WHAT DO NEETS NEED?
THE OVERALL EFFECT OF ACTIVE AND PASSIVE LABOR MARKET POLICIES

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Abstract

The overall effect of active and passive labor market policies is pivotal to motivate programs combining the two components. This paper evaluates a flagship French program for disadvantaged youth Not in Employment Education or Training (NEETs) that combines a year of cash transfers and activation policies. The results show a positive total effect of the program on employment (+21 percentage points, +64% relative to control in LATE terms) emerging after program termination. The analysis of mechanisms suggests a negative effect of the cash transfer component on employment and lock-in from training, compensated by a positive effect of activation policies.

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

JEL Codes: J64, J68, C23

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1 Introduction

Youth who are neither in employment, education, nor training (NEETs) have been a significant concern in advanced economies over the past decades.[1] Governments often support NEETs through social protection measures, such as cash transfers, but economists have long argued that these passive labor market policies may reduce incentives to work, potentially fostering welfare dependence (Moffitt, 1985). In contrast, active labor market policies are government programs designed to help the unemployed find work by improving employability and supporting job placement, such as through training or job search assistance. Active policies are viewed as a remedy to the negative effects of passive policies on job search, leading to a growing number of programs that combine both active and passive components (OECD, 2020; Pignatti and Van Belle, 2018). However, to what extent do the two components compensate for each other when combined, and what is the overall effect of active and passive policies?

The existing literature has rarely examined the overall effect of active and passive labor market policies in the same context. Evaluations of passive policies often suggest they lead to welfare dependence and negative employment outcomes (Card and Hyslop, 2005; Card et al., 2007). A few studies have explored the impact of adding passive policies to active ones (Aeberhardt et al.,

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[1]NEET rates for youths aged 15-24 averaged 12% in France, 20% in Italy, and 15% in Spain between 2008 and 2021 (Eurostat). For the US, the OECD estimates NEET rates at 8.8% for 15-19-year-olds and 18.3% for 20-24-year-olds in 2021. Higher rates are reported among women, less-educated individuals, and foreign-born persons. Another concern is that NEET spells can become a poverty trap with scarring" effects on youth employability (Oreopoulos et al., 2012).

2020; Schmieder and Trenkle, 2020), but in these cases, the active component remains fixed, focusing the analysis on the impact of the passive component, conditional on a given level of active policies. Other studies have concentrated on active labor market policies, generally finding a positive effect on employment (Card et al., 2010). Among these studies, some focus on populations already receiving passive support (e.g., when active policies target unemployment benefit recipients), but since there is no variation in the passive component, they estimate only the effect of adding active policies, keeping passive ones constant. Estimating the overall effect active and passive policies is instead key clarify to which extent active labor market policies can mitigate the potentially negative effects of passive policies. This requires comparing outcomes between individuals receiving at the same time a strong active and passive component vs. individuals not receiving neither of the two.

This paper evaluates the flagship program of the French government for disadvantaged NEETs between 16 and 25 years old, *Garantie Jeunes*. The program combines a year of generous cash transfers with intensive activation policies, namely soft-skills training, high-frequency counseling and short in-company work experiences. To evaluate the program, I construct a novel dataset containing information on 2 million youth from the information system of French Youth Employment Centers (YECs) and from social security records. The identification strategy exploits the program's staggered adoption between 2013 and 2017, where new areas of the French territory introduced the program each quarter. I estimate the effects of the program using both a fixed effects estimator and a difference-in-differences approach robust to heterogeneous effects across treatment groups.[2]

[2]The difference-in-differences estimator adapts the method of De Chaisemartin and D'Haultfœuille (2020a) to a setting where youths enter the population of interest – in this case, young NEETs in Youth Employment Centers – by cohorts, before being staggeredly exposed to treatment. This setting requires to estimate group-specific difference-in differences estimates over horizons since the cohort entered the population. This offers a benchmark for applying difference and differences estimators robust to heterogeneous treatment effects in cohort settings commonly found in applied microeconomics (for example, Martorell et al., 2016).

The results show that the overall effect of active and passive labor market policies on youth employment is strong and positive, but only after the program has concluded. Specifically, the intention-to-treat (ITT) effects on employment, hours worked and earnings, are insignificant in the first year but turn positive from the second year of exposure to *Garantie Jeunes*. Moreover, I show that the dynamic in the ITT estimates stems from a zero Local Average Treatment Effect (LATE) associated with youths still enrolled in the program, and a positive effect associated to youths who have completed the program, estimated at +21 percentage points in employment (+64% relative to the control group).

Subsequently, I explore the possible mechanisms underlying the overall effect of *Garantie Jeunes*. To study potential lock-in effects from time-consuming activation policies, I exploit the concentration of training activities in the first semester of enrollment in *Garantie Jeunes*. To assess the role of the cash transfer component in potentially discouraging job search, I leverage the fact that transfers can be fully combined with job earnings only up to €300 in monthly earnings. I find that the absence of an overall effect in the first semester stems from a roughly constant probability of earning more than €300, alongside a slight decline in the probability of earning below that. During the second phase of enrollment, when training has ended but participants continue receiving cash transfers, I observe an increase in the probability of earning above the minimum wage and below €300. However, I find a sharp reduction in the likelihood of earning between €300 and the minimum wage, indicating a strong labor supply response to variations in the cash transfer. Once the transfer ends, participants' employment rises significantly in all brackets. I interpret this pattern as evidence that the overall effect of the program reflects a negative impact of passive policies when youth receive the cash transfer, driven by high labor supply responsiveness to cash transfers, offset by a positive effect of active measures, although with a risk of short-term lock-in.

The paper expands the empirical literature on labor market programs for job-seekers, particularly focusing on the under-studied group of young, disadvantaged NEETs, by providing evidence on the overall effect of active and passive policies. While evaluations exist for policies that add a passive (respectively, active) component to an existing active (respectively, passive) program, the effect of jointly introducing active and passive labor market policies have rarely been evaluated.[3] Since certain job search assistance or job training programs include a stipend, some of their evaluations capture the overall effect of active and passive policies. One example is the *Year-Up* program in the US, where disadvantaged NEETs received a year of stipend and sectoral training.[4] Similarly to my findings for *Garantie Jeunes*, Fein and Hamadyk (2018) and Katz et al. (2022) highlight large positive effects of *Year-Up*, yet emerging mostly after completion. Our analysis helps explaining these results by showing that this dynamic in the effects stems from a negative effect of passive policies during enrollment in the program and a persistent positive effect of active policies.

