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Unemployment Insurance, Job Search Assistance and Subsidized Employment

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[1] Qui me permet de signaler qu'un résumé en français se trouve à la fin de cette thèse.

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Note to the reader

The three chapters of this dissertation are self-contained research articles and can be read separately. The terms ”paper”, ”study”, or ”article” are used to refer to chapters. All chapters are co-authored, which explains the use of the “we” pronoun.

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Introduction

The interplay between labor market dynamics and public policy is a critical area of research within labor economics. This thesis focuses on three major components of labor market policy and investigates how they affect the employment trajectories of individuals. Specifically, it examines part-time unemployment insurance, job search assistance and subsidized employment.

Below, I describe each chapter in more detail.

Chapter 1 : The Lock-in Effects of Information on Part-time Unemployment Benefits

Part-time unemployment benefits provided to persons working on non-regular jobs who are seeking a regular job play an increasingly important role in unemployment insurance systems.[2] The rise in the incidence of such alternative work arrangements as temporary work, part-time work, self-employment, and the new kinds of work relationship emerging in the "online gig economy" has increased the part-time unemployment take-up in several countries. In France, almost one over two unemployment benefit recipients works while on claim during his unemployment spell. Part-time unemployment benefits are also widespread in Belgium, Finland, Austria and Germany.[3]

In principle, part-time unemployment benefits aim at supplying incentives to job seekers who are looking for regular jobs to accept non-regular jobs in the meantime. This may increase overall employment and shorten unemployment spells if non-regular jobs act as stepping stones toward regular jobs. However, such benefits may also induce lock-in effects by discouraging unemployed workers from searching for regular jobs. Knowing the relative importance of stepping stone and lock-in effects which condition the access to regular employment is essential to evaluate the impact

[2]Regular jobs usually include permanent full-time jobs or full-time temporary jobs of long duration.

[3]See Ek Spector [2015] and Boeri and Cahuc [2023].

of part-time unemployment benefits on labor supply and on unemployment insurance expenditure. Unfortunately, little is known on these issues because the potential selection into part-time unemployment of individuals with non-observable characteristics correlated with their exit rate from unemployment makes the evaluation of part-time unemployment insurance very difficult.

To help understand the impact of part-time unemployment insurance, we ran a large randomized controlled experiment among about 115,000 recipients of unemployment insurance benefits in France in which we provide them with information about the existence of part-time unemployment benefits. Then, we deduce the impact of part-time unemployment insurance on the behavior of unemployed workers from the change in their behavior induced by the provision of information. The choice of this strategy is justified by the lack of knowledge about part-time unemployment insurance among job seekers. A survey conducted by the employment agency (Unédic [2012]) has shown that 41,2% of job seekers do not know of the existence of the program and that 33,6% are aware of its existence, but do not know the rules. In our experiment, we selected individuals who receive unemployment benefits for the very first time and who have been unemployed for less than 7 months at the beginning of the experiment to select people less likely to know the rules of part-time unemployment.[4] They were randomly allocated either to a treated group or to a control group. Individuals assigned to the treated group were sent emails that contained a description of the part-time unemployment insurance scheme. Individuals in the control group did not receive any message, while otherwise facing identical conditions in terms of employment services. To investigate how the treatment affects the behavior of unemployed workers, we combine administrative data from public employment services and from hiring intentions registers, which allow us to know whether individuals who exit unemployment do find jobs. Comparing the outcomes between treated and untreated individuals provides a clean identification of the average causal effects of providing information about part-time unemployment benefits.

To interpret the consequences of the provision of information, we propose a simple model of job search in which part-time unemployment insurance makes it possible to combine income from work with part of unemployment compensation and lengthen the potential duration of compensation thanks to the parts of unemployment compensation not received during periods of work while on claim. This model shows that the propensity to work while on claim declines with the marginal tax rate on labor earnings while on claim, which depends not only on the tax on

[4]The consequences of this selection on the impact of the treatment are analyzed below in the light of our results on the heterogeneity of the effect of the treatment.

current labor earnings, but also on the probability that the person will still be unemployed after the date of exhaustion of benefits and will thus recover the income from work while on claim taxed before the exhaustion date. The model shows how the marginal tax rate depends on the tax on current labor earnings and on the expectation of the duration of unemployment. We show that an information provision which raises the propensity to work while on claim can be interpreted as a revision of beliefs according to which the marginal tax rate drops. A drop in the marginal tax rate exerts effects on the search for regular jobs through two channels. The first is an *anticipation* channel reflecting the impact of part-time unemployment insurance schemes on the expected gains of unemployed workers. This channel necessarily reduces the exit rate from unemployment toward regular jobs if the drop in the marginal tax raises the expected gains of unemployed workers, which increases their reservation wage and reduces their job search effort. In addition, the model shows that this channel exerts a significant effect on the rate of exit from unemployment as the initial exhaustion date of benefits approaches, because the lengthening of the duration of unemployment benefits induced by work during the compensation period delays the increase in search effort that usually precedes the exhaustion date. The other channel arises from the *direct effect of work while on claim* on the exit rate from unemployment toward regular jobs. Working while on claim may generate more job opportunities than remaining on the dole. But working while on claim may also leave less time to look for regular jobs. Therefore, this second channel can either increase or decrease the exit rate from unemployment toward regular jobs when part-time unemployment becomes more remunerative.

We find that the information provision has a significant positive impact on the propensity to work on while on claim: The probability that treated individuals will take work while on claim increases by about 6% three months after receiving the information compared with non treated individuals. We explore the potential heterogeneity of the effects of the treatment to see whether the information provision has effects of different signs on the propensity to take work on non-regular jobs for different groups generated with the machine learning approach developed by Chernozhukov et al. [2018]. We find no evidence that the information provision has negative effects on the propensity to work while on claim. For all groups, the effects are either non-significant or positive, suggesting that the treatment changed the beliefs of job seekers whose behavioral inattention (Gabaix [2019]) induced them to underestimate the returns from work while on claim.

The hike in the propensity to work while on claim is associated with a drop in the exit rate from unemployment toward employment around the initial date of bene-

fit exhaustion.[5] In accordance with the predictions of the job search model, we find that the difference in the exit rate from unemployment toward employment between treated and untreated people is significant near the initial exhaustion date and then declines.[6] The effect is indeed significant: a 6% increase in the probability that job seekers will take work while on claim 3 months after the start of the treatment is associated with 1.5% drop in the probability that they will have exited unemployment toward regular employment the last month before the initial exhaustion date. Therefore, it is clear that lock-in effects of part-time unemployment dominate in our context.[7]

It is possible that part-time unemployment insurance decreases the exit to regular employment in general but increases the exit to good quality regular jobs, characterized by employment spells of a longer duration. However, we find that treated people are less likely to experience long (i.e. more than 3 or 6 months) employment spells than untreated ones.

Regarding the total number of days compensated by unemployment insurance, we find that these lock-in effects counterbalance the increase in work while on claim. More specifically, on the one hand, part-time unemployment benefits induce unemployed workers to work while on claim, which reduces the number of days compensated during the period of unemployment, but, on the other hand, they delay the exit to employment.[8] All in all, the net unemployment insurance expenditure remained unchanged over the entire period.

Although, it is possible that the provision of information improved welfare as treated individuals had better information, we show, on the basis of the search model, that its welfare impact is ambiguous. It depends on the impact of the part-time unemployment insurance parameters on the expectations of job seekers regarding the job offer availability. For example, the model shows that more generous part-time unemployment benefits can reduce the expected utility of job seekers and trigger more work while on claim if it is interpreted as signaling the scarcity of regular jobs. Therefore, in the absence of precise information on the expectations of job

[5]Note that the anticipation channel highlighted by the model implies that it is not possible to use the informational treatment as an instrument to study the effect of part-time work on the exit rate from unemployment since the treatment can change the exit rate from unemployment of individuals who do not work while on claim.

[6]As explained below, working while on claim moves the exhaustion date of the unemployment benefits scheduled at the date of entry into unemployment. Throughout this paper, by initial exhaustion date we mean the exhaustion date which is scheduled at the start of the unemployment spell.

[7]In our empirical analysis, the "lock-in" effect refers to the fact that the treatment caused job seekers to work more frequently while on claim (i.e. in non-regular jobs, by our definition, presented in Appendix C) than those in the treatment group *and* staying longer as recipients of unemployment benefits.

[8]Note once again, that this result is in line with the prediction of the job search model.

seekers, the assessment of the impact of unemployment insurance on their welfare is left for future research.

Chapter 2 : The Role of Caseworkers in the Labor Market Integration of Unemployed Youths: Evidence from France

Active labor market policies (ALMPs) have received significant attention over the past 40 years, yielding mixed results, especially for young people [Caliendo and Schmidl, 2016, Card et al., 2018, Kluve et al., 2019]. The design and successful delivery of these programs seem to be more crucial than their type. Caseworkers, as primary intermediaries that provide services within these programs, play a crucial role [Rosholm, 2014]. Recent studies have demonstrated substantial variations in the effectiveness of caseworkers on employment outcomes [Schiprowski, 2020, Cederlöf et al., 2021, Dromundo and Haramboure, 2021, Rasmussen, 2021].

The average percentage of unemployed young people who reported receiving helpful support in job finding from Public Employment Services (PES) was low in the European Union in 2022, around 20%[9]. However, this situation may differ in France. Between 66% and 80% of young people not in employment, education, or training (NEET) seek help from PES to receive assistance from caseworkers [Borel, 2022, Arnoult and Ghrairie, 2022]. The primary organization dedicated to assisting young individuals is known as the "mission locale." It comprises approximately 437 main centers and 7,000 related agencies, with a total workforce of 10,000 agents spread across France. These centers target NEETs aged 16 to 25, providing labor market assistance through individual meetings and group activities led by caseworkers. Additionally, they offer support for broader social issues, including health, transportation, housing, etc. Each year, over 1,000,000 young individuals have at least one meeting with a caseworker, and 80% of youths declare that they receive helpful support during their participation [UNML, 2021].

Evidence shows that young NEETs typically seek assistance when their labor market prospects are declining or very low[10]. The lack of knowledge about companies and career prospects, particularly for young people with limited qualifications and without a personal network, is a significant barrier to employment [Kramarz and Skans, 2014, Lo Bello and Morchio, 2022]. However, after registering with an

[9] See *Statistics on young people neither in employment nor in education or training* from Eurostat.
[10] see Figure 2.15 in Appendix A.

agency, their labor market situation improves, as indicated by an increase in the total number of employment or training days.

This study addresses two key questions: Do employment and training outcomes depend on the assigned caseworker? If so, do caseworkers employ different practices?

We rely on fine-grained administrative data at the caseworkers and youths level to analyze the effect of caseworker-provided assistance on the labor market integration of young people. Specifically, we take advantage of the quasi-random assignment of caseworkers to young people in large "mission locale" agencies. Regular caseworkers rotate among themselves, following a pre-planned schedule weeks or months in advance to welcome young people arriving at the agency for the first time. By default, the caseworker whom young people meet during the initial meeting becomes the caseworker responsible for conducting and supervising the entire job search process. We refer to these caseworkers as the '*main caseworkers*.'

Both quantitative and qualitative evidence suggest an assignment that appears random between caseworkers and young people in large agencies. We find no discernible pattern in the timing of caseworkers' initial meetings within an agency during a specific month or year. We then demonstrate that caseworkers' characteristics are not correlated with those of young people, especially when considering agency and the period of the first meeting. We have additional information for Paris agencies obtained through discussions with the board of directors of those agencies. Informal discussions revealed the process of allocating the first meetings among caseworkers. They negotiate among themselves to determine the day of their first meetings based on their schedules, pre-planned activities, and the total number of young people they assist (their caseload). Although some caseworkers can theoretically choose certain days rather than others, depending on the characteristics of the young people they expect to meet, they do not choose from among the young people who actually come on those days.

Overall, caseworkers' practices and activities vary considerably, even within the same agency. Simultaneously, we also observe differences in the average placement of young people in employment or training. Thus, we take advantage of the quasi-random assignment of caseworkers to investigate their causal impact on the subsequent labor market trajectories of the young people they serve.

We measure caseworkers' ability to facilitate young people's entry into jobs or training by evaluating their value-added in terms of the total number of employment days or training days over a two-year period following the initial meeting in a mission locale. Under the assumption that young people have similar potential labor market outcomes, contingent on the agency and period of the first meeting,

we can estimate unbiased value-added scores on either employment or training for each caseworker. As placebo tests, we also estimate the effect of caseworkers on young people's outcome *before* their first meeting and, reassuringly, find no significant impact on the number of employment or training days.

Once young people have registered with an agency and have been assigned to a main caseworker, we discover that being assigned to a caseworker with value-added one standard deviation (SD) above the mean increases the total number of employment days by an average of 0.05 SD over the next two years. This effect represents either an absolute increase of 12 days or a relative increase of 6%. This relative effect aligns with findings in the literature on caseworkers assisting adult job seekers. However, the absolute effect is lower, which we attribute to the specific population they serve, particularly young people classified as NEET, who, among several obstacles, struggle with a severe lack of confidence and exhibit behavioral biases that hinder their labor market integration [Jellab, 1998, Babcock et al., 2012].

To complete the analysis, we examine the effects of caseworkers on training outcomes, which are often expected to act as a stepping stone for this population [Escudero and Mourelo, 2017]. We find that being assigned to a caseworker one SD above the mean in the distribution of training value-added results in a 0.03 SD increase in the total number of training days. This corresponds to an absolute increase of 3 days or a relative effect of 8.5%. Interestingly, we find that the most effective caseworkers regarding employment are not necessarily the best at training placement, and vice versa. The evidence suggests that high-value-added caseworkers tend to specialize in one of the two dimensions.

We also examine the heterogeneity of caseworkers' effects, as they can vary according to the characteristics of the young people they serve. Males primarily benefit from caseworkers with higher employment value-added, while females gain more from caseworkers with higher training value-added. Furthermore, we find that the effect of caseworkers with higher value-added on employment diminishes with age, whereas the effect on training decreases with the educational level of young individuals. The effects of caseworkers with higher value-added differ across labor markets, with those excelling in employment primarily assisting youths in areas with high unemployment rates and low job-tightness. Conversely, caseworkers who excel in training mainly serve youths in areas with intermediate levels of unemployment.

Turning to the underlying mechanisms, we first examine how caseworkers differ in their practices with the young people they serve, including the intensity of individual meetings and contacts, job or training propositions, and placement into programmes. Overall, we find that the practices of high-value-added casework-

ers differ from those of lower-value-added caseworkers, although the magnitude of these differences is small.

We find that caseworkers with higher employment value-added do not necessarily increase the probability of young individuals finding permanent contracts. They make more employment propositions than low-value-added caseworkers, but the difference is too small to explain the effect on employment. Notably, they mainly assist young people under an intensive job placement program, in partnership with other caseworkers from the national public employment agency ("*Pôle emploi*"). Additionally, evidence suggests some competition among high-value-added caseworkers to place young individuals into employment. Partial outsourcing assistance appears to be an effectivepartially strategy for caseworkers. This option would allow young NEETs to benefit from advice shared not only by their own caseworkers but also by other caseworkers in different public structures.

Regarding caseworkers who excel in training placement, we find that higher value-added caseworkers only marginally, at least in absolute terms, increase the probability of enrolling in training programs that lead to national certification. They propose more training opportunities, but the magnitude of this increase is minimal: two additional training propositions per youth over a two-year period, totaling around 300 propositions, would lead to only a 2-percentage point shift in caseworkers' placements, enabling them to move from the lowest tercile to the highest tercile in the training value-added distribution.

More importantly, higher-value-added caseworkers conduct more collective workshops than their lower-value-added colleagues. These workshops appear to be attended by young people with lower employment probabilities. As a result, caseworkers leading these workshops likely better understand the wishes and needs of these youths, guiding them toward training programs to enhance their skill sets. However, we do not find that entry into training, due to high-value-added caseworkers, significantly improves the subsequent employment situations of youths within the two-year period. This result holds true whether entry into training occurs after 6 months or 1 year from the first meeting.

Finally, we explore the impact of different caseworker reassignment scenarios on young people's outcomes as well as the tax collected by the state. We rely on a machine learning algorithm, specifically random causal forests [Wager and Athey, 2018], to estimate treatment effects conditional on the set of youths' observed characteristics. The results show large heterogeneous effects of high-value added caseworkers from -100 days to +120 days depending on youths' characteristics. Based on conditional treatment effects, we simulated two scenarios. In the first scenario, we assigned high-value-added caseworkers to youths with the highest conditional

treatment effect to maximize the number of employment days. This reallocation could increase the average total number of employment days from 16 to 46, effectively tripling the mean effect of high-value-added caseworkers. In the second scenario, we assigned high-value-added caseworkers to youths who explicitly sought help with employment, had a low employment probability, and exhibited the highest conditional treatment effect. However, this reallocation only increased the mean effect of high-value caseworkers by 18% compared with the baseline scenario (from 16 to 19 days). We delve more precisely into the policy implications of these simulations in the conclusion.

Chapter 3 : The Benefits of Early Work Experience for School Dropouts: Evidence from a Field Experiment

Young people who leave high school before graduation, known as school dropouts, are over-represented among those who are not in employment, education, or training (NEET). In 2021, the dropout rate among NEETs was 37% in the US, compared to 16% for high school graduates, and 36.9% in the OECD countries, compared to 14.3% respectively.[11]

However, employers do not necessarily view dropouts as individuals who are completely adrift. Beyond the various factors that contribute to their decision to leave school, such as inadequate guidance after middle school [Hoxby and Avery, 2013, Arcidiacono et al., 2016, Guyon and Huillery, 2021], or an unsuitable specialization during their education [Kirkeboen et al., 2016, Clemens et al., 2021, Dahl et al., 2022], dropouts represent a heterogeneous group with diverse individual and professional experiences. Supporting this notion, a survey conducted in 2019 among French employers hiring young people in low-skilled occupations indicate that employers generally do not perceive dropouts negatively [Beffa and Broc, 2019]. They generally do not see them as individuals who have experienced significant misfortune or who have not put in enough effort at school or lack fundamental skills such as writing, listening, or mathematics. Instead, two-thirds of employers view dropouts as young people seeking opportunities in the labor market and pursuing specific work experiences.

In line with this perspective, dropouts face higher unemployment and inactivity probabilities and lower employment probabilities compared to other job seekers who possess adequate skills, particularly recent graduates [Rumberger and Lamb, 2003, Clemens et al., 2021]. This effect may be further exacerbated by the duration

[11] See the National Center for Education Statistics and Giret and Jongbloed [2021] for more details.

dependence of unemployment [Kroft et al., 2013, Eriksson and Rooth, 2014, Farber et al., 2019]. Yet, the situation may be less severe in occupations where employers struggle to find suitable candidates.

This study examines whether an early job-related work experience is sufficient to enhance the employment opportunities of school dropouts relative to graduates. Given the widespread existence of public programs designed to support disadvantaged youth, it is of interest to determine the effectiveness of such initiatives as potential stepping stones for early school leavers. Notably, our study focuses on recruiters' perceptions as one of the key mechanisms through which work experience can shape the employment trajectories of school dropouts.

To investigate this question, we conducted a correspondence study field experiment in France. Between January and July 2018, we submitted approximately 6,400 applications from fictitious candidates to actual job vacancies posted online. Additionally, between October and December 2018, we sent around 8,200 unsolicited applications to other firms employing individuals in the same occupations. The fictitious applicants were 18-year-old young men who had either graduated from a two-year vocational education program relevant to the targeted occupations or left school after middle school. While all dropout applicants were inactive during the first year after disengaging from school (at ages 16 to 17), their labor market experience during the second year (ages 17 to 18) was randomly assigned. One group of dropouts remained inactive during this second year, while the other group acquired one year of work experience in the targeted occupations.

Our experiment focused on two occupations, namely cook and mason. These occupations were selected based on their notable prevalence among vocational students and school dropouts, not only in France but also in other European countries. Moreover, these occupations are among those with a substantial percentage of workers who receive wage subsidies. In our experiment, our fictitious applicants possessed job-related experience gained through a national program called "Emploi d'Avenir", which operated between 2012 and 2018. This program targeted young people aged 16 to 25 without diplomas or with low levels of qualifications. Firms and non-market structures were eligible for wage subsidies if they hired young people through this program.[12] They also had the option to provide vocational training to young people, leading to a diploma equivalent to the one obtained through vocational education. The certification status among dropouts with work experience was also randomized.

Both graduates and dropouts with job-related experience listed identical skills

[12]Regarding potential negative stigma associated with this subsidized employment program, Cahuc et al. [2019] found no difference from an equivalent non-subsidized work experience.

on their CVs, encompassing a blend of both soft and hard skills. This emphasis on diverse skills underscores their ability to function proficiently not only within their respective occupations but also in diverse professional environments. In their cover letters, applicants elaborated on the acquisition of these skills, whether through educational pursuits or job-related experience. Conversely, dropouts who remained inactive solely presented soft skills on their CVs and conveyed their enthusiasm to acquire hard skills through their first professional experience in their cover letters.

The results indicate that the average job interview rate for school graduates is approximately 25%, while it is 18% for school dropouts. However, there is heterogeneity among dropouts, with those who signal work experience having an interview rate of 20%, compared to only 8% for those who remained inactive. Furthermore, the interview rate significantly differs among dropouts with work experience, with a rate of 19% when there is no certification and 22% when dropouts signal a diploma. Consequently, the chances of being invited for an interview decrease by 67% when young people drop out of high school, remain inactive, and compete for jobs against peers who have completed vocational education. This gap narrows when young people gain job-related experience. Specifically, one year of job-related work experience reduces the interview penalty from 67% to 25%. Moreover, this penalty decreases to 12% when dropouts acquire a diploma to certify their skills.

These differences in interview rates are not uniform across all settings. We utilize various indicators to assess opportunities in different local labor markets, such as the unemployment rate, the level of occupational tightness, and the proportion of dropouts with work experience among all workers. Our findings indicate that the situation for dropouts deteriorates in tight labor markets but improves in slack labor markets. This suggests that employers face more challenges in hiring qualified and experienced workers in tight labor markets, whereas the larger applicant pool in slack labor markets allows them to screen more new employees.

This explanation is particularly relevant for occupations where employers struggle to find suitable candidates and where skills can be acquired through on-the-job training. The link between acquired skills and integration into vocational occupations is indeed significant. By comparing the career paths of young novice workers in low-skilled occupations, some of whom have received more or less direct training in the field while others have not, Lainé [2005] demonstrates that the latter group is less likely to remain employed, more frequently engaged in precarious contracts, and experiences lower wage progression. Similarly, Girsberger et al. [2022] reveal that low-ability workers who have acquired specific manual skills in occupations similar to cook and mason have witnessed a 10% increase in their wages and a 50% reduction in their unemployment risk.

Lastly, we find that school dropouts with job-related work experience who send unsolicited applications to firms, without responding to specific job vacancies, achieve interview rates on par with graduates in slack labor markets. This positive effect applies regardless of whether skills are certified. However, we do not observe any effect of sending unsolicited applications in tight labor markets. We discuss the policy implications of our findings in the conclusion.

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Chapter 1

The Lock-in Effects of Information on Part-time Unemployment Benefits

This chapter corresponds to a paper[1] *co-authored with Hélène Benghalem and Pierre Cahuc.*

Abstract: We conducted a large randomized controlled experiment that provides information on part-time unemployment benefits to unemployment insurance recipients in France. The information provision had a significant positive impact on the propensity to work while on claim, but reduced the unemployment exit rate, showing important lock-in effects into unemployment associated with part-time unemployment benefits. The extension of the duration of compensated unemployment counterbalanced the increase in the number of days worked while on claim so that the net expenditure of unemployment insurance remained unchanged.

1.1 Introduction

Part-time unemployment benefits provided to persons working on non-regular jobs who are seeking a regular job play an increasingly important role in unemployment insurance systems.[2] The rise in the incidence of such alternative work arrangements as temporary work, part-time work, self-employment, and the new kinds of work relationship emerging in the "online gig economy" has increased the part-time unemployment take-up in several countries. In France, al-

[1] This article is forthcoming in the *Journal of Human Resources*, ≪ahead of print≫ version is accessible here.

[2] Regular jobs usually include permanent full-time jobs or full-time temporary jobs of long duration.

most one over two unemployment benefit recipients works while on claim during his unemployment spell. Part-time unemployment benefits are also widespread in Belgium, Finland, Austria and Germany.[3]

In principle, part-time unemployment benefits aim at supplying incentives to job seekers who are looking for regular jobs to accept non-regular jobs in the meantime. This may increase overall employment and shorten unemployment spells if non-regular jobs act as stepping stones toward regular jobs. However, such benefits may also induce lock-in effects by discouraging unemployed workers from searching for regular jobs. Knowing the relative importance of stepping stone and lock-in effects which condition the access to regular employment is essential to evaluate the impact of part-time unemployment benefits on labor supply and on unemployment insurance expenditure. Unfortunately, little is known on these issues because the potential selection into part-time unemployment of individuals with non-observable characteristics correlated with their exit rate from unemployment makes the evaluation of part-time unemployment insurance very difficult.

To help understand the impact of part-time unemployment insurance, we ran a large randomized controlled experiment among about 115,000 recipients of unemployment insurance benefits in France in which we provide them with information about the existence of part-time unemployment benefits. Then, we deduce the impact of part-time unemployment insurance on the behavior of unemployed workers from the change in their behavior induced by the provision of information. The choice of this strategy is justified by the lack of knowledge about part-time unemployment insurance among job seekers. A survey conducted by the employment agency (Unédic [2012]) has shown that 41,2% of job seekers do not know of the existence of the program and that 33,6% are aware of its existence, but do not know the rules. In our experiment, we selected individuals who receive unemployment benefits for the very first time and who have been unemployed for less than 7 months at the beginning of the experiment to select people less likely to know the rules of part-time unemployment.[4] They were randomly allocated either to a treated group or to a control group. Individuals assigned to the treated group were sent emails that contained a description of the part-time unemployment insurance scheme. Individuals in the control group did not receive any message, while otherwise facing identical conditions in terms of employment services. To investigate how the treatment affects the behavior of unemployed workers, we combine administrative data from public employment services and from hiring intentions registers, which allow us to

[3] See Ek Spector [2015] and Boeri and Cahuc [2023].

[4] The consequences of this selection on the impact of the treatment are analyzed below in the light of our results on the heterogeneity of the effect of the treatment.

know whether individuals who exit unemployment do find jobs. Comparing the outcomes between treated and untreated individuals provides a clean identification of the average causal effects of providing information about part-time unemployment benefits.

To interpret the consequences of the provision of information, we propose a simple model of job search in which part-time unemployment insurance makes it possible to combine income from work with part of unemployment compensation and lengthen the potential duration of compensation thanks to the parts of unemployment compensation not received during periods of work while on claim. This model shows that the propensity to work while on claim declines with the marginal tax rate on labor earnings while on claim, which depends not only on the tax on current labor earnings, but also on the probability that the person will still be unemployed after the date of exhaustion of benefits and will thus recover the income from work while on claim taxed before the exhaustion date. The model shows how the marginal tax rate depends on the tax on current labor earnings and on the expectation of the duration of unemployment. We show that an information provision which raises the propensity to work while on claim can be interpreted as a revision of beliefs according to which the marginal tax rate drops. A drop in the marginal tax rate exerts effects on the search for regular jobs through two channels. The first is an *anticipation* channel reflecting the impact of part-time unemployment insurance schemes on the expected gains of unemployed workers. This channel necessarily reduces the exit rate from unemployment toward regular jobs if the drop in the marginal tax raises the expected gains of unemployed workers, which increases their reservation wage and reduces their job search effort. In addition, the model shows that this channel exerts a significant effect on the rate of exit from unemployment as the initial exhaustion date of benefits approaches, because the lengthening of the duration of unemployment benefits induced by work during the compensation period delays the increase in search effort that usually precedes the exhaustion date. The other channel arises from the *direct effect of work while on claim* on the exit rate from unemployment toward regular jobs. Working while on claim may generate more job opportunities than remaining on the dole. But working while on claim may also leave less time to look for regular jobs. Therefore, this second channel can either increase or decrease the exit rate from unemployment toward regular jobs when part-time unemployment becomes more remunerative.

We find that the information provision has a significant positive impact on the propensity to work on while on claim: The probability that treated individuals will take work while on claim increases by about 6% three months after receiving the information compared with non treated individuals. We explore the potential het-

erogeneity of the effects of the treatment to see whether the information provision has effects of different signs on the propensity to take work on non-regular jobs for different groups generated with the machine learning approach developed by Chernozhukov et al. [2018]. We find no evidence that the information provision has negative effects on the propensity to work while on claim. For all groups, the effects are either non-significant or positive, suggesting that the treatment changed the beliefs of job seekers whose behavioral inattention (Gabaix [2019]) induced them to underestimate the returns from work while on claim.

The hike in the propensity to work while on claim is associated with a drop in the exit rate from unemployment toward employment around the initial date of benefit exhaustion.[5] In accordance with the predictions of the job search model, we find that the difference in the exit rate from unemployment toward employment between treated and untreated people is significant near the initial exhaustion date and then declines.[6] The effect is indeed significant: a 6% increase in the probability that job seekers will take work while on claim 3 months after the start of the treatment is associated with 1.5% drop in the probability that they will have exited unemployment toward regular employment the last month before the initial exhaustion date. Therefore, it is clear that lock-in effects of part-time unemployment dominate in our context.[7]

It is possible that part-time unemployment insurance decreases the exit to regular employment in general but increases the exit to good quality regular jobs, characterized by employment spells of a longer duration. However, we find that treated people are less likely to experience long (i.e. more than 3 or 6 months) employment spells than untreated ones.

Regarding the total number of days compensated by unemployment insurance, we find that these lock-in effects counterbalance the increase in work while on claim. More specifically, on the one hand, part-time unemployment benefits induce unemployed workers to work while on claim, which reduces the number of days compensated during the period of unemployment, but, on the other hand, they delay

[5]Note that the anticipation channel highlighted by the model implies that it is not possible to use the informational treatment as an instrument to study the effect of part-time work on the exit rate from unemployment since the treatment can change the exit rate from unemployment of individuals who do not work while on claim.

[6]As explained below, working while on claim moves the exhaustion date of the unemployment benefits scheduled at the date of entry into unemployment. Throughout this paper, by initial exhaustion date we mean the exhaustion date which is scheduled at the start of the unemployment spell.

[7]In our empirical analysis, the "lock-in" effect refers to the fact that the treatment caused job seekers to work more frequently while on claim (i.e. in non-regular jobs, by our definition, presented in Appendix C) than those in the treatment group *and* staying longer as recipients of unemployment benefits.

the exit to employment.[8] All in all, the net unemployment insurance expenditure remained unchanged over the entire period.

Although, it is possible that the provision of information improved welfare as treated individuals had better information, we show, on the basis of the search model, that its welfare impact is ambiguous. It depends on the impact of the part-time unemployment insurance parameters on the expectations of job seekers regarding the job offer availability. For example, the model shows that more generous part-time unemployment benefits can reduce the expected utility of job seekers and trigger more work while on claim if it is interpreted as signaling the scarcity of regular jobs. Therefore, in the absence of precise information on the expectations of job seekers, the assessment of the impact of unemployment insurance on their welfare is left for future research.

Our paper makes contributions to two strands of the literature. The first is the empirical literature on part-time unemployment insurance, which has used different approaches to identify the effects of part-time unemployment benefits. The seminal contribution of McCall [1996] exploits variations in the design of part-time unemployment benefits across U.S. states from 1986 to 1992. An increase in the disregard is estimated to raise the probability of part-time re-employment and to reduce expected joblessness.[9] Using kinks in the U.S. benefit-withdrawal schedule, Le Barbanchon [2020] provides evidence that workers take into account the value of future benefit entitlement when they make their labor supply decision. Ait Bihi Ouali et al. [2020] rely on a regression discontinuity design to show that an increase in the tax on earnings from work while on claim which occurred in France in 2006 reduced the propensity to work while on claim. Several studies, focused on European countries, rely on the timing-of-events approach (Abbring and Van Den Berg [2003]) or matching methods. They look at the effects of working on non-regular jobs on the access to regular employment in Austria (Böheim and Weber [2011], Eppel and Mahringer [2019], Belgium (Cockx et al. [2013]), Denmark (Kyyrä et al. [2013]), Finland (Kyyrä [2010]), France (Fremigacci and Terracol [2013], Auray and Lepage-Saucier [2021]), Germany (Caliendo et al. [2016]), Norway (Godøy and Røed [2016]), Slovakia (Van Ours [2004]), Switzerland (Gerfin et al. [2005]). They find mixed results, showing generally significant lock-in effects while individuals work on non-regular jobs and more positive effects on the access to regular jobs after non-

[8] Note once again, that this result is in line with the prediction of the job search model.

[9] Recipients accepting part-time jobs can earn up to a specific amount, called the "disregard", with no reduction in benefits during the reference period, which can be the week or the month. Above the disregard, the current benefits are reduced in proportion to the labor earnings. There is a disregard in several U.S. states, in Australia, Austria, Belgium, Canada, Czech Republic, Germany, Luxembourg, Poland.

regular jobs end. It is clear that these approaches can potentially identify the effects of working on non-regular jobs on the exit rate from unemployment, but cannot identify the effects of any part-time unemployment benefits scheme per se, insofar as they do not account for the *anticipation* channel described above. The papers of O'Leary [1997], Lee et al. [2019] and Altmann et al. [2021] are the most closely related to ours to the extent that they rely on randomized controlled experiments which allow a credible identification of the impact of part-time unemployment. O'Leary [1997] and Lee et al. [2019] analyze the consequences of the Washington State Unemployment Insurance Earnings Deduction Experiment in which for one year, starting in October 1994, Washington conducted a large randomized experiment to investigate the effects of reducing the amount of benefits deducted from claimants who worked while on claim. They find that the tax reduction had no positive effects on labor supply and increased the unemployment insurance expenditure because it raised the propensity to claim benefits. Altmann et al. [2021] provide information on part-time unemployment benefits to job seekers in Denmark. They find that the provision of information promotes employment in part-time and non-regular jobs for individuals close to benefit expiration, but reduces their employment and earnings in the longer run. Our contribution approaches the problem from a different angle since it analyzes the consequences of the provision of information.[10] It shows that the information provision had a significant positive impact on the propensity to work while on claim, but reduced the unemployment exit rate, showing important lock-in effects into unemployment associated with part-time unemployment benefits. Moreover, as we follow individuals over a period of 3 years – instead of one year for O'Leary [1997] and Lee et al. [2019] –, we can analyze the consequences of the information provision on unemployment survival over a sufficiently long period, which is key for the understanding of the consequence of a scheme that can increase the potential benefit duration well beyond the initial benefit exhaustion date.

Our contribution also adds to the literature devoted to the analysis of the consequences of information provision in a variety of economic applications, including job search (Altmann et al. [2018], Belot et al. [2018], Crépon et al. [2018], Darling et al. [2016]), labor supply and taxes (Chetty and Saez [2013], Blaufus et al. [2020], Abeler and Jäger [2015], Kostøl and Myhre [2021]), the take-up of social benefits (Currie [2006], Bhargava and Manoli [2015], Finkelstein and Notowidigdo [2019]), unemployment benefits (Blank and Card [1991], Fontaine and Ketteman [2019], Altmann et al. [2021]), housing allowances (Engström et al. [2015]), retirement savings plans (Dolls et al. [2019]) and training programs (Crépon et al. [2018]). From this perspective, our paper is the first, with that of Altmann et al. [2021], to provide in-

[10]The consequences of information provision is discussed in detail in Section 1.3.2

formation focused on part-time unemployment benefits. By design, the paper of Altmann et al. [2021] is mainly focused on the impact of the provision of information on beliefs, thanks to a survey that we do not have. However, its design makes it less informative than ours to look at the behavior of unemployed workers. From that perspective, these two contributions are complementary. In particular, we provide a model which shows how information provision can influence the beliefs held by individuals about part-time unemployment insurance rules from its impact on the behavior of unemployed workers. We find that the take-up of individuals who benefited from the information increased. This confirms the results of Altmann et al. [2021] and of the literature which finds that the take-up of most social benefits programs is reduced by the lack of information, especially when rules are complex. We find that the lack of information persists over a long horizon (at least 36 months) after the start of our experiment, suggesting that the spread of information about the program among uninformed unemployed workers is very slow. We also bring new information by analyzing spillover effects. We compare the behavior of non-treated individuals registered with employment agencies where half of individuals have been treated with the behavior of individuals registered with employment agencies where nobody has been treated. These two types of non-treated individuals behave similarly, meaning that information provision had no spillover effects among unemployed workers. This suggests that the information about part-time unemployment benefits did not spread from treated to non-treated individuals registered with the same unemployment agency and that the hike in non-regular employment of treated individuals did not crowd out that of individuals of the control group. This result can be compared to that of Crépon et al. [2013] and Gautier et al. [2018] who do find important spillover effects from job placement assistance programs.

The paper is organized as follows. Section 1.2 presents the part-time unemployment benefits program and the knowledge of unemployment workers about the program. Section 1.3 presents the theoretical framework which allows us to interpret the consequences of providing information about the program. The experimental design and the data are presented in Section 1.4. The impact of the informational treatment on part-time unemployment, on the unemployment exit rate, the hours worked, the quality of jobs, the unemployment insurance payout and welfare are discussed in Section 1.5. Section 1.6 provides concluding comments.

1.2 Institutional Background

1.2.1 Program structure

At the start of their unemployment spell, eligible unemployed workers get an initial unemployment insurance capital B_0 which allows them to get unemployment benefits, denoted by b. Both the initial capital and the unemployment benefits depend on the individual's past employment history.[11] The benefits paid each month are deducted from the capital B_t. This capital yields, in each month of unemployment t, unemployment benefits equal to

$$b(B_t) = \begin{cases} b > 0 & \text{if } B_t \geq b \\ \max(B_t, 0) & \text{otherwise} \end{cases} \tag{1.1}$$

The part-time unemployment insurance scheme allows unemployed workers to take work while on claim. There is no specific eligibility condition for part-time unemployment insurance. Claimants must only meet the usual eligibility requirements for unemployment insurance. They are allowed to work for any employer, including their past employers. For each euro earned from work, current benefits are reduced by the marginal benefit reduction rate τ=87%.[12] To put it differently, the part-time unemployment scheme allows individuals to combine their unemployment benefits and the share $1-\tau$ of their labor earnings z_t in the periods where they work while on claim. More precisely, the monthly income of a worker whose labor earnings amount to z_t in month t is equal to

$$\max[b(B_t) + (1-\tau)z_t, z_t] \tag{1.2}$$

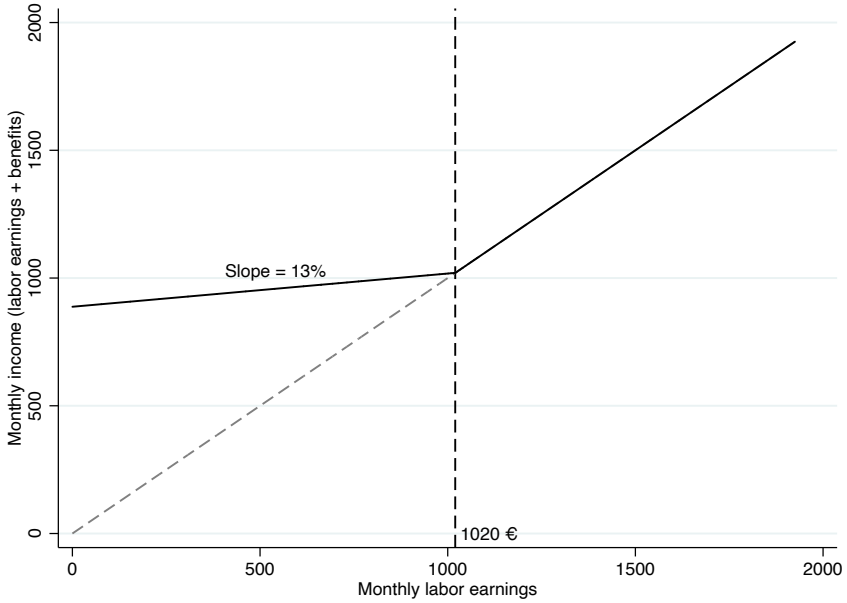
τ and b are set to ensure that by working while on claim job seekers cannot get a monthly income higher than the past monthly income used to compute their unemployment benefits. Hence, individuals whose labor earnings in the current month are larger than the monthly income used to compute their unemployment benefits, do not get unemployment benefits at the end of the month.

Figure 1.1 illustrates the part-time unemployment insurance schedule. From a static point of view, there are low incentives to work while on claim: the blue line, which displays the relation between the monthly labor earnings of people working while on claim and their income, is almost flat. Yet the reduction in benefits is not lost, it can be paid in a later month. The corresponding benefit reduction delays the

[11] See Appendix A for more details

[12] For the sake of simplicity, we describe the rules in *net* terms for a job seeker who earned the minimum wage before unemployment. Appendix A provides details on this point.

