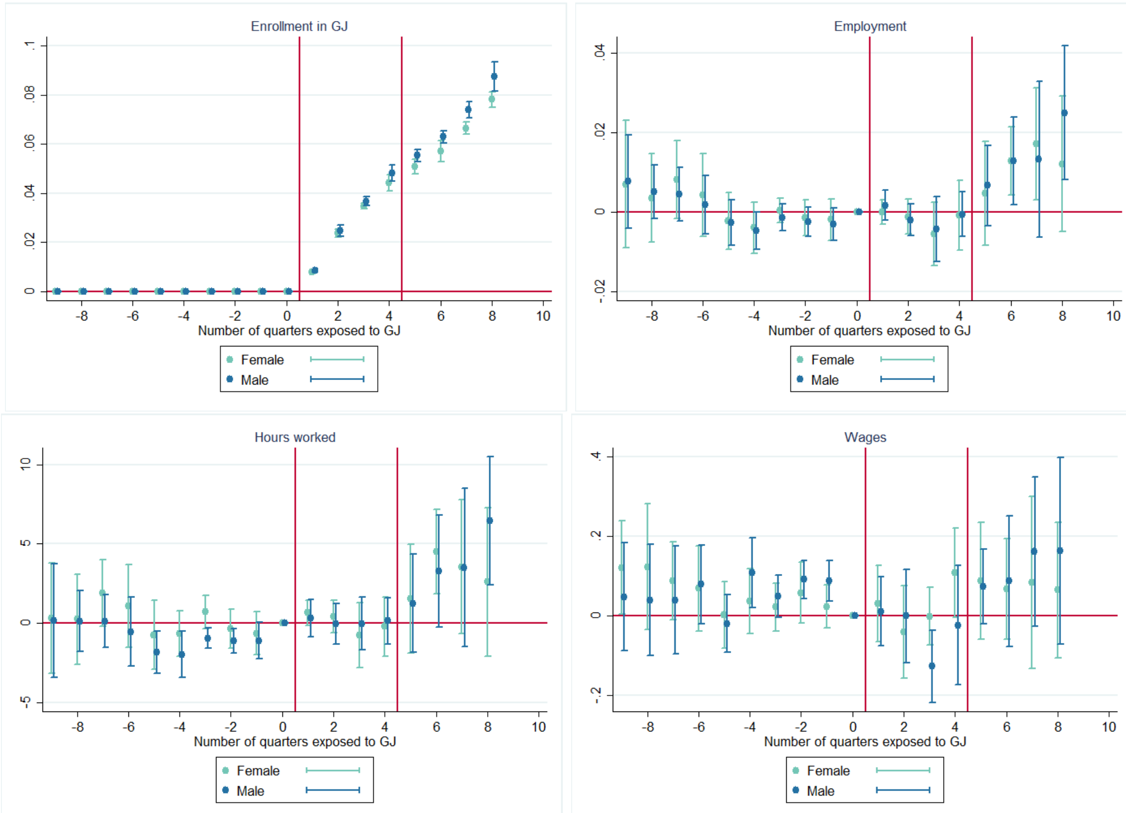
Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from registration at the YEC. "Active youths" are considered those whose file records any kind of action in the quarter. The red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

Figure 12: Average employment rates in the quarters precedent/following registration at YEC, controlling or not for age.



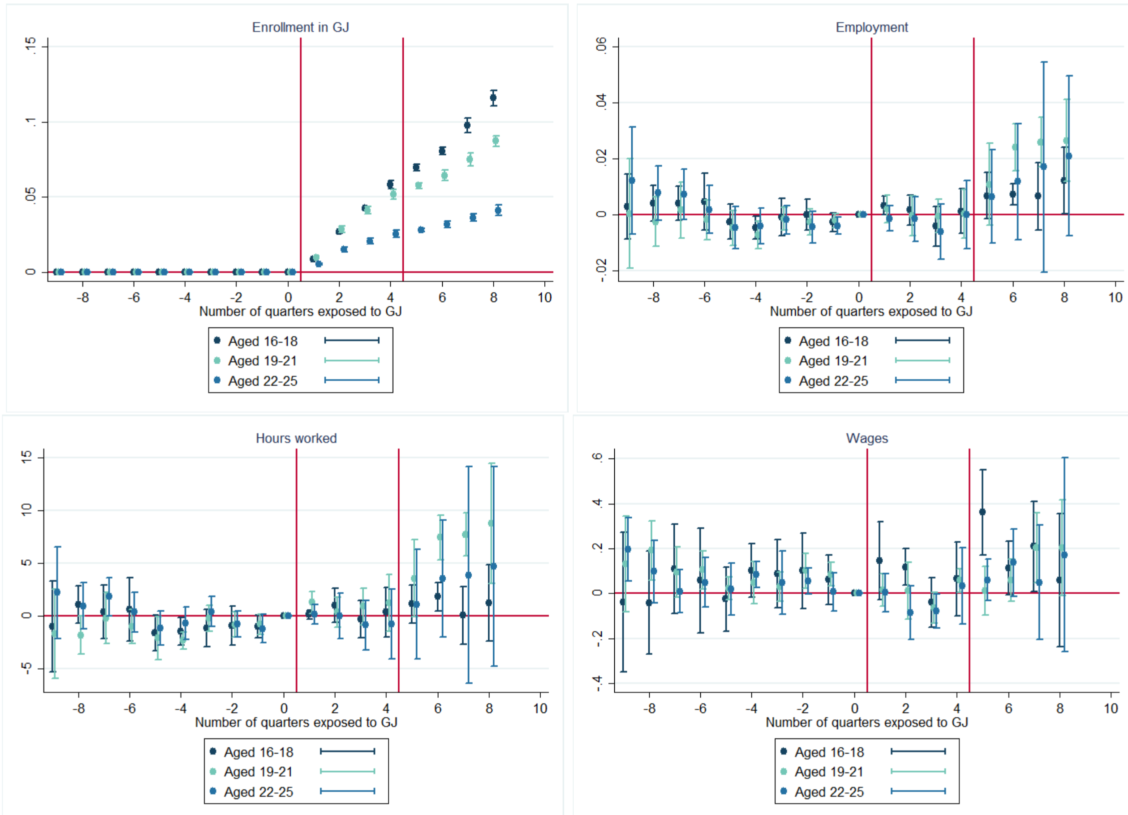
Notes. The figure plots coefficients of a regression of an employment dummy on quarters from registration, cohort and YEC fixed-effects (left panel), adding age fixed effects (right panel).