A second stream of related literature is the one on the design of welfare programs, arising from the risk that welfare benefits trigger moral hazard (Moffitt, 1985; Chetty, 2008). Theoretically, active labor market policies such as job search assistance can provide a monitoring device (Pavoni and Violante, 2007). This monitoring role of active labour market policies can be especially welfare-improving in combination with a passive policy, where monitoring interacts the threat of exclusion from benefits, so that programs offering both active and passive policies are preferable for reasonable estimates of the costs of monitoring (Boone et al., 2007). My results support and reinforce this hypothesis. In fact, not only is the effect of *Garantie Jeunes* is non-negative during en-

[3]Some studies on passive policies in isolation include Card and Hyslop (2005); Card et al. (2007); Verlaat et al. (2023); Verho et al. (2022); Aeberhardt et al. (2020). See also the review of Schmieder and Von Wachter (2016). Evaluations of active policies are reviewed in (Card et al., 2010). The marginal effect of active policies on top of passive ones is estimated for example by the empirical literature on the effects of sanctioning UI recipients for failure to participate in job search assistance programs (Van den Berg et al., 2004; Abbring et al., 2005). In Europe, some experimental programs combining active and passive measures are being evaluated (Aparicio Fenoll and Quaranta, 2022; Del Boca et al., 2021), but these target families rather than young NEETs.

[4]In other programs, such as Job Corps (Schochet et al., 2008; Schochet, 2021), the amount of the cash support is small relative to *Garantie Jeunes*.

rollment in the program, despite the negative effect of cash transfers, but also a positive effect arises after youth stop receiving the benefits. This second aspect suggests that active labour market policies can have a beneficial effect for disadvantaged youth that lack soft skills and network (Kramarz and Skans, 2014; Schlosser and Shanan, 2022).

Finally, the paper demonstrates the effectiveness of a key French labor market policy. Prior to this paper, *Garantie Jeunes* underwent a qualitative evaluation by Gautié (2018) and a pilot quantitative evaluation by Gaini et al. (2018). The pilot evaluation compared participants in the pilot territories to a group of similar youth based on observable eligibility characteristics, using a difference-in-differences but without testing pre-trends. While the evaluation found results comparable to mine, it did not explore the role of the passive and active components. My paper also contributes to a body of working papers evaluating either passive or active policies in the French context, focusing on a similar target population (Crépon et al., 2015; van den Berg et al., 2015; Aeberhardt et al., 2020). The negative marginal impact of the passive component of *Garantie Jeunes* aligns with findings by Aeberhardt et al. (2020) from a cash transfer experiment; however, I show that during the program, the active component mitigates this effect.

The article is structured as follows. Section 2 provides an overview of the relevant institutional background and of the program. Section 3 presents the data and the identification strategy. Section 4 estimates the overall effect of the program. Section 5 disentangles the overall effect into the marginal effect of cash transfers, lock-in from training and the marginal effect of activation policies, and discusses the results in comparison with related studies. Section 6 concludes.

2 Institutional Background

Garantie Jeunes was part of the European Union Youth Guarantee, which financed a number of different national programs aimed at promoting youth employment.[5] The French version of the program was launched in October 2013, co-financed by the French government, and targeted disadvantaged NEETs aged 16-25. Crucially, this population was excluded from the French minimum income scheme, which covers only youth above 25 years old. With the introduction of *Garantie Jeunes*, the socialist-led government aimed at extending a form of social insurance to younger NEETs, but was concerned about potential negative effects on employment. Hence, in light of previous evidence, the government decided to combine activation policies and time-limited cash transfers, which represented an innovative design in the French context (Gurgand and Wargon, 2013).

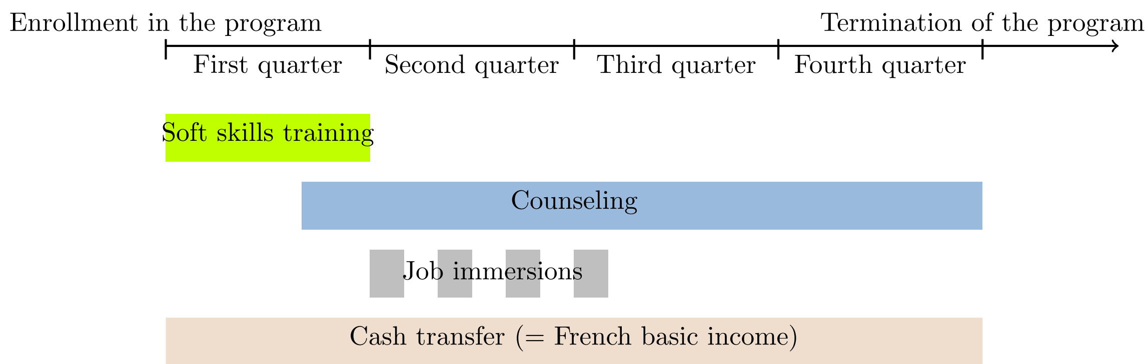
The structure of *Garantie Jeunes* is outlined in Figure 1.[6] Upon enrollment, participants are required to sign a contract of engagement with the *Garantie Jeunes* program, foreseeing exclusion from the program if not participating in the activities required. The first part of the program consists 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 related soft skills, such as presentation skills, job search strategies, applications, CVs, or motivation letters. 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. In the early stages of counseling, the counselor often suggests "job immersion" periods to the youth. These periods resemble very short internships, typically lasting a couple of weeks, during which the youth visits a partner firm with the aim of learning about the working environment and the industry.[7]

[5] For a European-wide review, see (Escudero and López, 2017).

[6] The average timing of activities and income benefits observed in the data is reported in Figure B.1 in the Appendix.

[7] Job immersions are regulated by specific conventions, and are not recorded as official employment in social security data, so the program doesn't imply a mechanical effect on participants employment during enrollment.

FIGURE 1. Outline of *Garantie Jeunes*



During the program, enrolled youths receive a monthly cash transfer equal to the amount provided by the French basic income for a single person (varying between €433.75 and €484.82 in 2013-2018). Importantly, if a participant finds a job before the end of the program, the cash transfer is not reduced if her labor earnings remain below €300. When labor earnings exceed €300, the monthly cash transfer decreases by approximately 54 cents for every additional euro earned, reaching zero at 80% of the French gross monthly minimum wage for full time workers (i.e. between €1,120 and €1,187 in 2013-2018), which is roughly equal to the minimum wage net of social contributions. Most of the youths stay enrolled in the program until the end, but 4% were expelled for not adhering to the terms of the contract of engagement with the program.[8] After a year, the program ends, and participants are allowed to extend the program only in exceptional cases (2% of enrolled youth).