Figure 1.1: The relation between earnings when working while on claim and income



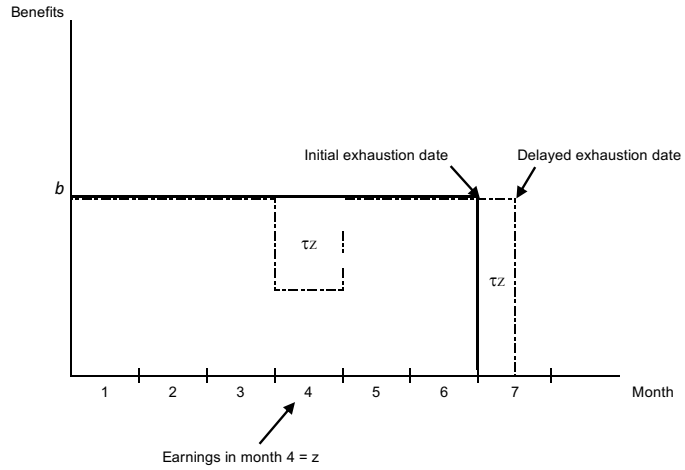
Note: This figure shows the relation between labor earnings (horizontal axis) and income (vertical axis) of individuals eligible for unemployment benefits whose monthly wage was equal to €1020 before their unemployment spell. Labor earnings and income are net of social contributions.

exhaustion date. Figure 1.2 illustrates the dynamic aspects of the part-time unemployment insurance schedule which are critical for understanding its incentivation effects. If job seekers are totally unemployed throughout their claim and receive their benefits each month, their benefits will lapse after their exhaustion date. When job seekers are only paid part of their benefits in a given month $b(B_t) - \tau z_t \geq 0$, the unpaid amount $\min[\tau z_t, b(B_t)]$ is not deducted from their insurance capital B_t. This implies that the earnings from the days worked while on claim make it possible to extend the duration of the claim. The exhaustion date can be delayed without any limitation. Hence, the unemployment insurance capital evolves according to the law of motion[13]

$$B_{t+1} = \max\left(B_t - b(B_t) + \min[\tau z_t, b(B_t)], 0\right) \tag{1.3}$$

[13]Note that part-time unemployment insurance drastically changes the possibility of renewing the entitlement period. In the absence of part-time unemployment, when the insurance capital is exhausted, individuals must have worked at least 150 hours while on claim over the last 28 months to be eligible for a new entitlement period. The new initial capital is computed on the basis of the daily wage of periods of work and according to the rule "one day of work yields one day of compensation". In the presence of part-time unemployment, the hours worked during the entitlement not only make it possible to delay the end date of entitlements but also to accumulate hours for the opening of a new entitlement period. We neglect the opening of a new entitlement period to lighten the presentation, but it is taken into account when we come to compute the marginal tax rate in Section J, Figures 1.26 and 1.27 .

Figure 1.2: The dynamic aspects of the schedule



Note: This figure displays the monthly benefits b of an individual eligible for 6 months of benefits at the start of her unemployment spell. She earns z for work while on claim in the fourth month. These earnings are taxed at rate τ, implying that benefits are reduced by the amount τz in month four. These saved benefits are carried over to the end of the initial entitlement period.

1.2.2 Knowledge of unemployed workers about the program

A survey conducted by the employment agency (Unédic [2012]) as well as interviews from the field (Issehnane et al. [2016]) show that the knowledge of unemployed workers about part-time unemployment benefits is very limited. The survey conducted in 2012 shows that 41,2% of job seekers do not know of the existence of the program and 33,6% know of its existence without being able to explain the rules framing the part-time unemployment benefits program. This lack of knowledge about the program is striking.

Le Barbanchon and Gonthier [2016] also conclude that a large proportion of job seekers do not know the rules. The authors study the rules prevailing before 2006. At this time, specific criteria had to be met to be eligible for part-time benefits. First, the number of hours worked could not exceed 136 hours per month, which amounts to 86% of a full-time job. Second, the corresponding gross wage could not go beyond 70% of the wage earned before the unemployment spell. This implies that earnings drop at those thresholds (136 hours per month or 70% of the last wage). These notches should create incentives to move from a point just above the notch, in particular in the dominated area, to a point just below the notch. However, the authors do not observe bunching at those cutoffs. The lack of knowledge regarding the rules may explain why a large proportion of job seekers do not bunch at cutoffs.

1.3 Theoretical framework

This section starts by presenting a simple job search model which explains the consequences of part-time unemployment benefits on the behavior of unemployed workers before analyzing the impact of the transmission and reception of information about the existence of this scheme.

1.3.1 The model

We analyze the behavior of unemployed workers who look for regular jobs that yield a present value higher than the present value of unemployment. The value of these jobs is denoted by W and the effort devoted to job search is denoted by e.[14] The per period utility derived from consumption $c \geq 0$ and search effort $e \geq 0$ is equal to

$$v(c) - e$$

where v is an increasing and concave function. Engaging in search effort e yields regular job arrival probability equal to $\lambda(e)$, where $\lambda(e) \in (0, 1)$ is an increasing and concave function of the search effort. Time is discrete and the discount factor is denoted by $\beta \in (0, 1)$.

To account for the possibility of working on non-regular jobs, it is assumed that, in each period, unemployed workers can get an offer to work on a one-period job.[15] In each period t, the earnings z_t associated with these jobs are independently drawn in a stationary distribution.[16] There is a fixed cost of working denoted by $\kappa > 0$. Moreover, we start by assuming that $\lambda(e)$, the exit rate from unemployment toward regular jobs, does not directly depend on the choice to work on non-regular jobs while on claim. This assumption, which will be modified later on, allows us to clearly exhibit an important source of lock-in effects.

As explained above, the part-time unemployment scheme allows individuals to combine their unemployment benefits and the share $1 - \tau$ of their labor earnings,

[14] We neglect the heterogeneity of regular jobs for the sake of simplification insofar as we do not have precise information on wages in our data. Adding this heterogeneity would not alter the qualitative predictions of the model regarding the impact of part-time unemployment benefits on the unemployment exit rate.

[15] It is possible to enrich the model by introducing a search effort for non-regular jobs in order to take into account the substitution between different job search channels (see e.g: Moscarini [2001], van den Berg and van der Klaauw [2006], Marinescu and Skandalis [2021]). This would accentuate the negative effect on the unemployment exit rate of part-time unemployment benefits linked to anticipations but would not modify the qualitative predictions concerning the unemployment exit rate. We do not integrate this aspect of the behavior of job seekers for the sake of simplicity insofar as we do not have precise data on job search activity.

[16] Note that this distribution may have a support including zero earnings which could be interpreted as a situation without job offer.

implying that their monthly income in period t is equal to $\max[b(B_t) + (1-\tau)z_t, z_t]$ where $b(B_t)$ is defined by equation (1.1) and B_t by the law of motion (1.3).

In every period, unemployed workers choose their search effort and whether to take work while on claim or not. The value function of unemployed workers is

$$U(B_t) = \mathbb{E}_t \left\{ \max_{(e_t \geq 0, \Omega_t)} v(c_t) - e_t + \beta \left[\lambda(e_t) W + (1 - \lambda(e_t)) U(B_{t+1}) \right] \right\} \tag{1.4}$$

where

$$c_t = \Omega_t \left(\max[b(B_t) + (1-\tau)z_t, z_t] - \kappa \right) + (1 - \Omega_t) b(B_t)$$

subject to the law of motion (1.3). $\mathbb{E}_t$ is the expectation operator with respect to the future values of z_t conditional on the information available in period t; and $\Omega_t \in \{0, 1\}$ is an indicator variable equal to 1 if the unemployed worker decides to work while on claim and to zero otherwise.

The optimal decision to work while on claim relies on the comparison of the gains, equal to the earnings z_t, with the costs equal to the sum of the taxed earnings and the fixed cost κ. The tax on earnings from work while on claim depends on the tax τ on current labor earnings and on the probability that the taxed earnings will be retrieved after the benefits exhaustion date. More precisely, the marginal tax rate is[17]

$$m_t = \tau \left[1 - \beta^{T-t} \mathbb{E}_t \left(\prod_{j=t}^{T-1} \left[1 - \lambda(e_j) \right] \right) \right] \tag{1.5}$$

where $T > t$ denotes the endogenous benefits exhaustion date such that $B_T = 0$.

The model implies that it is worth working while on claim if the net gains, equal to $(1 - m_t)z_t$, are larger than the costs κ, or

$$(1 - m_t) z_t > \kappa \tag{1.6}$$

The decision to work while on claim crucially depends on the marginal tax rate m_t, which has two components: the proportional tax rate τ on current earnings and the expected returns induced by current earnings reported at the end of the entitlement period, that will be obtained only if the person is still unemployed in this period. The marginal tax rate is higher for people who exit unemployment faster, because the probability that they will reach the exhaustion date while unemployed is smaller. The marginal tax rate decreases over time because the probability that they will reach the exhaustion date while unemployed increases over time.[18] This means that

[17] See Appendix B which presents the solution of the model.

[18] Figures 1.26 and 1.27 display the distribution of the estimates of the marginal tax rate for each individual × month observation and the evolution of the average marginal tax rate over the employ-

the incentives to work while on claim increase along the unemployment duration, implying that some individuals may decide to work while on claim only after a certain unemployment spell.

The forward-looking nature of the optimization problem of unemployed workers implies that the value function $U(B_t)$ of an individual who does not work while on claim in period t can depend on the future values of the marginal tax rate, if the individual anticipates that it could be worth working while on claim in the future. This means that part-time unemployment benefits can influence job search effort even for individuals who do not work while on claim, because the possibility of working while on claim in the future influences current job search behavior.

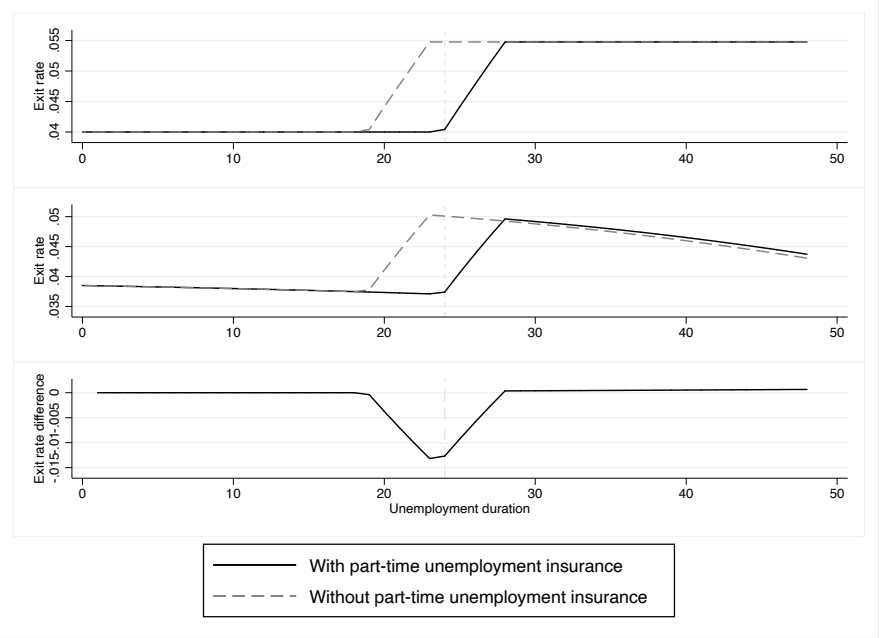
To illustrate the properties of the model, let us display the exit rate from unemployment in two cases: 1/ when the marginal tax rate m_t is smaller than one ($\tau = 0.85$) meaning that it can be worth working while on claim; 2/ when $m_t \geq 1$, meaning that there are no incentives to work while on claim. The model is calibrated to reproduce the typical shape of the exit rate from unemployment, which is approximately constant until the exhaustion date of benefits approaches, as shown in Figure 1.12, which displays the exit rate from unemployment to employment and the survival rate in unemployment for the non-treated individuals.[19] This corresponds to situations in which unemployed workers produce the minimum level of effort $e_t = 0$ until the exhaustion date approaches and then increase their effort from this date.[20] The model predicts that the search effort reaches its maximum value at the exhaustion date and remains constant thereafter. Marinescu and Skandalis [2021] show that this property is relevant on French data and that the drop in the rate of exit from unemployment to employment after the exhaustion date comes from composition effects, due to the exit around the exhaustion date of unemployed people who find a job more easily. To illustrate this point, the middle panel of Figure 1.3 displays the outflow rate from unemployment to employment for a population composed of two types of unemployed with different job search efficiency. The bottom chart shows the difference in exit rates between environments with and without part-time unemployment benefits for this population. The possibility of receiving unemployment benefits after the initial date acquired by work while on claim delays the date on which the unemployed increase their search effort. Thus, the bottom panel of Figures 1.3 and 1.13 clearly show that the impact of part-time unemployment benefits on the exit rate from unemployment to employment and on the unemployment survival rate is significant close to the benefit exhaustion date.

ment spell for the population of our field experiment.

[19] The differences between treated and non-treated individuals will be shown in Section 1.5.3.

[20] Note that the value zero of the minimum level of effort is a normalization. This minimum effort level can correspond to the minimum effort that the public employment agency can require.

Figure 1.3: The exit rate from unemployment in the job search model



Note: The job search model is simulated assuming that the monthly discount factor $\beta = 0.996$, which corresponds to an annual discount rate equal to 5%; $v(c) = \log(c)$; $\lambda(e) = \lambda_0 \left(\lambda_0 - \exp^{-\gamma e}\right)$; the value of benefits is normalized to one: $b = 1$; the replacement ratio is equal to 0.5 implying that the wage of regular jobs, the duration of which is infinite and which yield the value W is equal to 2; the initial potential benefit duration equals 24 months; the tax rate on earnings from work while on claim $\tau = 0.85$; the share α of current earnings reported at the end of the entitlement period, that will be obtained only if the person is still unemployed in this period, is equal to τ; the distribution of earnings from work while on claim z_t is uniform on the interval $[0, b]$; the fixed cost of work while on claim $\kappa = 0.12$; individuals still unemployed after the benefits exhaustion date get an income equal to 0.01 to ensure that they can choose to optimally produce a positive effort after the benefits exhaustion date. The top panel displays the case where $\gamma = 0.1$; $\lambda_0 = 0.05$; $\lambda_1 = 1.4$ which implies that the exit rate of unemployed workers increases around 24 months when there is part-time unemployment benefits and around 19 months otherwise. In this example, unemployed workers work 5 months while on claim in the 24 first months of unemployment when there is part-time unemployment benefits. The middle panel displays the exit rate from unemployment of a population composed of 95% of unemployed workers described in the top panel and 5% whose search effectiveness is too low to induce a positive search effort, implying that their exit rate from unemployment is equal to 1%. The bottom panel displays the difference between the exit rates of the middle panel.

To this point, the model implies that the possibility of working while on claim has lock-in effects which arise only from the increase in the value of unemployment induced by the possibility of working while on claim. The stepping stone effect, which has so far been left out of consideration, can arise from the relation between work on non-regular jobs and the arrival rate of regular job offers. This relation can be incorporated into the model by assuming that the arrival rate of regular job offers λ is a function of the job search effort and of the decision to work while on claim: $\lambda(e_t, \Omega_t)$. The stepping stone effect arises if the arrival rate of regular job offers increases when individuals work on non-regular jobs while on claim; this might happen because working on non-regular jobs improves work experience, sends good signals to employers, or facilitates the access to information about regular job offers

through networks. The stepping stone effect induces individuals to work while on claim more frequently and sooner in the unemployment spell.[21]

However, the relation between the arrival rate of regular job offers and work on non-regular jobs while on claim can also amplify the lock-in effect if working on non-regular jobs reduces the time available to hunt for regular jobs or sends a negative signal about the quality of workers to employers (Farber et al. [2017]). In this case, individuals have less incentives to work on non-regular jobs while on claim.

All in all, the model shows that there is a monotone mapping between the marginal tax rate on earnings from work while on claim and the propensity to work while on claim; and that part-time unemployment benefits programs influence the unemployment exit rate through two effects: the *anticipation effect*, which reflects the impact of the possibility of working while on claim on the search effort, and the *direct effect of working while on claim*, $d\lambda(e_t, \Omega_t)/d\Omega_t$.[22] Moreover, the model predicts that the impact of part-time unemployment benefits on the exit rate from unemployment to employment is significant close to the benefit exhaustion date.

1.3.2 The consequences of information provision

The provision of information to the treatment group is justified by the assumption that individuals are not fully informed and may have biased beliefs about the part-time unemployment insurance scheme. A natural interpretation of bias in beliefs is given by the literature on behavioral inattention (Gabaix [2019]) which assumes that the belief in the marginal tax rate, m_t, is a convex combination of the actual rate m_t^a and a "default" rate, m_t^d, i.e., $m_t = \alpha m_t^a + (1-\alpha)m_t^d$, where $\alpha \in [0,1]$ characterizes the degree of attention. In this framework, the provision of information increases the value of α.

Individuals can underestimate the gains from working while on claim, for instance because they think that they would lose all their labor earnings or their unemployment benefits if they were working while on claim. For these individuals, whose "default" tax rate is above the actual tax rate, the provision of information is equivalent to the announcement of a drop in the marginal tax rate, which should, ac-

[21] The gain from working while on claim with earnings z_t, equal to

$$\Delta \simeq [z_t(1-\tau) - \kappa]\, v'(b) + \tau z U'(B_t) + \beta\, [\lambda(e_t,1) - \lambda(e_t,0)]\, [W - U(B_{t+1})]$$

is higher in the presence of stepping stones effects, *i.e.* $\lambda(e_t,1) - \lambda(e_t,0) > 0$.

[22] Hence, as stressed by Kyyrä [2010], the timing-of-events approach, which estimates the direct effect of working while on claim can be relevant to an estimate of the effect of working on non-regular jobs under the existing unemployment insurance scheme, but cannot estimate the full impact of part-time unemployment insurance schemes.

cording to our model, boost part-time unemployment. Note that for those who did not use the scheme before the informational treatment simply because they were not aware that it existed, the "default" rate can be interpreted as being equal to one, since they thought that is was not possible to work while on claim before getting the information from our treatment.

For individuals who overestimate the gains, the information provision is equivalent to the announcement of an increase in the marginal tax rate which should induce less work on non-regular jobs while on claim.

The definition of the marginal rate m_t shows that it can be influenced in different ways by the informational treatment, which can change beliefs on the tax rate τ on current labor earnings but also any anticipation which affects the future value of unemployment U_{t+1}. For instance, the informational treatment can be interpreted by some individuals as a message on the frequency of the offers of non-regular or regular jobs. According to the model, if individuals interpret the informational treatment as a good signal indicating the availability of non-regular jobs, this raises their expected utility $\mathbb{E}_t(U_{t+1})$. They decrease their search effort for regular jobs and their marginal rate m_t drops. The informational provision might also be interpreted as bad signal indicating the scarcity of regular jobs, which now reduces the expected utility of job seekers.[23] But, like the good signal, it decreases the search effort for regular jobs and the marginal tax rate m_t.[24] Whatever, all these changes affect the marginal rate m_t, which is the key variable of interest to interpret the consequence of the informational treatment on the partial unemployment take-up as well as on the exit rate from unemployment.[25]

Hence, the fact that we find a positive impact of our treatment on the part-time unemployment benefits take-up, as will be shown below, means that treated individuals overestimated, on average, and for many different possible reasons, the marginal tax rate on earnings from work while on claim before the treatment.[26] The model shows that this decrease in the marginal tax rate has an impact of ambiguous sign on labor supply and on unemployment duration. Our empirical analysis aims

[23] To the best of our knowledge, this type of channel, which has been analyzed in the context of monetary economics which have exhibited signalling effects of monetary policy (Melosi [2016], Nakamura and Steinsson [2018]), has not been studied in labor economics.

[24] Note that an effective variation in the marginal tax rate τ on current labor earnings can be interpreted by job seekers as a sign of changes in the rate of arrival of job offers anticipated by the government. As such, the diversity of possible interpretations of the informational treatment can also concern changes in the effective rules of part-time unemployment insurance.

[25] It is very unlikely that our information provision could have been interpreted as a threat likely to modify job search behavior in the French context where there is almost no control of job search activity. In line with our interpretation, Crépon et al. [2018] do not find any threat effect of notifications of training proposals in France.

[26] Altmann et al. [2021] find similar results in Denmark.

at exploring this impact.

1.4 Experimental Design and Data

1.4.1 Treatment

Our experiment consists in sending information about the part-time unemployment benefits scheme to unemployed workers eligible for unemployment benefits and recently registered at the unemployment agency. We have selected people who registered for unemployment benefits for the first time from July 1, 2016 and who will have been unemployed at most 7 months at the start of the treatment. Individuals of the treated group received 3 successive emails on 31 January, 28 February, and 31 March 2017. The emails were sent from the employment agency's mailing platform. The main text of the emails is as follows:

We inform you that you can work without losing your unemployment benefits. This opportunity to combine your wage and benefits allows you to:

- *Have earnings higher than your benefits, though without exceeding the amount of your former gross wage. Pôle Emploi only reduces your benefits by 70 cents per gross euro earned.*
- *Be entitled to benefits for a longer period. The number of days of benefits not received due to the accumulation of your earnings while on benefits are credited to your account.*

At the exhaustion of your benefits, you will be able to get new entitlement to unemployment benefits if you have done at least 150 hours of salaried activity.

This main text is accompanied by an example which introduces a hypothetical worker and displays what happens to his benefits if he works while on claim. An attached file provides further information about the example. The message also comprises a link to a web page of the public employment agency where it is possible to simulate the disposal income as a function of labor earnings.[27]

[27] The exact contents of the emails is presented in appendix G. We sent two different types of email. One type presents the gains from labor earnings in net terms (i.e. after payment of the employee's social contributions. Income taxes, which depend on the situation of each person, cannot be computed at this stage) and the other type in gross terms. Insofar as we do not detect any statistically significant difference between the effects induced by these two types of message, we do not consider this heterogeneity of treatment in what follows.

1.4.2 Implementation

The experimental design relies on three experimental groups : treated workers (the "treated group"), untreated workers in treated areas (the "control group"), untreated job seekers in non-treated areas (the "super control" group).

The steps taken to implement the experiment were very similar to those described in Crépon et al. [2013]. Randomization was implemented at both the labor market and individual levels. There are 856 public unemployment agencies, scattered across France. Each agency represents a small labor market, within which we may observe treatment externalities which may arise from information spillovers or displacement effects. On the other hand, the agencies cover areas that are sufficiently large, and workers in France are sufficiently immobile, that we can assume that no spillovers take place across areas covered by different agencies. To identify spillovers, we used a "super control" group as in Crépon et al. [2013]. First we stratified our sample at the agency level.[28] Within each stratum we randomly divided the 856 agencies into two groups that covered areas similar in size and with comparable local populations. One of the two groups consists of the non-treated areas, i.e. the "super control" group. The other group consists of the treated areas. For each treated area, we stratified the job seekers. Within each stratum we randomly assigned treatment with a probability of one-half, making half of job seekers in the treated areas effectively treated (i.e. received emails). Table 1.1 summarizes the experimental lay-out.

Table 1.1: Experimental lay-out

1^{st} level of randomization : Local agencies assignment				
	Treated areas		Untreated areas	All
Assignment prob.	4/5		1/5	
Number of agencies	687		171	858
Number of job seekers	118 229		29 649	147 878
2^{nd} level of randomization : Job seekers assignment				
	Treated (T)	Control (C)	Super-control (SC)	All
Assignment prob.	1/2	1/2		
Number of job seekers	59 112	59 117	29 649	147 878

Note: The upper part of this table reports the assignment to treatment probability of local agencies, the number of agencies and the number of job seekers assigned to treatment. The bottom part displays the assignment to treatment probability of job seekers in agencies assigned to treatment and the number of workers belonging to the treatment group (i.e. who received the emails), to the control group (i.e. who did not receive the emails but who were located in agencies in which other job seekers received emails) and to the super-control group (i.e. who were located in agencies in which nobody received the emails).

[28] We present summary statistics for the variables that were used for stratification in Table 1.11.

1.4.3 Data

We use three sources of data. First, an administrative database on job seekers provided by the employment agency (*Fichier National des Allocataires*). These records provide the individual socio-demographic characteristics (age, gender, education level, family situation,living area) and detailed information about all previous registrations (date of registrations, reason for registration, start and end dates of unemployment spells, the level of unemployment benefits, earnings and hours of work while on claim...).

A second data set comes from the hiring intentions of firms (*Déclarations Préalables A l'Embauche)*. Prior to hiring each employee, any employer from the private and semi-private sector has to fill out a form indicating the starting day, the type (permanent contract or fixed-term contract) and the expected duration of the contract. This allows us to acquire information about the employment status of all randomized individuals. As this form only reports the intention to hire, we do not know whether the individual has actually been hired[29] and whether the individual stays in the firm for the entire expected contract duration.

Our third source of data is email tracking statistics. The information treatment was sent by email from the employment agency's mailing tool (*Gestion des Messages Entrants*). For all treated individuals, it lists basic email activity: whether they have opened the email and/or clicked within the email.

1.4.4 Sample and summary statistics

The randomization was implemented in December 2016. The effects of the experiment critically depend on the knowledge of job seekers about part-time unemployment insurance. Job seekers with multiple spells have a better knowledge of the unemployment system, and are thus less likely to react to our information intervention. We identified job seekers who are entitled for the very first time to unemployment benefits between 1 July 2016 and 30 November 2016. We excluded job seekers subject to very specific rules, such as recurrent temporary workers (in temp agencies), childminders, entrepreneurs, artists, and technicians working in the culture sector, as well as job seekers who had already worked while on claim between their entry into unemployment and November 2016.

The selection of newly compensated unemployed persons allows us to select those less likely to know the rules of part-time unemployment. These persons might react in a specific way to the treatment to the extent that newly unemployed people

[29] 90% of hiring intentions do become effective hires.

might react less to part-time unemployment benefits because their potential duration of benefits is longer than those of unemployed people with longer spells. The analysis of the heterogeneity effects of the treatment will shed light on this issue.

This procedure resulted in an experimental sample of 147,878 job seekers who have been randomized into treated (T), control (C) and super control groups (SC). In the treatment effect analysis, we apply an additional filter to the experimental sample, retaining only individuals who were still on claim and did not experience part-time unemployment between the randomization date and the first sending. Our final sample is then composed of 115,547 individuals.[30]

Table 1.11 in appendix I presents summary statistics regarding the final sample for the treated group, the control group and the super control group before program assignment, as well as balancing tests.[31] A large share of individuals (38%) is under 25 years old in our sample while they are 14% in the whole population of job seekers. This is not surprising given that we only select job seekers who have never been eligible for unemployment benefits. Nearly half of job seekers are women and 33% of the sample has a university degree. At the date of the first email (January 31, 2017), individuals have on average been unemployed for 108 days, which is consistent with the selection of job seekers who registered between 1 July 2016 and 30 November 2016. Finally, potential benefit duration - i.e. the difference between the *initial* date of benefit exhaustion and the starting date benefit of entitlement - is equal to 621 days on average and this potential duration is equal or longer than 2 years for 56% of job seekers (2 years being the maximum benefits duration for individuals under 53 years old). Figure 1.14 displays the distribution of potential benefit duration. 43% of individuals are entitled to 730 days of benefits against 30% in the whole population of job seekers. This also reflects the fact that we select individuals who are entitled to unemployment benefits for the first time. In this case, they are more likely to have experienced a long period of employment.

The last five rows of Table 1.11 present summary statistics about the employment agencies. The average number of job seekers by employment agency is 4,362 among

[30]The further away the entry into unemployment is from the treatment date, the higher the chances for an individual to have been filtered out from our final sample. Thus, for people with higher elapsed unemployment duration at treatment date, our sample is less representative of newly registered job seekers. We check below that the treatment effect does not significantly depend on the elapsed unemployment duration at treatment date.

[31]Table 1.12 in Appendix I reports summary statistics for the whole sample, before dropping the observations for individuals who were not on claim or who had already experienced part-time unemployment at the date of the first sending. It shows that the share of individuals who were still on claim and the share of those who had never experienced part-time unemployment at the date of the first sending are not statistically different in the treated group, the control group and the super control group before program assignment. Figures 1.20 and 1.21 in Appendix J provide additional descriptive statistics on work while on claim by group.

which 224 are in our sample. The unemployment rate is around 13.7%. Both the share of part-time unemployed workers and the share of recurrent job seekers is about 43%.

The last three columns of Table 1.11 report the *p*-values for the difference between those assigned to treatment (T) and those assigned to control (C) (column 5), the difference between those assigned to treatment (T) and the non assigned (C + SC), and for the joint significance of assignment status (T, C and SC). We do not observe any significant differences between our groups.

1.5 Results

This section provides information about the intensity of the informational treatment before looking at its impact on work while on claim, on unemployment, on unemployment insurance expenditure and on welfare.

1.5.1 Treatment intensity

Results concerning treatment intensity are reported in Table 1.6. The share of treated individuals who opened at least one email after the three mailings is about 85%. This figure is relatively high and can be related to the fact that we targeted first time claimants. Furthermore, among these 85%, the vast majority opened the first email. The proportion of claimants who used the simulator is much lower: about 7.5% used it at least once.

Regarding the heterogeneity of the opening rate, we observe that the share of job seekers who opened at least one email is high, above 70%, among all groups reported in Table 1.6. The most substantial differences in opening rates are associated with education: + 18,1 percentage points for individuals with higher education levels compared to people with lower education levels; age: + 7,6 percentage points for prime age people compared to seniors; gender: + 4,1 percentage points for women; and the daily reference wage: + 3,7 percentage points for people with a daily reference wage above the mean.

Although compliance in the experiment is difficult to interpret for people who have opened the emails and used the simulator, the recent contribution from Altmann et al. [2021] finds that the impact of providing information about unemployment insurance rules has a significant impact on the knowledge of unemployed workers in a context similar to ours, where they have pronounced information gaps about complex part-time unemployment benefits. To the extent that a large proportion of the unemployed opened the emails we sent, this suggests that our provision

of information may have altered the propensity to work while on claim.

1.5.2 Work while on claim

This section is devoted to the effects of the treatment on work while on claim. Work while on claim includes all those who work while continuing to receive unemployment benefits during the current month.[32] Hence, for our purposes hours of work while on claim are defined as the hours of work of individuals who do continue to receive unemployment benefits during the current month. We start by presenting our statistical model before looking at the effects of the treatment by comparing the treated, the control, and the super control groups. We also explore the potential heterogeneous effects of the treatment within the treated group. We then go on to compare the characteristics of individuals who work while on claim in the treated and control groups, in order to gauge the external validity of our results.

Statistical model

The intention to treat (ITT) estimates are obtained from the following model :

$$y_i = \alpha + \beta Z_i + \delta C_i + \gamma X_i + \epsilon_i \tag{1.7}$$

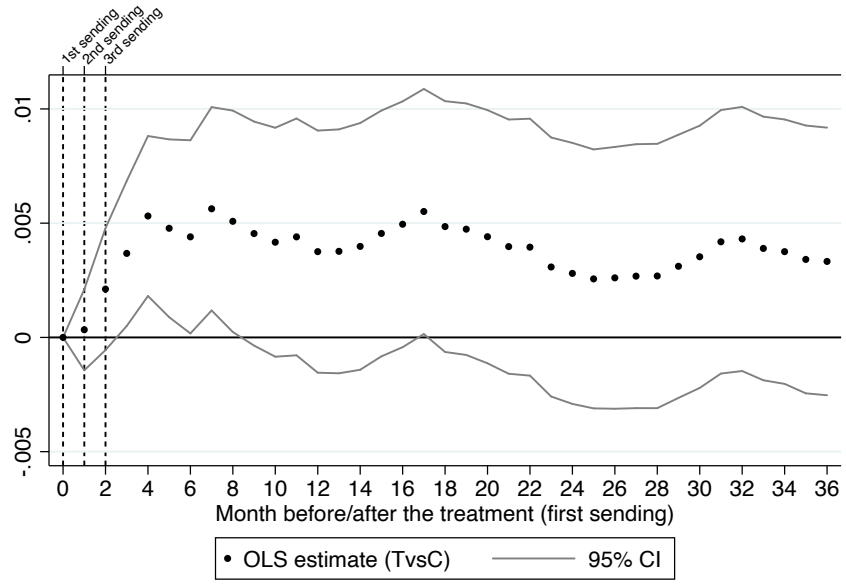
where Z_i is a dummy for being treated and C_i is a dummy for being in a treated area (i.e. being either in the treated group or in the control group but not in the super control group). Then, β is the difference between the treated group and the control group. δ is the difference between the control group and the super control group i.e. the effect of being untreated in a treated zone. X_i is a vector of control variables that includes the variables reported in the summary statistics (Table 1.11) as well as entry months and regional fixed effects.

[32] According to the regulations, individuals whose monthly earnings exceed the earnings used to compute their unemployment benefits do not get any unemployment benefits in the current month but are still on claim if they continue to register with the unemployment agency at the end of the month. By definition, an individual continues to be registered in the current month only if he registers during that month. Individuals who do not register during the current month lose the benefits associated with registration. Registration at the unemployment agency can be beneficial for reasons other than receiving unemployment benefits, e.g. getting counselling to find a better job, avoiding the time-consuming process of launching a fresh entitlement period from scratch, getting free access to several public services... We take the view that such individuals — registered at the unemployment agency and eligible for unemployment benefits but not actually receiving them because their earnings are too high — are not unemployed, and therefore not part-time unemployed.

Treated group versus control group

Regarding the difference between the treated group and the control group (β), we first consider the impact of the treatment on work while on claim at the extensive margin (i.e. the choice between working or not working while on claim), which is measured by the indicator variable equal to one from the first month in which the individual starts working while on claim. Figure 1.4 shows that the treatment has a quick positive impact on the extensive margin, which becomes significant three months after the first email, where work while on claim increases by 0.4 percentage points, which corresponds to an increase of 6% compared to non-treated individuals – see Table 1.13, Column 1. Work while on claim increases until six months after the first email by about 0.5 percentage points. After six months, the impact of the treatment stops increasing and remains positive. The fluctuations in the effect of the treatment, which is stronger in spring and summer, is associated with the seasonality of work while on claim illustrated on Figure 1.19.

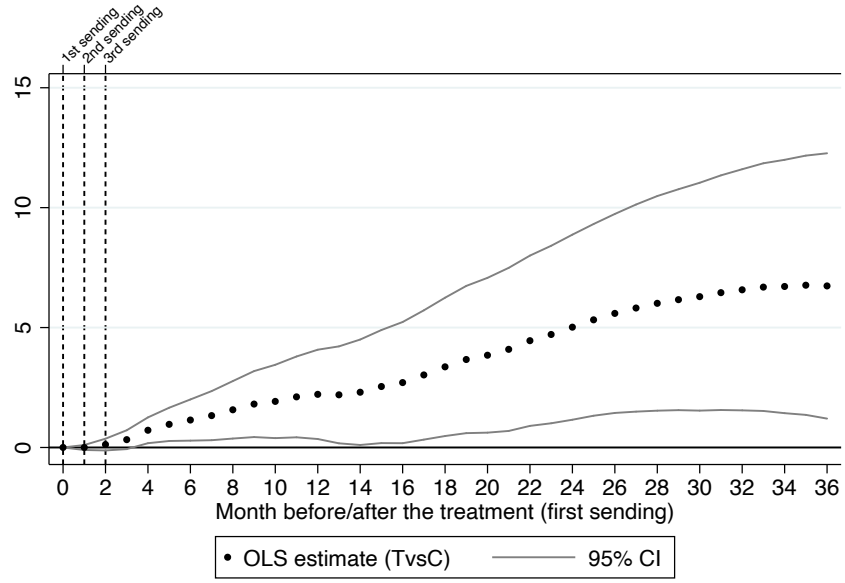
Figure 1.4: Intention to treat effects on work while on claim at the extensive margin



Note: Each black dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (1.7)) on the indicator variable equal to one from the first month in which the individuals starts working while on claim (the variable remains equal to one in months in which the individual does not work while on claim but has worked while on claim previously). The grey lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. All estimations include covariates that correspond to stratum variables reported in Table 1.11 as well as entry months and regional fixed effects. The results for 3, 6, 12 and 36 months durations are presented in Table 1.13 in Appendix I.

Figure 1.5 shows that the treatment has a significant effect on the cumulative number of hours of work while on claim. The impact amounts to about 7 supplementary hours after 36 months for people assigned to treatment. It is striking that

Figure 1.5: Intention to treat effects on the cumulative number of hours worked while on claim



Note: Each black dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (1.7)). The grey lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. All estimations include covariates that correspond to stratum variables reported in Table 1.11 as well as entry months and regional fixed effects.

the impact of the treatment does not dampen over a quite long period of time, up to three years. This suggests that whatever information members of the control group were able to acquire about part-time unemployment benefits over the period after the treatment did not sufficiently improve to catch up to the level of supplementary information provided by our emails.

Table 1.7 reports the results for the estimation of equation (1.7) for different outcomes and time horizons. From Panel A, we can see that the assignment to treatment increases the frequency of months in which individuals work while on claim by about 4.5% from 3 months to 36 months after the treatment. Panels B and C show that the treatment has about the same impact, in percentage terms, on the cumulative number of hours of work and on cumulative earnings from work while on claim, 3, 12 and 36 months after the treatment.

Table 1.8 reports the results for the effects of the treatment at the intensive margin, i.e. on the number of hours of work while on claim and on the earnings from work while on claim for the subset of job seekers who work at least one day while on claim. Table 1.8 shows that the impact of the treatment on the number of hours of work while on claim and on the earnings while on claim *conditional* on working while on claim is barely significant and small. This suggests that the treatment has

a negligible impact on work while on claim at the intensive margin.[33]

The robustness of these results to randomization based inference is presented in Appendix D and in Table 1.14. Overall, the p-values obtained with randomization inference tests are very close to the cluster-robust model based p-values, which is not surprising, considering the sample size in our experiment. Both conventional and randomized based inference thus support the conclusion that the treatment did have a statistically significant effect on the propensity to work while on claim.

Control group versus super control group

The propensity to work while on claim of the control group can be impacted by the informational treatment through two effects: *i*) The transmission of information from treated individuals, which can increase take-up in the control group, as well as in the treated group. *ii*) Displacement effects arising from the increase in the take-up of treated individuals. These displacement effects can decrease the take-up in the control group, as suggested by Crépon et al. [2013], who show that unemployed workers more intensively supported by public employment services crowd out other job seekers in a context similar to ours.

Figure 1.15 shows that the number of hours worked while on claim is not statistically different in the control and the super control group at all available time horizons. This result is confirmed by supplementary Tables 1.7 and 1.8 which show that there is no statistically significant difference between any outcome of the control group and of the super control group. Some coefficients reported in Table 1.8 are relatively large, but with p-values always larger than 19%. Further analysis, presented in Appendix F, shows that that there is no evidence that the part-time unemployment take-up of the control group increases, compared with the super control group, for all levels of the local unemployment rate. This suggests that there are no significant information spillovers to the control group arising from the treatment. Hence, we can be confident that the comparison of the outcomes of the treated group and the control group yields the net effect of the treatment on those who were assigned to it.

[33]It is possible that individuals induced to work while on claim by the treatment are different from those who work while on claim in the absence of our treatment. This issue is discussed in Appendix F. Moreover, as remarked above, for people with higher elapsed unemployment duration at treatment date, our sample is less representative of newly registered job seekers. However, as shown in Table 1.18, the treatment effect does not significantly depend on the elapsed unemployment duration at treatment date.

Heterogeneous effects of the treatment

To investigate the heterogeneity of the treatment effect in a disciplined fashion, we apply the machine-learning approach developed by Chernozhukov et al. [2018]. This allows us to analyze potential heterogeneous effects while being agnostic about the source of heterogeneity, which can arise from any combination of our covariates. More specifically, we test for the presence of heterogeneity and estimate average treatment effects sorted by groups as well as average characteristics of the most and least affected units. To analyze treatment effect heterogeneity, we restricted our analysis to observations from the treated group and the control group. Details for the estimation procedure are presented in Appendix E.

The absence of heterogeneity can be rejected (at 10% significance level) for one outcome, namely the probability to work while on claim at least once one year after the treatment – see Table 1.16. Apart from this, we do not detect any significant heterogeneity for the other outcomes of interest (cumulative part-time unemployment activity and exit from unemployment). Overall, these results provide only limited evidence of heterogeneity in the treatment effect. This may be due to the absence of such heterogeneity or to the inability of our machine-learning proxies to detect it.

Focusing on the heterogeneity in the treatment effect on the probability to work while on claim at least once one year after the treatment, Figure 1.16 reports the estimated conditional average treatment effect (CATE) for five heterogeneous groups induced by our machine-learning proxy. Although point estimates show some evidence of heterogeneous effects, differences across groups are not statistically different from the whole average effect. Looking at each group separately, confidence intervals indicate that the treatment had no significant effect on part-time unemployment benefits take-up among the four least affected groups, but a significantly positive effect among the most impacted group, which corresponds to the top 5% (p-value = 0.038 with Linear Regression proxy). If we focus on the most affected group vs. the least affected group (bottom 50%), we are able to reject the null hypothesis that the two coefficients are equal at 5% significance level (p-value = 0.047) (see Table 1.17). Finally, we can observe that all point estimates are positive (or very mildly negative), indicating that our informational treatment did not induce any group to work less while on claim.