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



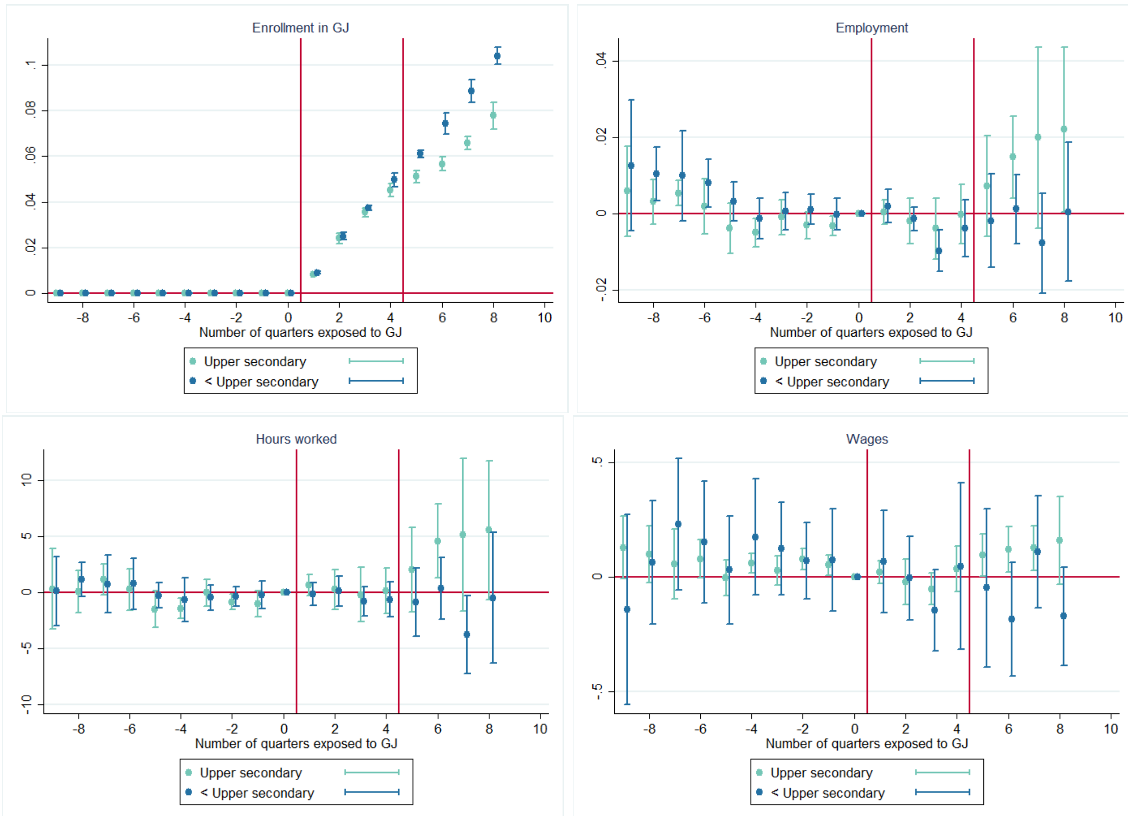
Notes. The figure reports results of the rolling diff-in-diff approach for different gender sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 7. Cell-specific effects were obtained as in Equation 6. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.

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



Notes. The figure reports results of the rolling diff-in-diff approach for different age sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 7. Cell-specific effects were obtained as in Equation 6. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.

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



Notes. The figure reports results of the rolling diff-in-diff approach for different sub-samples defined by higher education degree attained. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation 7. Cell-specific effects were obtained as in Equation 6. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.