French Youth Employment Centers (YECs) are in charge of the administration of the program. These employment centers are specifically responsible for youth between 16 and 25 years old and have been operating for several decades before the introduction of *Garantie Jeunes*, with approximately half a million youths registering to YECs every year. YEC registration is based on munici-

[8]Only 13% quit 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.

pality of residence 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 is much less demanding than *Garantie Jeunes*, as outlined in Figure B.2 in Appendix.[9] Importantly, YEC registration coincides with the beginning of job search for most of the youths.[10] Once youths are registered 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.[11]

The introduction of *Garantie Jeunes* was staggered over time, which provides our source of identification (Figure 2). The selection of provinces (*départements*) for the first waves were selected aiming at "ensuring geographical balance" and "embracing different contexts in terms of labour market and specific problems of youth" (Gurgand and Wargon, 2013). The program was then extended in six other waves until covering the whole French territory in January 2017. 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. Eventually, although the order of the different waves was not random, there is no apparent trend appearing comparing employment of youth across YECs of different waves of *Garantie Jeunes* extension (Figure B.5 to B.7 in the Appendix). Finally, it is important to note that YECs receive additional funding for administering *Garantie Jeunes*. The funds are distributed proportionally to the number of youths enrolled, and 10% of the funding is contingent upon

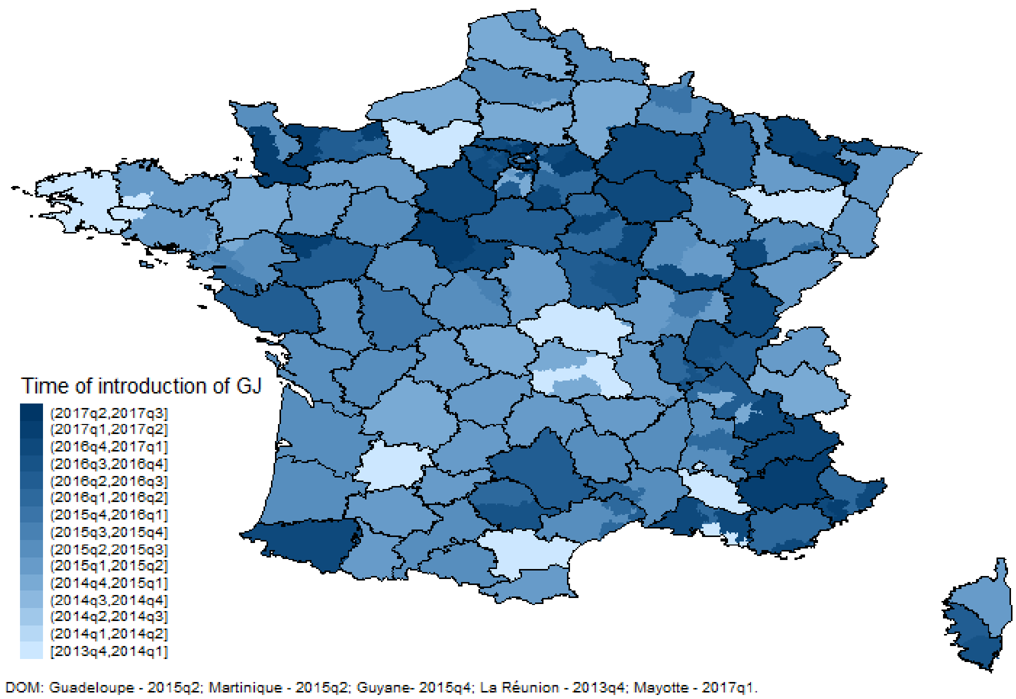
[9] Other programs offered at YECs included job search assistance in the form of counseling, although less frequent than *Garantie Jeunes* (*Projet Personnalisé d'Accès à l'Emploi*, *ANI Jeunes*, *Parrianage*) and subsidized employment in no-profit entities (*Emploi d'avenir*). The latter can still be offered to youth in *Garantie Jeunes*.

[10] In fact, the youth employment rate tends to rise from registration with YECs onward (Figure B.3 in the Appendix).

[11] Figure B.4 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", e.g. an email sent by the youth, an interview, or another activity with participation by the youth.

the submission of complete data and proof of enrollment.

FIGURE 2. Progressive extension of *Garantie Jeunes*.



Notes. Municipalities in different YECs catchment areas (black borders correspond to *départments*) by quarter of first case of enrollment in *Garantie Jeunes*. Overseas departments (DOM) are reported in the note.

Among the large number of youths registered at YECs, only few are concerned by *Garantie Jeunes*. Firstly, in order to be eligible, youths must be unemployed and out of education, live in a household with resources below the amount of the basic income, and receive no support from their parents. This requirements restrict the population of eligible youths to a minority of youth registered with YECs. Second, to enroll in *Garantie Jeunes* eligible youths must demonstrate motivation through an application process. Qualitative reports describing this process argue that the first selection mechanism involved proactive selection of youths by YECs, which often organized information sessions and pitched the program to specific youths. Then, a formal selection of applications is operated by local independent commissions.[12] In the end, youth who actually enroll in *Garantie Jeunes* are roughly half of the eligible ones according to Gaini et al. (2018).

Since its introduction in 2013, the program has grown in importance in France. Between 2017 and 2019, when *Garantie Jeunes* was offered in the whole French territory, about 90,000 youth enrolled in the program each year. In 2020, the program got further scaled-up as an answer to the Covid-19 pandemic, doubling the number of enrolled youths by easing the up-front selection. Finally, since March 2022, a new universalist version of the program named *Contrat d'Engagement Jeunes* covers all youths earning below basic income.

3 Research Design

3.1 Data, Sample and Measurement

To evaluate *Garantie Jeunes*, I build a novel dataset using two administrative sources available at the French Ministry of Labor and Social Affairs. The first one is the administrative system of YECs, called I-Milo. This dataset reports socio-demographics of youth and information on the activities undertaken by youth at YECs, from late 2010 until the present.[13] Second, to follow the employment path of youth also when they are not in contact with YEC, I use an extraction of French social security records. This dataset, which was prepared by the French Agency for Social Security under an agreement with the French Labor Ministry, includes information on all employment contracts signed during the period 2013-2018 by all youths who registered in YECs between 2013

[12] 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*.

[13] In addition, the dataset includes information provided by youth at the time of registration. 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. The dataset also contains information on French or foreign language proficiency, skills, and hobbies, but only for smaller samples.

and 2017. The available information includes date of start and termination of the employment spell, type of employment contract, total earnings and hours worked.

I merge these two sources to obtain a final dataset covering all youths who registered with YECs between January 2013 and December 2016 following their employment history and YEC activities from the time of registration with YECs until the end of 2017. This dataset encompasses approximately 2 million individuals whose characteristics are described in Table 1. Compared to the wider population of youths aged 16 to 25 in France, the group of youths registered at YECs is predominantly made up of individuals who have only completed secondary education, including vocational qualifications. This pattern aligns with the fact that YECs primarily serve less educated youths who aim to enter the labor market at a young age, often after fulfilling only minimum educational requirements.

In addition, Table 1 shows that youth registered at YECs do not significantly differ in terms of gender balance and the proportion of French nationals from same-aged French population. However, youth at YECs are characterized by early engagement in activities typically associated to adult life. On average, 35% of youths in YECs have already spent at least an hour working (compared to the national average of 30%), while 37% live independently (compared to the national average of 23%). Finally, youths who have been selected for the *Garantie Jeunes* program report a lower employment rate in the quarter prior to their registration with YECs compared to other youth at YECs.