Table 1.9 provides further evidence by comparing the characteristics of individuals in the most affected vs. less affected groups. Looking at demographic characteristics, the most affected are more likely to be young and to have an intermediate educational level. Regarding unemployment spell related variables, individuals in the most affected group are found to have a higher daily reference wage, entering

unemployment after shorter duration contracts and having a lower potential benefit duration. Looking at local environment characteristics, people are more impacted by the treatment when unemployment is lower and part-time unemployment more frequent. Some characteristics of people who respond most strongly to treatment – high pay, short benefit duration – are similar to those of people who reach benefit exhaustion date more frequently while still unemployed. This relationship is consistent with the predictions of the job search model. But the lack of statistical power, linked to the low impact of the treatment, does not allow us to analyze this subject further. The analysis of the heterogeneous effects of the treatment also suggests that the selection of newly compensated unemployed people – and thus with longer potential benefit duration than those with long unemployment spell – in our experiment may have reduced the impact of the treatment.

The heterogeneity of the impact of the informational treatment on the probability to work while on claim may arise from differences in dealing with information received by email, and from differences in the propensity to work while on claim. The results presented in Appendix F indicate that treated individuals who work while on claim have similar observable characteristics as non-treated individuals who have a high propensity to work while on claim. This suggests that the impact of the informational treatment does not arise from differences in dealing with information received by email, but from differences in the propensity to work while on claim associated with differences in the perceived marginal tax on earnings from while on claim.

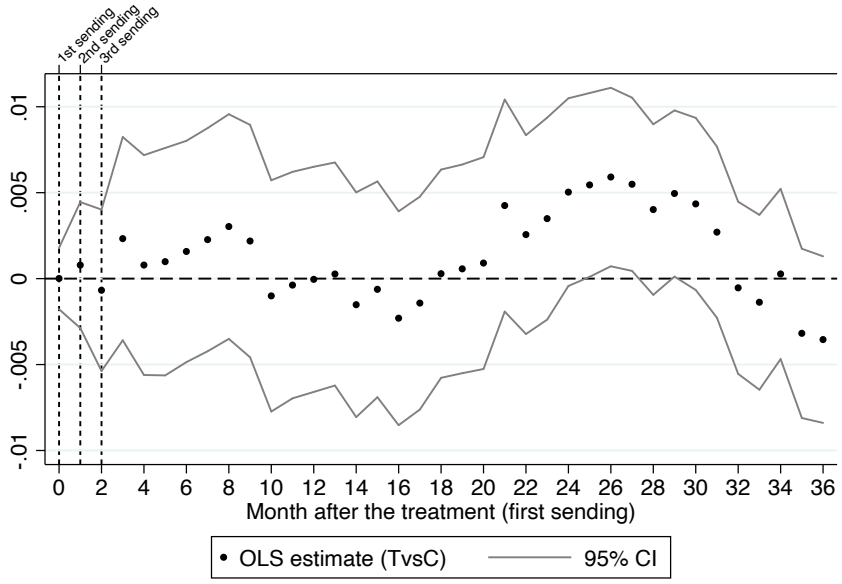
1.5.3 Unemployment

According to the theoretical job search model, part-time unemployment benefits have an ambiguous impact on unemployment duration, which can either decrease if work while on claim significantly boosts the access to regular jobs or increase if lock-in effects dominate. Specifically, as shown in Figure 1.13, the model predicts that the impact of part-time unemployment benefits on the unemployment survival rate is significant around the benefit exhaustion date if part-time unemployment benefits slows the exit rate. In what follows, we begin by analyzing the impact of the informational treatment on the survival in compensated unemployment, on the survival in unemployment –whether compensated, non-compensated or part-time –, and on job quality.

Compensated unemployment

In our context, it is clear that working while on claim is associated with longer unemployment spells. Figure 1.6 shows that the probability to be in compensated unemployment significantly increases among treated individuals around 26 months after the treatment date. Figure 1.7 further indicates that this pattern is driven by job seekers with potential benefit duration equal or above 2 years whose treatment effect is stronger in magnitude and significant on a longer period.[34]

Figure 1.6: Intention to treat effects on survival in compensated unemployment



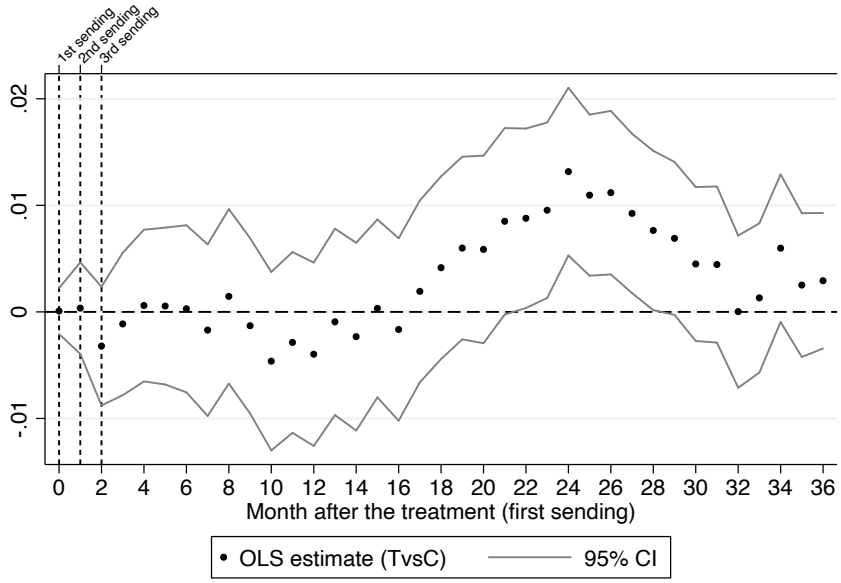
Note: Each black dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (1.7)) on the probability to have exited compensated unemployment in the month. The grey lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

The results displayed on Figures 1.6 and 1.7 are in line with the properties of the job search model – displayed on Figure 1.3 –, according to which the probability to remain in compensated unemployment starts to be higher in the treated group than in the control group *only* a few months before the initial date of benefits exhaustion and reaches a peak around this date.[35] This is consistent with a significant impact of the anticipation effect and/or a lock-in effect induced by the fact that working while on claim reduces the time spent looking for a job. This also suggests that the steep-

[34] By potential benefit duration we mean here the duration scheduled at the start of the entitlement period which corresponds to duration for individuals who do not work while on claim. As explained above, working while on claim delays the exhaustion date.

[35] Supplementary figures 1.22 and 1.23 report this result and show that it is of greater amplitude for job seekers entitled to 2 years or more of unemployment benefits.

Figure 1.7: Intention to treat effects on survival in compensated unemployment among job seekers with potential benefit duration superior or equal to 2 years



Note: Each black dot denotes the point estimate for intention to treat effect at a given unemployment spell based on OLS regressions (i.e. coefficient β in equation (1.7)) on the probability to be in compensated unemployment in the month. The grey lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

ing stone effect has at best a very limited impact. However, these results are only suggestive to the extent that the increase in the duration of compensated unemployment could entirely be mechanical. By taking-up part-time benefits, unemployed workers get a longer potential benefits duration.

Exit toward regular employment

The results concerning survival in compensated unemployment may simply reflect the fact that job seekers in the control group exhausted their unemployment benefits faster without necessarily finding regular jobs. We investigate this question by directly looking at exits toward regular employment, or in other terms at survival in unemployment including compensated, non-compensated and part-time unemployment.[36] Still in line with the job search model, survival in unemployment of treated individuals begins to increase relative to that of the control group around 18 months of unemployment and becomes significantly higher by 0.5 percentage points at 95% confidence level around 25 months (Figure 1.17). After 29 months, the treatment and control group difference in the survival rate diminishes. Then, it

[36]See Appendix C for the definition of variables.

vanishes after 33 months.

Table 1.2: Treatment effect on the probability to be in regular employment before the initial date of benefit exhaustion

			Potential Benefit Duration			
	All sample		< 730 days		≥ 730 days	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A : Prob. to be in regular employment in the last quarter						
Treated (β)	-0.0048	-0.0052*	0.0012	0.0000	-0.0093**	-0.0096**
	(0.0032)	(0.0031)	(0.0047)	(0.0044)	(0.0044)	(0.0043)
	[0.129]	[0.094]	[0.792]	[0.995]	[0.035]	[0.028]
In a treated area (δ)	-0.0025	-0.0019	-0.0052	-0.0070	-0.0006	0.0028
	(0.0055)	(0.0044)	(0.0075)	(0.0062)	(0.0063)	(0.0055)
	[0.648]	[0.660]	[0.487]	[0.263]	[0.927]	[0.609]
Mean super control	0.47		0.41		0.51	
Panel B : Prob. to be in regular employment in the last month						
Treated (β)	-0.0056*	-0.0059**	0.0033	0.0020	-0.0122***	-0.0125***
	(0.0031)	(0.0030)	(0.0047)	(0.0045)	(0.0043)	(0.0042)
	[0.068]	[0.046]	[0.493]	[0.648]	[0.004]	[0.003]
In a treated area (δ)	0.0024	0.0015	-0.0019	-0.0052	0.0055	0.0072
	(0.0053)	(0.0042)	(0.0074)	(0.0060)	(0.0062)	(0.0055)
	[0.655]	[0.725]	[0.798]	[0.385]	[0.371]	[0.193]
Mean super control	0.40		0.34		0.44	
Covariates	No	Yes	No	Yes	No	Yes
N	115547	115547	50887	50887	64660	64660

Note : Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors are reported in parenthesis, they are robust to heteroskedasticity and clustered at the local agency level. p-values are reported below standard errors in brackets. Covariates include all stratum variables reported in table 1.11 as well as entry months and regions fixed effects. N indicates the number of observations which is equal to the number of individuals. Outcome in panel A is a dummy equal to one if the individual is in regular employment during the last quarter before the initial date of benefit exhaustion. The outcome in panel B is a dummy equal to one if the individual is in regular employment in the last month before the benefit exhaustion date.

The impact of the treatment on the exit rate toward regular employment is further documented by Table 1.2 which shows that the treatment has a negative impact on the probability to have a regular job in the last quarter and in the last month before the initial date of benefits exhaustion. Hence, the treatment has significant and sizeable lock-in effects: it increases by 6% the share of job seekers working while on claim about 3 months after the start of the treatment and reduces by 1.5% (Table 1.2, Panel B col. 1) the probability to have a regular job the last month before the initial exhaustion date. The lock-in effects are larger for individuals whose potential benefit duration is longer: the treatment decreases the probability to have a regular job the last month before the exhaustion date by 2.8% (compared to 1.5%) when the potential benefit duration is superior or equal to 2 years whereas the effect on the propensity to work while on claim 3 months after the start of the treatment is only slightly higher than for the overall sample (+6.7% compared to 6%).

The robustness of these results to randomization based inference is presented in Appendix D and in Table 1.15. As previously, for the effects of the treatment on

the propensity to work while on claim, the p-values obtained with randomization inference tests are very close to the cluster-robust model based p-values. Therefore, both conventional and randomized based inference indicate that the treatment had a statistically significant effect on both the exit from compensated unemployment and the exit toward regular jobs.

Job quality

It is possible that the informational treatment decreases the exit to regular employment in general but increases the exit to good quality regular jobs. To explore this possibility, we decompose the exit toward regular jobs according to the length of the employment spell. As we only have information on people in employment when they return to the unemployment lists, our measure of job quality is limited. We do not observe the wage and we indirectly measure the duration of employment by analyzing the length of time that people who leave unemployment to work remain off the unemployment lists.[37] As an alternative measure, one could think of contract type to analyze job quality. However, the hires recorded as "permanent contracts" often end up to last only a few weeks for individuals in our sample. Hence, we do not consider it as truly informative about job quality and we prefer to focus on the effective employment spell duration.

In particular, we consider the probability to be in an employment spell that lasts at least 3 months (Figure 1.18), at least 6 months or at least 12 months – see Figures 1.24 and 1.25 displayed in appendix for the sake of space. For all durations, we observe a drop in the probability to be employed for treated individuals which is around the same period (twenty to thirty months after the treatment) as for the drop in regular employment in general (see Figure 1.17). Thus, these results suggest that the lock-in effects found in the previous section do not mask stepping stone effects toward better quality jobs, but rather are driven by a lower exit rate to longer employment spells.

1.5.4 Unemployment insurance expenditure

In order to evaluate the impact of the informational treatment on unemployment insurance expenditure, we compute the difference in cumulative unemployment benefits net of taxes between the treated and control groups. Since we have limited information on tax receipts, we also provide information about the effect of the treatment on cumulative unemployment benefits gross of taxes as well as the number of

[37] See Appendix C for the definition of variables.

days of compensated unemployment.[38]

Table 1.10 shows that the treatment has no statistically significant effects on cumulative unemployment insurance payments, either net (Panel A) or gross (Panel B), at any time horizon.[39] Three years after the start of the treatment, the cumulative benefits-net-of-taxes difference between the treated and the control group is very small and not significant: it is equal to the tiny amount of 62 euros (p-value $= 0.50$ for the null hypothesis of difference equal to zero) compared to the average cumulative amount equal to 13,746 euros. Table 1.10, Panel B, also shows that there is a non-significant increase in cumulative benefits in the treated group from the first year after the treatment. The positive sign reflects the lock-in effects, implying that the increase in part-time unemployment (which should generate a drop in benefit payments) is counterbalanced by less exits toward employment. Then, as time elapses, cumulative benefits tend to be larger in the treated group, although the difference is not statistically significant. Panel C of Table 1.10 confirms the previous results by showing that the cumulative number of days of compensated unemployment does not significantly differ between the treated group and the control group.

The absence of a significant difference in unemployment insurance expenditures between treated and control group confirms that both the positive (lock-in effects) and negative (supplementary days of work while on claim) effects of the treatment on the expenditure cancel each other out.[40]

1.5.5 Welfare

According to the model, improving information about unemployment insurance can only improve welfare because this helps to make better decisions. Therefore, the informational treatment improves welfare if it provides better knowledge on

[38] Tax receipts from unemployment insurance are computed by applying the unemployment insurance payroll tax rate to labor earnings, equal to 6.5%, for all hourly wages below about 25 euros, and to zero above this threshold. We have no information on earnings of individuals who definitively exit unemployment in our period. The monthly earnings are estimated by assuming that they are equal to the past daily wage used to compute the unemployment benefits times 30, which corresponds to the monthly earnings of a person working full time for the corresponding daily wage. It is likely that this overestimates the amount of tax receipts since all job seekers do not work full-time when they exit unemployment, meaning that we get a lower bound of the effect of the treatment on unemployment insurance expenditure net of taxes. For this reason, we provide results for the impact of the treatment on gross unemployment insurance payments (i.e. neglecting tax receipts), which yields an upper bound for the effect of the treatment on unemployment insurance expenditure net of taxes.

[39] Appendix H provides information indicating that the treatment has effects on hours of work that are consistent with those found on unemployment insurance payments.

[40] It should be noted that our experience does not take into account the possible increase in entries into unemployment induced by part-time unemployment benefits since the information was transmitted to those entering unemployment. Recent empirical evidence in France suggests that this channel could increase unemployment insurance expenditure (Khoury [2021]).

the unemployment insurance parameters without changing the expectation on the arrival rate of job offers. In this context, improved welfare is associated with a longer spell because the informational treatment induces a drop in the expected tax τ on current earnings from work while on claim and job seekers find it optimal to work more while on claim and to search for regular employment less intensely.

But as discussed above, it is possible that the informational treatment was interpreted as a negative signal suggesting the scarcity of regular jobs, which now reduces the expected utility of job seekers, but induces them to work more while on claim and to look less intensely for regular jobs. The job search model clearly shows that this effect is not specific to our experiment. It is a consequence of part-time unemployment insurance which is likely to modify the expectations of the unemployed on job offers. Therefore, assessing the impact of part-time unemployment insurance on the welfare of the unemployed requires knowing how it affects their expectations regarding their employment prospects.[41] According to another mechanism, not accounted for by our job search model, the information treatment may also have a negative impact on welfare even though it improves knowledge of unemployment insurance parameters if working while on claim creates lock-in effects in unemployment that are not anticipated by job seekers. These two issues,which are beyond the scope of this paper, are left for future research.

1.6 Conclusion

Our paper shows that the transmission of information about part-time unemployment benefits which raises the take-up rate induces significant lock-in effects into compensated unemployment and reduce the exit rate toward stable employment. Since the consequences of our information treatment on the behavior of unemployed people who are unaware of the existence of part-time unemployment are equivalent to those of a reform that introduces part-time unemployment for unemployed people who are aware of it, it is striking that these lock-in effects exist despite the positive correlation between the part-time unemployment take-up and the exit from compensated unemployment, documented by the previous literature in France (Fremigacci and Terracol [2013] and Auray and Lepage-Saucier [2021]) and in other countries.[42] Our results, are in line with those contributions focused on the causal impact of part-time unemployment insurance (Lee et al. [2019], Le Barbanchon [2020], Altmann et al. [2021]) which also find significant lock-in effects associated with part-

[41] Papers on the signalling effects of monetary policy of Melosi [2016] and Nakamura and Steinsson [2018] might be a source of inspiration.

[42] See the references provided above in the introduction.

time unemployment benefits. Insofar as we find that the lock-in effects are important and can last for several years, our contribution underlines the importance of taking them into account in order to design effective part-time unemployment insurance schemes.

Our contribution, focused on unemployment duration and unemployment insurance expenditure, does not draw conclusions on the impact of part-time unemployment insurance on welfare. Such an analysis, which is beyond the scope of this paper, is an important area for future research to the extent that part-time unemployment insurance, which is already an important component of unemployment insurance in many countries, is expected to play a growing role in the face of the development of unstable jobs.

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Appendices

A Unemployment Insurance in France

Eligibility conditions

To qualify for unemployment benefits, the claimant must satisfy the following conditions:

- reside in France,
- have worked at least 122 days or 610 hours (4 months) in the last 28 months (or in the last 36 months for job seekers aged 50 and over) before becoming unemployed,
- have involuntarily lost his/her job (termination by the employer, the end of a fixed-term employment contract or an assignment contract, termination by mutual agreement or resignation for a valid reason),
- be registered as a job seeker with "Pôle emploi",
- be actively seeking employment.

Potential benefit duration

The potential benefit duration is computed based on the principle of "a day of work equals a day of compensation". Claimants must have worked at least 4 months before becoming unemployed. Benefits are then paid for a minimum period of 4 months and a maximum period of 24 months for job seekers aged under 50, and 36 months for job seekers aged over 50.

Benefits

Benefits are calculated on the basis of a daily reference wage. The reference wage is based on earnings subject to contributions during the 12 calendar months prior to the last day of paid work[43]. It is calculated as follows:

$$\text{Daily reference wage} = \frac{\text{Earnings during the past 12 months}}{\text{Number of working days during the past 12 months (up to 365 days)}}$$

The daily benefit is equal to the highest of the following amounts:

[43] Up to a limit of 4 times the social security ceiling (13,076 euros per month).

- 40.4% of the daily reference wage + a set amount (11.84 euros in 2017)
- 57% of the daily reference wage

This amount cannot be below 28.86 euros or exceed 75% of the daily reference wage.

Monthly benefits, denoted b in the text, are then computed as the number of days in a month times daily benefit.

Part-time benefits

The part-time unemployment insurance scheme allows unemployed workers to work on non-regular jobs while on claim. They are allowed to work for any employer, including their past employers. For the sake of simplicity, the text only describes the rules in *net* terms for a job seeker who earned the minimum wage before unemployment. Nevertheless, the rules have been designed in *gross* terms. The marginal benefit reduction rate in gross terms is 70%, meaning that for each euro earned from work, 0.70 cents are deducted from the benefits.

When both the social contributions paid on the wage and on the benefits are deducted, the net financial gain of working is much lower as explained in the text. The contributions on wage amount to around 23% of the gross wage. Moreover, the social contributions on benefits for a job seeker who earned the minimum wage before unemployment represent 4.5%. For job seekers who earned more than the minimum wage before unemployment, the social contributions on benefits represent 9.6%. The net marginal benefit reduction rate is then comprised between 82% ($= \frac{70\%}{1-23\%}(1-9.6\%)$) and 87%($= \frac{70\%}{1-23\%}(1-4.5\%)$).

Evolution of the unemployment insurance capital

At the beginning of her claim, the job seeker is informed about her monthly benefits b and about her potential benefit duration. The initial unemployment insurance capital B_0 is equal to the potential benefit duration times the level of benefits. If job seekers are totally unemployed all along their claim and receive their benefits each month, their benefits will lapse after their potential benefit duration. When job seekers are only paid part of their benefits in a given month, the unpaid amount is rolled over to a later month in the claim, so the capital depreciates at a slower pace. Working while on claim is thus a way to delay the initial exhaustion date. The exhaustion date can be delayed without any limitation. Besides, after the initial benefit entitlement has expired, individuals can be eligible for a new entitlement period at

the exhaustion of the unemployment benefits related to their current entitlement period. To do so, job seekers must meet less restrictive eligibility requirements. They must have worked at least 1 month while on claim (instead of 4 months for a first claim). The new potential benefit duration is still calculated on the principle of "a day of work while on claim equals a day of compensation".

B Job search model solution

Maximization of program (4) with respect to the search effort e_t yields the first order condition:

$$-1 + \lambda'(e_t)\beta\left[W - U(B_{t+1})\right] + \eta_t = 0 \tag{1.8}$$

where $\eta_t \geq 0$ stands for the multiplier associated with the constraint $e_t \geq 0$. This equation defines the optimal search effort e_t in each period. In order to analyze how e_t evolves over time one needs to know how $U(B_{t+1})$ evolves. We know that B_t decreases over time. Therefore, it suffices to know the sign of the derivative of U to know how e_t evolves. One can show that $U'(B) > 0$. The envelope theorem implies that

$$U'(B_t) = \begin{cases} \beta\left[1 - \lambda(e_t)\right]U'(B_{t+1}) & \text{if } B_t \geq b \\ v'(c_t) & \text{otherwise} \end{cases} \tag{1.9}$$

In equation (1.9), the case where $U'(B_t) = v'(c_t)$ arises the period just before the total exhaustion of the unemployment insurance capital B_t. It shows that $U'(B_t) = v'(c_t) > 0$ in this period. Then, solving backward, condition $U'(B_t) = \beta\left[1 - \lambda(e_t)\right]U'(B_{t+1})$ in the top of the right hand side of equation (1.9) shows that $U'(B_t) > 0$ in all periods, implying that $U(B_t)$ decreases over time since B_t decreases over time.

Let us first consider the case where $\eta_t > 0$. We get from equation (1.8)

$$\eta_t = 1 - \lambda'(0)\beta\left[W - U(B_{t+1})\right]$$

We just showed that $U(B_t)$ decreases with t for all $t < T$ and reaches the minimum value $U(0)$ for all $t \geq T$ where T stands for the benefits exhaustion date T. Thus, this equation implies that $\eta_t > 0$ and then $e_t = 0$ for all t iff

$$1 - \lambda'(0)\beta\left[W - U(0)\right] > 0.$$

This situation can arise when the gap between the value of employment and the value of unemployment after the date of exhaustion of benefits is small.

Now, let us consider the case where $U(0)$ is small enough to yield a positive search effort at the benefits exhaustion date T and assume that $\exists t < T$ such that $e_t > 0$. In this case, e_t is defined by equation (1.8) with $\eta_t = 0$. Since $\lambda''(e_t) < 0$, differentiation of equation (1.8) implies that

$$\frac{\mathrm{d}e_t}{\mathrm{d}U(B_{t+1})} = \frac{\lambda'(e_t)}{\lambda''(e_t)\left[W - U(B_{t+1})\right]} < 0 \tag{1.10}$$

and then that the search effort e_t increases over time since $U(B_t)$ decreases over time.

Therefore, the optimal search effort can take 3 different types of time profiles

depending on the values of parameters:

1. $e_t = 0$ for all t if $e_T = 0$. This situation arises when the expected gains from job search effort are low.

2. $e_t = 0$ for all $t \leq t_0 \in]0, T[$, $e_t > 0$ for all $t > t_0$, $e_t < e_{t+1}$ for all $t \in [t_0, T[$ and $e_t = e_T$ for all $t \geq T$ if $e_T > 0$,. This situation arises when the expected gains from job search effort at the start of the unemployment spell are low because the initial expected value of unemployment is high, but declines enough over time to trigger positive search effort before reaching the date of exhaustion of benefits.

3. $e_t > 0$ for all t, $e_t < e_{t+1}$ for all $t < T$ and $e_t = e_T$ for all $t \geq T$ if $e_T > 0$,. This situation arises when the expected gains from job search effort are high from the start of the unemployment spell.

Now, let us look at the choice of working while on claim. Since we look for the reservation level of earnings from work while on claim in situations where individuals accumulate unemployment benefits b and earnings from work while on claim z_t, which arise when $z_t < b + (1 - \tau)z_t$, we can focus on the case $z_t < b/\tau$ without loss of generality to determine this reservation level. Maximization of program (4) with respect to Ω_t implies that individuals prefer to work while on claim (i.e. choose $\Omega_t = 1$) if and only if this yields utility gains $\Delta > 0$. The first order approximation of the utility gains from work while on claim with earnings z_t can be computed using equation (4):

$$\Delta \simeq [z_t(1 - \tau) - \kappa] \, v'(b) + \beta \, [1 - \lambda(e_t)] \, U'(B_{t+1}) \mathrm{d}B_{t+1}$$

Equation (3) implies that $\mathrm{d}B_{t+1} = \tau z_t$, when an individual earns z_t from working while on claim compared with the situation in which she does not work. Using equation (1.9) we get:

$$\Delta \simeq [z_t(1 - \tau) - \kappa] \, v'(b) + \tau z_t U'(B_t) \tag{1.11}$$

The first term of the right hand side, $[z_t(1 - \tau) - \kappa] \, v'(b)$, corresponds to the increase in the marginal utility of the current period induced by the increase in current consumption and the second term, $\tau z_t U'(B_t)$, corresponds to the increase in the future expected consumption. From equation (1.9) we know that $U'(B_t)$ increases along the unemployment spell, because $\beta(1 - e_t\lambda) < 1$ implies that $U'(B_t) < U'(B_{t+1})$. This property, together with equation (1.11), implies that the incentives to work while on claim increase over time.

Now, let us show that equation (1.11) implies that the effects of the part-time unemployment insurance scheme on the propensity to work while on claim depend on a single parameter, the marginal taxation rate, which encapsulates all the parameters of the part-time unemployment insurance scheme.

The expected discounted income from work while on claim in period t for an individual who gets benefits until the exhaustion date T – i.e. period T where $B_T = 0$ and $B_{T-1} > 0$ according to the law of motion (3)– is equal to the instantaneous income, $z_t(1-\tau)$, plus the future income that the individual can expect T if she is still unemployed in that period $T-1$. Note that, for the sake of simplicity in this discrete time framework, we assume that the taxed earnings from work while on claim increase the income in the last period before the exhaustion date, and neglect the situation where these taxed earnings move the exhaustion date, without loss of generality. Thus, the expected discounted income from earnings z from working while on claim in period t in the neighborhood of $c_T = b$ is equal to

$$y_t = z_t(1-\tau) + \tau z_t \beta^{T-t} \mathbb{E}_t \left(\prod_{j=t}^{T-1} [1 - \lambda(e_j)] \right)$$

By definition, the marginal taxation rate in period t, denoted by m_t, is equal to $1 - (\mathrm{d}y_t/\mathrm{d}z_t)$, which yields, from the previous equation

$$m_t = \tau \left[1 - \beta^{T-t} \mathbb{E}_t \left(\prod_{j=t}^{T-1} [1 - \lambda(e_j)] \right) \right] \tag{1.12}$$

Using equation (1.9) to compute $U'(B_t)$ recursively from the last period T in which unemployed benefits are collected, we get, in the neighborhood of $c_T = b$:

$$U'(B_t) = \beta^{T-t} \mathbb{E}_t \left(\prod_{j=t}^{T-1} [1 - \lambda(e_j)] \right) v'(b) \tag{1.13}$$

From equations (1.12) and (1.13), we get

$$\tau U'(B_t) = v'(b)\,(\tau - m_t) \tag{1.14}$$

Substituting this expression of $\tau U'(B_t)$ in equation (1.11) yields

$$\Delta \simeq z_t v'(b) \left(1 - m_t - \frac{\kappa}{z_t} \right) \tag{1.15}$$

where m_t is defined by equation (1.12). Equation (1.15) implies that it is worth work-

ing while on claim in period t if and only if

$$z_t(1 - m_t) > \kappa$$

C Definition of variables

Outcomes	Tables and Figures	Definitions
Work while on claim		
Probability to work while on claim	Tables 6, 1.4, 1.5, 1.12, 1.13, 1.16, 1.17, 1.18, 1.19, 1.20, 1.21, 1.19 and Figures 7, 10	A job seeker has worked while on claim during a given month if the hours of work during the month and the monthly unemployment benefits are both positive.
Cumulative number of months with work while on claim	Tables 4, 1.3, 1.5, 1.14, 1.16, 1.17, 1.18, 1.19, 1.20, 1.21, 1.20	Sum of months with a positive number of hours of work while on claim (i.e. positive number of hours of work and positive benefits)
Cumulative number of hours with work while on claim	Tables 4, 5, 1.3, 1.5, 1.14, 1.16, 1.17, 1.18, 1.19, 1.20, 1.21, 1.21 and Figures 8, 9, 1.10, 1.11	Sum of hours of work while on claim (i.e. hours of work when benefits are positive)
Cumulative earnings with work while on claim	Tables 4, 5, 1.3, 1.5, 1.14, 1.16, 1.17, 1.18, 1.19, 1.20, 1.21	Sum of earnings over months with a positive number of hours of work while on claim (i.e. positive number of hours of work and positive benefits)
Cumulative number of hours worked while on claim at the intensive margin	Tables5, 1.14	Same as above, for the subset of job seekers who worked while on claim at least one day.
Cumulative earnings (in euro) from work while on claim at the intensive margin	Table5, 1.14	Same as above, for the subset of job seekers who worked while on claim at least one day.

Outcomes	Tables and Figures	Definitions
Compensated unemployment		
Survival in compensated unemployment	Figures 11, 1.22	A job seeker has exited compensated unemployment in the month if she perceive no benefit during this month.
Survival in compensated unemployment among job seekers with PBD superior or equal to 2 years	Figures 12, 1.23	Same as above, for the subset of job seekers with a a potential benefit duration (PBD) superior or equal to 2 years.
Exit toward regular employment		
Survival in unemployment (compensated, non-compensated or part-time)	Figure 13	A job seeker has exited unemployment toward regular employment if he or she is not registered anymore as unemployment and if we observe a hire. Since our data on hires is not exhaustive, we also consider that a job seeker has exited toward regular employment if he or she is not registered anymore as unemployed but still has unclaimed benefits.
Probability to be in regular employment in the last quarter	Tables 7, 1.15	Let T denote the initial exhaustion month. A job seeker is in regular employment in the last quarter if he or she has exited toward regular employment during month T, $T-1$ or $T-2$ and is still off the unemployment lists at month T.
Probability to be in regular employment in the last month	Tables 7, 1.15, 1.16, 1.17	Let T denote the initial exhaustion month. A job seeker is in regular employment in the last month if he or she has exited during month T.
Hours of work		
Cumulative number of hours of work	Figure 1.10	Sum of hours of work (whether it is while on claim or not)
Cumulative number of hours of work among job seekers with PBD superior or equal to 2 years	Figure 1.11	Same as above, for the subset of job seekers with a a potential benefit duration (PBD) superior or equal to 2 years.

Outcomes	Tables and Figures	Definitions
Job quality		
Probability to have a regular job for at least 3 months	Figure 14	A job seeker has exited unemployment toward regular employment and remain out of the register of job seekers during at least 3 months.
Probability to have a regular job for at least 6 months	Figure 1.24	A job seeker has exited unemployment toward regular employment and remain out of the register of job seekers during at least 6 months.
Probability to have a regular job for at least 12 months	Figure 1.25	A job seeker has exited unemployment toward regular employment and remain out of the register of job seekers during at least 12 months.
Unemployment insurance expenditure		
Unemployment insurance payments (in euro)	Table 8	Benefits paid to job seekers.
Unemployment insurance payments (in euro) net of taxes	Tables 8	Benefits paid to job seekers minus the taxes collected by the Unemployment Insurance from work while on claim (contributions for the unemployment insurance amounts to 6.5% of the gross wage).
Number of days of compensated unemployment	Table 8	Days for which a job seeker perceive benefits. The number of days of compensated unemployment equals the number of days in the months minus the number of days not compensated because of work while on claim.

D Randomization Inference

This appendix evaluates the robustness of our results to randomization based inference.

Contrary to conventional inference (cluster-robust p-value based on large sample approximations) which aims to account for sampling uncertainty, randomization based inference accounts for the uncertainty created by the treatment assignment itself. This method, first proposed by Fisher [1936], is increasingly used in experimental papers as an alternative method to perform statistical inference (Bloom et al. [2006], Ichino and Schündeln [2012], Fujiwara and Wantchekon [2013]). Moreover, Young [2019] recently demonstrated that a substantial part of seemingly significant results, obtained with conventional methods, appear to be insignificant when statistical tests are conducted with randomization based methods.

The idea behind randomization inference is intuitive. It makes use of the knowledge that the researcher has on the randomization process to generate placebo estimates of the treatment effect. Thus, the observed ITT estimate, coming from the actual treatment assignment, can be compared to the distribution of these placebo estimates to test for its statistical significance.

Implementation

First, we randomly re-assigned "treatment" in the same way as was done in the experimental setting, that is, a 2 levels stratified sampling as described in subsection 4.2. Then, we re-estimate the two placebo treatment effect parameters: β_r (Treated vs Control) and δ_r (Control vs Super-control) based on the same estimating equation as equation (7):[44]

$$y_i = \alpha_r + \beta_r Z_{r,i} + \delta_r C_{r,i} + \gamma_r X_i + \eta_{r,i}$$

where $Z_{r,i}$ is a dummy for being assigned to the treated group and $C_{r,i}$ is a dummy for being assigned to a treated area (i.e. being either in the treated group or in the control group but not in the super control group) in random re-assignment r.

We repeat this procedure 5000 times.[45] Finally, for a given outcome, randomization based p-value are obtained by computing the share of randomized based placebo estimates that are superior or equal (in absolute value) to the corresponding experimental estimate. For instance, we have for $\hat{\beta}$:

[44] All the results reported below are based on the specification including covariates.

[45] As a comparison Young [2019], used 10 000 repeftitions but did not detect any appreciable difference above 2000 draws.

$$p - \text{value}^{\text{RI}}(\hat{\beta}) = \frac{\sum_{r=1}^{R} \mathbb{1}(\hat{\beta}_r \geq \hat{\beta})}{R}$$

where R is the total number of random draws (i.e. R = 5000 in our setting).

Results

Tables 1.14 and 1.15 present the results of randomization inference tests. In particular, Table 1.14 presents the results for part-time unemployment and Table 1.15 presents the results for unemployment. We only present the results for outcomes on which we measured a statistically significant treatment effect with cluster-robust p-value.

Overall, the p-values obtained with randomization inference tests are very close to the cluster-robust model based p-values. To some extent this was expected, considering the relatively large sample size in our experiment. In particular, almost all (i.e. 7 out of 8) estimates that are statistically significant at 5% with model based inference are still significant at 5% with randomization based inference. Both conventional and randomized based inference thus support the view that the treatment had a statistically significant effect on both the propensity to work while on claim and the probability to exit from unemployment (i.e. lock-in effect).

E Heterogeneous treatment effects

This appendix describes the estimation of heterogeneous treatment effects following the approach of Chernozhukov et al. [2018].

The Conditional Average Treatment Effect (CATE) function is:

$$s_0(X) = E[Y(1)|X] - E[Y(0)|X]$$

where X denotes a vector of covariates and Y is the outcome of interest.

We start by splitting evenly the whole sample into a *main* subsample, used to predict $s_0(X)$, and an *auxiliary* subsample, used to estimate the key features of $s_0(X)$. The auxiliary sample is used to predict $s_0(X)$ with machine learning (e.g. Elastic Net, Random Forest). We estimate the model separately for observations in the treatment and control groups, resulting in two prediction models. We then compute the estimated outcome for each observation in the main sample under both treatment statuses, i.e. $\hat{Y}^T(X_i)$ and $\hat{Y}^C(X_i)$ and the estimated propensity score $\hat{p}(X_i)$. Finally we compute $\hat{S}(X_i) = \hat{Y}^T(X_i) - \hat{Y}^C(X_i)$ our proxy for the true CATE, $s_0(X_i)$. However, except under strong assumptions about the ML estimator, this proxy predictor is likely to be an inconsistent estimate of $s_0(X_i)$. This motivates the second step of the procedure where the ML proxy is *post-processed* into the estimates of the key features of $s_0(X_i)$.

To estimate the best linear predictor of the conditional average treatment effect function we run the following weighted regression

$$y_i = \alpha + \beta_1(Z_i - \hat{p}(X_i)) + \beta_2(Z_i - \hat{p}(X_i))(\hat{S}(X_i) - \mathbb{E}\hat{S}(X_i)) + \theta\hat{Y}^C(X_i) + \epsilon_i \quad (1.16)$$

where Z_i is an indicator variable equal to 1 for treated individuals, $\mathbb{E}$ denotes the empirical expectation with respect to the main sample and the weights are equal to

$$w(X_i) = \frac{1}{\hat{p}(X_i)(1 - \hat{p}(X_i)))}$$

Chernozhukov et al. [2018] show that $\beta_1 + \beta_2(\hat{S}(X_i) - \mathbb{E}\hat{S}(X_i))$ identifies the best linear predictor of the conditional average treatment effect $s_0(X_i)$. Besides, β_1 identifies the average treatment effect (ATE) and rejecting the null hypothesis that $\beta_2 = 0$ therefore means that there is both heterogeneity and $\hat{S}(X_i)$ captures a relevant part of this heterogeneity. Table 1.16 presents our estimates of the best linear predictor of the conditional average treatment effect.

Next we estimate the sorted group average treatment effects. Here the parameters of interest are $\mathbb{E}[s_0(X_i)|G]$, where G is an indicator of group membership based

on our proxy predictor $\hat{S}(X_i)$. As shown by Chernozhukov et al. [2018], we can recover these parameters by estimating the following weighted regression:

$$y_i = \alpha + \sum_{k=1}^{5} \gamma_k (Z_i - \hat{p}(X_i)) * \mathbb{1}(G_k) + \theta \hat{Y}^C(X_i) + \epsilon_i \tag{1.17}$$

where the weights are the same as in equation (1.16) and $\mathbb{1}(G_k)$ is equal to 1 if $\hat{S}(X_i)$ lies in the k^{th} interval and 0 otherwise. We cut $\hat{S}(X_i)$ at 50^{th}, 75^{th}, 90^{th}, 95^{th} percentiles. In particular, Group 1 corresponds to the observations that lie in the bottom 50% of $\hat{S}(X_i)$ and Group 5 corresponds to the observations that lie in the top 5% $\hat{S}(X_i)$. Table 1.17 displays the results we obtained by estimating equation (1.17).

F Heterogeneity of treatment effects on the super control group

It is possible that the lack of spillover documented in Tables 4 and 5 arises from the absence of any effects of the treatment on the control group. But it is also possible that the two effects cancel each other out. Crépon et al. [2013] identify displacement effects from variations in the share of treated individuals in each unemployment agency. This does not help us to identify the relative impact of the two effects since the strength of both effects is expected to increase with the share of treated individuals: when more individuals are treated, both information transmission and displacement effects may increase.

However, Crépon et al. [2013] find displacement effects only in weak labor markets where the unemployment rate is high. Thus, in labor markets with a low unemployment rate, only the transmission of information is likely to have a significant impact on the control group if there are informational spillovers. This means that one should observe a positive impact of the treatment on the part-time unemployment take-up of the control group in labor markets where the unemployment rate is low if the provision of information spreads to the control group. To test this assumption, we estimate the following model for individuals in the control and the super control groups:

$$y_i = \alpha_0 + \alpha_1 C_i + \alpha_2 U_i + \alpha_3 (C_i \times U_i) + \alpha_4 X_i + \epsilon_i \tag{1.18}$$

where y_i is a measure of part-time unemployment take-up of individual i, U_i is an indicator function equal to one if individual i is located in a commuting zone in the bottom tercile of local unemployment rates ; C_i is a dummy for being in the control group – i.e. in a treated area but not in the treated group since it is excluded from the sample here. $(C_i \times U_i)$ denotes the interaction between C_i and U_i. As previously, X_i

is a vector of control variables that includes the variables reported in the summary statistics (Table 2) as well as unemployment entry months and regional fixed effects. Coefficient α_3 is positive if the provision of information spreads to the control group.

Table 1.3 shows that there is no evidence that the part-time unemployment take-up of the control group increases, compared with the super control group, when the local unemployment rate is low. This suggests that there are no significant information spillovers to the control group arising from the treatment. Accordingly, the absence of spillover – from both displacement effects and information transmission – reported in Tables 4 and 5 is likely the consequence of the absence of any significant impact of the informational treatment on the control group.

Table 1.3: Spillover effects on part-time unemployment

	3 months		12 months		36 months	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A : Cumulative number of months with work while on claim						
Control	-0.0015	0.0006	0.0047	0.0142	-0.0367	-0.0114
	(0.0043)	(0.0041)	(0.0187)	(0.0162)	(0.0545)	(0.0430)
	[0.725]	[0.891]	[0.799]	[0.379]	[0.501]	[0.790]
Low	0.0025	-0.0035	0.0545**	-0.0219	0.2887***	-0.0278
	(0.0066)	(0.0075)	(0.0236)	(0.0276)	(0.0691)	(0.0754)
	[0.707]	[0.638]	[0.021]	[0.429]	[0.000]	[0.713]
Low X Control	-0.0005	-0.0003	-0.0018	0.0012	0.0316	0.0309
	(0.0079)	(0.0073)	(0.0301)	(0.0267)	(0.0879)	(0.0723)
	[0.946]	[0.970]	[0.953]	[0.965]	[0.719]	[0.669]
Mean super control	0.10		0.57		1.70	
Panel B : Cumulative number of hours worked while on claim						
Control	-0.2188	-0.0466	-0.4772	0.2655	-3.7746	-1.3492
	(0.3031)	(0.2926)	(1.5610)	(1.3726)	(5.2196)	(4.1148)
	[0.471]	[0.873]	[0.760]	[0.847]	[0.470]	[0.743]
Low	0.7206	-0.0510	8.7680***	0.2101	39.7589***	3.5531
	(0.5002)	(0.5525)	(2.1647)	(2.3707)	(7.4701)	(7.7616)
	[0.150]	[0.926]	[0.000]	[0.929]	[0.000]	[0.647]
Low X Control	-0.0246	-0.0472	-0.8199	-1.0094	-1.1072	-3.0131
	(0.5859)	(0.5532)	(2.7197)	(2.4288)	(9.3550)	(7.6591)
	[0.966]	[0.932]	[0.763]	[0.678]	[0.906]	[0.694]
Mean super control	5.75		40.76		135.85	
Panel C : Cumulative earnings (in euro) from work while on claim						
Control	-2.9422	-0.9830	-7.4255	0.2810	-38.7499	-12.8498
	(3.9135)	(3.6114)	(22.1168)	(17.4544)	(73.9316)	(53.5499)
	[0.452]	[0.786]	[0.737]	[0.987]	[0.600]	[0.810]
Low	13.4765*	2.6427	142.4420***	15.2339	641.1864***	106.4218
	(7.1747)	(7.7183)	(31.2417)	(31.7882)	(112.2593)	(104.2902)
	[0.061]	[0.732]	[0.000]	[0.632]	[0.000]	[0.308]
Low X Control	-4.9249	-5.9131	-24.7816	-33.4379	-71.7663	-122.5675
	(8.0957)	(7.6851)	(38.4159)	(33.3205)	(137.1450)	(107.4938)
	[0.543]	[0.442]	[0.519]	[0.316]	[0.601]	[0.255]
Mean super control	69.46		501.78		1709.82	
Covariates	No	Yes	No	Yes	No	Yes
N	69356	69356	69356	69356	69356	69356

Note: This table reports the estimates of coefficients α_1, α_2 and α_3 of equation (1.18). Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. *p*-values are reported in brackets. Dependent variables are the same as in Table 4. Each duration (i.e. 3, 12, and 36 months) indicates the elapsed time since treatment. Covariates include all stratum variables reported in Table 2 as well as entry months and regions fixed effects. The sample comprises the control group and the super control group only. "Control" (coefficient α_1) is a dummy for individuals in treated area but not treated. "Low" (coefficient α_2) is a dummy for areas in the bottom tercile of the unemployment rate. "Low $\times$ Control" (coefficient α_3) is the interaction term. The number of observations N corresponds to the number of individuals.

Characteristics of individuals working while on claim in the treated group

It is possible that the informational treatment impacted individuals particularly sensitive to information received by email, implying that those induced to work while on claim by the treatment are very different from those who work while on claim in the absence of our treatment. Knowing whether individuals induced to work while on claim because they received our information about part-time unemployment benefits resemble other individuals working while on claim is important when it comes to gauging the external validity of our analysis; or, to put it differently, when it comes to gauging whether the effect of the treatment can be compared to the effect of changes in the marginal tax on earnings from work while on claim. We examine this issue in two different ways. First, we compare the characteristics of individuals working while on claim in the treatment and in the control groups. Second, we use the super control group to predict the individual characteristics associated with the propensity to work while on claim and we analyze how treated individuals react to the treatment depending on these characteristics.

Comparison of individuals working while on claim in the treated group and control group Table 1.4 reports the means of the characteristics of individuals who worked while on claim at least once six months after the treatment, which corresponds to the period in which the treatment has the largest impact on the number of job seekers working while on claim. It is clear that the characteristics of treated individuals working while on claim do not differ from those of other individuals also working while on claim, except for the duration of the last contract before the entry into unemployment. Individuals of the treated group in part-time unemployment had contracts whose duration was more frequently below 3 months before starting their unemployment spell compared with other individuals in part-time unemployment. This means that the informational treatment has larger effects on the propensity to work on non-regular jobs for individuals who worked on such jobs in the past. This is likely because those individuals are more inclined or have more opportunities to work on non-regular jobs. Apart from this difference, the characteristics of individuals of the treated group in part-time unemployment are not statistically different from those of other individuals who work while on claim.

Treatment impact conditional on predicted characteristics associated with work while on claim Now, let us analyze whether the informational treatment has a stronger impact on the probability to work while on claim for individuals more

likely to work while on claim in the absence of the treatment. We start by regressing the probability to work while on claim on the covariates displayed in the summary statistics (Table 2) as well as month of entry into unemployment and regional fixed effects for individuals belonging to the super control group.[46] This allows us to rely on out-of-sample untreated units to predict the probability to work while on claim conditional on these covariates.[47] Overall, Table 1.5 shows that the impact of the treatment on all measures of the intensity of the propensity to work while on claim is more important for individuals whose observable characteristics are associated with a probability above the median to work while on claim. This indicates that the treatment induces individuals to work while on claim whose observable characteristics are similar to those who have a high propensity to work while on claim, which is a situation that should arise if the marginal tax on work while on claim drops.

[46] Tables 1.19, 1.20, 1.21 report the results of this first stage for our outcomes of interest measured one 3, 12 and 36 months after the start of the treatment respectively. We can perceive that most of the characteristics associated with a higher probability to work while on claim at least once are also the characteristics that are prevalent in the most affected group from the CLAN analysis in Table 6. The only exception is the potential benefit duration, which is positively associated with part-time unemployment whereas it is on average lower in the most affected group.

[47] Abadie et al. [2018] stress the importance of using out-of-sample untreated units to proceed to this type of analysis.

Table 1.4: Summary statistics on individuals working while on claim at least once 6 months after the start of the treatment

	Means				p-value of the difference		
	All	T	C	SC	T - C	T - (C + SC)	T = C = SC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Job seekers characteristics							
Female	.504	.508	.501	.5	.503	.431	.728
Age	31.169	31.08	31.213	31.266	.547	.451	.751
Young	.418	.422	.413	.42	.345	.447	.636
Prime age	.462	.461	.466	.456	.639	.895	.774
Senior	.12	.117	.121	.125	.474	.308	.563
Lower secondary education	.236	.234	.239	.235	.53	.625	.805
Upper secondary education	.488	.488	.489	.486	.92	1	.978
Higher education	.276	.278	.272	.279	.477	.661	.732
Last contract inf to 12 m	.353	.357	.347	.354	.272	.365	.535
Last contract inf to 3 m	.103	.108	.098	.104	.077	.137	.18
Potential benefit duration	611.635	611.155	612.156	611.589	.833	.852	.975
PBD inf to 730 days	.448	.451	.448	.445	.759	.664	.905
PBD sup or eq to 730 days	.552	.549	.552	.555	.759	.664	.905
Daily Reference Wage	60.125	60.546	59.673	60.155	.281	.422	.547
DRW below the mean	.66	.663	.663	.648	.994	.554	.581
DRW above the mean	.34	.337	.337	.352	.994	.554	.581
Days since entry in unemp	105.976	106.241	105.793	105.789	.569	.548	.835
Tenure inf to 3 months	.423	.426	.423	.416	.772	.586	.754
Tenure between 4 and 6 months	.577	.574	.577	.584	.772	.586	.754
Local agencies characteristics							
Number of participants	214.148	217.323	214.428	206.974	.177	.18	.33
Number of claimants	4356.972	4371.09	4340.28	4361.041	.322	.706	.637
Share of part time unemp	.444	.443	.443	.449	.797	.46	.571
Share of long-term unemp	.431	.431	.431	.431	.866	.962	.988
Exit rate from unemp	.064	.064	.064	.064	.535	.547	.781
Unemployment rate	13.817	13.761	13.917	13.733	.102	.48	.296
N	13240	5419	5218	2603			

Columns (1), (2), (3) and (4) report the means of characteristics of individuals working while on claim at least once after the start of the treatment in our final sample, for the treatment, the control and the super control group, respectively. Columns (5)–(8) report the *p*-values for the difference between assigned to treatment (T) and assigned to control (C) (column 5), the difference between assigned to treatment (T) and non assigned (C + SC), and for the joint significance of assignment status (T, C and SC). See Table 2 for a description of the variables.

Table 1.5: Treatment heterogeneity conditional on predicted part-time unemployment activity

	After 3 months	After 12 months	After 36 months
	(1)	(2)	(3)
Panel A : Prob. to work while on claim at least once			
Treated	0.001	-0.000	0.001
	(0.0018)	(0.0032)	(0.0037)
	[0.611]	[0.892]	[0.873]
Treated × Above median	0.010**	0.010*	0.007
	(0.0040)	(0.0055)	(0.0060)
	[0.011]	[0.069]	[0.218]
Mean super control	0.06	0.19	0.30
Panel B : Cumulative number of months with work while on claim			
Treated	0.001	0.006	0.038
	(0.0032)	(0.0114)	(0.0250)
	[0.833]	[0.574]	[0.133]
Treated × Above median	0.013**	0.048**	0.100
	(0.0063)	(0.0234)	(0.0649)
	[0.037]	[0.039]	[0.123]
Mean super control	0.10	0.57	1.70
Panel C : Cumulative number of hours worked while on claim			
Treated	-0.102	-0.565	1.696
	(0.1952)	(0.8271)	(1.8660)
	[0.601]	[0.494]	[0.364]
Treated × Above median	1.591***	7.105***	12.116*
	(0.5294)	(2.1583)	(6.3581)
	[0.003]	[0.001]	[0.057]
Mean super control	5.75	40.76	135.85
Panel D : Cumulative earnings (in euro) from work while on claim			
Treated	-0.445	-7.187	14.584
	(2.1089)	(8.4265)	(18.8816)
	[0.833]	[0.394]	[0.440]
Treated × Above median	21.132***	102.325***	210.609**
	(7.0557)	(28.4939)	(84.0406)
	[0.003]	[0.000]	[0.012]
Mean super control	69.46	501.78	1709.82
Covariates	Yes	Yes	Yes
N	92391	92391	92391

Note: Levels of significance: $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. *p*-values are reported in brackets. Each panel (outcome) $*$ column (duration) displays the results from a different regression. Each regression include the list of covariates reported in the summary statistics (see Table 2) as well as entry months and regions fixed effects. "Treated" designates individuals who were assigned to treatment (ITT estimate). "Above median" designates individuals for whom the predicted outcome is above the median. For each outcome $*$ duration, the predicted outcome is estimated by an OLS regression using individuals from the super control group only. Individuals from the super control group are not included in the regressions presented in this table to avoid potential bias arising from endogenous stratification as described in Abadie et al. [2018]. The number of observations N corresponds to the number of individuals.

G Emails contents

Figure 1.8: Screenshot of the message received by job seekers (example with gains in gross terms)



Bonjour,

Vous êtes aujourd'hui demandeur d'emploi indemnisable au titre de l'Allocation de Retour à l'Emploi (ARE). **Nous vous informons que vous pouvez travailler sans perdre votre allocation chômage.** Cette possibilité de cumuler votre salaire et votre allocation vous permet:
- **De disposer d'un revenu plus élevé** que votre seule allocation mais sans dépasser le montant de votre ancien salaire brut. Pôle emploi ne retire que 70 centimes d'allocation par euro brut gagné.
- **D'être indemnisé plus longtemps**. Le nombre de jours d'allocations non perçues en raison de votre cumul reste acquis.

A la fin de vos allocations, vous pouvez bénéficier de nouveaux droits grâce à cette activité dès que vous avez exercé 150 heures d'activité réduite.

Illustration:
Mme Dubois augmente son revenu mensuel de 180 euros brut si elle travaille 9 jours dans le mois au SMIC.

Mme Dubois bénéficie d'une allocation de 930 euros pour un mois de 31 jours sans activité. Elle travaille 9 jours sur un mois donné pour un salaire brut de 600 euros. Pôle emploi retire 70 centimes par euro brut gagné. Pôle emploi retire donc 420 euros brut (=0,7 x 600 euros) et continue à verser 510 euros d'allocation. Mme Dubois obtient un revenu mensuel brut de 1110 euros (600 euros de salaire brut + 510 euros d'allocation brute restante), **supérieur de 180 euros** aux allocations perçues pour un mois de chômage complet (930 euros).

Simuler le montant de votre allocation en cas de reprise d'activité

En pratique:
Chaque mois, l'activité professionnelle doit être déclarée au moment de votre actualisation mensuelle. Une copie du bulletin de salaire doit être envoyée aux services de Pôle emploi.

Pour plus d'information:
Les règles de cumul de votre allocation avec un salaire sont détaillées en pièce jointe.

Cordialement,
L'équipe Pôle emploi

Attention :
Ce courriel vous est envoyé automatiquement, merci de ne pas utiliser la fonction "répondre à l'expéditeur".

Vous disposez d'un droit d'accès et de rectification aux informations qui vous concernent auprès de Pôle emploi conformément à la loi du 6 janvier 1978, modifiée, relative à l'informatique,aux fichiers et aux libertés.

Figure 1.9: Screenshot of the message received by job seekers (example with gains in net terms)



Bonjour,

•••

Mme Dubois augmente son revenu mensuel de 60 euros net si elle travaille 9 jours dans le mois au SMIC.

Mme Dubois bénéficie d'une allocation de 930 euros brut pour un mois de 31 jours sans activité, soit 889 euros net. Elle travaille 9 jours sur un mois donné pour un salaire brut de 600 euros, soit 462 euros net. Pôle emploi retire 70 centimes par euro brut gagné. Pôle emploi retire donc 420 euros brut (=0,7 x 600 euros) et continue à verser 510 euros brut d'allocation, soit 487 euros net. Mme Dubois obtient un revenu mensuel net de 949 euros (462 euros de salaire net + 487 euros d'allocation nette restante), **supérieur de 60 euros** aux allocations perçues pour un mois de chômage complet (889 euros).

Simuler le montant de votre allocation en cas de reprise d'activité

En pratique:
Chaque mois, l'activité professionnelle doit être déclarée au moment de votre actualisation mensuelle. Une copie du bulletin de salaire doit être envoyée aux services de Pôle emploi.

Pour plus d'information:
Les règles de cumul de votre allocation avec un salaire sont détaillées en pièce jointe.

Cordialement,
L'équipe Pôle emploi

Attention :
Ce courriel vous est envoyé automatiquement, merci de ne pas utiliser la fonction "répondre à l'expéditeur".

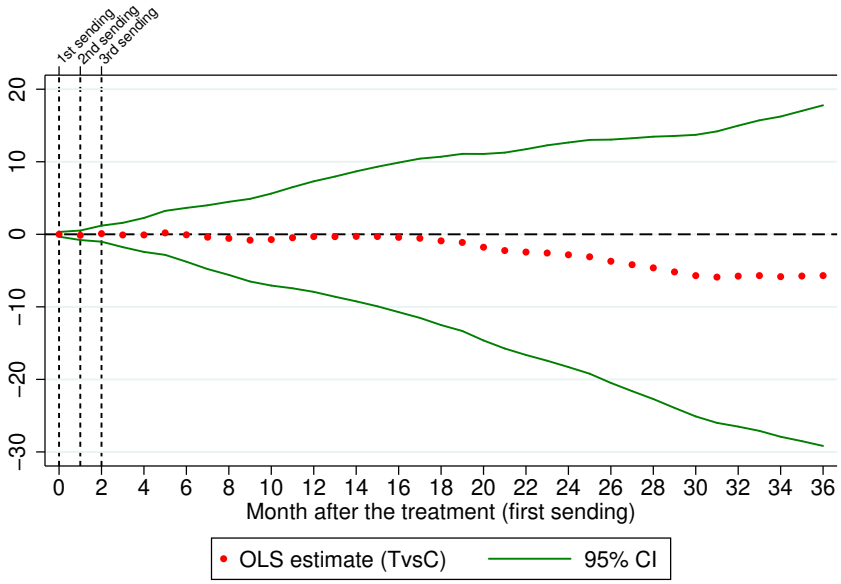
Vous disposez d'un droit d'accès et de rectification aux informations qui vous concernent auprès de Pôle emploi conformément à la loi du 6 janvier 1978, modifiée, relative à l'informatique,aux fichiers et aux libertés.

H Hours of work

In order to better gauge the impact of the informational treatment on overall labor supply, we compute the difference in the number of hours worked (both in part-time unemployment and in regular employment) between the treated group and control group. These results are mainly indicative, to the extent that we know the exact number of hours worked for individuals who are registered at employment agencies but we have no information on the number of hours worked of individuals who definitely exit unemployment in our period. In this case, the number of hours worked is computed by assuming that individuals who exit toward regular employment are working at the same intensity as before their unemployment spell.

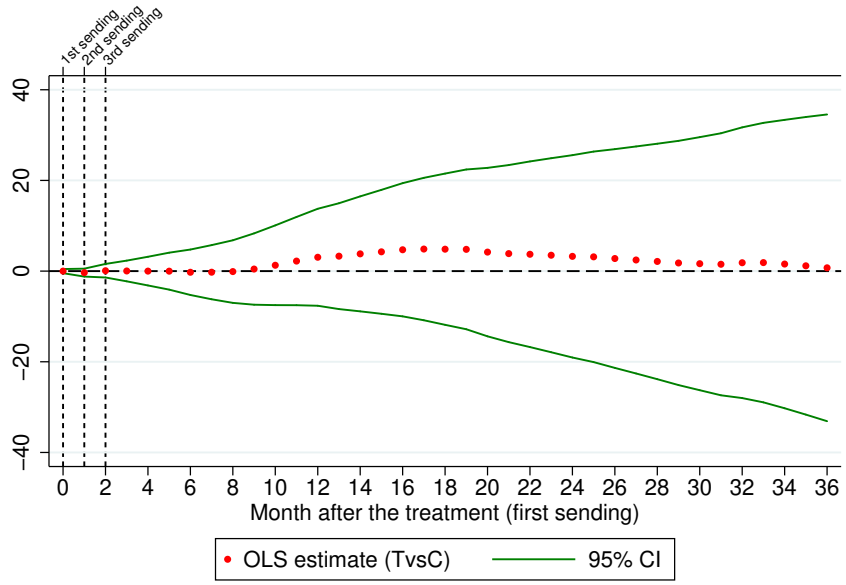
Figure 1.10 shows that there is a non-significant decrease in cumulative working hours in the treatment group 3 years after treatment. The absence of an increase in hours worked in the first year after the start of treatment suggests, in accordance with Figure 13, that small lock-in effects appear from the start of the unemployment spell insofar as the treatment has a positive effect on the cumulative number of hours worked during the compensation period from four months after the start of treatment (see Figure 8). Treated unemployed, for whom the potential duration of benefits is at least equal to 24 months show an increase (although not significant) in their number of hours of work accumulated until the date of initial exhaustion of benefits (see Figure 1.11). But it is then wiped out because they are more often unemployed close to the exhaustion date than the untreated unemployed.

Figure 1.10: Intention to treat effects on the number of accumulated hours of work



Note: Each red dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (7) on the number of accumulated hours of work. The green lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Figure 1.11: Intention to treat effects on the number of accumulated hours of work among job seekers with potential benefit duration superior or equal to 2 years



Note: Each red dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (7) on the number of accumulated hours worked. The green lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Table 1.6: Treatment intensity in the final sample

	% of treated ind. who opened the email				% of treated ind. who used the simulator			
	At 1^{st} sending	At 2^{nd} sending	At 3^{rd} sending	At least once	At 1^{st} sending	At 2^{nd} sending	At 3^{rd} sending	At least once
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All	75.4	69.3	67.8	84.8	3.4	2.6	2.3	7.5
Female	78.1	72.1	70.6	87	3.5	2.9	2.5	8
Male	72.9	66.7	65.2	82.9	3.3	2.4	2.1	7
Young	74.3	67.8	66.2	85.6	3.3	2.4	2.4	7.4
Prime age	77.6	71	69.6	86.3	3.3	2.4	2	7.1
Senior	71.6	67.6	66.2	78.7	3.9	3.6	3	9
Lower education level	63.1	58.5	57.5	73.7	3.5	2.5	2.5	7.6
Intermediate education level	75.4	68.9	67.5	85.5	3.4	2.7	2.3	7.6
Higher education level	84	77.4	75.5	91.8	3.2	2.6	2.2	7.4
Last contract $\leq$ 12 months	73.6	67.6	66.3	84.2	3.5	2.4	2.4	7.7
Last contract $\leq$ 3 months	72.3	66.8	65.7	83	3.7	2.4	2.3	7.9
Potential Benefit Duration $<$ 730 days	73.5	67.3	65.9	84.1	3.5	2.4	2.3	7.5
Potential Benefit Duration $\geq$ 730 days	76.9	70.8	69.3	85.3	3.3	2.8	2.3	7.5
Daily Reference Wage below the mean	73.3	67.1	65.7	83.6	3.3	2.5	2.3	7.3
Daily Reference Wage above the mean	79.8	73.8	72.2	87.3	3.6	2.7	2.3	7.9
Days since entry in unemp. $\leq$ 3 months	76.1	70	68	84.8	3.3	2.6	2.4	7.6
Days since entry in unemp. between 4 and 6 months	74.9	68.8	67.7	84.8	3.4	2.6	2.3	7.5
Nb. of individuals: 115,547								

Note: Columns (1), (2), (3), and (4) report the share of treated participants who opened the first email (1), the second email (2), the third email (3) or at least one email (4). Columns (5), (6), (7), and (8) report the share of treated participants who used the simulator in the first email (5), in the second email (6), in the third email (7) or at least in one email (8). See Table 1.11 for a description of the variables.

Table 1.7: Treatment effect on part-time unemployment: extensive margin

	3 months		12 months		36 months	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A : Cumulative number of months with work while on claim						
Treated (β)	0.0052*	0.0052*	0.0254**	0.0260**	0.0782***	0.0812***
	(0.0027)	(0.0027)	(0.0109)	(0.0108)	(0.0293)	(0.0290)
	[0.053]	[0.051]	[0.020]	[0.016]	[0.008]	[0.005]
In a treated area (δ)	-0.0017	0.0004	0.0035	0.0163	-0.0303	0.0082
	(0.0037)	(0.0033)	(0.0166)	(0.0130)	(0.0502)	(0.0366)
	[0.642]	[0.912]	[0.834]	[0.209]	[0.546]	[0.823]
Mean super control	0.10		0.57		1.70	
Panel B : Cumulative number of hours worked while on claim						
Treated (β)	0.3230	0.3259	2.1532**	2.2149**	6.4473**	6.7340**
	(0.2016)	(0.2001)	(0.9633)	(0.9485)	(2.8676)	(2.8181)
	[0.109]	[0.104]	[0.026]	[0.020]	[0.025]	[0.017]
In a treated area (δ)	-0.2362	-0.0676	-0.8573	0.0625	-4.6537	-1.6120
	(0.2607)	(0.2422)	(1.4521)	(1.1837)	(5.0166)	(3.6733)
	[0.365]	[0.780]	[0.555]	[0.958]	[0.354]	[0.661]
Mean super control	5.75		40.76		135.85	
Panel C : Cumulative earnings (in euro) from work while on claim						
Treated (β)	5.6210**	5.6575**	33.0513**	33.7244***	104.3254***	107.4585***
	(2.5364)	(2.5167)	(12.8756)	(12.6225)	(39.8029)	(38.4577)
	[0.027]	[0.025]	[0.010]	[0.008]	[0.009]	[0.005]
In a treated area (δ)	-4.7117	-2.9677	-17.3072	-8.7657	-70.3628	-44.2654
	(3.5402)	(3.2363)	(20.2628)	(15.6455)	(71.5434)	(49.5247)
	[0.184]	[0.359]	[0.393]	[0.575]	[0.326]	[0.372]
Mean super control	69.46		501.78		1709.82	
N	115547	115547	115547	115547	115547	115547
Covariates	No	Yes	No	Yes	No	Yes

Note: Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. p-values are reported in brackets. Each duration (i.e. 3, 12, and 36 months) indicates the elapsed time since treatment. Covariates include all stratum variables reported in Table 1.11 as well as entry months and regional fixed effects. "Treated" designates individuals who were assigned to treatment (ITT estimate), "In treated area" refers to those registered at employment agencies where half of individuals have been treated and "super control" designates individuals registered at employment agencies where nobody has been treated. The number of observations N corresponds to the number of individuals.

Table 1.8: Treatment effect on part-time unemployment: intensive margin

	3 months		12 months		36 months	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A : Cumulative number of hours worked while on claim at the intensive margin						
Treated (β)	-0.0200	-1.4426	7.1282*	5.5718	16.7444**	11.3161
	(2.3061)	(2.2151)	(3.9264)	(3.4865)	(8.3361)	(7.5458)
	[0.993]	[0.515]	[0.070]	[0.110]	[0.045]	[0.13 4]
In a treated area (δ)	-0.8517	0.7068	-2.6126	-2.5034	0.4418	-0.9476
	(3.0287)	(2.6508)	(6.1278)	(4.6174)	(14.1505)	(9.3651)
	[0.779]	[0.790]	[0.670]	[0.588]	[0.975]	[0.919]
Mean super control	89.20		215.80		446.51	
Panel B : Cumulative earnings (in euro) from work while on claim at the intensive margin						
Treated (β)	27.4618	-1.6892	122.7403**	88.6023**	289.2814**	191.0127*
	(29.5058)	(26.7263)	(54.6951)	(44.5939)	(117.8574)	(100.0897)
	[0.352]	[0.950]	[0.025]	[0.047]	[0.014]	[0.057]
In a treated area (δ)	-40.0733	-18.0860	-68.2410	-74.0666	-34.6073	-73.2656
	(46.4326)	(33.8810)	(96.6964)	(57.1514)	(223.4429)	(121.3045)
	[0.388]	[0.594]	[0.481]	[0.195]	[0.877]	[0.546]
Mean super control	1076.53		2656.41		5619.95	
Covariates	No	Yes	No	Yes	No	Yes
N	7435	7435	21840	21840	34317	34317

Note: This table reports the estimates of the impact of the treatment on the cumulative number of hours of work while on claim and on the cumulative earnings from work while on claim for the subset of job seekers who worked while on claim at least one day. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. *p*-values are reported in brackets. Each duration (i.e. 3, 12, and 36 months) indicates the elapsed time since treatment. Covariates include all stratum variables reported in Table 1.11as well as entry months and regional fixed effects. "Treated" designates individuals who were assigned to treatment (ITT estimate), "In treated area" refers to those registered at employment agencies where half of individuals have been treated and "super control" designates individuals registered at employment agencies where nobody has been treated. The number of observations N corresponds to the number of individuals.

Table 1.9: Summary statistics for those most and least affected by the treatment
Outcome: Prob. to work while on claim at least once

	Linear Regression			Elastic Net		
	Most Affected	Least Affected	Difference	Most Affected	Least Affected	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Job seekers characteristics						
Female	0.480	0.462	0.020	0.491	0.452	0.035
	-	-	[0.127]	-	-	[0.003]
Elderly	0.123	0.187	-0.064	0.101	0.203	-0.099
	-	-	[0.000]	-	-	[0.000]
Young	0.485	0.330	0.151	0.471	0.326	0.147
	-	-	[0.000]	-	-	[0.000]
Intermediary aged	0.380	0.489	-0.102	0.412	0.474	-0.066
	-	-	[0.000]	-	-	[0.000]
Lower education	0.196	0.281	-0.086	0.163	0.286	-0.119
	-	-	[0.000]	-	-	[0.000]
Upper education	0.527	0.392	0.143	0.520	0.379	0.147
	-	-	[0.000]	-	-	[0.000]
Higher education	0.269	0.324	-0.045	0.291	0.336	-0.038
	-	-	[0.000]	-	-	[0.001]
Last contract inf to 3 m	0.274	0.024	0.256	0.315	0.023	0.285
	-	-	[0.000]	-	-	[0.000]
Last contract inf to 12 m	0.494	0.269	0.235	0.540	0.273	0.271
	-	-	[0.000]	-	-	[0.000]
Daily reference wage	69.34	57.85	11.730	83.62	56.84	26.350
	-	-	[0.000]	-	-	[0.000]
PBD	567.1	640.0	-73.81	557.5	649.8	-95.27
	-	-	[0.000]	-	-	[0.000]
Local agencies characteristics						
Number of participants	179.4	226.9	-46.65	198.4	231.8	-33.43
	-	-	[0.000]	-	-	[0.000]
Number of claimants	3901	4319	-430.6	3998	4400	-430.4
	-	-	[0.000]	-	-	[0.000]
Share of part-time unemployed	0.444	0.429	0.011	0.427	0.429	-0.002
	-	-	[0.000]	-	-	[0.416]
Share of recurrent job seekers	0.426	0.427	-0.001	0.420	0.429	-0.008
	-	-	[0.554]	-	-	[0.000]
Unemployment rate	13.37	14.05	-0.668	13.04	14.02	-0.961
	-	-	[0.000]	-	-	[0.000]

Note: The outcome is measured 12 months after the treatment date. The results are presented for the two best ML methods regarding this outcome : Linear Regression and Elastic Net. The most affected group refers to the top 5% of the distribution of $\hat{S}(X_i)$ whereas the least affected group refers to the bottom 50%. The parameter estimates and p-values - displayed in brackets - are computed as medians over 100 splits, with nominal levels adjusted to account for the splitting uncertainty.

Table 1.10: Treatment effect on unemployment insurance payments

	1st year		2nd year		3rd year		All years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A : Unemployment insurance payments (in euro) net of taxes								
Treated (β)	20.2819	12.6762	18.7764	11.4147	42.2967	37.6859	81.3550	61.7768
	(61.5429)	(38.5711)	(58.2378)	(44.2870)	(44.1259)	(38.8300)	(137.8379)	(92.0106)
	[0.742]	[0.743]	[0.747]	[0.797]	[0.338]	[0.332]	[0.555]	[0.502]
In a treated area (δ)	205.3856	13.5991	54.3455	-54.3729	-36.9611	-53.6301	222.7700	-94.4039
	(327.8550)	(59.7480)	(244.0963)	(58.5165)	(96.1035)	(57.4613)	(648.0012)	(142.9254)
	[0.531]	[0.820]	[0.824]	[0.353]	[0.701]	[0.351]	[0.731]	[0.509]
Controls var	No	Yes	No	Yes	No	Yes	No	Yes
Mean super control	7950.84		4228.02		1566.92		13745.78	
Panel B : Unemployment insurance payments (in euro)								
Treated (β)	18.5352	10.9208	15.7558	8.2395	43.6383	38.7334	77.9293	57.8937
	(60.1002)	(35.5741)	(56.4778)	(40.3723)	(42.8495)	(36.1003)	(136.1165)	(83.2331)
	[0.758]	[0.759]	[0.780]	[0.838]	[0.309]	[0.284]	[0.567]	[0.487]
In a treated area (δ)	204.5963	11.5053	61.9056	-54.7882	-23.3730	-50.4534	243.1289	-93.7363
	(336.1321)	(54.9017)	(263.1796)	(53.2496)	(116.3471)	(52.8787)	(701.5659)	(128.7493)
	[0.543]	[0.834]	[0.814]	[0.304]	[0.841]	[0.340]	[0.729]	[0.467]
Controls var	No	Yes	No	Yes	No	Yes	No	Yes
Mean super control	8383.06		4981.52		2447.24		15811.83	
Panel C : Number of days of compensated unemployment								
Treated (β)	0.1479	0.1106	-0.0439	-0.0598	0.6404	0.6061	0.7444	0.6569
	(0.8390)	(0.7206)	(0.8216)	(0.6886)	(0.6386)	(0.5886)	(1.7570)	(1.4271)
	[0.860]	[0.878]	[0.957]	[0.931]	[0.316]	[0.303]	[0.672]	[0.645]
In a treated area (δ)	1.5650	0.0032	-0.5512	-0.9503	-1.5087	-0.9996	-0.4949	-1.9468
	(1.7620)	(1.0568)	(1.4923)	(1.0428)	(1.0131)	(0.8371)	(3.2277)	(2.2341)
	[0.375]	[0.998]	[0.712]	[0.362]	[0.137]	[0.233]	[0.878]	[0.384]
Controls var	No	Yes	No	Yes	No	Yes	No	Yes
Mean super control	211.46		112.32		54.87		378.64	
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
N	115547	115547	115547	115547	115547	115547	115547	115547

Note : This table reports the effect of the treatment on unemployment insurance payments in the first, second, third year after the start of the treatment and during all 3 years after the start of the treatment. Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. p-values are reported in brackets. Covariates include all stratum variables reported in Table 1.11 as well as entry months and regions fixed effects. "Treated" designates individuals who were assigned to treatment (ITT estimate), "In treated area" refers to those registered at employment agencies where half of individuals have been treated and "super control" designates individuals registered at employment agencies where nobody has been treated. The number of observations N corresponds to the number of individuals.

Table 1.11: Summary statistics on the final sample

	Means				p-value of the difference		
	All	T	C	SC	T - C	T - (C + SC)	T = C = SC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Job seekers characteristics							
Female	.472	.473	.473	.467	.967	.496	.335
Age	32.645	32.639	32.632	32.683	.935	.831	.972
Young	.378	.375	.377	.386	.398	.82	.406
Prime age	.461	.464	.462	.449	.545	.475	.318
Senior	.161	.161	.16	.165	.77	.46	.7
Lower education level	.239	.239	.236	.242	.256	.21	.406
Intermediate education level	.432	.427	.432	.444	.101	.926	.081
Higher education level	.329	.334	.332	.313	.485	.362	.301
Last contract duration $\leq$ 12 months	.338	.335	.336	.344	.743	.656	.675
Last contract duration $\leq$ 3 months	.089	.088	.09	.091	.249	.559	.465
Potential benefit duration	621.096	621.506	621.507	619.456	.999	.793	.948
... < 730 days	.44	.44	.441	.441	.652	.793	.9
... $\geq$ 730 days	.56	.56	.559	.559	.652	.793	.9
Daily Reference Wage	62.948	63.137	63.166	62.138	.93	.652	.901
... $\leq$ the mean	.678	.678	.677	.678	.961	.973	.999
... > the mean	.322	.322	.323	.322	.961	.973	.999
Unemployment entry month							
July 2016	.157	.158	.156	.159	.234	.196	.42
August 2016	.163	.164	.165	.158	.622	.181	.137
September 2016	.279	.279	.279	.281	.823	.699	.909
October 2016	.229	.228	.231	.229	.273	.296	.51
November 2016	.171	.17	.17	.174	.769	.462	.623
Local agencies characteristics							
Unemployment rate	13.761	13.771	13.757	13.749	.676	.955	.912
Share of part time unemployment	.434	.433	.432	.438	.309	.35	.425
Share of long-term unemp	.429	.429	.429	.429	.398	.979	.668
Exit rate from unemp	.064	.064	.064	.064	.193	.431	.337
Number of claimants	4361.794	4366.773	4377.762	4320.004	.305	.624	.477
Number of participants	224.45	226.913	227.873	212.704	.213	.108	.127
N	115547	46191	46200	23156			

Note: This table reports descriptive statistics for the sample of individuals in January 2017, after dropping observations for individuals who were not on claim or who had already worked while on claim on 31 January 2017. Columns (1), (2), (3) and (4) report the means of individual characteristics for the treatment, the control and the super control sub-samples, respectively. Columns (5)–(7) report the *p*-values for the difference between assigned to treatment (T) and assigned to control (C) (column 5), the difference between assigned to treatment (T) and non assigned (C + SC), and for the joint significance of assignment status (T, C and SC). *Female* equals 1 if the participant is female. *Age* is the age of the participant when the first email was sent. *Young* equals 1 if the participant is younger than 25 years old. *Prime age* equals 1 if the participant is between 25 and 50 years old. *Senior* equals 1 if the participant is above 50 years old. *Lower education level* equals 1 if the participant did not pass the Baccalauréat. *Intermediate education level* equals 1 if the participant passed the Baccalauréat. *Higher education level* equals 1 if the participant has a university degree. *Potential benefit duration* represents the maximum duration of unemployment when the participant does not work while on claim. *Daily reference wage* represents the daily wage earned prior unemployment. *Days since entry* represents the number of days since entry into unemployment when the first email was sent. *Unemployment entry month* represents the starting month of the participant's unemployment spell. Variables mentioning $<$ *mean* ($>$ *mean*) equal 1 for participants whose value of the variable in question is respectively below or above the mean. *Last contract duration* $\leq n$ *months* equals 1 for participants who entered into unemployment after a contract shorter than n months. *Unemployment rate*: unemployment rate in the area of the employment agency in December 2016. *Share of part-time unemp*: agency share of job seekers working while on claim in December 2016. *Share of long-term unemp*: agency share of job seekers whose unemployment duration is longer than one month in the area of the employment agency in December 2016. *Exit rate from unemp*: agency average unemployment exit rate in December 2016. *Number of claimants*: number of job seekers by agency in December 2016. *Number of participants*: number of individuals included in our sample by agency.

Table 1.12: Summary statistics on the overall sample

	Means				*p*-value of the difference		
	All	T	C	SC	T - C	T - (C + SC)	T = C = SC
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Job seekers characteristics							
Worked while on claim before treatment	.127	.126	.126	.13	.868	.371	.414
Still on claim at treatment date	.901	.901	.9	.905	.858	.354	.404
Female	.477	.479	.479	.472	.946	.403	.138
Age	31.5	31.511	31.498	31.484	.863	.97	.977
Young	.419	.416	.418	.43	.436	.514	.265
Prime age	.442	.446	.445	.429	.735	.24	.237
Senior	.139	.139	.137	.141	.531	.342	.632
Lower education level	.224	.224	.222	.228	.321	.237	.462
Intermediate education level	.435	.431	.433	.446	.332	.531	.195
Higher education level	.341	.345	.345	.326	.887	.219	.365
Last contract duration $\leq$ to 12 months	.367	.365	.365	.375	.941	.374	.567
Last contract duration $\leq$ to 3 months	.106	.105	.106	.109	.898	.669	.754
Potential benefit duration	601.958	602.089	602.836	599.949	.632	.522	.807
... < 730 days	.469	.47	.468	.471	.659	.677	.891
... $\geq$ 730 days	.531	.53	.532	.529	.659	.677	.891
Daily Reference Wage	60.245	60.457	60.472	59.371	.957	.603	.866
... $\leq$ the mean	.669	.667	.669	.673	.492	.964	.698
... > the mean	.331	.333	.331	.327	.492	.964	.698
Unemployment entry month							
July 2016	.156	.157	.154	.156	.146	.17	.315
August 2016	.161	.161	.163	.157	.352	.091	.126
September 2016	.288	.288	.288	.289	.89	.774	.938
October 2016	.232	.231	.233	.231	.389	.398	.648
November 2016	.163	.163	.162	.167	.781	.401	.522
Local Agencies characteristics							
Unemployment rate	13.705	13.712	13.712	13.678	.983	.922	.994
Share of part time unemp	.435	.434	.434	.44	.245	.329	.318
Share of recurrent job seekers	.429	.429	.429	.428	.37	.958	.612
Exit rate from unemp	.064	.064	.064	.064	.215	.526	.416
Number of claimants	4365.983	4367.24	4378.652	4338.219	.227	.701	.398
Number of participants	223.505	225.884	226.813	212.166	.172	.119	.129
N	147878	59112	59117	29649			

Note: This table reports descriptive statistics for the sample of individuals on January 2017 before dropping observations for individuals who were not on claim or who had already worked while on claim on 31 January 2017. Columns (1), (2), (3) and (4) report the means of individual characteristics for the treatment, the control and the super control sub-samples, respectively. Columns (5)–(7) report the *p*-values for the difference between assigned to treatment (T) and assigned to control (C) (column 5), the difference between assigned to treatment (T) and non assigned (C + SC), and for the joint significance of assignment status (T, C and SC). See Table 2 for the definition of each covariate.