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 *Garantie Jeunes*
Num. of youths (stock)	9327476	2005650	118984
Num. of youths (quarter inflow)		128110	14899
Less than secondary edu.	0.394	0.192	0.259
At most upper secondary edu.	0.434	0.682	0.713
Avg. age	20.3	20.1	18.8
Sh. of Women	0.491	0.491	0.461
French nat.	0.915	0.895	0.927
Empl. last quarter	0.297	0.348	0.211
Living autonomously	0.23	0.377	0.354

Notes. The table compares the characteristics of youths in registered with YECs and enrolled in *Garantie Jeunes* with those of the French population of the same age. 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 column reports descriptives on youth enrolling in *Garantie Jeunes*. All information from second and third column is measured at the quarter of registration at YECs.

I aggregate data on the employment history of youth quarterly and calculate quarterly earnings and hours based on the employment contract's duration, while trimming outliers at 99%. For employment, I define a dummy variable equal to one if the youth has reported at least one hour of work during the quarter. I organize my data by grouping youths into cohorts based on their registration quarter with YECs. Additionally, I assign each YEC to a specific wave of *Garantie Jeunes* introduction, determined by the quarter in which the first enrollment in *Garantie Jeunes* takes place at the YEC.[14]

3.2 Setup and Illustration of the Setting

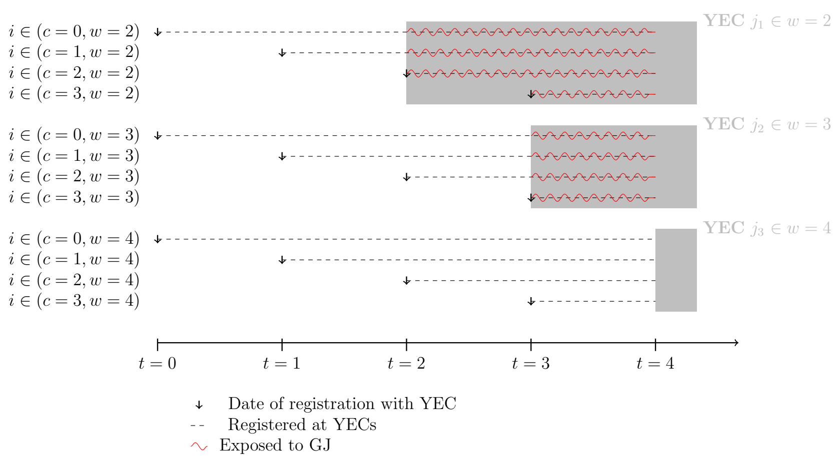
Let youths be denoted by i, each registering with a YEC j at different points in time, forming "cohorts" of registration with YECs denoted by c. Then, youth are observed over time since their registration with YECs, denoting

[14] Raw descriptives of the structure of the dataset including the number of youth in each wave and cohort are provided in Tables B.1-B.3 in the Online Appendix

time elapsed since registration $h = t - c + 1$, where t is calendar time in quarters and $h \in \{1, ...\}$. YECs adopt the program staggeredly, according to treatment "waves", denoted by w. Once exposed to the program, eligible youth can apply and be selected to enroll in the program.

To convey the intuition, Figure 3 reports a simplified illustration of the setting, including only 12 youths, in 4 cohorts of registration with YECs, and 3 different YECs. Each line in the exhibit represents a youth in the population, grouped by YECs. Program adoption is the gray shaded area. Following staggered adoption of the program, youth registering 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

Let $Y_{i,j,c,h}(g)$ be the potential outcome for youth i in YECs j, in cohort c, and observed h quarters after registration, if they are exposed for g quarters to *Garantie Jeunes*. 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 and when not exposed, over i, j, c and h:

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

Recall that each YEC j belongs to a wave w of adoption of *Garantie Jeunes*, staggered over time. Hence, g will be determined by $G_{w,c,h} : (w, c, h) \rightarrow g = \min(c + h - w, h)$. In other words, the cohort structure of our dataset and the staggered adoption of the program implies that the number of periods of exposure to *Garantie Jeunes* is determined univocally by the treatment wave of the YEC, by the cohort of registration and by the time passed since registration with YECs. In fact, the time of exposure equals either the time passed since adoption of the program by the YEC or the full time since a youth has registered with YECs (in case the youth registered with a YEC which was already offering the program).

3.3.1 Fixed Effects Approach

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

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

Where $Y_{i,j,c,h}$ is the outcome of interest, $\gamma_{c,h}$ and $\mu_{j,h}$ are cohort and YEC fixed effects interacted with time-since-registration with YECs. Note that by interacting all fixed effects with time-since-registration with YECs h, the model compares youths at the same time since registration with YECs. To test that *Garantie Jeunes* doesn't entail a change in the characteristics of youth registering with YECs, Section 4.1 reports a set of balance checks.[15]

[15]Note also that the population of youth at YECs is large compared to the number of participants in *Garantie Jeunes*, as can be seen by comparing Table B.2 and B.3 in the

Identification of β^g stems from comparing cohorts which have been exposed for g quarters to the program to cohorts not yet exposed, comparing youth at the same point in their job search (i.e. at the same h). When running regression (1), standard errors are double-clustered at the YEC-time since registration level, following Cameron and Miller (2015).[16]

3.3.2 Difference-in-Differences Approach

As an alternative, it is possible to use a difference-in-differences estimator which is robust to heterogeneous treatment effects, unlike fixed effects estimators (De Chaisemartin and D'Haultfœuille, 2020a). The difference-in-differences estimator employed closely follows De Chaisemartin and D'Haultfœuille (2020a), with adaptations made to address the fact that the difference-in-differences should in this case be estimated not only across a two-way panel dimension (i.e. cohorts and adoption waves), but also across varying durations since the cohort's entry into the population.

Assumptions and propositions are detailed in Online Appendix A.1. Denote $Y_{w,c,h} := \mathbb{E}(Y_{i,j,c,h}|i \in w, c, h)$ as the conditional expected outcome for all youths in cell w, c, h, i.e. registered with a YEC j belonging to treatment wave w, in cohort c, and registered to YECs since h quarters. My estimator first estimates cell-specific $DID_{w,c,h}$, obtained by taking the difference between expected outcomes of youths in cell (w, c, h) minus the latest cohort from the same treatment wave where youths are not-yet exposed after h quarters since registration with YECs (first difference), and the difference in outcomes in the same cohorts but in YECs where both cohorts are not-yet-exposed (second difference). Formally:

Appendix.

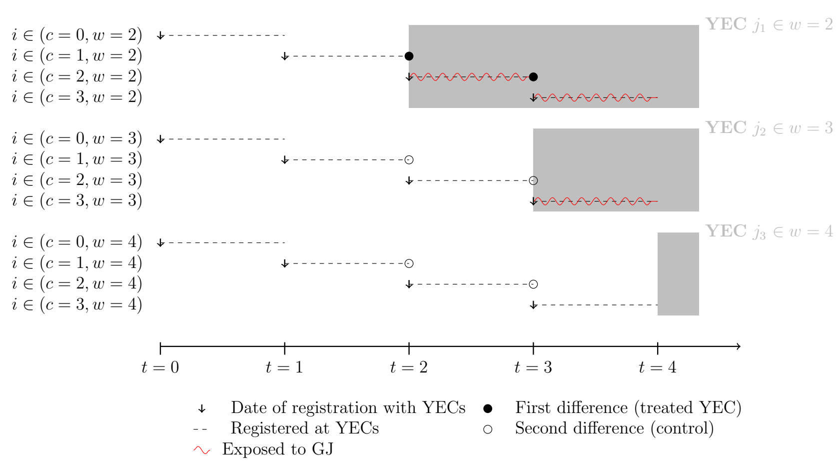
[16]Following Borusyak and Jaravel (2017), I also make sure to drop always treated groups w for each specific h, and to estimate effects only when a never-treated group is available (i.e. drop cohorts after the last wave w gets treated, for every h). Fully dynamic estimates are obtained by dropping the last wave of adoption of *Garantie Jeunes*, which is extremely small (<1% of observations) and hence potentially too noisy to represent a suitable never-treated group.

$$DID_{w,c,h} := Y_{w,c,h} - Y_{w,c',h} - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_{w},c}} (Y_{w',c,h} - Y_{w',c',h}) \quad \forall (w,c,h) : G_{w,c,h} = g > 0 \tag{2}$$

Where $G_{w,c',h} = 0$ but $G_{w,c'+1,h} = 1$, and Ω_w is the set of waves such that $G_{w',c,h} = G_{w',c',h} = 0$, for each $w' \neq w$ and $c' \neq c$. $n_{w'}$ is the number of individuals of cohort c in wave w' while $N_{\Omega_w,c}$ is the total number of individuals of cohort c in all waves $w' \in \Omega_w$.

To get the intuition, Figure 4 reports the observations used to estimate the effect for youth in cell $(h = 1, w = 2, c = 2)$, who are exposed to the program for one period $(g = 1)$. This estimator, denoted $DID_{w=2,c=2,h=1}$, compares the average outcome 1 period after registration with YECs for youth in cohort $c = 2$ minus the average outcome for the latest cohort not-yet-treated $c = 1$ minus the same difference but in YECs where both cohort $c = 2$ and $c = 1$ are not exposed.

FIGURE 4. Illustration of the difference-in-differences estimator $DID_{w=2,c=2,h=1}$



To obtain an estimator of ITT^g, I then average all $DID_{w,c,h}$ where (w, c, h) is such that $G_{w,c,h} = g$, weighted by the relative number of youth in cell (w, c, h), obtaining an estimator of the ITT effect of being exposed for g quarters, DID^g.

$$DID^g := \sum_{(w,c|h):G_{w,c,h}=g} \frac{n_{w,c}}{\sum_{(w,c|h):G_{w,c,h}=g} n_{w,c}} DID_{w,c,h} \tag{3}$$

I estimate 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).

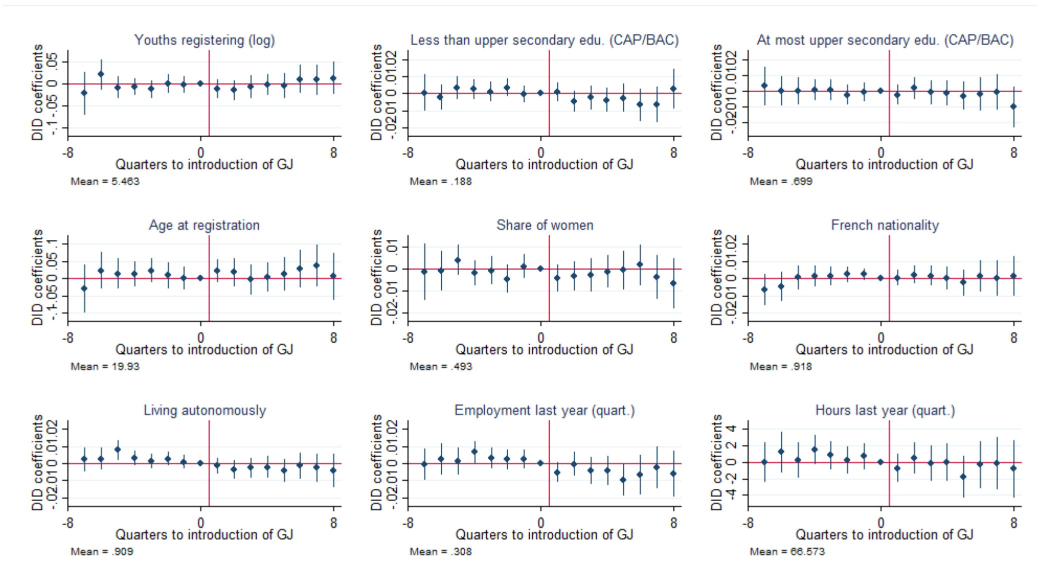
3.4 Testing Sample Stability

An important assumption underlying the identification of ITT effects is that cohorts of youth entering YECs before and after the introduction of *Garantie Jeunes* should be comparable (see Assumption 3 in the Online Appendix). That is, the composition of youths registering to YECs must not change with the introduction of *Garantie Jeunes.* This hypothesis can be tested by running the following regression at the YECs level:

$$Y_{j,c} = \sum_{q \neq -1} \beta^q \mathbb{1}(Q_{w,c} = q) + \gamma_c + \mu_j + \epsilon_{j,c} \tag{4}$$

Where $Y_{j,c}$ is a set of outcomes describing characteristics of youths in cohort c entering a YEC j belonging to adoption wave w, measured at the time of registration with YECs. $Q_{w,c} = w - c$ is the time since the introduction of *Garantie Jeunes* in the YEC. Intuitively, β^q in Equation 4 captures potential differences in the characteristics of youth of cohort c and wave w registering to YECs q quarters before/after the introduction of *Garantie Jeunes.* Reassuringly, no particular trend emerges in the estimates, signaling no significant changes in the inflow probability into YECs, in the characteristics of youth registering, and in their employment choices before registration at YECs, before or after the introduction of *Garantie Jeunes* in YECs. The magnitude of the estimated coefficients in Figure 4 is also relatively low compared to the average value in the sample of the youth characteristic used as outcome, reported at the bottom of each graph. This suggests that the composition of youth registered with YECs don't significantly change with *Garantie Jeunes* introduction. In fact, YECs were established long before *Garantie Jeunes*, offering a range of programs and services to disadvantaged youth, and a large share of young NEETs was registering with them already before the introduction of *Garantie Jeunes*.

FIGURE 5. Evolution of baseline characteristics of cohorts registering at YECs



Notes. The graph reports the estimated change in the characteristics of cohorts of youth registering at YECs before and after the introduction of *Garantie Jeunes*, including cohort and YEC fixed effects as in Equation (4). The vertical red line marks the introduction of *Garantie Jeunes*, so that coefficients after the red line are relative to cohorts registering to YECs after the introduction of *Garantie Jeunes*. The mean refers to the mean outcome the quarter before the introduction of *Garantie Jeunes*. Standard errors are clustered at the YEC level and confidence intervals are reported at 95% confidence level.