Table 1.13: Treatment effect on the probability to work while on claim

	3 months		6 months		12 months		36 months	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A : Prob. to work while on claim at least once								
Treated (β)	0.0037**	0.0037**	0.0044**	0.0044**	0.0037	0.0038	0.0033	0.0033
	(0.0016)	(0.0016)	(0.0022)	(0.0022)	(0.0027)	(0.0027)	(0.0030)	(0.0030)
	[0.025]	[0.023]	[0.046]	[0.041]	[0.177]	[0.164]	[0.277]	[0.264]
In a treated area (δ)	-0.0021	-0.0006	0.0005	0.0037	-0.0017	0.0026	-0.0107*	-0.0038
	(0.0024)	(0.0020)	(0.0034)	(0.0026)	(0.0045)	(0.0032)	(0.0063)	(0.0040)
	[0.384]	[0.765]	[0.874]	[0.147]	[0.708]	[0.417]	[0.090]	[0.345]
Mean super control	0.06		0.11		0.19		0.30	
Panel B : Prob. to work while on claim at least two months								
Treated (β)	0.0013	0.0013	0.0033**	0.0033**	0.0045**	0.0046**	0.0037	0.0038
	(0.0010)	(0.0010)	(0.0017)	(0.0016)	(0.0022)	(0.0022)	(0.0027)	(0.0027)
	[0.221]	[0.219]	[0.044]	[0.041]	[0.043]	[0.037]	[0.184]	[0.163]
In a treated area (δ)	-0.0002	0.0005	0.0003	0.0023	0.0011	0.0045*	-0.0057	0.0002
	(0.0013)	(0.0013)	(0.0023)	(0.0019)	(0.0034)	(0.0026)	(0.0052)	(0.0036)
	[0.887]	[0.719]	[0.910]	[0.233]	[0.734]	[0.083]	[0.275]	[0.965]
Mean super control	0.03		0.06		0.12		0.23	
Panel C : Prob. to work while on claim at least three months								
Treated (β)	0.0003	0.0003	0.0030**	0.0030***	0.0038**	0.0039**	0.0047**	0.0049**
	(0.0005)	(0.0005)	(0.0012)	(0.0012)	(0.0018)	(0.0018)	(0.0024)	(0.0024)
	[0.624]	[0.616]	[0.011]	[0.009]	[0.035]	[0.029]	[0.050]	[0.037]
In a treated area (δ)	0.0005	0.0005	-0.0006	-0.0001	0.0008	0.0029	-0.0041	0.0001
	(0.0007)	(0.0006)	(0.0016)	(0.0015)	(0.0027)	(0.0023)	(0.0044)	(0.0032)
	[0.434]	[0.430]	[0.694]	[0.955]	[0.771]	[0.203]	[0.345]	[0.969]
Mean super control	0.01		0.03		0.08		0.17	
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
N	115547	115547	115547	115547	115547	115547	115547	115547

Note: Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. p-values are reported in brackets. Each duration (i.e. 3, 6, 12 and 36 months) indicates the elapsed time since treatment. Covariates include all stratum variables reported in Table 2 as well as entry months and regional fixed effects. "Treated" designates individuals who were assigned to treatment (ITT estimate), "In treated area" refers to those registered at employment agencies where half of individuals have been treated and "super control" designates individuals registered at employment agencies where nobody has been treated. The number of observations N corresponds to the number of individuals.

Table 1.14: Treatment effect on part-time unemployment : model vs randomization based inference

	3 months			12 months			36 months		
	Coeff.	*p*-value		Coeff.	*p*-value		Coeff.	*p*-value	
	estimate	model based	rand. inference	estimate	model based	rand. inference	estimate	model based	rand. inference
Panel A : Extensive margin									
Panel A.1 : Cumulative number of months with work while on claim									
Treated (β)	0.0052	0.0505	0.061	0.0260	0.0156	0.015	0.0812	0.0052	0.005
In a treated area (δ)	0.0004	0.9116	0.903	0.0163	0.2090	0.210	0.0082	0.8230	0.816
Panel A.2 : Cumulative number of hours worked while on claim									
Treated (β)	0.3246	0.1043	0.115	2.2044	0.0196	0.022	6.7753	0.0156	0.021
In a treated area (δ)	-0.0628	0.7950	0.807	0.0595	0.9598	0.962	-1.5359	0.6735	0.672
Panel A.3 : Cumulative earnings (in euro) from work while on claim									
Treated (β)	5.6575	0.0246	0.027	33.7244	0.0075	0.007	107.4585	0.0052	0.007
In a treated area (δ)	-2.9677	0.3591	0.337	-8.7657	0.5753	0.572	-44.2654	0.3714	0.366
Covariates		Yes			Yes			Yes	
N		115547			115547			115547	
Panel B : Intensive margin									
Panel B.1 : Cumulative number of hours worked while on claim									
Treated (β)	-1.4552	0.5109	0.499	5.5382	0.1105	0.136	11.5298	0.1239	0.139
In a treated area (δ)	0.7782	0.7681	0.765	-2.5141	0.5842	0.577	-0.7025	0.9397	0.939
Panel B.2 : Cumulative earnings (in euro) from work while on claim									
Treated (β)	-1.6892	0.9496	0.953	88.6023	0.0469	0.061	191.0127	0.0563	0.070
In a treated area (δ)	-18.0860	0.5935	0.581	-74.0666	0.1950	0.207	-73.2656	0.5459	0.574
Covariates		Yes			Yes			Yes	
N		7435			21840			34317	

Note: This table presents the results obtained for the outcomes related to part time unemployment for both extensive margin (*Panel A*) and intensive margin (*Panel B*), that is, only for people who worked at least one hour while on claim in the period. Each duration (i.e. 3, 12, and 36 months) indicates the elapsed time since treatment. For each duration, the first two columns display the coefficient estimate and the model based *p*-value that are presented in subsection 1.5.2 and the third column corresponds to the *p*-value based on a two-sided randomization inference test statistic.

Table 1.15: Treatment effect on regular employment : model vs randomization based inference

	Potential Benefit Duration								
	All sample			< 730			≥ 730		
	Coeff.	p-value		Coeff.	p-value		Coeff.	p-value	
	estimate	model based	rand. inference	estimate	model based	rand. inference	estimate	model based	rand. inference
Panel A : Prob. to be in regular employment in the last month									
Treated (β)	-0.0059	0.0452	0.053	0.0020	0.6477	0.635	-0.0125	0.0031	0.002
In a treated area (δ)	0.0015	0.7247	0.693	-0.0052	0.3843	0.346	0.0072	0.1924	0.164
Panel B : Prob. to be in regular employment in the last quarter									
Treated (β)	-0.0052	0.0935	0.093	0.0000	0.9949	0.995	-0.0096	0.0273	0.020
In a treated area (δ)	-0.0019	0.6598	0.611	-0.0070	0.2625	0.215	0.0028	0.6091	0.589
Covariates		Yes			Yes			Yes	
N		115547			50887			64660	

Note: This table presents the results obtained for the outcomes related to unemployment in table 7. Each duration (i.e. 3, 12, and 36 months) indicates the elapsed time since treatment. For each duration, the first two columns display the coefficient estimate and the model based p-value that are presented in subsection 1.5.3 and the third column corresponds to the p-value based on a two-sided randomization inference test statistic.

Table 1.16: Best Linear Predictor of the conditional average treatment effect

ATE (β_1)		HET (β_2)		Best ML method
Coeff.	p-value	Coeff.	p-value	
(1)	(2)	(3)	(4)	(5)
Prob. to work while on claim at least once - 12 months after treatment				
0.004	[0.650]	0.266	[0.080]	Linear Regression
Cumulative nb. of months worked in part-time unemployment - 12 months after treatment				
0.025	[0.196]	0.090	[1.000]	Linear Regression
Cumulative earnings from work while on claim - 12 months after treatment				
31.92	[0.143]	0.335	[0.362]	Elastic Net
Out of unemployment in last month before benefit exhaustion				
-0.005	[0.417]	-0.086	[0.536]	Boosting

Note: The parameter estimates and p-values - displayed in brackets - are computed as medians over 100 splits, with nominal levels adjusted to account for the splitting uncertainty.

Table 1.17: GATES of Most and Least Affected Groups

Heterogeneity group			Best ML method
Top 5% (γ_5)	Bottom 50% (γ_1)	Difference ($\gamma_5 - \gamma_1$)	
(1)	(2)	(3)	(4)
Prob. to work while on claim at least once - 12 months after treatment			
0.038	-0.001	0.038	Linear Regression
[0.038]	[1.000]	[0.048]	
Cumulative nb. of months worked in part-time unemp. - 12 months after treatment			
0.113	0.021	0.093	Linear Regression
[0.274]	[0.619]	[0.428]	
Cumulative earnings from work while on claim - 12 months after treatment			
194.50	15.32	176.200	Elastic Net
[0.514]	[0.793]	[0.610]	
Out of unemployment in last month before benefit exhaustion			
-0.018	-0.002	-0.017	Boosting
[0.699]	[1.000]	[0.862]	

Note: The parameter estimates and p-values - displayed in brackets - are computed as medians over 100 splits, with nominal levels adjusted to account for the splitting uncertainty.

Table 1.18: Treatment effect interacted with the elapsed unemployment duration at treatment date

Outcome measured 6 months after the treatment	Prob. to work while on claim at least once	Cumulated nb. of months worked while on claim	Cumulated nb. of hours worked while on claim	Cumulated earnings from work while on claim
	(1)	(2)	(3)	(4)
Treated	0.005*	0.017**	0.917	15.477**
	(0.0028)	(0.0073)	(0.6099)	(7.7612)
	[0.061]	[0.023]	[0.133]	[0.046]
Above median	-0.009***	-0.015**	-1.592***	-13.733**
	(0.0025)	(0.0064)	(0.5302)	(6.7017)
	[0.000]	[0.017]	[0.003]	[0.041]
Treated X Above median	-0.002	-0.004	0.463	4.423
	(0.0038)	(0.0101)	(0.8483)	(10.7809)
	[0.627]	[0.670]	[0.585]	[0.682]
Mean super control	0.11	0.24	16.14	198.40
Covariates	Yes	Yes	Yes	Yes
N	115547	115547	115547	115547

Note: Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. p-values are reported in brackets. Each column displays the results from an OLS regression of the associated outcome based on equation (7). "Treated" designates individuals who were assigned to treatment (ITT estimate). "Above median" designates individuals whose elapsed unemployment duration at treatment date is above median, which corresponds to ∼4,5 months. Each regression include the list of covariates reported in the summary statistics (see Table 2) as well as regions fixed effects. Entry months fixed effects are not included in these regressions to avoid collinearity issues with the "Above median" regressor. The number of observations N corresponds to the number of individuals.

Table 1.19: Correlations between individual / local characteristics and part-time unemployment activity 3 months after the treatment in the super control group

Outcome measured 3 months after the treatment	Prob. to work while on claim at least once	Cumulated nb. of months worked while on claim	Cumulated nb. of hours worked while on claim	Cumulated earnings from work while on claim
	(1)	(2)	(3)	(4)
Job seekers characteristics				
Female	0.012***	0.023***	1.044**	14.795**
	(0.0033)	(0.0055)	(0.4290)	(6.2267)
Young	0.006	0.002	-0.868*	-5.699
	(0.0040)	(0.0070)	(0.4909)	(6.4642)
Senior	-0.021***	-0.032***	-2.207**	-24.535*
	(0.0058)	(0.0103)	(0.8530)	(12.4490)
Higher education	-0.015***	-0.026***	-0.801	-4.669
	(0.0040)	(0.0068)	(0.4973)	(6.2285)
Lower secondary education	-0.009**	-0.017**	-1.177***	-10.093*
	(0.0042)	(0.0070)	(0.4414)	(5.3815)
Potential benefit duration	0.000	0.000	0.001	0.016
	(0.0000)	(0.0000)	(0.0014)	(0.0193)
Daily Reference Wage	0.000**	0.000***	0.035***	0.698***
	(0.0000)	(0.0001)	(0.0072)	(0.1613)
Last contract inf. to 3 m.	0.009	0.018	1.483*	15.822
	(0.0075)	(0.0116)	(0.8350)	(9.8805)
Last contract inf. to 12 m.	0.013**	0.012	0.941	10.170
	(0.0055)	(0.0094)	(0.6926)	(9.3814)
Local agencies characteristics				
Number of participants	-0.000	-0.000	-0.006	-0.034
	(0.0001)	(0.0001)	(0.0051)	(0.0830)
Number of claimants	0.000*	0.000*	0.000	0.002
	(0.0000)	(0.0000)	(0.0003)	(0.0038)
Share of part-time unemp.	0.049	0.055	1.488	0.951
	(0.0431)	(0.0704)	(5.5228)	(88.3497)
Exit rate from unemp	-0.133	0.022	3.992	-75.241
	(0.3174)	(0.4961)	(37.5766)	(492.3760)
Share of recurrent job seekers	-0.057	-0.067	3.723	68.471
	(0.0684)	(0.1109)	(7.2266)	(103.8840)
Unemployment rate	-0.000	-0.000	-0.052	-0.826
	(0.0004)	(0.0006)	(0.0429)	(0.5294)
N	23156	23156	23156	23156
R^2	0.007	0.006	0.006	0.009

Note: Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. Each column displays the results from an OLS regression of the associated outcome on the listed covariates as well as entry months and regional fixed effects. The number of observations N corresponds to the number of individuals.

Table 1.20: Correlations between individual / local characteristics and part-time unemployment activity 12 months after the treatment in the super control group

Outcome measured 12 months after the treatment	Prob. to work while on claim at least once	Cumulated nb. of months worked while on claim	Cumulated nb. of hours worked while on claim	Cumulated earnings from work while on claim
	(1)	(2)	(3)	(4)
Job seekers characteristics				
Female	0.035***	0.193***	13.562***	169.492***
	(0.0052)	(0.0231)	(2.1664)	(28.2983)
Young	0.008	-0.103***	-12.496***	-117.510***
	(0.0068)	(0.0250)	(2.3262)	(31.3109)
Senior	-0.109***	-0.300***	-24.269***	-285.148***
	(0.0100)	(0.0446)	(4.1396)	(58.4379)
Higher education	-0.039***	-0.124***	-4.981*	-16.307
	(0.0067)	(0.0274)	(2.5520)	(32.9411)
Lower secondary education	-0.004	-0.048*	-5.076**	-38.191
	(0.0059)	(0.0248)	(2.1947)	(28.0274)
Potential benefit duration	0.000***	0.000***	0.027***	0.307***
	(0.0000)	(0.0001)	(0.0061)	(0.0788)
Daily Reference Wage	0.000**	0.002***	0.355***	6.632***
	(0.0001)	(0.0004)	(0.0510)	(0.9395)
Last contract inf. to 3 m.	0.012	0.039	1.774	14.672
	(0.0111)	(0.0345)	(2.8504)	(33.7341)
Last contract inf. to 12 m.	0.002	-0.040	-2.660	-39.970
	(0.0089)	(0.0348)	(3.0241)	(38.5039)
Local agencies characteristics				
Number of participants	-0.000***	-0.001***	-0.075***	-0.889**
	(0.0001)	(0.0002)	(0.0237)	(0.3426)
Number of claimants	0.000***	0.000***	0.003***	0.043**
	(0.0000)	(0.0000)	(0.0012)	(0.0170)
Share of part-time unemp.	0.165***	0.578***	24.608	210.025
	(0.0605)	(0.2168)	(21.7140)	(324.3254)
Share of recurrent job seekers	0.091	0.426	54.738	638.739
	(0.0978)	(0.3727)	(35.4824)	(476.4667)
Exit rate from unemp.	0.290	0.507	94.238	809.370
	(0.4377)	(1.7558)	(174.9111)	(2261.9591)
Unemployment rate	0.001	-0.002	-0.307	-5.034*
	(0.0005)	(0.0022)	(0.1964)	(2.6034)
N	23156	23156	23156	23156
R^2	0.018	0.019	0.026	0.035

Note: Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. Each column displays the results from an OLS regression of the associated outcome on the listed covariates as well as entry months and regional fixed effects. The number of observations N corresponds to the number of individuals.

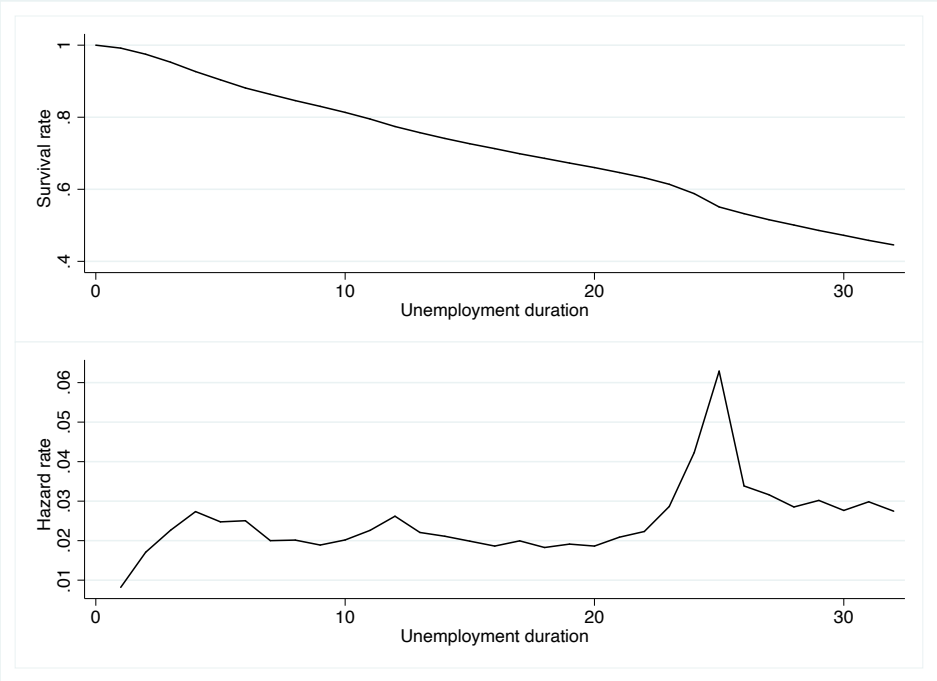
Table 1.21: Correlations between individual / local characteristics and part-time unemployment activity 36 months after the treatment in the super control group

Outcome measured 36 months after the treatment	Prob. to work while on claim at least once	Cumulated nb. of months worked while on claim	Cumulated nb. of hours worked while on claim	Cumulated earnings from work while on claim
	(1)	(2)	(3)	(4)
Job seekers characteristics				
Female	0.058***	0.754***	58.392***	733.635***
	(0.0062)	(0.0641)	(6.2670)	(80.8263)
Young	0.018**	-0.430***	-43.769***	-354.968***
	(0.0075)	(0.0667)	(6.5472)	(87.0175)
Senior	-0.154***	-0.538***	-50.469***	-581.447***
	(0.0126)	(0.1187)	(12.4086)	(182.4914)
Higher education	-0.076***	-0.366***	-23.598***	-150.368
	(0.0080)	(0.0713)	(7.6686)	(102.1160)
Lower secondary education	0.009	-0.126*	-18.880**	-149.567
	(0.0076)	(0.0743)	(7.4010)	(94.7158)
Potential benefit duration	0.000***	0.002***	0.125***	1.399***
	(0.0000)	(0.0002)	(0.0155)	(0.2088)
Daily Reference Wage	0.000***	0.010***	1.548***	29.057***
	(0.0001)	(0.0012)	(0.1536)	(2.7619)
Last contract inf. to 3 m.	0.027*	0.109	3.562	3.077
	(0.0139)	(0.0809)	(7.1952)	(89.1138)
Last contract inf. to 12 m.	-0.003	-0.166**	-14.454*	-198.793*
	(0.0102)	(0.0793)	(8.2918)	(110.1325)
Local agencies characteristics				
Number of participants	-0.000**	-0.003***	-0.268***	-3.381***
	(0.0001)	(0.0007)	(0.0780)	(1.0473)
Number of claimants	0.000	0.000**	0.009**	0.125**
	(0.0000)	(0.0000)	(0.0042)	(0.0581)
Share of part-time unemp.	0.164*	2.404***	177.679**	2327.809**
	(0.0923)	(0.6474)	(75.8056)	(1098.0432)
Exit rate from unemp	1.199**	1.979	48.165	-3301.319
	(0.5670)	(5.2555)	(547.9057)	(7248.3622)
Share of recurrent job seekers	0.239*	1.044	92.367	528.277
	(0.1225)	(1.0487)	(111.2905)	(1566.6654)
Unemployment rate	0.001	-0.008	-1.070*	-18.106**
	(0.0007)	(0.0060)	(0.5970)	(8.1095)
N	23156	23156	23156	23156
R^2	0.031	0.043	0.051	0.065

Note: Levels of significance: * < 0.10, ** < 0.05, *** < 0.01. Standard errors, reported in parenthesis below the coefficients, are robust and clustered at the local agency level. Each column displays the results from an OLS regression of the associated outcome on the listed covariates as well as entry months and regional fixed effects. The number of observations N corresponds to the number of individuals.

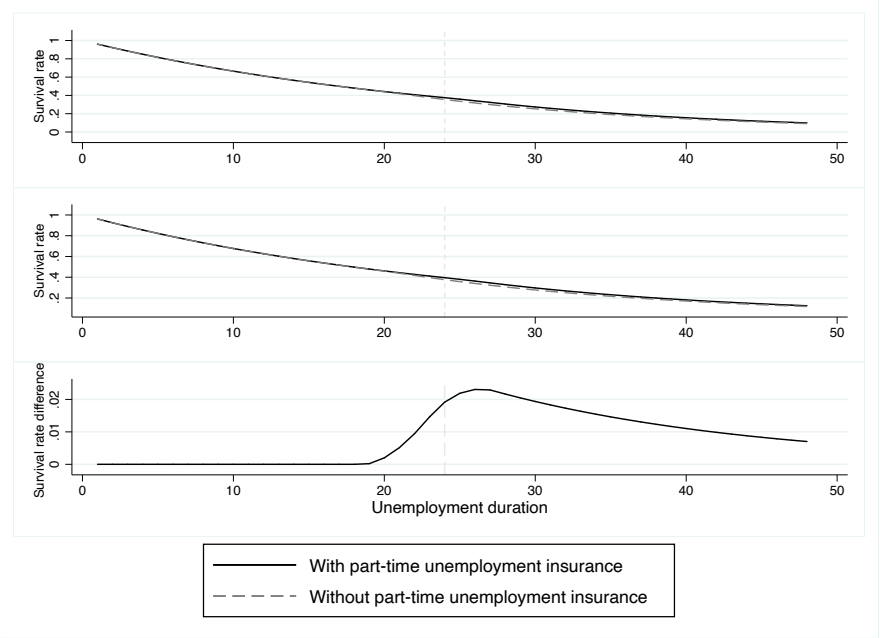
J Supplementary Figures

Figure 1.12: Survival rate in unemployment and exit rate from unemployment to employment of non-treated individuals



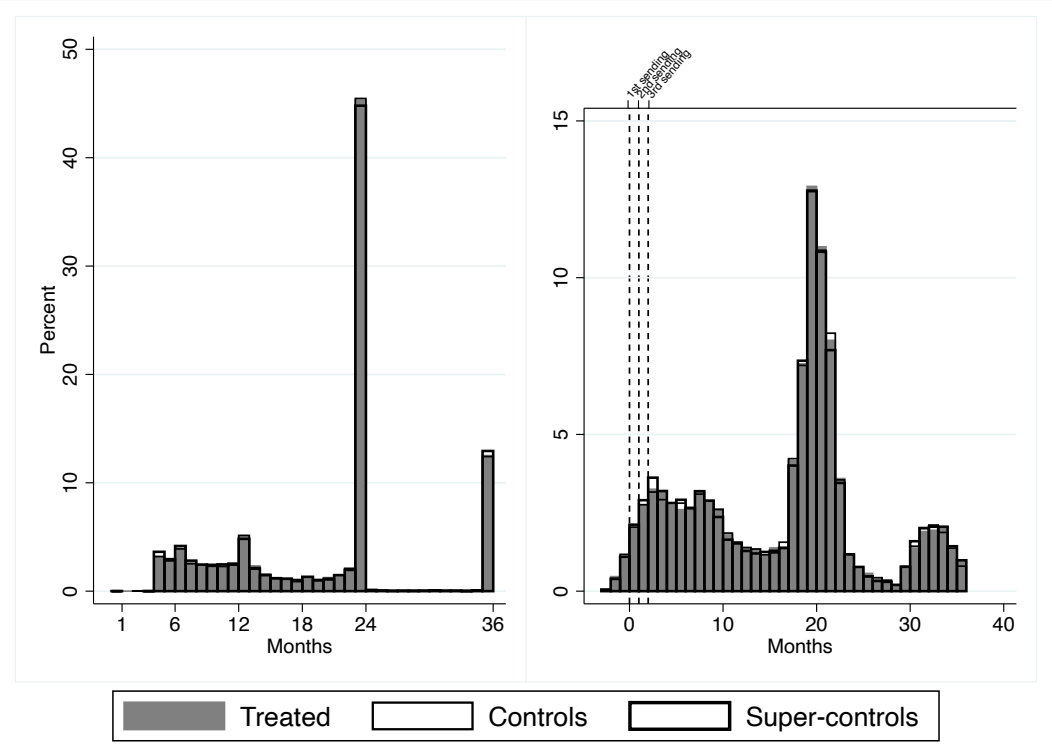
Note: This figure displays the Kaplan-Meier estimate of the survival rate in unemployment and the monthly exit rate from unemployment to employment of unemployed workers whose initial potential benefit duration is equal to 24 months. Unemployment comprises compensated and non-compensated unemployment meaning that all individuals are considered as unemployed until they find a regular job.

Figure 1.13: The unemployment survival rate in the job search model



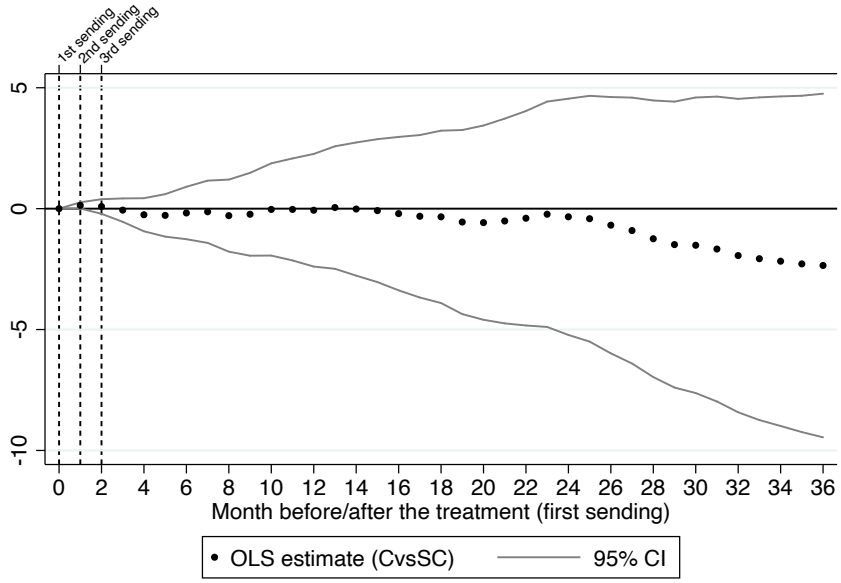
Note: The job search model is simulated assuming that the monthly discount factor $\beta = 0.996$, which corresponds to an annual discount rate equal to 5%; $v(c) = \log(c)$; $\lambda(e) = \lambda_0 \left(\lambda_1 - \exp^{-\gamma e}\right)$; the value of benefits is normalized to one: $b = 1$; the replacement ratio is equal to 0.5 implying that the wage of regular jobs, the duration of which is infinite and which yield the value W is equal to 2; the initial potential benefit duration equals 24 months; the tax rate on earnings from work while on claim $\tau = 0.85$; the share α of current earnings reported at the end of the entitlement period, that will be obtained only if the person is still unemployed in this period, is equal to τ; the distribution of earnings from work while on claim z_t is uniform on the interval $[0, b]$; the fixed cost of work while on claim $\kappa = 0.12$; individuals still unemployed after the benefits exhaustion date get an income equal to 0.01 to ensure that they can choose to optimally produce a positive effort after the benefits exhaustion date. The top panel displays the case where $\gamma = 0.1$; $\lambda_0 = 0.05$; $\lambda_1 = 1.4$. In this example, unemployed workers work 5 months while on claim in the 24 first months of unemployment when there is part-time unemployment benefits. The middle panel displays the unemployment survival rate of a population composed of 95% of unemployed workers described in the top panel and 5% whose search effectiveness is too low to induce a positive search effort, implying that their exit rate from unemployment is equal to 1%. The bottom panel displays the difference between the survival rates of the middle panel.

Figure 1.14: Potential benefit duration at registration date (left panel) and treatment date (right panel)



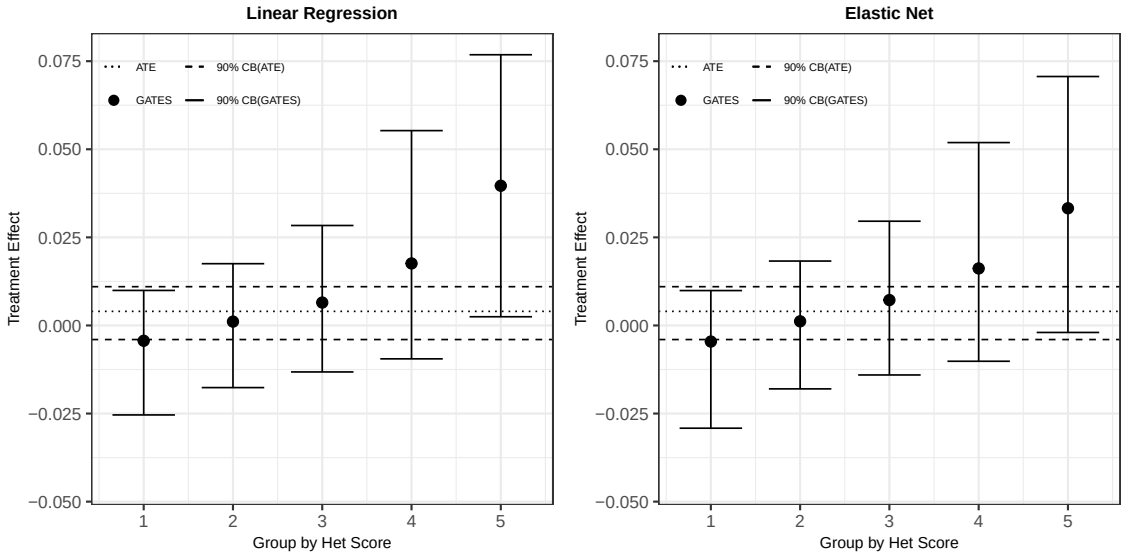
Note: This figure displays the histogram of potential benefit duration at registration date (left panel) and treatment date (right panel) for the treated group, the control group and the super control group.

Figure 1.15: Comparison of the cumulative number of hours worked while on claim between control and super control group



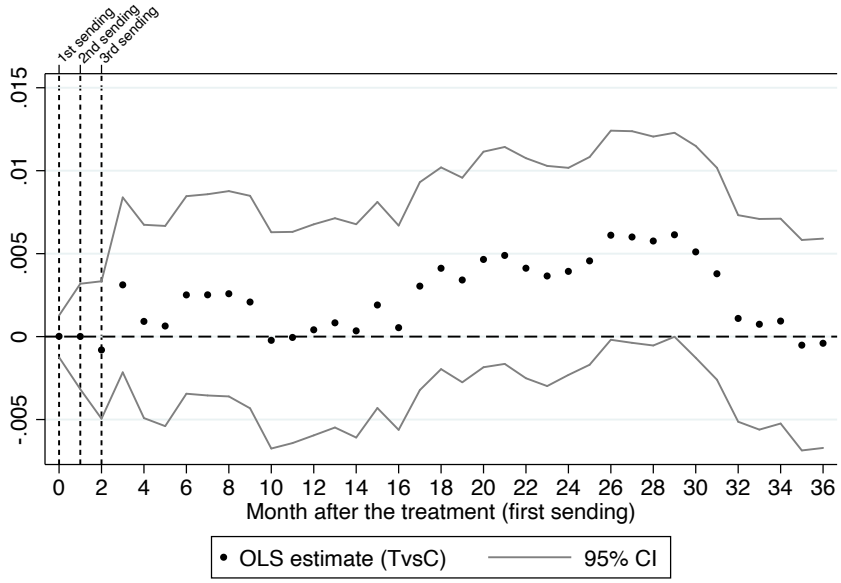
Note: Each black dot denotes the point estimate for being assigned to the control group compared to super control group at a given time horizon based on OLS regressions (i.e. coefficient δ in equation (1.7)). The grey lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. All estimations include covariates that correspond to stratum variables reported in Table 1.11 as well as entry months and regional fixed effects.

Figure 1.16: GATES of prob. to work while on claim at least once



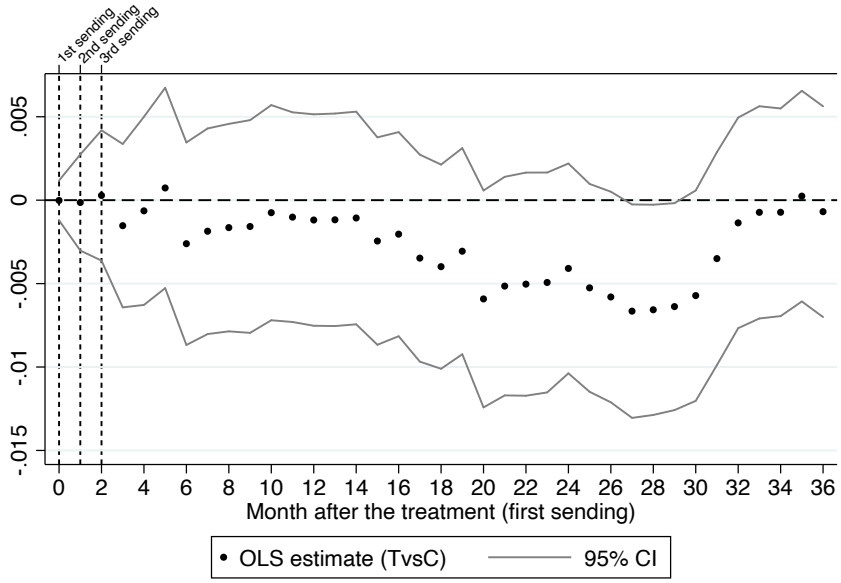
Note: The outcome - probability to work while on claim at least once - is measured 12 months after the treatment date. The results are presented for the two best ML methods regarding this outcome : Linear Regression and Elastic Net. Heterogeneity groups are formed using the ML proxy distribution $\hat{S}(X_i)$ which we cut at 50^{th}, 75^{th}, 90^{th}, 95^{th} percentiles. For example, Group 1 corresponds to the bottom 50% of $\hat{S}(X_i)$ and Group 5 to the top 5%. The parameter estimates and confidence intervals are computed as medians over 100 splits, with nominal levels adjusted to account for the splitting uncertainty.

Figure 1.17: Intention to treat effects on survival in unemployment (compensated, non-compensated or part-time)



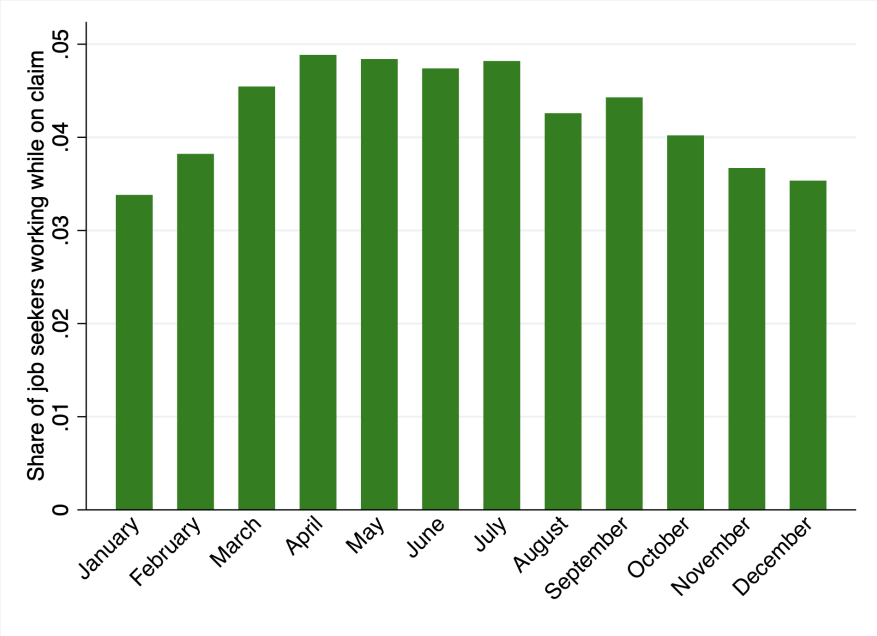
Note: Each black dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (1.7)) on the indicator variable equal to one if the individual is not matched with the return-to-work indicator in the month. The grey lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Figure 1.18: Intention to treat effects on the probability to have a regular job for at least 3 months



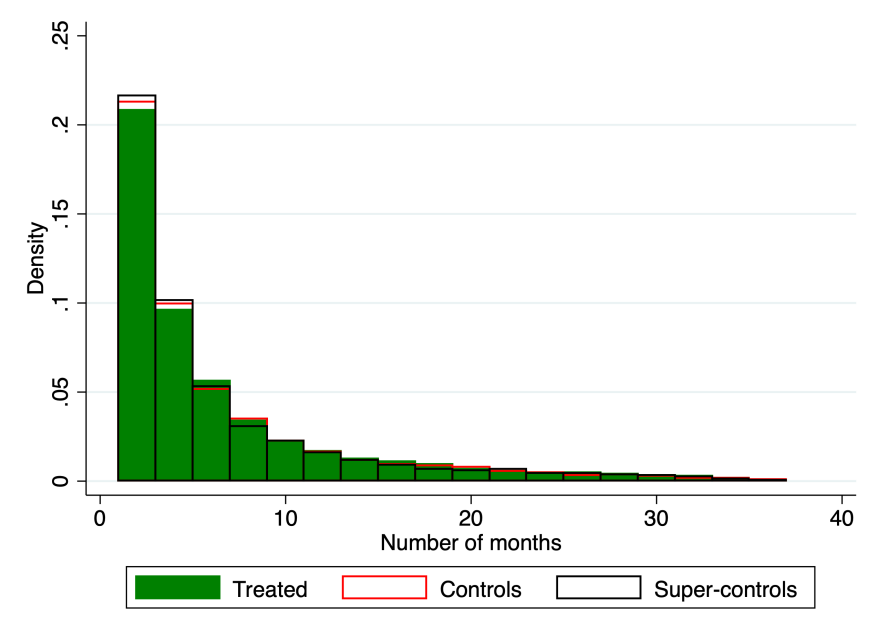
Note: Each black dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (1.7)) on the indicator variable equal to one either if the individual is not compensated in the current month and is matched with the return-to-work indicators with permanent jobs or temporary jobs lasting at least 3 months. The grey lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Figure 1.19: Frequency of work while claim by calendar month



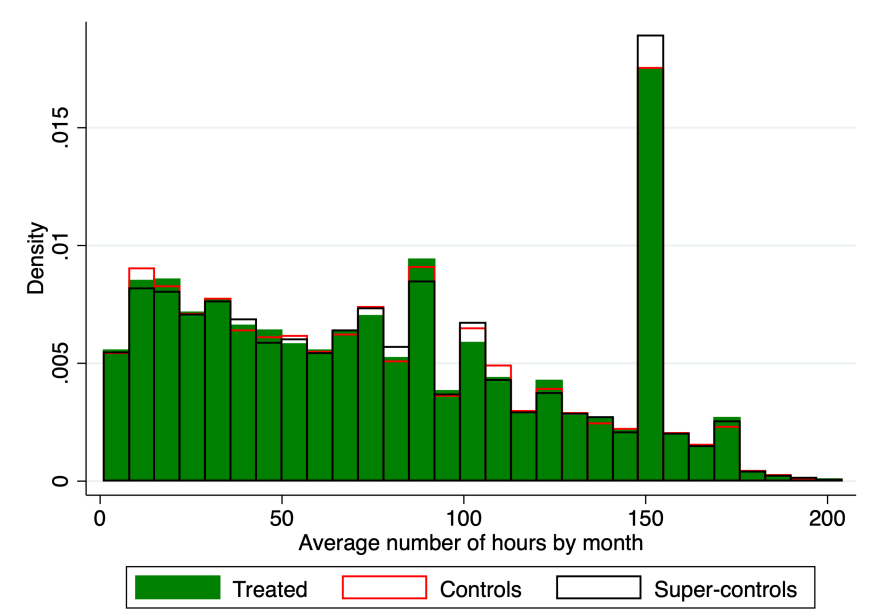
Note: This figure displays the calendar month average value of the indicator variable equal to one when the job seeker works while on claim for individuals belonging to the control group or the super control group.

Figure 1.20: Distribution of the number of months in part-time unemployment among those who worked while on claim



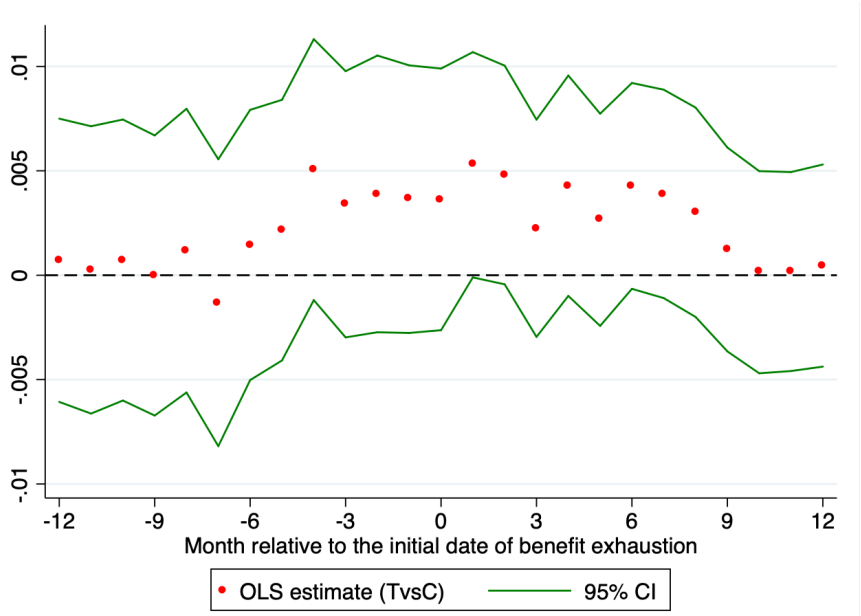
Note: This figure displays the distribution of the number of months with work while on claim by group over the 36 months of the study, conditional on working while on claim. The small number of observations per bin implies that the differences observed between groups are usually not significant. Only 2 bins display a significant difference between the supercontrol and the control groups. As for differences between the treated group and the supercontrol group or the control group, the few significant differences indicate that the treated are less present in the bottom of the distribution.

Figure 1.21: Distribution of the monthly number of hours worked in part-time unemployment among those who worked while on claim



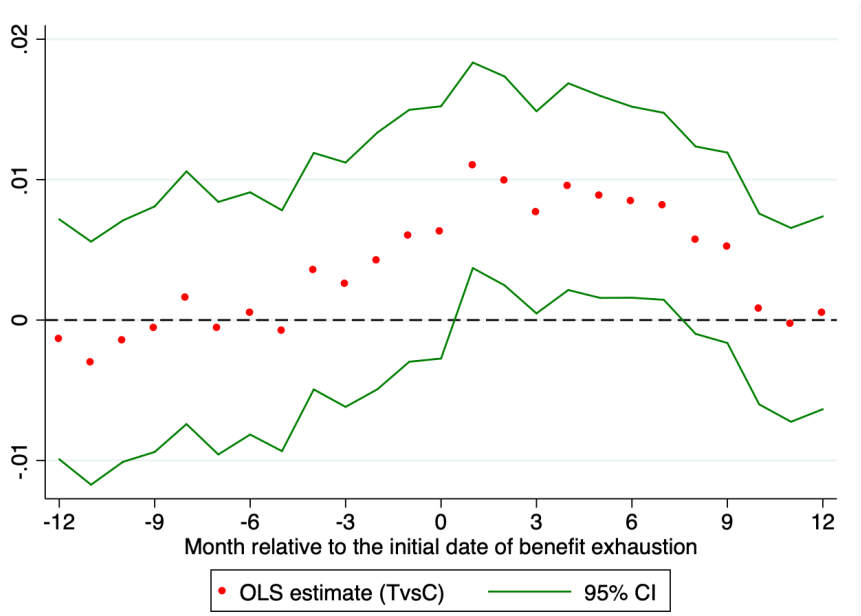
Note: This figure displays the distribution of the average number of hours worked while on claim by group over the 36 months of the study, conditional on working while on claim. The small number of observations per bin implies that the differences observed between groups are usually not significant. Only 2 bins display a significant difference between the supercontrol and the control groups. As for differences between the treated group and the supercontrol group or the control group, the few significant differences indicate that the treated are less present in the bottom of the distribution.

Figure 1.22: Intention to treat effects on survival in compensated unemployment



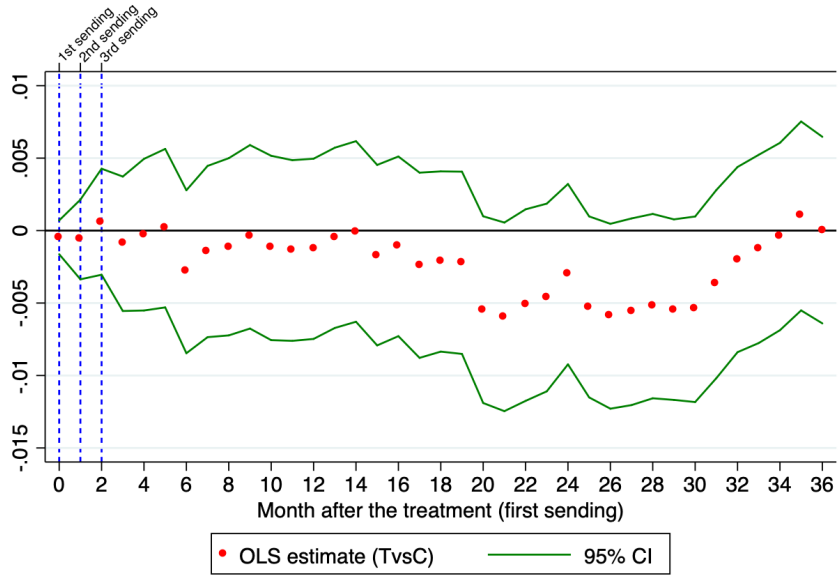
Note: Each red dot denotes the point estimate for intention to treat effect at a given unemployment spell based on OLS regressions (i.e. coefficient β in equation (7) on the probability to be in compensated unemployment in the month. The green lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Figure 1.23: Intention to treat effects on survival in compensated unemployment among job seekers with potential benefit duration superior or equal to 2 years



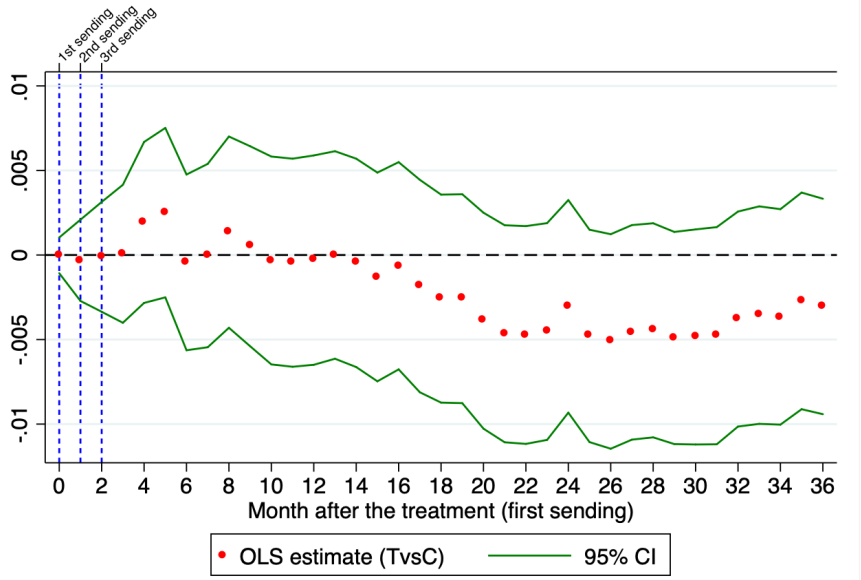
Note: Each red dot denotes the point estimate for intention to treat effect at a given unemployment spell based on OLS regressions (i.e. coefficient β in equation (7) on the probability to be in compensated unemployment in the month. The green lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Figure 1.24: Intention to treat effects on the probability to have a regular job for at least 6 months



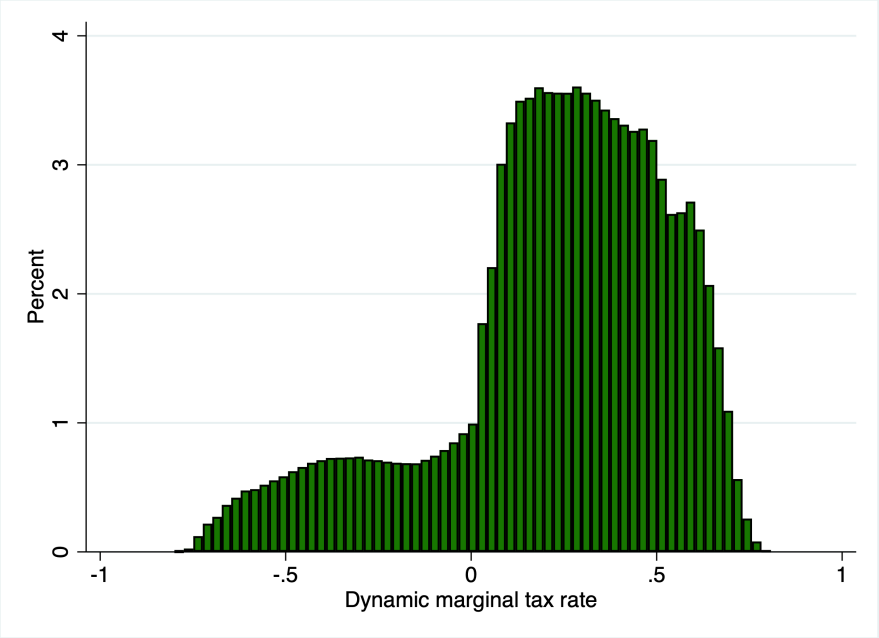
Note: Each red dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (7) on the indicator variable equal to one either if the individual is not compensated in the current month and is matched with the return-to-work indicators with permanent jobs or temporary jobs lasting at least 6 months. The green lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Figure 1.25: Intention to treat effects on the probability to have a regular job for at least 12 months



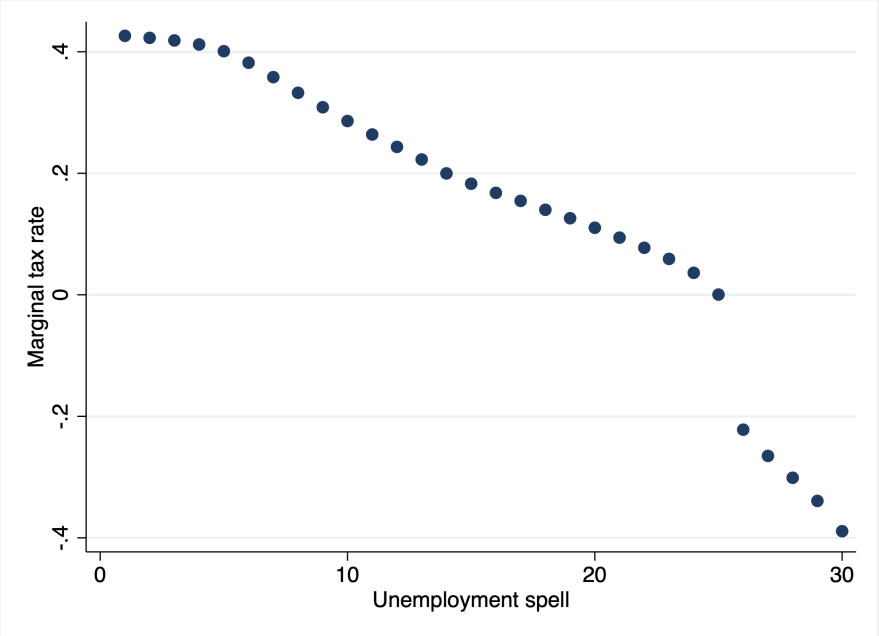
Note: Each red dot denotes the point estimate for intention to treat effect at a given time horizon based on OLS regressions (i.e. coefficient β in equation (7) on the indicator variable equal to one either if the individual is not compensated in the current month and is matched with the return-to-work indicators with permanent jobs or temporary jobs lasting at least 12 months. The green lines denote 95% confidence interval for the corresponding point estimate where standard errors are clustered at the agency level. Estimations do not include covariates but include entropy balancing weights that ensure identical outcome between the treated group and the control group at date zero (Hainmueller [2012]).

Figure 1.26: The distribution of marginal tax rates



Note: This figure displays the distribution of the marginal tax rate for each individual $\times$ month observation. The marginal tax rate is equal to $\tau - (\tau + \rho)\,\beta^{T-t} \prod_{j=t}^{T-1} [1 - \lambda(e_j)]$. It is different from the definition provided in equation (1.5), where the parameter ρ does not appear, because we take into account the rule according to which when the insurance capital is exhausted, individuals can be eligible for a new entitlement period. To do so, they must have worked at least 150 hours while on claim over the last 28 months. The new initial capital is computed on the basis of the daily wage of periods of work while on claim and according to the rule "one day of work yields one day of compensation". For each individual and each month, the benefits exhaustion date, which depends on the cumulative number of hours of work while on claim, is computed according to the legal rules. The individual survival probability until the benefits exhaustion date, equal to $\prod_{j=t}^{T-1} [1 - \lambda(e_j)]$ in equation (1.5), is estimated from a Cox proportional hazards model with covariates including gender, age, education, the reference wage, and the local unemployment rate. The monthly discount factor β is equal to =0.996, which corresponds to an annual discount rate equal to 5%.

Figure 1.27: Evolution of the average marginal tax rate over the employment spell



Note: This figure displays the average marginal tax rate month by month from the start of the unemployment spells. The marginal tax rate is equal to $\tau - (\tau + \rho)\,\beta^{T-t} \prod_{j=t}^{T-1} [1 - \lambda(e_j)]$. It is different from the definition provided in equation (1.5), where the parameter ρ does not appear, because we take into account the rule according to which when the insurance capital is exhausted, individuals can be eligible for a new entitlement period. To do so, they must have worked at least 150 hours while on claim over the last 28 months. The new initial capital is computed on the basis of the daily wage of periods of work while on claim and according to the rule "one day of work yields one day of compensation". For each individual and each month, the benefits exhaustion date, which depends on the cumulative number of hours of work while on claim, is computed according to the legal rules. The individual survival probability until the benefits exhaustion date, equal to $\prod_{j=t}^{T-1} [1 - \lambda(e_j)]$ in equation (1.5), is estimated from a Cox proportional hazards model with covariates including gender, age, education, the reference wage, and the local unemployment rate. The monthly discount factor β is equal to =0.996, which corresponds to an annual discount rate equal to 5%.

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Chapter 2

The Role of Caseworkers in the Labor Market Integration of Unemployed Youths: Evidence from France

This chapter is based on a joint work with Jérémy Hervelin (CY Cergy Paris University, THEMA)

Abstract: This study examines the impact of caseworkers on the employment and training outcomes of young people facing challenging transitions from school to work in France. By utilizing a unique administrative database and exploiting a quasi-random assignment mechanism, we are able to assess the value-added of each caseworker. Our findings reveal that caseworkers who perform above average significantly increase the number of employment days by 6% and training enrollment by 7.5% within two years. We further explore the reasons why caseworkers perform differently. We find that they tend to specialize in employment or training placement. Higher value-added caseworkers secure either temporary contracts or non-certified training. Finally, we use machine learning algorithms to measure the conditional effects of treatment and estimate outcomes under different caseworker reassignment scenarios. We find that random allocation is far from optimal in terms of the total number of days worked or the tax collected by the state.

2.1 Introduction

Active labor market policies (ALMPs) have received significant attention over the past 40 years, yielding mixed results, especially for young people

[Caliendo and Schmidl, 2016, Card et al., 2018, Kluve et al., 2019]. The design and successful delivery of these programs seem to be more crucial than their type. Caseworkers, as primary intermediaries that provide services within these programs, play a crucial role [Rosholm, 2014]. Recent studies have demonstrated substantial variations in the effectiveness of caseworkers on employment outcomes [Schiprowski, 2020, Cederlöf et al., 2021, Dromundo and Haramboure, 2021, Rasmussen, 2021].

The average percentage of unemployed young people who reported receiving helpful support in job finding from Public Employment Services (PES) was low in the European Union in 2022, around 20%[1]. However, this situation may differ in France. Between 66% and 80% of young people not in employment, education, or training (NEET) seek help from PES to receive assistance from caseworkers [Borel, 2022, Arnoult and Ghrairie, 2022]. The primary organization dedicated to assisting young individuals is known as the "mission locale." It comprises approximately 437 main centers and 7,000 related agencies, with a total workforce of 10,000 agents spread across France. These centers target NEETs aged 16 to 25, providing labor market assistance through individual meetings and group activities led by caseworkers. Additionally, they offer support for broader social issues, including health, transportation, housing, etc. Each year, over 1,000,000 young individuals have at least one meeting with a caseworker, and 80% of youths declare that they receive helpful support during their participation [UNML, 2021].

Evidence shows that young NEETs typically seek assistance when their labor market prospects are declining or very low[2]. The lack of knowledge about companies and career prospects, particularly for young people with limited qualifications and without a personal network, is a significant barrier to employment [Kramarz and Skans, 2014, Lo Bello and Morchio, 2022]. However, after registering with an agency, their labor market situation improves, as indicated by an increase in the total number of employment or training days.

This study addresses two key questions: Do employment and training outcomes depend on the assigned caseworker? If so, do caseworkers employ different practices?

We rely on fine-grained administrative data at the caseworkers and youths level to analyze the effect of caseworker-provided assistance on the labor market integration of young people. Specifically, we take advantage of the quasi-random assignment of caseworkers to young people in large "mission locale" agencies. Regular caseworkers rotate among themselves, following a pre-planned schedule weeks or

[1] See *Statistics on young people neither in employment nor in education or training* from Eurostat.
[2] see Figure 2.15 in Appendix A.

months in advance to welcome young people arriving at the agency for the first time. By default, the caseworker whom young people meet during the initial meeting becomes the caseworker responsible for conducting and supervising the entire job search process. We refer to these caseworkers as the *'main caseworkers.'*

Both quantitative and qualitative evidence suggest an assignment that appears random between caseworkers and young people in large agencies. We find no discernible pattern in the timing of caseworkers' initial meetings within an agency during a specific month or year. We then demonstrate that caseworkers' characteristics are not correlated with those of young people, especially when considering agency and the period of the first meeting. We have additional information for Paris agencies obtained through discussions with the board of directors of those agencies. Informal discussions revealed the process of allocating the first meetings among caseworkers. They negotiate among themselves to determine the day of their first meetings based on their schedules, pre-planned activities, and the total number of young people they assist (their caseload). Although some caseworkers can theoretically choose certain days rather than others, depending on the characteristics of the young people they expect to meet, they do not choose from among the young people who actually come on those days.

Overall, caseworkers' practices and activities vary considerably, even within the same agency. Simultaneously, we also observe differences in the average placement of young people in employment or training. Thus, we take advantage of the quasi-random assignment of caseworkers to investigate their causal impact on the subsequent labor market trajectories of the young people they serve.

We measure caseworkers' ability to facilitate young people's entry into jobs or training by evaluating their value-added in terms of the total number of employment days or training days over a two-year period following the initial meeting in a mission locale. Under the assumption that young people have similar potential labor market outcomes, contingent on the agency and period of the first meeting, we can estimate unbiased value-added scores on either employment or training for each caseworker. As placebo tests, we also estimate the effect of caseworkers on young people's outcome *before* their first meeting and, reassuringly, find no significant impact on the number of employment or training days.

Once young people have registered with an agency and have been assigned to a main caseworker, we discover that being assigned to a caseworker with value-added one standard deviation (SD) above the mean increases the total number of employment days by an average of 0.05 SD over the next two years. This effect represents either an absolute increase of 12 days or a relative increase of 6%. This relative effect aligns with findings in the literature on caseworkers assisting adult

job seekers. However, the absolute effect is lower, which we attribute to the specific population they serve, particularly young people classified as NEET, who, among several obstacles, struggle with a severe lack of confidence and exhibit behavioral biases that hinder their labor market integration [Jellab, 1998, Babcock et al., 2012].

To complete the analysis, we examine the effects of caseworkers on training outcomes, which are often expected to act as a stepping stone for this population [Escudero and Mourelo, 2017]. We find that being assigned to a caseworker one SD above the mean in the distribution of training value-added results in a 0.03 SD increase in the total number of training days. This corresponds to an absolute increase of 3 days or a relative effect of 8.5%. Interestingly, we find that the most effective caseworkers regarding employment are not necessarily the best at training placement, and vice versa. The evidence suggests that high-value-added caseworkers tend to specialize in one of the two dimensions.

When considering the timing of these effects, we observe that young people benefit from caseworkers with higher employment value-added throughout the two-year assistance period. Conversely, they gain more from caseworkers with higher training value-added during the second year. Looking at the extensive margins, that is, on the probability of being at least one day in employment or training, the effect of a caseworker with value-added one SD above the average are +3% and 7.5% respectively. Additionally, these positive effects persist when assessing other employment and training outcomes, such as the probability of being employed for at least one day per month or the total number of training hours.

We also examine the heterogeneity of caseworkers' effects, as they can vary according to the characteristics of the young people they serve. Males primarily benefit from caseworkers with higher employment value-added, while females gain more from caseworkers with higher training value-added. Furthermore, we find that the effect of caseworkers with higher value-added on employment diminishes with age, whereas the effect on training decreases with the educational level of young individuals. The effects of caseworkers with higher value-added differ across labor markets, with those excelling in employment primarily assisting youths in areas with high unemployment rates and low job-tightness. Conversely, caseworkers who excel in training mainly serve youths in areas with intermediate levels of unemployment.

Turning to the underlying mechanisms, we first examine how caseworkers differ in their practices with the young people they serve, including the intensity of individual meetings and contacts, job or training propositions, and placement into programmes. Overall, we find that the practices of high-value-added caseworkers differ from those of lower-value-added caseworkers, although the magnitude of

these differences is small.

We find that caseworkers with higher employment value-added do not necessarily increase the probability of young individuals finding permanent contracts. They make more employment propositions than low-value-added caseworkers, but the difference is too small to explain the effect on employment. Notably, they mainly assist young people under an intensive job placement program, in partnership with other caseworkers from the national public employment agency ("*Pôle emploi*"). Additionally, evidence suggests some competition among high-value-added caseworkers to place young individuals into employment. Partial outsourcing assistance appears to be an effectivepartially strategy for caseworkers. This option would allow young NEETs to benefit from advice shared not only by their own caseworkers but also by other caseworkers in different public structures.

Regarding caseworkers who excel in training placement, we find that higher value-added caseworkers only marginally, at least in absolute terms, increase the probability of enrolling in training programs that lead to national certification. They propose more training opportunities, but the magnitude of this increase is minimal: two additional training propositions per youth over a two-year period, totaling around 300 propositions, would lead to only a 2-percentage point shift in caseworkers' placements, enabling them to move from the lowest tercile to the highest tercile in the training value-added distribution.

More importantly, higher-value-added caseworkers conduct more collective workshops than their lower-value-added colleagues. These workshops appear to be attended by young people with lower employment probabilities. As a result, caseworkers leading these workshops likely better understand the wishes and needs of these youths, guiding them toward training programs to enhance their skill sets. However, we do not find that entry into training, due to high-value-added caseworkers, significantly improves the subsequent employment situations of youths within the two-year period. This result holds true whether entry into training occurs after 6 months or 1 year from the first meeting.

Finally, we explore the impact of different caseworker reassignment scenarios on young people's outcomes as well as the tax collected by the state. We rely on a machine learning algorithm, specifically random causal forests [Wager and Athey, 2018], to estimate treatment effects conditional on the set of youths' observed characteristics. The results show large heterogeneous effects of high-value added caseworkers from -100 days to +120 days depending on youths' characteristics. Based on conditional treatment effects, we simulated two scenarios. In the first scenario, we assigned high-value-added caseworkers to youths with the highest conditional treatment effect to maximize the number of employment days. This reallocation

could increase the average total number of employment days from 16 to 46, effectively tripling the mean effect of high-value-added caseworkers. In the second scenario, we assigned high-value-added caseworkers to youths who explicitly sought help with employment, had a low employment probability, and exhibited the highest conditional treatment effect. However, this reallocation only increased the mean effect of high-value caseworkers by 18% compared with the baseline scenario (from 16 to 19 days). We delve more precisely into the policy implications of these simulations in the conclusion.

Our paper makes a significant contribution to the literature on caseworker effects. While the value-added methodology is primarily employed in the field of education economics to measure the impact of teachers on student outcomes [Rivkin et al., 2005, Chetty et al., 2014b, Gilraine and Pope, 2021], and in the economics of law to investigate the influence of specific judges on individual sentences or wage penalties for firms [Dobbie et al., 2018, Cahuc et al., 2020], it has more recently been used to assess the impact of caseworkers from job centers on the unemployment duration of job seekers. [Schiprowski, 2020] discovers that job seekers who unexpectedly missed a meeting with caseworkers, belonging to the upper half of the value-added distribution, experienced an additional 12 days of unemployment in Switzerland. This effect corresponds to an increase of approximately 5% in the unemployment spell duration. Rasmussen [2021] finds that job seekers assigned to caseworkers who were one standard deviation above the mean had a shorter unemployment duration of approximately one week in Denmark, equivalent to a treatment effect of about 5%. Similarly, Cederlöf et al. [2021] report that a one standard deviation increase in the value-added of caseworkers leads to a 5-8% improvement in the job-finding rate of job seekers in Sweden. Very recently, Dromundo and Haramboure [2021] found that having caseworkers from job centers ("Pôle emploi") in the Paris region who are one standard deviation above the mean increases the probability of finding a job by around 8.5%. In our study, we find that caseworkers dedicated to a population facing the greatest difficulty in entering the labor market help youths experience more employment days (approximately +6%) or training days (approximately +8.5%) when they are one standard deviation above the average caseworker. We also observe an asymmetrical effect of high and low value-added caseworkers when grouped into terciles. High employment (training) value-added caseworkers increased the employment (training) days of their youths by 19 (8) days on average during a two-year period, compared to middle value-added caseworkers. By contrast, low value-added caseworkers caused a loss of 21 (7) days of employment (training) for young people.

We also help to shed light on the reasons why some caseworkers seem to be more

effective in helping young people integrate into the job market. [3] Existing literature suggests that caseworkers may possess different personality traits, with those sharing characteristics with their job seekers potentially being more effective, especially in programs relying more on threats than support [Behncke et al., 2010b, van den Berg et al., 2020]. However, evidence regarding the effect of caseworkers' personality traits is scarce, and recent findings indicate that the practices of caseworkers are of higher importance Cederlöf et al. [2021]. Less accommodating caseworkers are often more successful in placing job seekers in employment [Behncke et al., 2010a, Huber et al., 2017]. This may be attributed to factors such as the number of job applications required [Arni and Schiprowski, 2019], the transmission of job vacancies [Glover, 2021, Cheung et al., 2023], placement in training classrooms [Schmieder and Trenkle, 2020, Humlum et al., 2023], or information shared during individual meetings [Schiprowski, 2020, Cheung et al., 2023, Maibom et al., 2017]. These findings are specific to caseworkers assisting adult job seekers in job centers. In our case, we find that high value-added caseworkers tend to specialize in either employment or training, primarily placing young people in temporary jobs or non-certified training, respectively. They also make more employment or training propositions than lower value-added caseworkers. Their practices also differ from other aspects of the profession, such as the number of administrative tasks performed or the number of collective workshops conducted. However, the magnitude of these differences is small. When focusing on Paris agencies, we find that higher employment value-added caseworkers have significantly more contacts with their youths outside the walls of the agencies. In addition to placement, it may be crucial for caseworkers to stay in touch with this vulnerable population to maintain a certain level of motivation and job search efforts.

Finally, we contribute to the literature on school-to-work transitions [Ryan, 2001, Bertschy et al., 2009, Kramarz and Skans, 2014, Doruk and Pastore, 2020, Lo Bello and Morchio, 2022, Comi et al., 2022] by focusing on a crucial resource in the implementation of labor market policies, namely caseworkers. Specifically, we quantify the magnitude of caseworkers' effect on the transitions of young people into employment or training, highlighting their role in the effectiveness of job search assistance programs [Neumark and Rothstein, 2005, Kluve et al., 2019]. We also find evidence suggesting competitive dynamics among caseworkers. In larger public agencies, competition among caseworkers to place youths in employment or training is more intense, especially in labor markets with high unemployment rates or

[3] Caseworkers may have a (positive) influence on youth that is not directly measurable through labor market outcomes. In this study, we focus on caseworkers vary in their ability to enhance young people's prospects within specific dimensions related to the labor market.

low job availability. Because the level of caseworker assistance may fluctuate based on job seekers's motivation [Guul et al., 2020] or because caseworkers tend to maintain their strategies without considering evidence of program effectiveness [Bolhaar et al., 2020], we conducted policy simulations to assess the impact of alternative assignment methods. Our findings suggest that assigning caseworkers to young individuals with the highest conditional treatment effect, as measured by a random forest model based on their observable characteristics, could result in more employment days for young people and increased payroll tax revenue for public authorities.

The remainder of this paper is organized as follows. Section 3.2 presents the institutional background related to the "mission locale" agencies. Section 2.3 describes our data sources, the construction of our analysis sample, and the activity of young people and caseworkers in these agencies. Section 2.4 explains the methodology used to construct the estimators for caseworkers value added. Section 3.4 shows the impact of caseworkers assistance on youth employment and training situations, and its heterogeneity. Section 2.6 breaks down the underlying mechanisms down and describes what differentiates more able to less able caseworkers, and evaluates the effect of reallocating caseworkers to youths on employment days. Section 3.5 concludes.

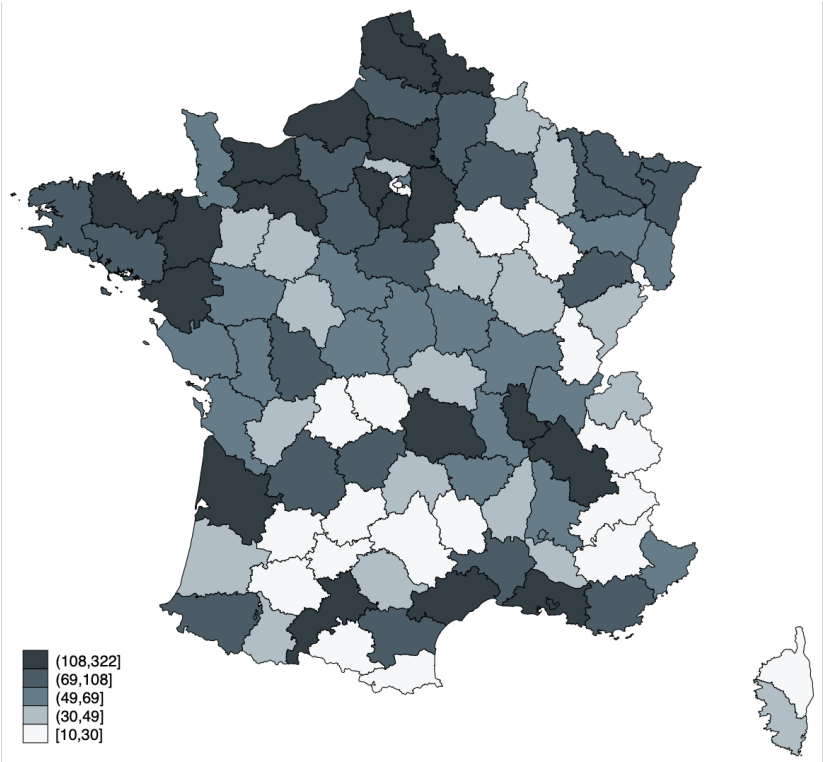
2.2 Institutional background

European countries implemented the Youth Guarantee (YG) scheme in 2013 to assist young individuals who are NEET (Not in Education, Employment, or Training) more effectively. Public agencies, acting as intermediaries, have received increased financial and personnel resources. Consequently, many young people have turned to these agencies to seek job search assistance in the hope of entering the labor market.

2.2.1 The "Missions Locales" network

"*Missions Locales pour l'insertion professionnelle et sociale des jeunes*", commonly known as "missions locales", assist a significant 10-15% of the 16-25 age population in France each year. They primarily focus on young people who are no longer in school and often face significant barriers to entering the labor market. In addition to employment assistance, these centers provide comprehensive social support in areas such as health, housing, and administrative tasks. Established in 1982, the "missions locales" were later integrated into the French public employment service in

Figure 2.1: Location of "*Missions Locales*" centers and agencies in Metropolitan France



Note: There are at least ten agencies that youths can join to enroll within each French *departement*.
Source: IMILO (extraction date: October 2021), authors' calculations.

2005.

In 2022, the network comprises a total of 437 centers, spread across 6,800 affiliated agencies and other registration places accessible to young people seeking assistance[4]. As depicted in Figure 2.1, these agencies are distributed throughout mainland France, with at least ten agencies per French department, although their geographical distribution varies. The network employs approximately 10,000 people, over half of whom are caseworkers.

2.2.2 First meeting and follow-up in agencies

Each year, around 400,000 young people go to a "mission locale" agency for the first time to seek assistance, as shown in Figure 2.16 in Appendix A. Most of these young people decide to go on their own or based on recommendations from family or friends. About 20% are directed to agencies either by the national public employment agency ("*Pôle emploi*") or by workers from the "mission locale" agency. Additionally, agreements have been established with other public institutions, including

[4]Source: Union nationale des Missions Locales (UNML), (link).

the Ministry of Education and the Ministry of Defense, to inform and guide young people to the nearest agency, particularly those who have left school [Hervelin, 2022]. These agencies also have extensive connections with other local partners.

To register with an agency, young people must attend an initial meeting with a caseworker. This initial interview typically lasts 30 to 45 minutes, during which the caseworker collects information about the young person's socio-demographic situation and their needs related to employment or broader social issues. In most cases, at least in large agencies, there are no specific rules governing the allocation of caseworkers to young people during their first meetings. Caseworkers are usually assigned based on availability, with the next available caseworker being allocated. This means that caseworkers are not chosen based on their characteristics or preferences, and the assignment can be considered quasi-random. In some cases, young people may contact the agency in advance to make an appointment, but the allocation of caseworkers in this scenario also follows the same principle[5].

As noted by Adjeoda [2021] for the agencies in Paris, caseworkers do not choose or know young people they receive at first meeting, and vice versa. The rotation of caseworkers is set several weeks in advance depending on their caseload and other planned activities in their agency such as individual meetings with other youths or collective workshops[6].

After the first meeting, approximately 83% of young people return for at least a second meeting with their caseworker. Caseworkers provide a range of assistance, including individual meetings, workshops to improve oral and written skills, collective information sessions, and various forms of communication such as emails, SMS, or phone calls. Caseworkers are expected to demonstrate good social skills including understanding, availability, dynamism, etc[7]. Usually, a minimum of two years of higher education is usually required to be recruited as caseworker, and they start with a gross wage around €2,000. As they play a central role in directing young

[5]Only 14.6% of the first meetings in 2017 and 2018 were performed after an appointment made in advance.

[6]As we have no direct information on the organization of the first meeting in other agencies, we assume that it is the same as in Paris, for which we were able to obtain information directly from the field. Adjeoda [2021] also notes: "*Instead of going directly to an agency to meet a caseworker, young people can also contact the Mission Locale in advance to make an appointment. In this case, too, the 'allocated' caseworker will be one of those assigned to the first meetings that day, regardless of the young person's characteristics*". Clearly, this feature – that there is no sorting between young people and their caseworkers within agencies – is crucial for our empirical strategy. In particular, this means that potential outcomes of young people are equal across caseworkers and thus that caseworkers value added estimates measure the causal effect of caseworkers on young people's outcomes. We provide evidence supporting the quasi-randomness of caseworkers' allocation at first meetings and detail our empirical strategy in section 2.4.

[7]"*Understanding*" is ranked first by young people when asked what they expect from their caseworkers. Source: UNML, national survey 2019.

people to suitable programs or providing them with job or training opportunities, we construct a sample that allows us to analyze their effect.

2.3 Data, sample construction, and descriptive evidence

We begin by presenting the data sources we utilized for our analysis, followed by a description of the sample restrictions we implemented to create our final sample. Lastly, we provide descriptive evidence on the activities of youth and caseworkers within the agencies.

2.3.1 Data Sources

To conduct our analysis, we combine two administrative sources provided by the statistical office of the French Ministry of Labor (MT-DARES).

IMILO. Our primary data source is derived from the national information system of "missions locales" (IMILO). It includes socio-demographic data on young people, including details about their educational background, geographic location, housing conditions, and social security status, among other factors. IMILO is used by caseworkers to monitor the progress of young people within the agency, and it contains comprehensive records of young people's activities within the agency[8]. This includes information on interactions with caseworkers, such as meetings, remote contacts (email, SMS, phone calls), participation in workshops, and involvement in specific programs. While our knowledge of caseworkers is limited to their activity within the agency, we have access to information like their date of birth, gender, and full names. To supplement socio-demographic data for both youth and caseworkers, we utilized the Namsor API[9], which classifies personal names by country of origin[10].

FORCE. Our secondary data source is an administrative database containing employment and vocational training information for individuals who have been registered or in contact with public employment services (PES), including youth centers. This database allows us to construct labor market outcomes for young people both before and after their enrollment in an agency. It provides details about periods of unemployment, vocational training, and includes additional information such as

[8]Public funding is linked to agencies activity, which is recorded in this information system. There is a direct incentive for caseworkers to fill in these information accurately.

[9]Namsor is an organization which uses machine learning techniques on historical Census data to classify names by ethnicity, gender, and religion (see https://namsor.app). It has been used in the literature recently by Bursztyn et al. [2021]

[10]We categorize (predicted) ethnic background into main geographical regions: North-Africa, Sub-Saharan Africa, Asia, Europe (except from France) and France.

the type of employment contract (permanent or temporary) and the type of training (certified or not). It's important to note that this database covers employment in the private sector but does not include experience in the public sector or self-employment.

2.3.2 Final sample

Employment and training data (FORCE) starts in 2017, we thus restricted our sample to cohorts of young people who registered with agencies after January 1, 2017. In order to observe labor market trajectories over a sufficiently long period after their first meeting, we exclude individuals who visited an agency after December 31, 2018.[11] This results in approximately 800,000 young people for whom we have information on both their follow-up at the agency and their subsequent labor market situations. However, we had to impose a number of restrictions to construct our final sample.

First, we retained only the largest agencies and affiliated centers located in mainland France. Specifically, we decided to keep the agencies in the top 25% in terms of the number of first meetings held between January 2017 and December 2018. As illustrated in Figure 2.17 in Appendix B, it corresponds to agencies that welcomed between 4,000 and 18,000 young people newly registered over the period. The reason to remove the remaining 75% of the agencies is to avoid including agencies that may have specific organization because of the limited number of young people they welcome every year. For the largest agencies, we assume they are organized as the mission locale of Paris, for which we have direct background information describing quasi-random assignment of its caseworkers. The appendix reproduces our main results for the Paris agencies only.

Second, we drop observations with missing values in socio-demographic and labor market characteristics. We also excluded young people who did not have a regular one-to-one first meeting with a caseworker when they initially registered with an agency.[12] As we focus on young people seeking for job related assistance, we only keep young people who are NEET at the time of their first meeting (about 90%).

Finally, we imposed additional restrictions to ensure econometric feasibility of our analysis. This included narrowing the sample to young people who registered

[11] We decided to exclude data from 2020 and beyond due to the impact of the Covid-19 pandemic, which significantly disrupted the economy and the organization of agencies, including first meetings.

[12] Approximately 92% of young people initially registered in an agency through an individual meeting, while 7% did so via a specific collective information session, and 1% through a collective workshop.

with a "mission locale" agency between January 2017 and December 2018.[13] We also excluded agency × month cells with fewer than four youths, two caseworkers, and two youths per caseworker to ensure that each cell had a minimum of four observations (2 youths × 2 caseworkers). Finally, we considered caseworkers who conducted first meetings for at least one month per quarter over the two-year period. This condition guaranteed that caseworkers were present throughout the period used to compute our value-added estimators.[14]

Table 2.12 in Appendix B provides a comprehensive list of the restrictions we imposed and the resulting number of observations at each step. This process yielded a final sample of 60,384 youths, 445 caseworkers, and 97 agencies.

2.3.3 Descriptive evidence

We now provide an overview of youths and caseworkers with respect to the entire population within the selected agencies and all agencies in France.

Youths' characteristics and activity

Table 2.13 in Appendix C presents a summary of youth characteristics in both the full samples and our analysis sample. Overall, there are hardly any differences in youth characteristics. There are slightly more males than females, 10% of youths did not attend high school, less than 50% possess a driver's license, about 15% have a foreign nationality or an African ethnicity, and the majority of young people are referred to an agency by their relatives.

Table 2.1 provides statistics on youth activity and labor market situations. Young people have limited work experience before registering with an agency and attending their first caseworker meeting. Table 2.1 shows that, on average, young people worked from 16 to 27 days in the six months prior to registration, but only received one day's training on average. However, these figures increased during the 24 months following registration. While the period after registration is longer than before, the number of days employed steadily increases to approximately 50 days on average six months after registration, rising to around 210 days on average after 24 months.

Over this 2-year period, youths who remain in the agencies, on average, attend

[13] For the econometric analysis in Section 3.4, we considered only youths who registered between July 2017 and December 2018 to allow for a six-month period before and twenty-four months after the initial meeting with a caseworker. We extended the analysis to January 2017 for greater precision in computing caseworker fixed effects.