3.5 Identification of LATEs

While ITT estimators estimate the effect of exposure to *Garantie Jeunes*, a more policy-relevant parameter is the effect associated to being actually enrolled in *Garantie Jeunes*, i.e. the LATE. As in standard settings, LATE can be identified under additional assumptions. The first one is the exclusion restriction of a difference in differences: when *Garantie Jeunes* is introduced in different YECs, no other changes in policies or confounders influence the outcome at the same time. This is likely verified as *Garantie Jeunes* was one of the main policies affecting young NEETs in that period and the only one following staggered adoption. Second, the identification of the LATEs is based on the assumption that there exist no spillover effects of treated individuals on non-treated individuals. As described in Section 2, each YECs receives a special budget for the program and YECs are not supposed to reduce their activities for other groups. Moreover, the share of youth participating in *Garantie Jeunes* is small in proportion to all NEETs in YECs and to all unemployed youth in the labour market.

First, I can estimate a LATE on all compliers exposed for g quarters to the program:

$$LATE^g \quad = \quad \mathbb{E}(Y_{i,j,c,h}(g) - Y_{i,j,c,h}(0)|D_{i,j,c,h}(g) > 0)$$

Where $D_{i,j,c,h}(g)$ is the number of quarter elapsed since a youth, after being exposed to the program, has enrolled in the program, with $D_{i,j,c,h}(0) = 0$ when youth are not exposed and not enrolled in the program. Note that $D_{i,j,c,h}(g) \leq g$, because youth can enroll in the program only from the quarter when they start being exposed.

Proposition 3 in Online Appendix A.1 points out that $LATE^g$ can be estimated by simple rescaling of ITT estimates by the share of compliers, as it's standard when no unexposed youth can take-up the treatment (one-sided non compliance).[17]

Yet, $LATE^g$ estimates the average program effect associated to *any* complier, after g quarters that youth could have enrolled in the program. This means it includes a mixture of compliers at different stages of the program, and some

[17] 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 unexposed wave and no defiers/always takers in the control group.

who have already completed the program, as youth can enroll in *Garantie Jeunes* at various times after the beginning of their exposure to the program. Hence, I then aim at estimating effect associated with compliers *at a specific* stage of the program (i.e. by time elapsed since *enrollment* in the program). In particular, I disentangle the program effect on compliers who are in the first vs. the second semester of program enrollment, or after termination of the program. Such estimand will be a LATE estimand depending on d, i.e. on the number of periods since actual enrollment in *Garantie Jeunes*, and can be written as:

$$LATE^d \quad = \quad \mathbb{E}(Y_{i,j,c,h}(g) - Y_{i,j,c,h}(0)|D_{i,j,c,h}(g) = d)$$

Proposition 4 in Online Appendix A.1 suggests that we can recover $LATE^d$ using a 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:

$$\begin{aligned} DID_{w,c,h} = {} & \delta(0 < d \leq 2) Pr(0 < D_{i,j,c,h}(g) \leq 2|w, c, h) \\ & + \delta(2 < d \leq 4) Pr(2 < D_{i,j,c,h}(g) \leq 4|w, c, h) \\ & + \delta(d > 4) Pr(D_{i,j,c,h}(g) > 4|w, c, h) + \varepsilon_{w,c,h} \end{aligned} \tag{5}$$

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 5 clarifies the intuition behind this last step of my methodology: the $\hat{\delta(.)}$ coefficients are estimating "how much" the cell-specific ITT $DID_{w,c,h}$ changes following a change in the share of youths at a particular stage of the program in that cell. [18]

[18] This last step relies on the assumption that participants entering *Garantie Jeunes* earlier

4 Results

4.1 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 6 reports the results obtained both using a fixed effect regression as in (1) and using the difference-in-differences methodology. First, results using fixed effects and difference-in-differences are extremely similar. Then, by analyzing the first stage in the upper left panel it appears 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 earnings 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.

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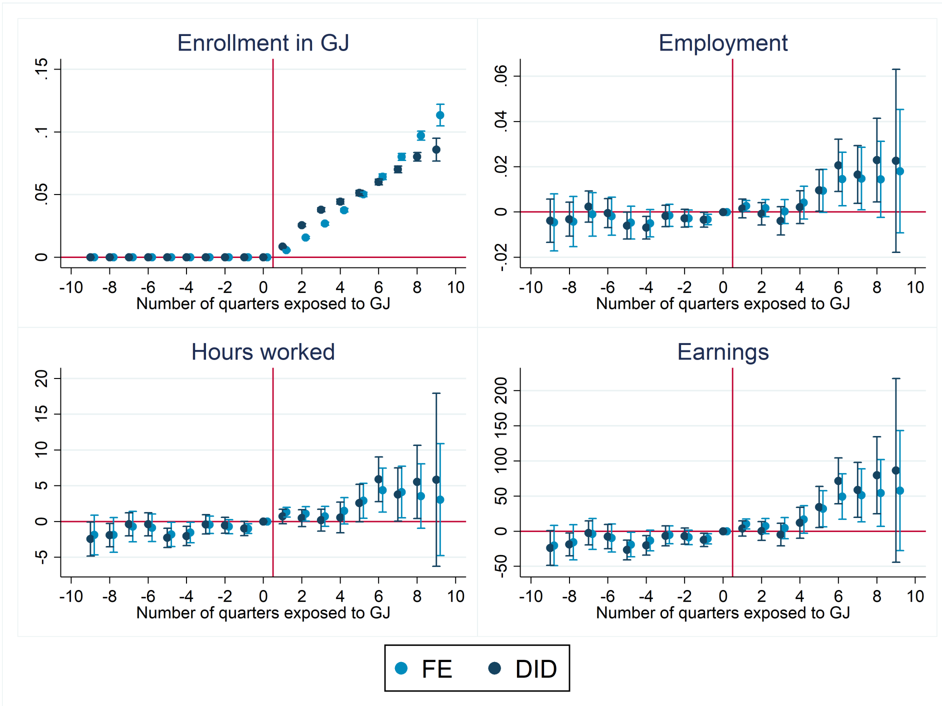
or later after exposure have comparable average outcomes. Table B.4 suggests that indeed average employment paths of youth entering the program at different points after exposure are remarkably similar.

FIGURE 6. Intent to treat (ITT) estimates of the effect of exposure to *Garantie Jeunes*.



Notes. The FE series reports the estimated program effect obtained by regressing the outcome on dummies for different quarters of exposure to *Garantie Jeunes*, cohort × time since registration in YECs fixed effects, YEC × time since registration in YECs fixed effects. The DID series reports the estimated program effect for youth before and after exposure to *Garantie Jeunes*, obtained using a difference in differences approach following Equation (3). The upper left 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 are dummies for each quarter since exposure to *Garantie Jeunes*. The other three panels report the reduced-form coefficients: the dependent variables are employment, hours worked and labor earnings, while the horizontal axis corresponds to different levels of exposure to *Garantie Jeunes*. The vertical red line marks the beginning of exposure to *Garantie Jeunes*. 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.

To get a more precise idea of the magnitudes of the effects, Table 2 reports the average of the quarterly effects obtained with the difference-in-differences methodology, for the first semester, second semester and 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 +2.88 hours on a quarterly basis and earnings by €44.5.

TABLE 2. Intent to treat (ITT) estimates aggregated.

	Enrollment in GJ	Employment	Hours worked	Earnings
	(1)	(2)	(3)	(4)
ITT 1st semester of exposure	0.0158***	-0.000459	0.329	0.135
	(0.000572)	(0.00163)	(0.435)	(4.48)
Total n.obs	4003538	4003420	3960094	3957283
ITT 2nd semester of exposure	0.0401***	-0.00331	-0.174	-2.49
	(0.00085)	(0.00264)	(0.644)	(7.19)
Total n.obs	3890678	3890532	3834252	3829157
ITT 2nd year of exposure	0.0631***	0.0115**	2.88**	44.5***
	(0.000859)	(0.00508)	(1.37)	(15.1)
Total n.obs	5574885	5574568	5476643	5470916
Control mean 1st semester in YEC		0.386	64.0	679.8
Control mean 2nd semester in YEC		0.468	99.8	1052.4
Control mean 2nd year in YEC		0.486	125.5	1338.9

Notes. The table reports the weighted averages of the $DID_{w,c,h}$ coefficients where exposure is between 1 and 2 quarters, between 2 and 4 quarters, or above 4 quarters. Quarterly estimates are obtained using the difference-in-differences approach outlined in Online Appendix A.1, where I estimate a full set of $DID_{w,c,h}$, for every $(w, c|h)$ cell, and then aggregate $DID_{w,c,h}$ 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.

Subsequently, in the upper panel of Table 3 I estimate LATEs on all compliers, conditional on the time of exposure to *Garantie Jeunes*. Specifically, the coefficients indicate that compliers in the second year of exposure increase their probability of employment by 18 percentage points, quarterly hours worked by 46, and earnings by approximately €700.

TABLE 3. Local average treatment effect (LATE) by exposure and enrollment

	Employment (1)	Hours worked (2)	Earnings (3)
LATE 1st semester of exposure	-0.0287	20.7	8.46
	(0.103)	(27.5)	(283)
LATE 2nd semester of exposure	-0.0825	-4.32	-61.7
	(0.0652)	(15.9)	(177)
LATE 2nd year of exposure	0.182**	45.7**	704***
	(0.0793)	(21.5)	(237)
LATE 1st semester of enrollm.	-0.0969*	10.1	-39.2
	(0.0559)	(14.4)	(159)
LATE 2nd semester of enrollm.	-0.0307	-5.55	-110
	(0.0679)	(22.3)	(213)
LATE after termination	0.211**	46.9*	833***
	(0.0947)	(26.3)	(296)
Compliers mean 1st semester in GJ	0.327	34.04	365.0
Compliers mean 2nd semester in GJ	0.408	59.94	658.2
Compliers mean after completing GJ	0.542	109.7	1221.

Notes. The upper panel reports reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and earnings for compliers, obtained a the ratio of reduced-form to first-stage effects. The middle panel reports the LATE effect of being at different stages of *Garantie Jeunes*, obtained according to Equation 5. The lower panel reports average employment rates for compliers in the treatment group. Standard errors are bootstrapped and reported in parenthesis.

Finally, to understand the actual dynamic effect of the program on youth when they enroll into *Garantie Jeunes*, I can use Equation 5 to estimate the LATE associated to compliers in the first semester of program enrollment, the second semester of enrollment, or after program termination (Table 3, lower part). The LATE estimated on compliers in the second year after enrollment (LATE after completion) is +21 percentage points in employment, +47 hours worked and +€833 of earnings.[19] Hence, the estimated LATE at different stages of

[19]Note that an underlying assumption of estimating LATEs through Equation (5) is that treatment effect $DID_{w,c,h}$ is independent from other variables than the share of youth enrolled in different stages of program. To test this, in Tables B.5-B.7 we evaluate the robustness of the results using OLS instead of Minimum Distance and including in Equation (5) fixed effects for wave w, time since registration h, and cohort c. The estimates remain broadly consistent, especially for employment and earnings.

youth *enrollment* in the program indicate that the positive effect observed in the second year of *exposure* is driven by the share of youth who has completed *Garantie Jeunes*. We can compare the estimated LATEs to average employment of compliers in the treatment group, and see that estimates imply a 64% increase of employment probabilities, and an even larger relative increases in hours worked and earnings after completing the program.[20]

The estimated LATE effects on employment after completion of *Garantie Jeunes* are large and positive, but results are driven by very precarious forms of employment. Table B.8 in the Online Appendix reports the ITT and LATE effect on employment in open-ended jobs, temporary jobs, agency jobs (quite frequent in this population) and apprenticeship. The effect on open-ended employment is insignificant and close to zero, while the overall employment effect mostly comes from temporary jobs (+.5 percentage points in ITT) and agency jobs (+.4 percentage points ITT). Finally, I run heterogeneity by youth characteristics (Figure B.8-B.10 in the Online Appendix). The effect in ITT terms does not vary by gender, but 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.[21]

[20]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.

[21]The estimated effects after program termination are also consistent both in significance and magnitude with the ones found by the pilot evaluation of *Garantie Jeunes* by Gaini et al. (2018). In that paper, the authors focused only on the first wave of the program, and used a matched survey to identify a suitable control. They estimate a LATE of +22.2 in the probability of employment (over a control mean of 25%) on the fifth quarter after enrollment in the program. Few differences arise for program effects before completion. For the first quarter of exposure, our estimates are similar but less significant compared to Gaini et al. (2018). While they find small positive effect on employment already in the second and third quarter, I find program effects close to zero before completion. This might be due to the fact that Gaini et al. (2018) measure employment through a survey question asking for "having worked at least one hour in the quarter". This measure can capture short work immersions proposed to youths by YEC as part of *Garantie Jeunes*, hence mechanically increase in the second and third quarter of the program. These short in-company work experiences are not reported in the administrative data used in this paper.