[14] Tables 2.23 and 2.23 in Appendix F demonstrates the robustness of the main effects presented in Section 3.4 across sample restrictions for employment and training respectively.

Table 2.1: Summary statistics on the follow-up of young people and labor market outcomes

	Complete sample	Selected sample	
		Full	Analysis
	(1)	(2)	(3)
Labor market outcomes			
Before joining agency (6-month period)			
Nb of days of employment	26.96	24.67	16.70
	(53.36)	(51.47)	(39.34)
Nb of days of training	1.13	1.22	0.82
	(11.13)	(11.66)	(9.26)
Nb of days of open unemployment	30.12	30.15	29.30
	(54.93)	(54.97)	(54.27)
After joining agency (24-month period)			
Nb of days of employment from month 0 to month 24 after 1st meeting	217.96	208.83	208.92
	(239.67)	(237.53)	(225.56)
Nb of days of training from month 0 to month 24 after 1st meeting	26.93	28.16	30.20
	(76.65)	(77.87)	(79.32)
Nb of days of open unemployment from month 0 to month 24 after 1st meeting	210.31	211.39	211.77
	(236.08)	(236.32)	(231.02)
Follow up at the Mission Locale			
Nb of individual meetings	7.96	7.86	7.61
	(9.03)	(8.94)	(8.82)
Nb of collective workshops	3.91	3.63	3.72
	(9.79)	(9.20)	(9.40)
Nb of information sessions	0.47	0.50	0.37
	(1.69)	(1.73)	(1.67)
Nb of digital contacts	15.64	16.79	13.77
	(23.32)	(25.69)	(18.90)
Enrolled in a national program	0.71	0.72	0.75
	(0.45)	(0.45)	(0.44)
Enrolled in a specific program	0.81	0.81	0.82
	(0.40)	(0.39)	(0.38)
N	808,222	426,372	60,384

Note: Column (1) shows statistics for the complete sample. Columns (2) and (3) show statistics for the complete selected sample and the restricted selected sample for the econometric analysis as described in Section 2.3.2 respectively.
Source: IMILO (extraction date: October 2021) and FORCE, authors' calculations.

7 individual meetings with a caseworker.[15] They participate in 3 collective workshops, fewer than 1 information session, and have 13 digital contacts with their caseworker. Figure 2.18 illustrates the share of youths with at least one event. The proportion of young people having had at least one individual interview within one month falls rapidly, dropping to 20% five months after the first interview and stabilizing at 10% after one year.

[15]Figure 2.20 in Appendix C demonstrates that when young people attend 7 meetings with a caseworker, over 60% of them are with the main caseworker, i.e., the one who conducted the first meeting.

There is no specific rules regarding the intensity of individual meetings if young people do not register in specific programs [16]. Thus, agencies usually consider youths as "active" as long as they had at least one event with their referring caseworker during the last six months. Using this definition to determine the survival of youths in agencies, Figure 2.19 shows that 75% of youths remain active in their agency one year after their first meeting, decreasing to 30% after two years. Additional statistics show that youth who remain active after two years have accumulated less labor market experience than those who exited earlier on average.

Finally, Figure 2.21 presents the number and type of requests made by youths, depending on the number of months relative to their first meeting. Young people primarily request information related to employment, such as specific job details or training content. In line with this, caseworkers make a similar number of labor market-related propositions, except during the first month of assistance, when caseworkers provide 3 labor market-related propositions for every 1.5 requests.

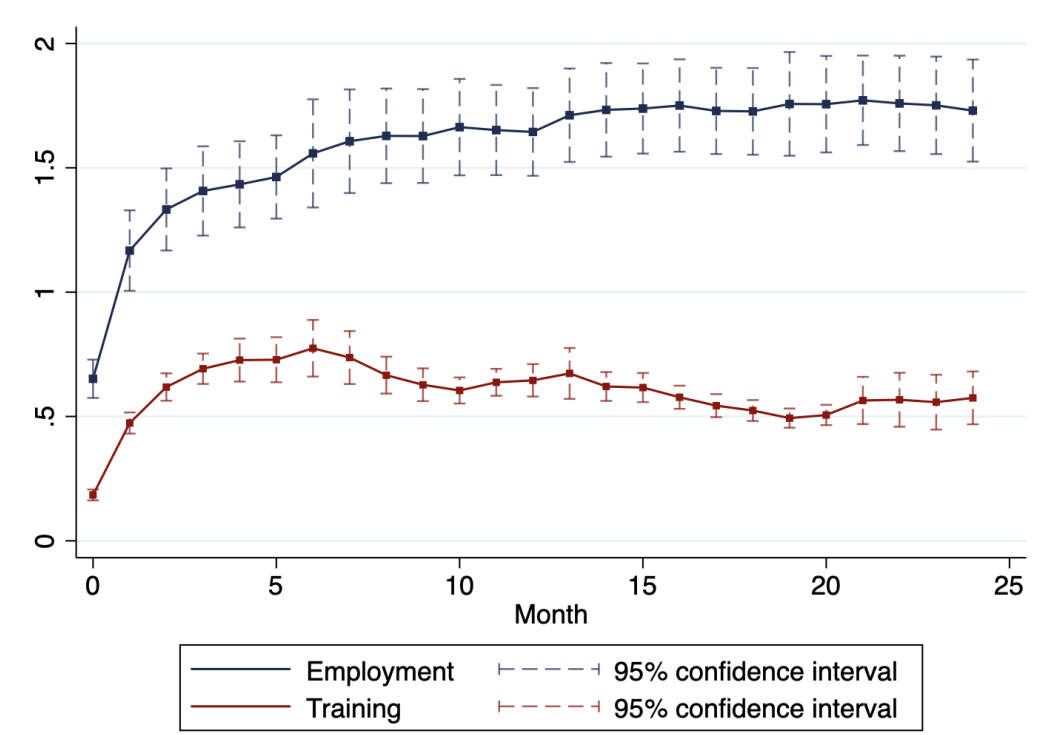
Caseworkers' characteristics and activity

Now, let's turn our attention to caseworkers. We find very few differences in the limited socio-demographic characteristics we have between our samples, as illustrated in Table 2.14. Between 75% and 80% of caseworkers are female, and they are, on average, around 41 years old when they first engage with youths. Only 10% or less of these caseworkers have an African-related origin, as determined by their names.

In terms of activity, caseworkers in our analysis sample, on average, handle 140 youths in their caseload, which is 50% higher than caseworkers in the full samples. This difference is due to the restrictions we apply when constructing our analysis samples. We only retain caseworkers who were present for at least one month during each quarter between January 2017 and December 2018. This condition ensures that we are analyzing the effect of caseworkers who provide assistance on a consistent basis. Since caseworkers in these public agencies can also help youths with other social-related issues, they may not oversee the entire paths of youths within the agencies but assist them with specific matters. Focusing on labor market-related caseworkers, Figure 2.24 displays how the caseload per caseworker, divided into terciles, evolves over time. Although there are few variations over time, the main difference lies in the size of the caseload across caseworkers. Those in the first tercile have an average of 100 youths, while those in the third tercile have an average of 200 youths.

[16] Figure 2.22 indicates that up to 1/3 of young people do not register for any specific program during the two-year period.

Figure 2.2: Evolution of the mean value of the within-agency caseworker placement variance



Note: This graph is obtained by first computing the mean of employment and training situation of youths for each caseworker per month. Then, we compute the variance of these means within each agency per month. Finally, we compute the mean value of the within-agency variations per month.

Figure 2.25, which illustrates the number of meetings per caseworker, and Figure 2.26, which shows the number of digital contacts per caseworker, provide additional insights. The activity of caseworkers in terms of individual meetings reveals that caseworkers in the third tercile conduct twice as many meetings as those in the first tercile, with an average of 80 meetings per month versus 40. The discrepancy is even more substantial regarding digital contacts between caseworkers and youths. Caseworkers in the third tercile have between 100 and 200 digital contacts (emails, SMS, or phone calls) per month with their youths over the course of a year, while those in the first and second tercile have less than 10 and 25 contacts per month, respectively.

This is in line with the observations from the field of Adjeoda [2021] at the mission locale of Paris where she attended 45 first meetings led by 12 caseworkers from October 2020 to February 2021. She observed that the differences in the content of meetings were largely explained by caseworkers' approach, more than by the profile of young people they receive. Notably, she found that some caseworkers adopt a didactic approach and try to encourage youth to take action, such as preparing an

appropriate CV and looking for job offers, while others focus mainly on administrative tasks and do not ask young people to prepare for the second meeting.

These differences among caseworkers may impact the labor market integration of young people. Figure 2.2 displays the evolution of the mean value of the within-agency caseworker placement variance. It is evident from this figure that the situations of young people evolve differently across caseworkers within the same agencies. We demonstrate in the next section that these differences are mainly attributable to caseworkers.

2.4 Empirical strategy

We first provide evidence supporting our identifying assumption that there is no systematic sorting between youths and caseworkers within agencies and time cells. We then present our methodology for estimating caseworkers' value added (VA).

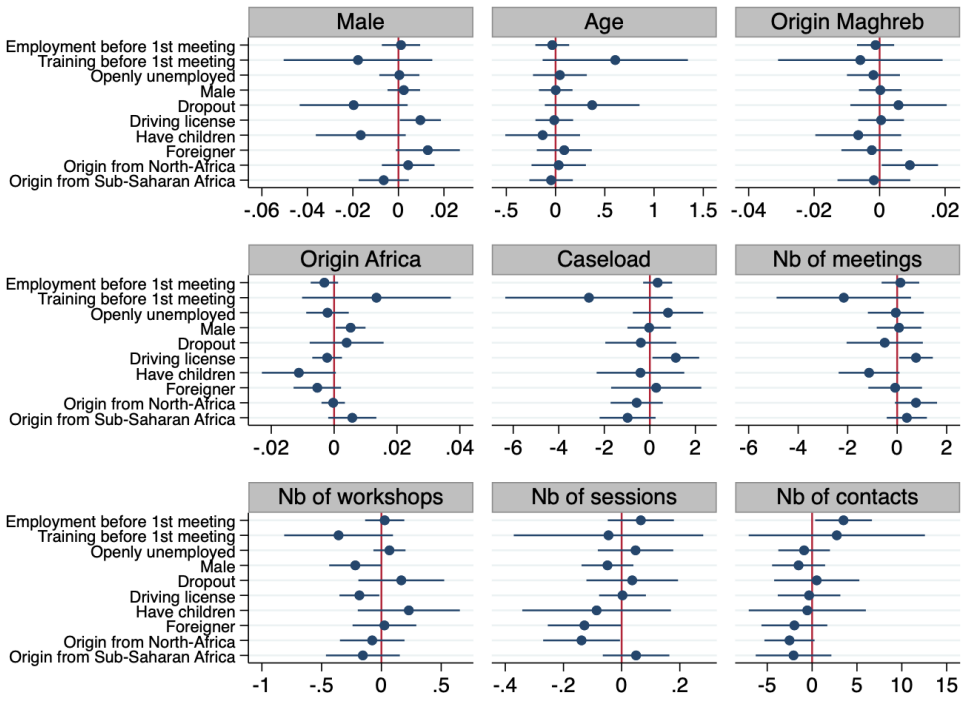
2.4.1 Quasi-exogeneity of caseworkers assignment

For the largest agencies in France, we have direct background information indicating that there are no specific rules that would make certain caseworkers more likely to be selectively assigned to certain youths. Young people seeking assistance from these agencies must appear in person at one of the agencies. On the day the youths arrive, they meet with one of the caseworkers who has been assigned to that day to welcome new youths. Caseworkers are assigned to calendar days rather than to youths directly, on a rotation basis planned several weeks in advance. The timing of caseworker assignments is primarily based on the caseworker's workload at the time they decide. It's important to note that the caseworker automatically becomes the youth's primary supervisor and contact for the remainder of the youth's follow-up.

Overall, there is one or two different caseworkers present each day to conduct the first meetings from Monday morning to Friday afternoon, excluding one morning or one afternoon in the week because of team meetings.[17] Table 2.16 and Table 2.17 in Appendix D show the correlations between the day of the week and youth and caseworker characteristics, respectively. We observe very little sorting between the day of the week and some youth characteristics. For instance, young people with more previous employment experience come more often on Thursdays, and those with more previous training experience come less often than youths with less

[17] Figure 2.27 in Appendix D indeed shows that there are fewer first meetings performed during Fridays in the sample of selected agencies.

Figure 2.3: Correlation between youths' and caseworkers' characteristics



Note: Each square shows the estimates from an OLS regression of a specific caseworker characteristic on all its newcoming youths' characteristics.

experience, respectively. Turning to caseworkers, we see, for instance, that older caseworkers conduct more first meetings on Thursdays, and caseworkers with Sub-Saharan African origin conduct more first meetings on Wednesdays but less on Tuesdays.

However, caseworker assignments are expected to be exogenous to youth characteristics within a given agency and time cell. Within agencies, on the one hand, because caseworkers are typically assigned to only one agency over the period (at least in terms of first meetings) and because youth characteristics are not evenly distributed across agencies. Within time cells, on the other hand, because caseworkers differ in their distribution of assignments over time, so does the distribution of youth characteristics.

Our identifying assumption is based on these factors, suggesting that caseworker assignments are quasi-random, contingent on the agency and time period. To assess the validity of this claim, we initially propose to scrutinize the correlation between the characteristics of youths and those of their assigned caseworkers within the selected agencies. In each panel, Figure 2.3 illustrates the estimated coefficients derived from an OLS regression involving a specific caseworker's characteristics and those of the youths. Youth characteristics encompass socio-demographics as

listed in Table 2.13, along with predetermined labor market characteristics. Regarding caseworkers' characteristics, we consider gender, age, and ethnic background inferred from their names, in addition to variables related to their activity and experience at the agency. On the whole, the evidence reveals a significant proportion of very small and insignificant coefficients, implying that there is no systematic sorting between caseworkers and youths.

2.4.2 Caseworkers value added

To construct caseworkers' VA estimates, we employ the methodology introduced by Chetty et al. [2014a], which we adapt to our specific context. As caseworkers' assignments are expected to be quasi-random within a particular agency and quarter[18], we condition our analysis on cohort fixed effects. These effects are defined as the group of individuals who had their initial meeting with a caseworker in the same quarter and within the same agency.

Methodology: Computing value-added estimates

As caseworkers' effectiveness is multidimensional, we compute VA estimates associated to different outcomes related to employment and training.

For each outcome (e.g. the total number of days of employment 24 months after the first meeting with a caseworker), we start by computing youth outcome residuals $\hat{Y}_i$ by regressing student outcome Y_i on cohort fixed effects $\gamma_{a,t}$, youth predetermined characteristics X_i as well as caseworker fixed effects μ_j. Including caseworker fixed effects in the model allows us to estimate residuals relying on within caseworker variation.

We estimate the following regression.

$$Y_i = \mu_j + \beta X_i + \gamma_{a,t} + \epsilon_i \tag{2.1}$$

Youth outcome residuals are then computed as

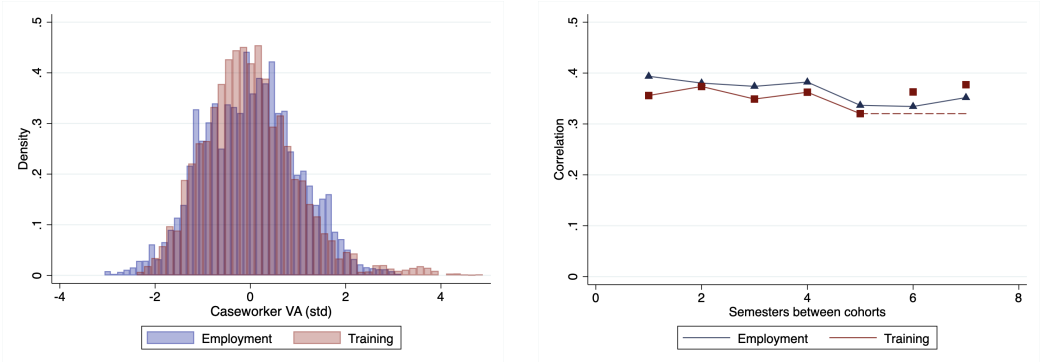
$$\hat{Y}_i = Y_i - \hat{\beta} X_i - \hat{\gamma_{a,t}} \tag{2.2}$$

Let $\bar{Y}_{j,t}$ denote the mean residual youth outcome for caseworker j and cohort t. We further define $\bar{Y}_j$ as the mean residual youth outcome across all of caseworker's j cohorts, and $\bar{Y}_{j,-t}$ as the same quantity but excluding cohort t.

[18] As a robustness check, we demonstrate in Tables 2.23 and 2.24 in Appendix F that our results remain consistent when considering different time spans, such as a month or a semester.

Figure 2.4: Distribution of VA estimates and auto-correlation vector

(a) Distribution of VA estimates (b) Auto-correlation vector



Note: The VA estimates are obtained with equation (2.3) using the total number of days of employment 24-month after the first meeting for employment, and using a similar outcome on training for training.

If our identifying assumption is verified, $\bar{Y}_{j,-t}$ is an unbiased estimate of the causal effect of caseworker j on cohort t youths' outcome. However, it may not be an optimal out of sample predictor of caseworker effectiveness. Thus, following Chetty et al. [2014a] we construct our caseworker value added estimate as the best linear predictor of $\bar{Y}_{j,t}$ based on the residuals of all other cohorts $\bar{Y}_{j,-t}$.

Specifically, we compute caseworker predicted effectiveness for cohort t as

$$\hat{\mu}_{j,t} = \Phi \bar{Y}_{j,-t} \tag{2.3}$$

where Φ is the vector of coefficient that minimizes the mean-squared error of the forecasts of the residual outcomes. It is estimated by regressing $\bar{Y}_{j,t}$ on $\bar{Y}_{j,-t}$

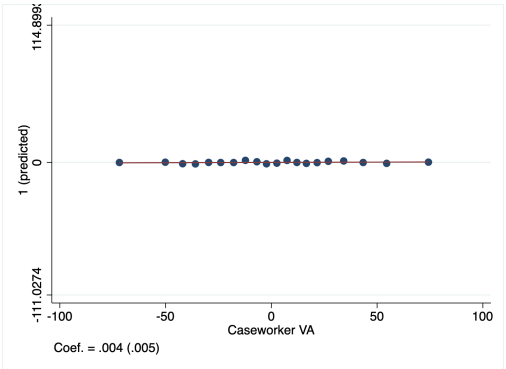
Validity of value-added estimates

To validate our caseworker VA estimates, we assess forecast bias and out-of-sample prediction accuracy, following the approach introduced by Chetty et al. [2014a].

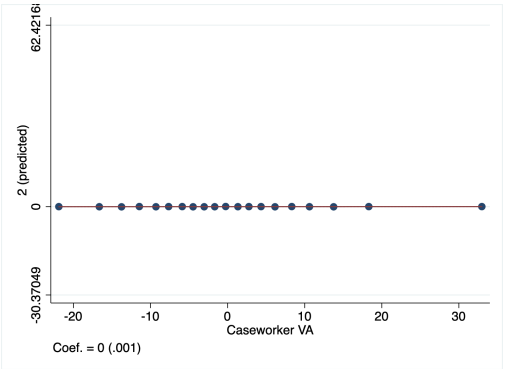
The concept of a forecast bias test is to gauge the extent to which variation in VA is influenced by sorting on unobserved characteristics. To accomplish this, we employ ex-ante outcomes' values, i.e., those observed before the first meeting with a caseworker, as a proxy for all the unobserved factors that might impact future outcomes and potential sorting. Given the time span during which we observe individuals in our sample, we can compute all our outcomes up to 6 months before their initial caseworker meeting. We begin by regressing the actual outcome on the ex-ante outcome, followed by residualizing this predicted outcome using the same set of controls and fixed effects as in equation (2.1). Subsequently, we assess forecast

Figure 2.5: Effects of caseworker VA on predicted outcomes

(a) Employment



(b) Training



Note: The figures show binscatters of caseworker value-added and youth predicted outcomes. The figure on the left shows youths' predicted outcome based on the cumulative number of days of employment 6 months before the first meeting. The figure on the right shows youths' predicted outcome based on the cumulative number of days of training 6 months before the first meeting.

bias by regressing this residualized predicted outcome against the corresponding caseworker VA. Results are presented in Figures 2.5a and 2.5b for employment and training, respectively. These figures illustrate that our caseworker value-added measures are not associated with any of the predictions based on ex-ante measures of the outcomes.

Furthermore, as value-added estimates ($\hat{\mu}_{j,t}$) are designed to be the optimal linear predictors of residualized outcomes ($\hat{Y}_i$), they should yield a coefficient of 1 by construction. We verify this by regressing $\hat{Y}_i$ on $\hat{\mu}_{j,t}$. As shown by Figure 2.6 that depicts the relationship between $\hat{Y}_i$ and $\hat{\mu}_{j,t}$ non-parametrically, we observe an almost perfectly linear relationship for both outcomes, indicating that a linear prediction model effectively fits the data.

Identifying equation and interpretation

To assess the influence of caseworkers on youths' outcomes, we employ the following regression model:

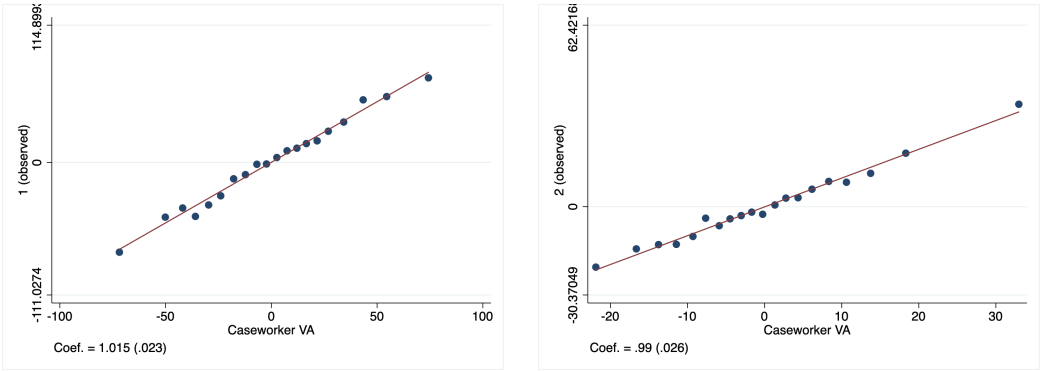
$$Y_i = \alpha + \beta X_i + \delta \hat{\mu}_{j,t} + \gamma_{a,t} + \epsilon_i \tag{2.4}$$

We standardize caseworkers' VA estimates, enabling us to interpret δ as the impact of a one standard deviation increase in caseworker VA.

Caseworkers' VA is measured for either employment or training. For employment, we estimate VA based on the total number of employment days during a 2-year period following the first meeting with caseworker j. Concerning training,

Figure 2.6: Effects of caseworker VA on observed outcomes

(a) Employment (b) Training



Note: The figures show binscatters of caseworker value-added and youth actual outcomes. The figure on the right shows youth observed employment outcome, i.e. cumulative number of days of employment within the 24 months after the first meeting. The figure on the right shows youth observed training outcome, i.e. cumulative number of days of training within the 24 months after the first meeting.

we gauge VA using a similar outcome. As a sensitivity test, we also calculate VA for other employment and training outcomes and show their correlations with our primary measures in Tables 2.3 and 2.5 in Appendix F.

It is crucial to acknowledge that caseworkers' impact on youths is likely to be multidimensional, and they may have a positive influence on youth that is not directly measurable through labor market outcomes. Therefore, our VA estimates should not be interpreted as a measure of the overall caseworker "quality," but rather as an assessment of how caseworkers vary in their ability to enhance young people's prospects within specific dimensions related to the labor market.

2.5 Caseworkers effects

This section presents the primary findings regarding the impact of caseworkers on the labor market outcomes of young people. We also explore the heterogeneity of these effects based on youths' characteristics and local labor market conditions[19].

2.5.1 On employment

Table 2.2 presents our primary findings concerning the total number of days of employment, which we also use to measure our VA estimate on employment, as de-

[19] All the results are replicated for the sample of the mission locale of Paris, for which we had direct information about the exogeneity of the assignment process from the management team. They are qualitatively similar to those obtained for our selected agencies. Figures and tables are shown in Appendix E.

Table 2.2: Caseworkers impact on youth employment

Employment (std)	Before 1st meeting		After 1st meeting			
	(1)	(2)	(3)	(4)	(5)	(6)
Caseworker VA (std)	0.0150	0.0092	0.0528***	0.0550***	0.0498***	0.0532***
	(0.0107)	(0.0122)	(0.0128)	(0.0151)	(0.0124)	(0.0146)
Employment before 1st meeting					0.2030***	0.2032***
					(0.0058)	(0.0060)
Observations	47,297	44,780	47,297	44,780	47,297	44,780
R-squared	0.0927	0.0922	0.0859	0.0852	0.1272	0.1266
Outcome mean	19.1	19.1	210.3	210.3	210.3	210.3
Outcome standard deviation	41.4	41.4	225.9	225.9	225.9	225.9
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
95% Winsorization of VA	No	Yes	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the caseworker level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation (2.3). Individual characteristics include all the characteristics listed in Table 2.13 in Appendix C.

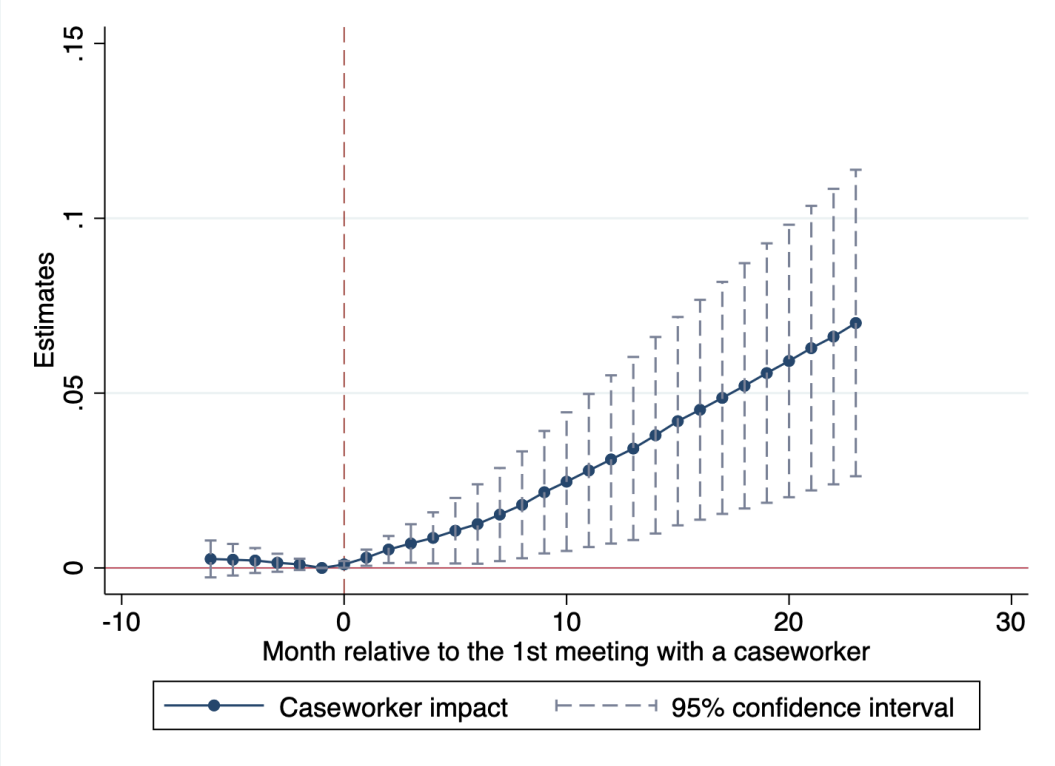
scribed above.

Similar to the forecast bias, we directly perform placebo tests by considering the number of employment days before the first meeting with the assigned caseworker. Indeed, ex-ante labor market outcomes should not be related to caseworkers' VA if our identifying assumption is valid. We find that caseworker VA estimates are both small in magnitude and statistically insignificant.

After the first meeting, however, we find that being assigned to a caseworker whose VA is one standard deviation (SD) above average leads to a significant increase in the total number of days of employment within the two-year period. The magnitude of the effect is around 0.05 SD. In line with the placebo tests, introducing the past employment days has no effect on caseworkers' VA estimates, although it is highly correlated with ex-post outcomes, as we could expect. The results are robust to a 95% winsorization of the sample based on caseworkers' VA. Taking the estimates of column (6) from Table 2.2, which is our preferred specification, and given the mean and standard deviation of the outcome (210 and 225 days, respectively), this corresponds to an average treatment effect on the treated (ATT) of almost 6%. This magnitude is in line with what is found in the few other studies looking at caseworkers helping adult job seekers in different countries [Rasmussen, 2021, Cederlöf et al., 2021, Schiprowski, 2020].

This impact is not uniform in time. We show how the effect of being assigned to a caseworker who is one standard deviation above average evolves over time in Figure 2.7. We first see that the effect is again not significant in the period before the first meeting, which is reassuring since they have supposedly not met, nor been sorted with the youths. After registration and assignment, though, we see that the effect of having a caseworker who is one standard deviation above average is moderate during the first six months of assistance, which is below 0.03 percentage

Figure 2.7: Effect on the cumulative number of days of employment over time



Note: Month 0 corresponds to the month of the first meeting.

points (pp). This effect steadily increases afterward up to the end of the second year of assistance and reaches 0.07 SD, which corresponds to an ATT of 7.5%.

We now consider alternative outcomes to evaluate the effect of caseworker VA on the employment situations of youths. More specifically, during the 24-month period after registration, we examine the impact of caseworkers on the probability of being employed for at least one day, the probability of being employed for at least one day per month, the number of days of employment per month, the total number of days of employment in stable jobs (including permanent contracts and temporary contracts lasting more than six months), and the total number of days of employment in permanent jobs only. Table 2.3 presents the results from regressions similar to column (6) of Table 2.2. We find that the effect of caseworkers who are one SD above the average caseworker is always significantly positive across the outcomes. For instance, we see an impact of +2.6% on the probability of being employed for at least one day and 4.4% on the probability of working at least one day each month. Interestingly, we see that the effect of 1 SD above the mean caseworker is less pronounced on the total number of employment days under a permanent contract, with a mean effect of 4.8%

Table 2.3: Caseworkers impact on other youth employment outcomes

Outcome	Mean	Sd	Caseworker VA	ATT	Significance
	(1)	(2)	(3)	(4)	(5)
Working at least 1 day	.7074	.4550	.0409	+2.6%	***
Working at least 1 day per month	.3412	.4741	.0317	+4.4%	***
Number of days of employment per month	8.82	13.33	.0313	+4.7%	***
Total number of days of employment	210.30	225.90	.0532	+5.7%	***
Total number of days of employment in stable job	144.52	211.64	.0390	+5.7%	***
Total number of days of employment in permanent job	82.30	170.87	.0232	+4.8%	**

Note: Estimates are obtained with OLS regressions including individual characteristics, agency × month fixed effects, employment situation before the first meeting, and caseworkers winsorized at the 95% level. Standard errors are below coefficients in parentheses and clustered at the caseworker level. Permanent job include *CDI* contract. Stable job include *CDI* contract and *CDD* that lasts more than six months.

2.5.2 On training

In line with the methodology employed for analyzing employment outcomes, an analogous analysis is conducted concerning training outcomes. To quantitatively assess the influence of caseworkers on training, a regression analysis is once again performed, regressing the training outcomes of youths against the variable "caseworker VA," using equation (2.4). The findings pertaining to the total number of days of training are presented in Table 2.4.

Furthermore, placebo tests are conducted, focusing on the number of days of training prior to the initial meeting with the assigned caseworker. These tests reveal that caseworker VA estimates are nearly zero and statistically insignificant. However, after the first meeting, it is observed that being assigned to a caseworker whose VA is one SD above the average results in a significant increase in the total number of training days within the two-year period. This effect exhibits strong robustness, approximately equal to 0.03 SD. Consistent with the placebo tests, the introduction of past training days does not impact caseworkers' VA estimates, although it is correlated with ex-post outcomes. Additionally, the results remain robust when considering a 95% winsorization of the sample based on caseworker VA. Taking the estimate from column (6) in Table 2.4, which remains our preferred specification, and considering the mean and standard deviation of the outcome (31 and 80 days, respectively), this corresponds to an ATT of approximately 8.5%.

This result is a novel contribution to the existing literature and carries significant implications for young individuals. Given their enrollment in public agencies with labor market difficulties and low educational levels, placing them in training programs to acquire skills valued by employers is generally regarded as a second best objective.

Moreover, event-study reveals that the impact of being assigned to a caseworker one standard deviation above the average undergoes notable evolution, as depicted in Figure 2.8. It is observed that, unlike the employment context, although the ef-

Table 2.4: Caseworkers impact on youth training

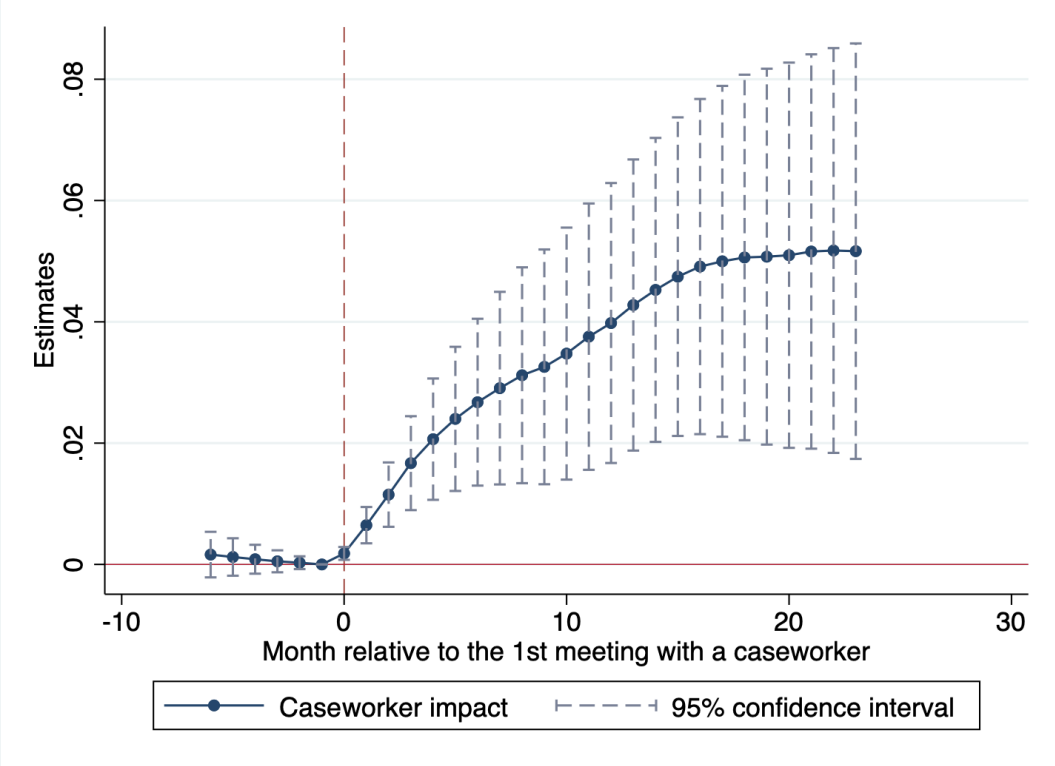
Training (std)	Before 1st meeting		After 1st meeting			
	(1)	(2)	(3)	(4)	(5)	(6)
Caseworker VA (std)	0.0109	0.0057	0.0386***	0.0330***	0.0383***	0.0329***
	(0.0115)	(0.0130)	(0.0119)	(0.0113)	(0.0119)	(0.0114)
Training before 1st meeting					0.0221***	0.0220***
					(0.0042)	(0.0043)
Constant	-0.6289***	-0.6356***	-0.5025***	-0.5187***	-0.4886***	-0.5047***
	(0.0741)	(0.0729)	(0.0635)	(0.0663)	(0.0638)	(0.0667)
Observations	47,297	44,954	47,297	44,954	47,297	44,954
R-squared	0.0158	0.0163	0.0253	0.0241	0.0258	0.0247
Outcome mean	1.0	1.0	30.8	30.8	30.8	30.8
Outcome standard deviation	10.2	10.2	79.9	79.9	79.9	79.9
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
95% Winsorization of VA	No	Yes	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the caseworker level. Caseworker VA is computed on the number of days of training 24 months after the first meeting according to equation 2.3. Individual characteristics include all the characteristics listed in Table 2.13 in Appendix C.

fect's magnitude is noticeably lower, caseworkers exhibit an influence that intensifies during the initial phase of assistance, reaching 0.04 SD. Subsequently, this effect stabilizes during the second year, hovering around 0.05 SD. This observation is interpreted as the imperative role caseworkers play in facilitating training placements at the outset due to the lack of skills hindering the attainment of suitable employment in the labor market.

Similar to the analysis of employment outcomes, alternative training-related outcomes are considered during the 24-month period following registration. Specifically, the impact of caseworkers on the probability of being engaged in training for at least one day, the probability of participating in training for at least one day per month, the number of training days per month, the total number of days in certified long-duration training, and the total number of training hours in both all training and certified-only training are investigated. Table 2.5 summarizes the results obtained from regressions analogous to those in column (6) of Table 2.4. It is found that the effect of caseworkers who are one SD above the average caseworker remains significant and consistent across various outcomes, ranging from a +7.3% impact on the probability of being in training for at least one day per month to a +9.2% effect on the number of training days per month. However, it is noteworthy that these effects become non-significant when examining certified training. As discussed in Section 2.6.2, these findings suggest that the impact of higher VA caseworkers on young people is primarily associated with non-certified training, which typically has a shorter duration compared to certified training programs.

Figure 2.8: Effect on the cumulative number of days of training over time



Note: Month 0 corresponds to the month of the first meeting.

2.5.3 Heterogeneity

Our primary findings reveal that certain caseworkers exert a significant influence on the subsequent labor market performance of young individuals. In this section, we delve into the heterogeneity of these effects, considering the characteristics of both the young individuals and the labor market. Across various subgroups, we employ the same regression model as presented in Equation 2.4.

In terms of youth characteristics, we investigate heterogeneity by gender, age, education, and ethnicity, as inferred from their first and last names. As illustrated in Figure 2.9, our estimates generally exhibit sufficient precision to statistically reject the notion that the impact of caseworkers is uniform across subgroups and regardless of the specific outcome under consideration. We will leverage this insight to simulate multiple scenarios of caseworker reallocation among young individuals, based on conditional treatment effects, as discussed in Section 2.6.3.

Although the determinants of these heterogeneous effects are beyond the scope of this paper, it remains informative to highlight them. Figure 2.9a illustrates the effects on both employment and training for males and females, Figure 2.9b presents outcomes for different age groups, Figure 2.9c showcases effects for various education levels, and Figure 2.9d examines youths with African-related ethnic back-

Table 2.5: Caseworkers impact on other youth training outcomes

Outcome	Mean	Sd	Caseworker VA	ATT	Significance
	(1)	(2)	(3)	(4)	(5)
Training at least 1 day	.2017	.4013	.0367	+7.3%	***
Training at least 1 day per month	.0490	.2158	.0199	+8.8%	***
Number of days of training per month	1.25	5.85	.0197	+9.2%	***
Total number of days of training	30.80	79.90	.0329	+8.5%	***
Total number of days of certified training	7.87	42.74	.0081	+4.4%	
Total number of hours of training	13.22	47.54	.0257	+9.2%	**
Total number of hours of certified training	4.23	31.78	.0127	+9.5%	

Note: Estimates are obtained with OLS regressions including individual characteristics, agency × month fixed effects, training situation before the first meeting, and caseworkers winsorized at the 95% level. Standard errors are below coefficients in parentheses and clustered at the caseworker level. Long-duration training are training that lasts more than six months.

grounds.