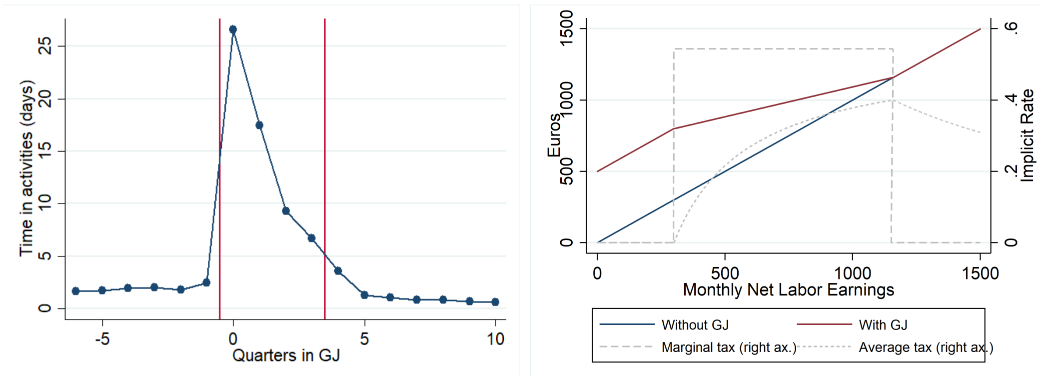
5 Mechanisms

The mechanisms behind the particular dynamic of the effect highlighted in the previous section – no effect in the first year of exposure and positive effect after termination – can be several. In the literature, participation in activation measures is associated to improved employability, however with the risk of a negative initial "lock-in" effect on employment due to reduced time for job search during the program (Gautier et al., 2018). In turn, passive policies can negatively affect employability through the elasticity of labor supply, as individuals reduce job search if higher job earnings reduce access to cash benefits (Card et al., 2007; Chetty, 2008).

To investigate these possible mechanisms, I exploit the variation in the timing of activation measures and in the cash transfer phase-out with job earnings, summarized in Figure 7. The left panel in Figure 7 reports the estimated number of working days during which youth are busy with activities with YECs (training, interview or job immersions,...), before and after enrollment in *Garantie Jeunes*. In the first two quarters of the program, youths are busy 26 and 17 days in a quarter respectively, limiting the time to actually look for a job (i.e. they risk a "lock-in" effect).

The right panel in Figure 7 reports the schedule of youth 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, and is then reduced quite steeply for every additional Euro of job earnings, reaching zero at 80% of the gross minimum wage (€1120 in 2013, €1159 on average in 2013-2016). Hence, during the year when youth are enrolled in the program, youth can attain the red schedule of labor income gross of the cash transfer. The phase-out of the cash transfer with labor earnings significantly flattens the schedule of monthly income with *Garantie Jeunes* between €300 and the threshold of 80% of the gross minimum wage. In fact, for every additional Euro earned the cash transfer is reduced by about 54 cents, implying 54% marginal tax rate and up to 40% average rate. At the end of the program, youth potential income goes back to the blue 45 degree line.

FIGURE 7. 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 monthly income attainable while enrolled in the program or not. The 80% of the gross minimum wage threshold is €1159 in the figure, the average in the 2013-2016 period.

Given these variations in the treatment, we aim at studying how the front-loading of time-consuming activation policies and the discontinuities in cash transfers are reflected in labor earnings of participants in *Garantie Jeunes*. To this purpose, I estimate LATE effects since enrollment in *Garantie Jeunes* but using as outcome the probability of earning a net monthly amount below €300, between €300 and €1100, or above €1100 for at least one month in the quarter.[22] Note that because €1100 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.

Table 4 reports the results. In the first semester after enrollment, when youths are busy in soft-skill training and job immersions, I find a decrease in employ-

[22] Net monthly amount are estimated from the dataset received from the French Agency for Social Security, estimating the monthly amount from the total duration and total gross earnings from the employment spell. The net amount is obtained by dividing the gross earnings by 1.2, to account for mandatory social security contributions (income tax is zero below 15 000 annual earnings).

ment, slightly significant for very short part-time jobs. This could be interpreted as youths being too busy in activation policies to have time for searching and taking up less remunerative jobs, while still accepting or targeting more highly remunerative jobs. In the second semester since enrollment, instead, youths have completed the most time-consuming part of the program, but are still eligible for the cash transfer. In this case, the estimated LATEs suggest a small and insignificant positive effect on the probability of earning below €300 and on the probability of earning above €1100, but also a significant 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. This corresponds to a generally positive effect of the program on employability and job quality after completion, when youth have acquired program soft skills, developed their search technology, and stopped receiving cash transfers. This evidence suggests that the overall positive effect of active and passive policies after termination of the program arises from active policies effectively counterbalancing the negative effect of passive ones, and improving employment once the program has terminated.

TABLE 4. LATE effects of *Garantie Jeunes* on the probability of reporting at least once in the quarter monthly job earnings in different income brackets.

Local Average Treatment Effect

	Monthly labor income		
	€1-€300	€300-€1150	over €1150
	(1)	(2)	(3)
LATE 1st semester of enrollm.	-0.0930*	-0.000547	0.00975
	(0.0522)	(-0.000547)	(0.0526)
LATE 2nd semester of enrollm.	0.0504	-0.194**	0.0916
	(0.0471)	(0.0918)	(0.121)
LATE after termination	-0.0841	0.191*	0.142
	(0.0685)	(0.110)	(0.116)

Notes. The table reports estimates of LATE effects obtained by estimating Equation 5 using as outcome the probability of earning in different income brackets. Standard errors are reported in parenthesis and re-estimating the model on bootstrapped datasets. Equation 5 is estimated using Equally Weighted Minimum Distance.

6 Conclusions

In this paper I studied the effects of combining active and passive labor market policies for young disadvantaged NEETs, evaluating the French program *Garantie Jeunes*. The results highlight a strong positive effect on employment after completion of the program, but no effect during enrollment. The increase in employment is however driven by temporary jobs. I show that the results can be explained by a negative marginal effect of cash transfers, lock-in from initial training and a positive marginal effect of activation policies. Employability gains due to activation policies compensate for lock-in and for the negative marginal effect of cash transfers during enrollment in the program, and drive the overall positive effect of the program after youths terminate it.

The results imply that a combination of active and passive policies effectively improves employability of disadvantaged NEETs, as argued by comparative policy reports such as OECD (2020); Pignatti and Van Belle (2018). In terms of policy implications, however, the cost-effectiveness of combining active and passive policies should not be taken for guaranteed, as costs of activation policies can be high. Online Appendix Section A.4 runs a cost-benefit analysis of *Garantie Jeunes* based on Hendren and Sprung-Keyser (2020) and finds that benefits of the program are only 19% larger than its costs.

The mechanisms analysis further suggests that youths are sensitive to the amount of the cash benefits hence that passive policies tend to reduce employment in this context. However, the active component in the policy is shown to be an effective remedy, as its positive effect is strong enough to compensate the negative effects of cash transfers during the program and driving the positive effect afterwards. This represents a valuable insight for programs combining active and passive policies of several kinds, although the limits of such results in terms of external validity should be tested by further research. For example, the negative effects of cash benefits could be lower for sub-populations more attached to the labor force, while the positive effect of activation policies can be weaker outside of the disadvantaged and motivated NEETs of *Garantie Jeunes*. On this latter aspect, the new universalist version of *Garantie Jeunes* which started in March 2022 could provide an opportunity to evaluate the overall effect of active and passive labor market policies on a broader population.

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