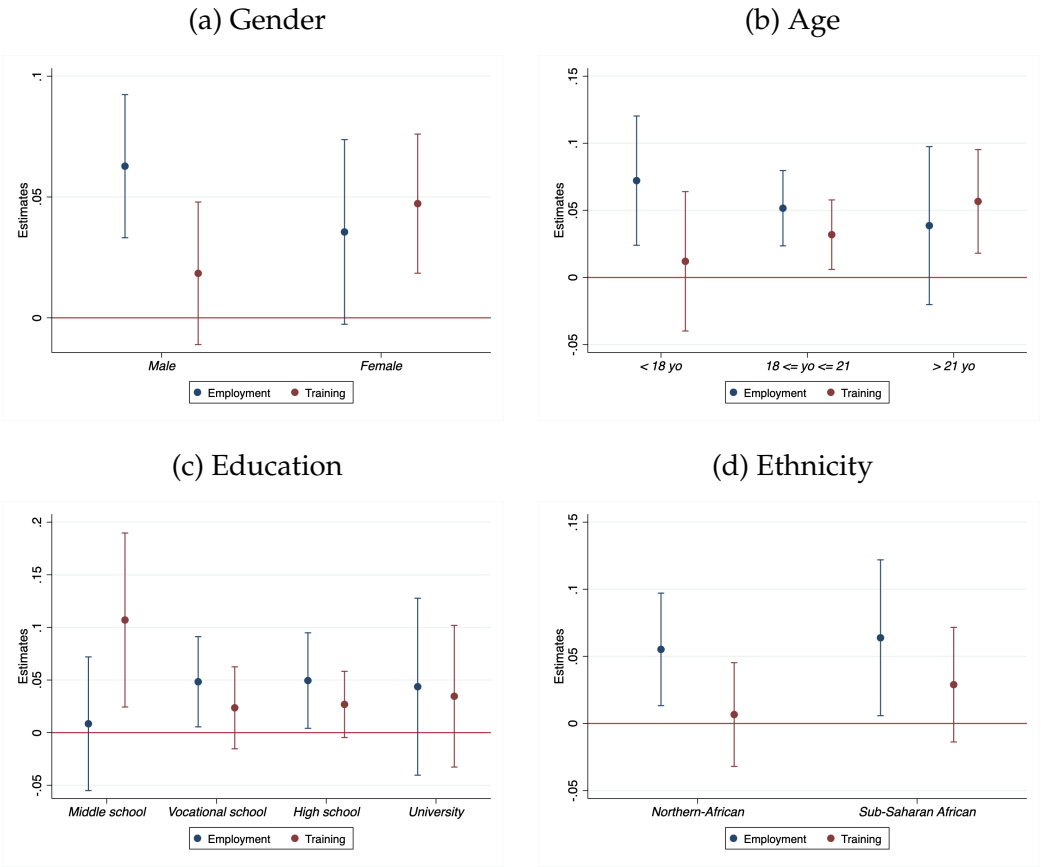
Our findings reveal that females tend to benefit more from caseworkers with high VA scores in terms of training, which facilitates their entry into training programs. On the other hand, males experience greater benefits from caseworkers with high VA scores in employment. This discrepancy may be attributed to the higher number of females enrolling in agencies with a middle-school level of education compared to males. Consequently, caseworkers with superior abilities in training placement are more pertinent for females as they seek to enhance their skill sets.

Concerning age, the influence of caseworkers, as measured by our VA in employment, appears to decrease as individuals grow older, contrary to initial expectations. Notably, older youths seem to benefit less from caseworkers with high VA scores in employment compared to younger individuals. Conversely, they derive greater advantages from caseworkers with high VA scores in training, possibly due to the relatively small variance in past employment across age groups, indicating that older youths may need to update their skills, a need better addressed by high VA caseworkers.

Additionally, we observe a more pronounced impact of caseworkers with higher training VA scores on youths with a middle school level of education. However, this impact is non-significant for individuals with other educational backgrounds. These young individuals typically possess adequate skills to enter the labor market, leading caseworkers with strong training placement abilities to potentially consider skill enhancement unnecessary. Regarding employment, we discern an inverted U-shaped effect of more capable caseworkers across different educational levels.

Lastly, young individuals of African origin experience the most significant benefits from caseworkers with high VA scores in employment, facilitating their entry into the job market, while the effect on training remains near zero and non-significant.

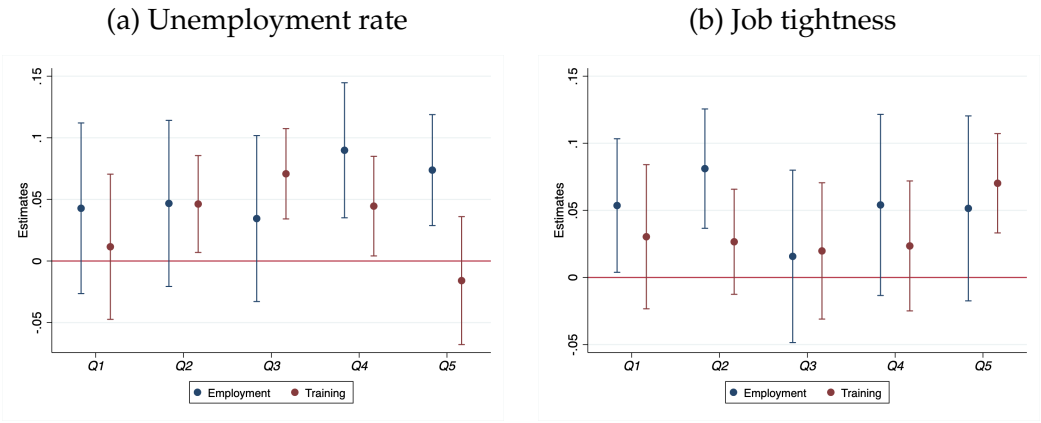
Figure 2.9: Heterogeneous effects of caseworker across youths' characteristics



Note: The estimates are obtained from regressions equivalent to Column (6) in Table 2.2.

Beyond the analysis of youth characteristics, we explore the impact of caseworkers with VA scores one SD above average across various labor markets. Figure 2.10a demonstrates differentiated effects across areas with varying unemployment rates, while Figure 2.10b presents correlated results across regions with different job tightness levels. Surprisingly, the impact of caseworkers with higher VA scores in employment is more pronounced in areas characterized by high unemployment rates and lower job tightness, even though one might expect more intense job competition in these regions.

Figure 2.10: Heterogeneous effects of caseworker across different labor markets



Note: The estimates are obtained from regressions equivalent to Column (6) in Table 2.2.

2.6 Discussion

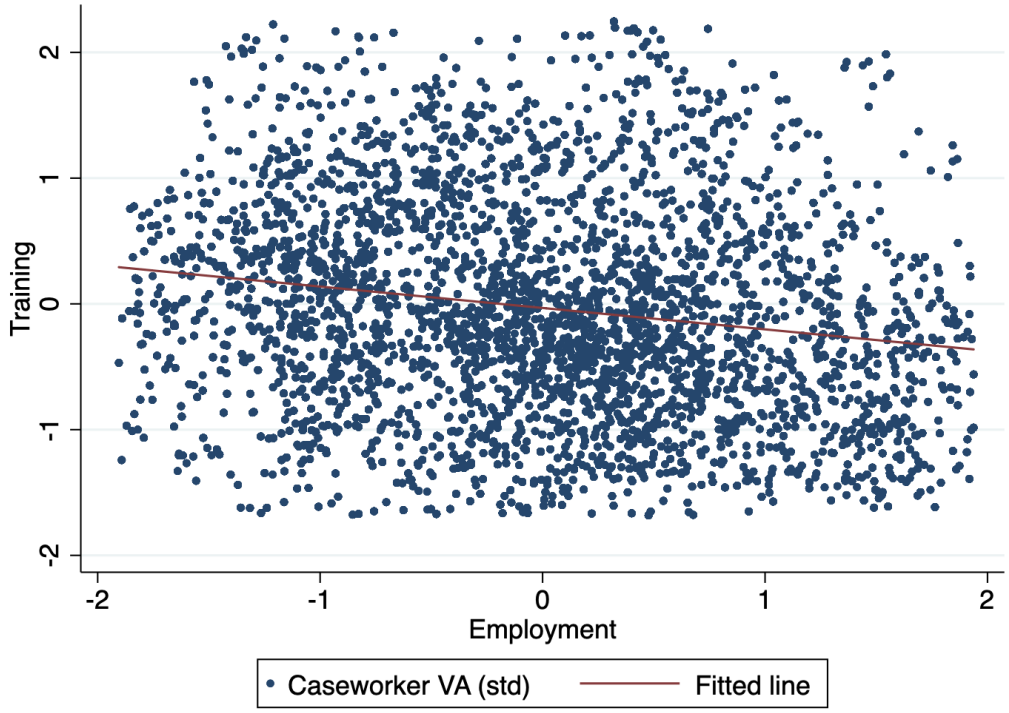
We now delve into the potential causes and consequences associated with caseworkers exhibiting different levels of value-added. Initially, we categorize caseworkers into distinct group levels. Subsequently, we scrutinize the various practices employed by these groups to shed light on the determinants of their abilities. Finally, we conduct various policy simulations to analyze the effects of alternative assignment mechanisms.

2.6.1 Discrete treatment

As shown in Figure 2.11, caseworkers who excel in placing young individuals into employment are not necessarily the same caseworkers who excel in placing them into training. Notably, there is a slight negative correlation between our two value-added estimators, suggesting that caseworkers tend to specialize in one of the two dimensions without significantly affecting the other.

Unfortunately, we lack sufficient data on the socio-demographic characteristics of caseworkers, preventing us from investigating whether more capable caseworkers possess distinct personality traits compared to their peers. Other studies may provide insights into this matter. For instance, Cederlöf et al. [2021] find that cognitive ability and personal experience of unemployment are unrelated to caseworker performance when assisting adult job seekers. Their sole finding is that caseworkers with more than two years of experience perform better than those with less experience. Due to data limitations, we leave this question open in our article, especially considering that personality traits may be correlated with caseworkers' practices, some of which have been found to be correlated with performance.

Figure 2.11: Correlation between VA measures on employment and training



Note: Value added is measured on the number of days of employment 24 months after the first meeting in the y-axis, and on the number of days of training 24 months after the meeting in the x-axis.

To facilitate interpretation, we discretize VA estimates for both employment and training measures. Initially, we compute the mean VA estimate for each caseworker. Within agencies, we group caseworkers into terciles based on their mean VA value[20]. We categorize caseworkers as follows:

- Tercile 1: High VA caseworkers → Treatment group 1
- Tercile 2: Middle VA caseworkers → Control group
- Tercile 3: Low VA caseworkers → Treatment group 2

Table 2.6 presents the impact of High and Low VA caseworkers, in comparison with Middle VA caseworkers, on the main outcomes of young individuals. The estimates indicate that High employment VA caseworkers do not significantly affect training outcomes. Similarly, High training VA caseworkers do not significantly impact employment outcomes. This suggests that caseworkers not only tend to specialize in either employment or training but that specialization in one dimension does not significantly impede their effectiveness in the other.

[20]We adopt this approach since we limited our sample to agencies with at least three caseworkers, which is the actual number of caseworkers observed in some agencies.

Table 2.6: Impact of High VA vs Low VA Caseworkers on youth outcomes

Outcomes	Employent				Training			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High vs Middle VA caseworker (employment)	18.4630***	19.3542***			-0.3115	-0.3877		
	(1.3072)	(1.4791)			(1.1359)	(1.2063)		
Low vs Middle VA caseworker (employment)	-21.1514***	-21.3754***			1.5411*	1.6426*		
	(1.5113)	(1.6988)			(0.9081)	(0.9609)		
High vs Middle VA caseworker (training)			-0.0390	0.8229			7.9575***	8.0320***
			(2.6132)	(2.6445)			(0.5401)	(0.5267)
Low vs Middle VA caseworker (training)			2.4013	2.6638			-6.9881***	-7.1929***
			(2.7943)	(2.9564)			(0.4151)	(0.4465)
Observations	60,384	57,210	60,384	57,230	60,384	57,210	60,384	57,230
R-squared	0.0919	0.0914	0.0872	0.0881	0.0248	0.0252	0.0300	0.0292
Outcome mean	213	213	208	207	30	30	31	31
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
95% Winsorization threshold	No	Yes	No	Yes	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the caseworker level. Caseworker VA is either computed on the number of days of employment, or training, 24 months after the first meeting according to equation (2.3). Then, VA estimators are dichotomized as described in Section 2.6.1. Individual characteristics include all the characteristics listed in Table 2.13 in Appendix C.

On their respective dimensions, the results complement those presented in Section 3.4. High employment VA caseworkers increase the total number of employment days by an average of 19 days compared to Middle VA caseworkers. In contrast, Low employment VA caseworkers decrease this outcome by an average of 21 days. Therefore, the effects on employment appear asymmetric, with the losses due to low VA caseworkers outweighing the gains from high VA caseworkers by 2 days.

Conversely, in the context of training, gains exceed losses by an average of 1 day. High training VA caseworkers increase the total number of training days by an average of 8 days compared to Middle VA caseworkers, while Low training VA caseworkers decrease this total by an average of 7 days.

2.6.2 Potential mechanisms

In the following sections, we try to understand the heterogeneous impact of caseworkers on young people labor market outcomes.

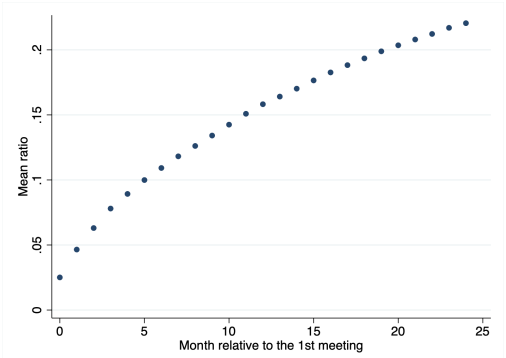
Temporary jobs and non-certified training

High employment VA caseworkers help more young people gain employment days, while High training VA caseworkers assist more youths in acquiring training days. This naturally raises the question of whether employment is done under temporary or permanent contracts, and if the training is certified or not.

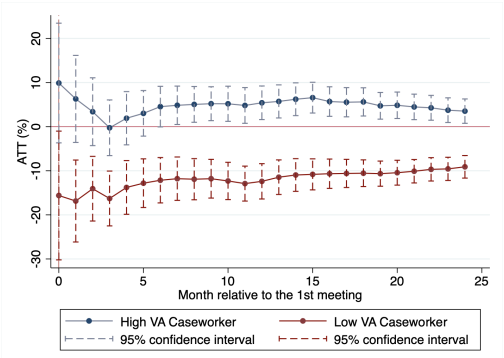
Figure 2.12a illustrates the progression of the ratio of employment days in permanent contracts to the total employment days within the 24-month period after the first meeting with a Middle employment VA caseworker. Notably, the majority of young people initially experience employment under temporary contracts, although the proportion of permanent contracts increases over time. After 2 years,

Figure 2.12: Effect of High and Low vs Middle VA caseworker (employment) on permanent employment

(a) Outcome mean



(b) ATT



Note: The estimates are obtained from regressions equivalent to Column (6) in Table 2.2. The outcome is the ratio of the number of days of employment under permanent contract over the number ot days of employment under any contract.

approximately 22% of employment days are associated with permanent contracts, meaning that the probability of working under a permanent contract after two years is around 22%.

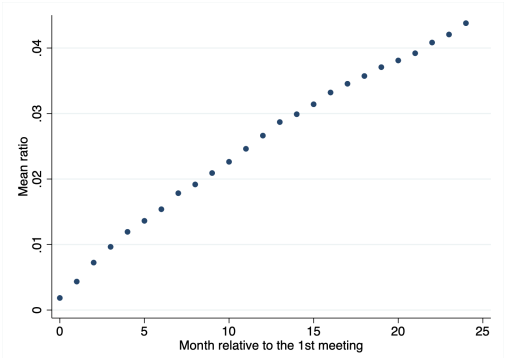
Figure 2.12b complements this view by displaying the average effect of High or Low employment VA caseworkers, as compared to Middle VA caseworkers, on this ratio over time. Notably, the impact on permanent contracts differs between caseworker types. High employment VA caseworkers increase this ratio by an average of 5% over the 2-year period, while Low employment VA caseworkers decrease it by 10%. However, it's essential to note that, despite the positive or negative influence of High or Low VA caseworkers, the overall changes remain relatively small. Young people assigned to High VA caseworkers see their probability of working under a permanent contract rise from 22% to 23%, while those assigned to low VA caseworkers see their probability decrease from 22% to 20%.

We replicate a similar analysis on certified training, depending on the training VA group of caseworkers. Figures 2.13a show the evolution of the ratio of training days in certified training to the total training days (certified or not) within the 24-month period after the first meeting with a Middle training VA caseworker. It's evident that even fewer young people experience training that leads to certification, although the proportion of certifying training increases over time. After 2 years, only about 4% of training days are associated with certification, implying that the probability of enrolling in a certifying training program after two years is approximately 4%.

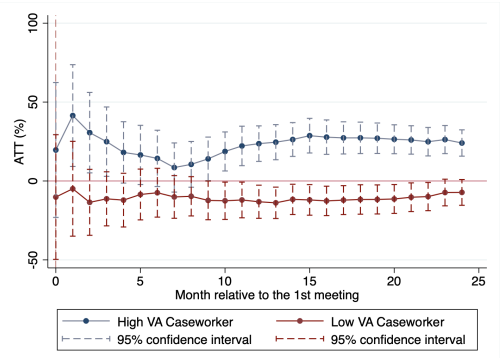
Figure 2.13b complements this view by showing the average effect of High or

Figure 2.13: Effect of High and Low vs Middle VA caseworker (training) on certified training

(a) Outcome mean



(b) ATT



Note: The estimates are obtained from regressions equivalent to Column (6) in Table 2.2. The outcome is the ratio of the number of days of training under long-duration training over the number of days of training under any duration.

Low training VA caseworkers, as compared to Middle VA caseworkers, on this ratio over time. Similar to the impact on permanent contracts, the effect on certified training varies across caseworker types. High training VA caseworkers increase this ratio by an average of 25% over the 2-year period, while Low training VA caseworkers decrease it by 5%. The latter effect of Low training VA caseworkers is barely significant at the 95% level, suggesting that only High training VA caseworkers have a significant impact on certified training. However, the absolute magnitude of the effect remains relatively small, as the share of certified training is very low. Young people assigned to High training VA caseworkers see their probability of enrolling in training programs that deliver certification rise from 4% to 5%.

Furthermore, we examine whether enrolling in training, due to a High training VA caseworker, increases the probability of subsequent employment. Figure 2.33a in Appendix H shows that, on average, for every day of training, young people work for approximately 10 days. However, this ratio is unaffected by the type of caseworker assigned to the youths, as shown in Figure 2.33b. As an additional robustness check, we employ a two-stage least square instrumental variable (IV) strategy to investigate the impact of High training VA caseworkers on training enrollment and subsequent employment. The top panel of Table 2.25 in Appendix H indicates a positive and significant impact of High training VA caseworkers on training enrollment. Nevertheless, the bottom panel of the same table shows that this increase in training enrollment does not lead to a significant impact on subsequent employment, consistent with the findings presented in Figure 2.33b and in Section 2.6.1.

In conclusion, caseworkers with High employment VA do not necessarily place

young people more often in permanent contracts, and High training VA caseworkers do not necessarily enroll them in certified training programs. High VA caseworkers may employ other practices that set them apart from Middle and Low VA caseworkers.

Direct assistance and placement in program

Table 2.7 presents correlations between our VA estimators, both in terms of employment (columns 1 and 2) and training (columns 3 and 4), and various caseworker practices. These practices include the number of individual meetings conducted during the 2-year period, the number of collective workshops, information sessions, administrative tasks, digital contacts (email, SMS, phone calls), and the number of job, training, project, and other propositions at the caseworker-youth level[21].

Given the specialization of caseworkers in either employment or training placement, there are distinct practices between caseworkers who excel in one dimension without negatively affecting the other. First, it is evident that the number of individual meetings is not associated with caseworkers' performance. This finding contrasts with the primary result of Schiprowski [2020], where it was shown that the loss of one unplanned meeting due to absence increased job-seekers' unemployment duration by 5%. In our case, one possible explanation is that the quantity of meetings may not be as crucial as their quality, particularly regarding young people, which our data does not fully capture. Alternatively, the diversity in youth situations may account for the variation, with some individuals requiring more meetings to secure appropriate placements.

Subsequently, caseworkers who perform better in employment tend to engage in fewer administrative tasks, while those who excel in training conduct more collective workshops. In these workshops, caseworkers primarily train young people to identify labor market opportunities independently. Caseworkers who invest more time in these activities may have more opportunities for in-depth analysis and discussions with young people. Since young people often struggle to enter the labor market, they tend to participate more in specialized training.

Caseworkers who excel in training tend to propose more training opportunities, and likewise for those who excel in employment, although the impact is minimal. On average, Low employment VA caseworkers offer eight job propositions during the 2-year assistance period. Consequently, the results indicate that adding one extra job proposition would only increase caseworkers' employment VA by 0.0011 SD, keeping them within the Low employment VA group. Regarding training proposi-

[21] Results remain qualitatively consistent when averaging at the caseworker level.

Table 2.7: Correlation of caseworkers VA with practices

High vs Low VA caseworker	Employment		Training	
	(1)	(2)	(3)	(4)
Nb of individual meetings	0.0012	0.0034	-0.0008	-0.0006
	(0.0030)	(0.0029)	(0.0027)	(0.0027)
Nb of collective workshops	0.0140	0.0108	0.0243***	0.0232**
	(0.0101)	(0.0094)	(0.0091)	(0.0090)
Nb of information sessions	-0.0059	-0.0368	-0.0175	-0.0216
	(0.0308)	(0.0230)	(0.0324)	(0.0329)
Nb of administrative tasks	-0.0110	-0.0171**	-0.0057	-0.0103
	(0.0081)	(0.0073)	(0.0080)	(0.0078)
Nb of digital contacts	0.0011	0.0005	0.0009	0.0013
	(0.0047)	(0.0046)	(0.0040)	(0.0038)
Nb of job propositions	0.0011***	0.0010***	0.0006	0.0007
	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Nb of training propositions	-0.0027	-0.0027	0.0105***	0.0103***
	(0.0022)	(0.0022)	(0.0033)	(0.0034)
Nb of project propositions	-0.0024	-0.0022	-0.0029*	-0.0027*
	(0.0014)	(0.0014)	(0.0015)	(0.0015)
Nb of other propositions	-0.0018**	-0.0020**	-0.0014*	-0.0015*
	(0.0009)	(0.0009)	(0.0008)	(0.0008)
Observations	40,110	37,668	39,783	37,242
R-squared	0.1112	0.1376	0.1185	0.1413
Individual characteristics	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes
95% Winsorization of VA	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. Caseworker VA is computed on employment or training outcome 24 months after the first meeting according to equation 2.3.

tions, the average is 1.7 propositions for Low training VA caseworkers. Adding two more propositions would raise the probability of transitioning from Low training VA to High training VA by just 2 percentage points, which is still a small effect.

Furthermore, we observe that High training VA caseworkers make fewer project and other-type propositions compared to Low training VA caseworkers. This may suggest that caseworkers who prioritize training propositions aligned with the needs of young people have less time available for other labor market-related propositions.

Interestingly, maintaining digital contact via SMS, email, or phone calls with young people is not correlated with higher value-added, whether in terms of employment or training. This result contradicts findings when examining Paris agencies exclusively. Table 2.22 in Appendix E shows that the number of digital contacts is the only practice that differs between High employment VA and Low employment VA caseworkers in Paris. The magnitude of the effect is substantial, as conducting one additional digital contact per month with all their young people for a year

Table 2.8: Correlation of caseworkers VA with placement into programs

High vs Low VA caseworker	Employment		Training	
	(1)	(2)	(3)	(4)
Program: Diagnosis	-0.0078	-0.0018	0.0206	0.0078
	(0.0212)	(0.0205)	(0.0208)	(0.0205)
Program: PACEA	-0.0169	0.0032	0.0985	0.1320*
	(0.0609)	(0.0620)	(0.0837)	(0.0769)
Program: CEP	0.0015	-0.0196	-0.0934	-0.1233
	(0.0585)	(0.0598)	(0.0849)	(0.0783)
Program: PPAE	0.0215	0.0220	-0.0130	-0.0161
	(0.0152)	(0.0159)	(0.0160)	(0.0167)
Program: GJ	-0.0121	-0.0159	-0.0117	-0.0108
	(0.0122)	(0.0124)	(0.0130)	(0.0134)
Program: AIJ	0.0287**	0.0314***	0.0108	0.0148
	(0.0116)	(0.0118)	(0.0131)	(0.0129)
Program: Other	-0.0099	-0.0102	0.0323*	0.0175
	(0.0103)	(0.0107)	(0.0187)	(0.0136)
Observations	40,110	37,668	39,783	37,242
R-squared	0.1097	0.1346	0.1159	0.1379
Individual characteristics	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes
95% Winsorization of VA	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. Caseworker VA is computed on employment or training outcome 24 months after the first meeting according to equation 2.3.

would increase the probability of transitioning from the Low employment VA group to the High group by 42 pp. Qualitative insights from discussions with caseworkers in Paris agencies indicated that maintaining contact with youths was a vital tool for keeping them on track and boosting their confidence. However, this mechanism is not observed in our selected agencies, suggesting that practices among caseworkers may vary across centers due to differences in youth and labor market characteristics.

Regarding placement into programs, we find minimal correlations with our VA estimators, as summarized in Table 2.8. On one hand, caseworkers who place their youths in the "*Accompagnement Intensif Jeunes*" (AIJ) program have higher employment VA. While this program is not run within the agency but rather by the French public employment agency ("*Pôle emploi*") with other types of caseworkers, one possible explanation is that High employment VA caseworkers effectively outsource assistance in the matching process to other caseworkers overseeing the AIJ program, with the expectation that young people will find suitable solutions through this program.

Finally, we observe a positive correlation between the PACEA program and high

training VA. The PACEA program, which stands for "*Parcours contractualisé d'accompagnement vers l'emploi et l'autonomie*" in French (*Contractualized Path to Employment and Autonomy*), is not a program per se. Introduced in January 2017, it served as a framework for caseworkers to formalize the assistance provided to young people and outlined the expectations that young people were required to follow. This framework was not compulsory in practice until January 2019. However, caseworkers eager to use it could establish a clear assistance path within the first month after the initial meeting, aligning with young people's needs. It appears that caseworkers who utilized this framework were also those who were more invested in finding appropriate solutions for young people, particularly in the case of training.

Persistence of VA and competition among caseworkers

Now, we turn our attention to the persistence of VA estimates over time and investigate the presence of competition among caseworkers. It's conceivable that a caseworker who initially places fewer young people in employment or training may reverse this pattern in the following months. Therefore, we measure VA in employment and training not only at 24 months after the first meeting but also at 6 months after the initial meeting. We refer to the VA at 6 months as the short-term VA and the VA at 24 months as the long-term VA, examining these parameters for each caseworker in our analysis sample.

Furthermore, we explore the potential for competition among caseworkers using a simple linear-in-means-type model [Manski, 1993, Bramoullé et al., 2009]. To do so, we calculate the number of caseworkers who were in the High VA group after 6 months and remained in the High VA group after 24 months within each agency. Given that our primary outcome variable is the VA group after 24 months at the caseworker level, we exclude the observed caseworker's situation when calculating this number. We do not only control for the mean caseload size of each caseworker but also account for the size and structure of the agency to isolate the effect of other caseworkers and mitigate the so-called reflection problem.

On one hand, it is plausible for Low VA caseworkers to benefit from interactions with consistently High VA caseworkers. These highly effective caseworkers, skilled at placing young people in employment or training, may share their strategies for assisting youths with Low VA caseworkers. On the other hand, if there are a limited number of job or training opportunities in the local labor market, the High VA caseworkers' activity may displace the efforts of Low VA colleagues.

To assess the impact in our case, we estimate the following equation:

Table 2.9: Persistence of Low VA and effect of other High VA caseworkers (employment)

Low VA after 24 months (T^{24m}_{jac})	All	Unemployment rate		Job tightness	
	(1)	(2)	(3)	(4)	(5)
Low VA after 6 months (T^{6m}_{jac})	0.4651***	0.4762***	0.4286***	0.4539***	0.4511***
	(0.0572)	(0.0821)	(0.0817)	(0.0807)	(0.0842)
Nb of permanent High VA caseworkers (Φ_{ac})	0.1182***	0.0929*	0.1795***	0.1802***	0.0929*
	(0.0391)	(0.0523)	(0.0448)	(0.0431)	(0.0517)
Mean caseload ($\bar{C}_{jac}$)	-0.0009	-0.0011	-0.0006	-0.0004	-0.0014
	(0.0008)	(0.0009)	(0.0012)	(0.0011)	(0.0009)
Size of the agency (S_{ac})	-0.0022	-0.0015	-0.0038	-0.0032	-0.0020
	(0.0026)	(0.0034)	(0.0043)	(0.0045)	(0.0030)
Constant	0.2225*	0.2368	0.1762	0.1170	0.4173**
	(0.1229)	(0.1702)	(0.2059)	(0.1915)	(0.1481)
Observations	484	233	229	234	228
R-squared	0.2710	0.2741	0.2638	0.2889	0.2506
Center FE (Ψ_c)	Yes	Yes	Yes	Yes	Yes
Sample restriction	None	Below median	Above median	Below median	Above median

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation 2.3.

$$T^{24m}_{jac} = \alpha + \beta T^{6m}_{jac} + \delta \Phi_{ac,-j} + \lambda \bar{C}_{jac} + \gamma S_{ac} + \Psi_c + \epsilon_{jac}$$

where T^{24m}_{jac} is equal to 1 if caseworker j, in agency a of center c, belongs to the low VA group, and 0 if she belongs to the middle or high VA group, based on the total number of days of employment or training of her youths during the 24-month period. T^{6m}_{jac} is equal to 1 if caseworker j, in agency a of center c, belongs to the low VA group, and 0 if she belongs to the middle or high VA group, based on the total number of days of employment or training of her youths during the first 6-month period. $\Phi_{ac,-j}$ is the number of caseworkers in the agency who were in the high VA group after the first 6-month period and remained in the high VA group after the 24-month period, leaving-out caseworker j. $\bar{C}_{jac}$ is the mean caseload of the caseworker j in agency a of center c. S_{ac} is the total number of caseworkers who did first meetings and potentially assisted young people (either permanently or temporarily) in the agency a of center c. Ψ_c is a vector of center fixed effects. ϵ_{jac} is an idiosyncratic shock level at the caseworker level.

Results for employment VA are presented in Table 2.9. In Column (1), the results are for all caseworkers combined, while columns (2) and (3), and (4) and (5), examine these effects considering the unemployment rate, and job tightness levels respectively. Initially, we observe that belonging to the Low employment VA group after 6 months has a lasting impact. The probability of remaining in this Low employment VA group after 24 months increases by 46.5 pp. This effect remains consistent across local labor markets, whether the unemployment rate is below or above the median, or job tightness is below or above the median.

The probability of remaining in the Low employment VA group for a caseworker

Table 2.10: Persistence of Low VA and effect of other High VA caseworkers (training)

Low VA after 24 months (T_{jac}^{24m})	All	Unemployment rate		Job tightness	
	(1)	(2)	(3)	(4)	(5)
Low VA after 6 months (T_{jac}^{6m})	0.4323***	0.5140***	0.3649***	0.3893***	0.4890***
	(0.0588)	(0.0954)	(0.0641)	(0.0664)	(0.0992)
Nb of still High VA caseworkers (Φ_{ac})	0.1316***	0.1065***	0.1903***	0.1938***	0.1002***
	(0.0237)	(0.0361)	(0.0265)	(0.0262)	(0.0330)
Mean caseload ($\bar{C}_{jac}$)	0.0004	0.0008	-0.0001	0.0001	0.0005
	(0.0007)	(0.0012)	(0.0011)	(0.0010)	(0.0012)
Size of the agency (S_{ac})	-0.0040*	-0.0054	-0.0022	-0.0011	-0.0045
	(0.0023)	(0.0039)	(0.0025)	(0.0025)	(0.0030)
Constant	0.1672	0.0475	0.1704	0.1104	0.1320
	(0.1056)	(0.1829)	(0.1595)	(0.1473)	(0.2004)
Observations	474	227	225	231	221
R-squared	0.2435	0.3176	0.2032	0.2320	0.2844
Center FE (Ψ_c)	Yes	Yes	Yes	Yes	Yes
Sample restriction	None	Below median	Above median	Below median	Above median

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation 2.3.

with Low employment VA is further influenced by the number of permanently High employment VA caseworkers. The estimates suggest that having one additional permanently High employment VA caseworker in the agency increases the probability for a Low employment VA caseworker to remain in the Low VA group by an average of 11.8%. It appears that the activity of High VA caseworkers primarily displaces the activity of Low VA caseworkers rather than favoring them. This effect is even more pronounced when considering different labor markets. Notably, the effect of the number of permanently High employment VA caseworkers is lower in areas with a low unemployment rate (approximately 9 pp) and higher in areas with a high unemployment rate (approximately 18 pp). Similar results are observed when analyzing job tightness across different areas, indicating that caseworkers seem to be in greater competition for placing youths in employment when job opportunities are scarce compared to when job availability is high.

Moreover, not only does the number of permanently High employment VA caseworkers impact Low VA caseworkers, but it also affects High employment VA caseworkers themselves. Table 2.26 in Appendix I shows that High employment VA caseworkers have a higher probability of remaining in the High VA group after 24 months compared to Low VA caseworkers, especially in tight labor markets. However, competition from other High employment VA caseworkers is intense, resulting in a decrease of 28.4 pp for each additional High employment VA caseworker. This effect becomes more pronounced in slack labor markets, where there are numerous job seekers and limited job vacancies.

In parallel, Table 2.10 presents results for training VA. As with employment VA, we observe that being a Low training VA caseworker leads to a significant persistence effect, with a 43 pp increase in the probability of remaining a Low training VA

caseworker in the long run. Unlike employment VA, this persistence effect varies with the economic context, being less pronounced in slack areas where the unemployment rate is high or job tightness is low. This variation may be attributed to the presence of more training slots in areas where individuals face greater difficulty in finding suitable jobs, enabling caseworkers to place youths into training more effectively after several months. We also observe competition effects among caseworkers in the training context. The more permanently High training VA caseworkers in an agency, the higher the probability of remaining in the Low training VA group for a Low training VA caseworker. This competition effect is less sensitive to the economic situation of the local area. Table 2.27 in Appendix I offers additional results when caseworkers initially belong to the High training VA group. The conclusions align with those for employment VA caseworkers, except that persistence in the High VA group becomes less automatic in areas with a high unemployment rate or low job tightness.

2.6.3 Policy simulations

We now examine the implications of our results through a simulation exercise in which we estimate the impact on young people's wages and tax revenues of different scenarios for the assignment of caseworkers.

Our approach begins by identifying the total number of agencies and welcome places with at least three caseworkers and the corresponding number of youths associated with "mission locale" centers in our analysis sample[22]. Subsequently, we create a new sample with young individuals in agencies whose observable characteristics are, on average, similar to the observed characteristics in those agencies[23]. For each observation, we predict the total number of employment days within a two-year period based on these characteristics. To achieve this, we utilize training samples from 80% of our analysis database. In these training samples, we adjust the observed number of employment days for each youth by the average effect of the associated caseworker. We employ several machine learning algorithms, including linear regression, decision tree, random forest, support vector machines, and neural networks.

The mean values of total employment days predicted by each algorithm, along with the associated goodness of fit measured by the R^2 indicator, are presented in Table 2.28 in Appendix J. In this context, we select the predictions provided by the

[22]The restrictions previously outlined and applied in Section 2.3.2 were implemented to ensure a suitable sample for econometric analysis. We alleviate these restrictions here.

[23]These characteristics encompass the variables listed in Table 2.13 as well as a dummy variable for past employment days, i.e., days employed before registering with an agency.

neural network algorithm, as the mean prediction closely aligns with the observed outcome (213.77 days and 212.32 days, respectively). Nevertheless, our results remain robust when considering the predictions from other algorithms.

Next, we perform a similar prediction exercise for wages. Regrettably, our original database does not include wage data, and as such, we cannot observe this information for the young individuals in our study. To address this limitation, we rely on the pooled French labor force surveys from 2017 to 2020 to predict the mean net wage. Based on the same listed characteristics used for employment predictions, we estimate the monthly net wage for each youth in both our analysis and simulated samples. Table 2.29 in Appendix J displays the mean values of the monthly net wage predicted by the same set of machine learning algorithms. In this case, we choose the predictions returned by the random forest algorithm, with an average monthly net wage estimated at 1,273.51 EUR in our simulated sample, while the mean predicted wage in our analysis sample is 1,252.14 EUR.

For the final prediction exercise, we estimate the conditional treatment effect of having a High employment VA caseworker or a Low employment VA caseworker for each observation in our simulated sample. To do so, we determine the treatment effect of High and Low VA caseworkers at the group level using a causal forest model on our analysis sample. This model follows the approach described in Davis and Heller [2017], employing the honest approach originally developed in Wager and Athey [2018]. This class of model is suitable because of the quasi-random assignment of caseworkers to young individuals, as detailed in Section 2.4. Additionally, as observed in Section 2.5.3, the effect of higher VA employment caseworkers differs across youth characteristics. The causal forest model allows for a deeper analysis of the differentiated effects of High and Low VA caseworkers by interacting our treatment variables with the youths' characteristics. In our specification, the causal forest comprises 1,000 trees with a minimum of 10 observations per leaf. Figures 2.34 and 2.35 in Appendix J illustrate the conditional average treatment effect on the treated of High employment VA caseworkers and Low employment VA caseworkers, respectively. These figures clearly demonstrate varying effects as young individuals, used to train the algorithm, are ranked from the lowest to the highest effect. For instance, Figure 2.34 reveals that a High employment VA caseworker may decrease the total number of employment days by 100 for the youth with the lowest effect, and increase this total by 120 days for the youth with the highest effect[24].

[24]The average treatment effect on the treated (ATT), as measured by the mean of the conditional treatment effects, slightly underestimates the mean effect of high employment VA caseworkers with a value of +16 days, as opposed to the $\sim +19$ days estimated with OLS in Table 2.6 in Section 2.6.1. However, the mean effect of low employment VA caseworkers seems well-recovered by the causal forest model, with a mean value of -21 days, which is identical to the mean effect estimated in Table

Table 2.11: Reallocation of caseworkers to young people

Sample	Selected	Simulated		
	(1)	(2)	(3)	(4)
High VA caseworkers (1/3)				
ATT	15.87	16.16	46.60	19.26
Mean wage (net)	1,254.88	1,273.78	1,238.55	1,209.23
ΔPayroll tax	+12,136,426	+34,305,064.23	+96,906,298.20	+38,231,446.73
Low VA caseworkers (1/3)				
ATT	-21.57	-21.66	-24.07	-24.37
Mean Wage (net)	1,250.18	1,274.12	1,269.30	1,335.08
ΔPayroll tax	-24,159,526.65	-47,249,511.69	-54,131,647.39	-56,805,847.73
Nb of observations	60,384	148,801	148,801	148,801
Assignment mechanism	As-If Random	Random	Optimal	Assortative

Note: ATT is measured on the total number of days of employment during a 2-year period after first meeting. Net wages are previously predicted from random forest algorithms using the pooled French labor force surveys from 2017 to 2020. Wages are normalized in EUR base 2020 assuming that all wages earned between 2017 and 2019 grew at the same annual rate than the national net minimum wage. Payroll tax are computed with a tax rate of 53.22% on the raw wage.

With the knowledge of the effects of High and Low employment VA caseworkers for each youth, considering their observable characteristics, we can simulate various policies based on different assignment mechanisms of caseworkers to young individuals and evaluate the effects on employment days, income as a proportion of the monthly net wage, and payroll tax collection by retrieving raw wages[25].

Within each agency in our simulated sample, we divided our caseworkers into three groups: i) High employment VA, ii) Middle employment VA, and iii) Low employment VA. Since this is a simulated sample, we do not have access to the VA scores of individual caseworkers. However, we can estimate their average impact on each youth based on the conditional treatment effect, as explained earlier. We explore three distinct allocation scenarios in comparison to the baseline random assignment observed in our study: i) random allocation, ii) optimal allocation, and iii) assortative allocation.

In the first scenario, caseworkers were randomly assigned to youths, regardless of their VA group[26]. In the second scenario, caseworkers were allocated to young people based on their conditional treatment effect. In other words, youths were ranked according to their conditional treatment effect, and high employment VA

2.6.

[25]We assume that the employment situations of young individuals are carried out under classic temporary or permanent contracts that are not subsidized by public authorities. This assumption is strong, as caseworkers from "mission locale" agencies can assist young individuals in finding subsidized contracts. Unfortunately, we do not possess this information at present.

[26]We assume that caseworkers evenly distribute the number of youths among them. As we observed no significant differences in caseload size or the number of initial meetings among caseworkers in different VA groups, we assume that caseworkers have an equal number of youths. For example, in an agency with 9 caseworkers and 180 youths, each caseworker has 20 youths.

caseworkers were assigned to those with the highest conditional treatment effect[27]. The third scenario represents an intermediate approach, where young people were ranked based on their employment request (yes, then no[28]), predicted employment days in ascending order, and the conditional treatment effect in descending order. This policy aims to align with the ethical principles of public services by giving High employment VA caseworkers priority for young people who seek employment, have a low likelihood of finding employment, and have the potential for a significant positive impact from High VA caseworkers.

Table 2.11 displays the estimates associated with the baseline scenario in column (1), and our simulated policies in columns (2) to (4). In the actual scenario, the payroll tax collection, as measured by the raw wages earned by youths during the employment days due to caseworkers, is primarily negative. The contribution of the approximately 20,000 youths assisted by high employment VA caseworkers allows the public administration to collect 12.1 million euros, compared to the situation where they would have been assisted by middle VA caseworkers. However, the 20,000 youths who were assisted by low VA caseworkers lost an average of 21 employment days, resulting in missing payroll tax collection of approximately 24.1 million euros. A similar pattern emerges in the first scenario, where random allocation is used in the simulated data. Payroll tax levels are higher in this scenario due to the larger number of youths.

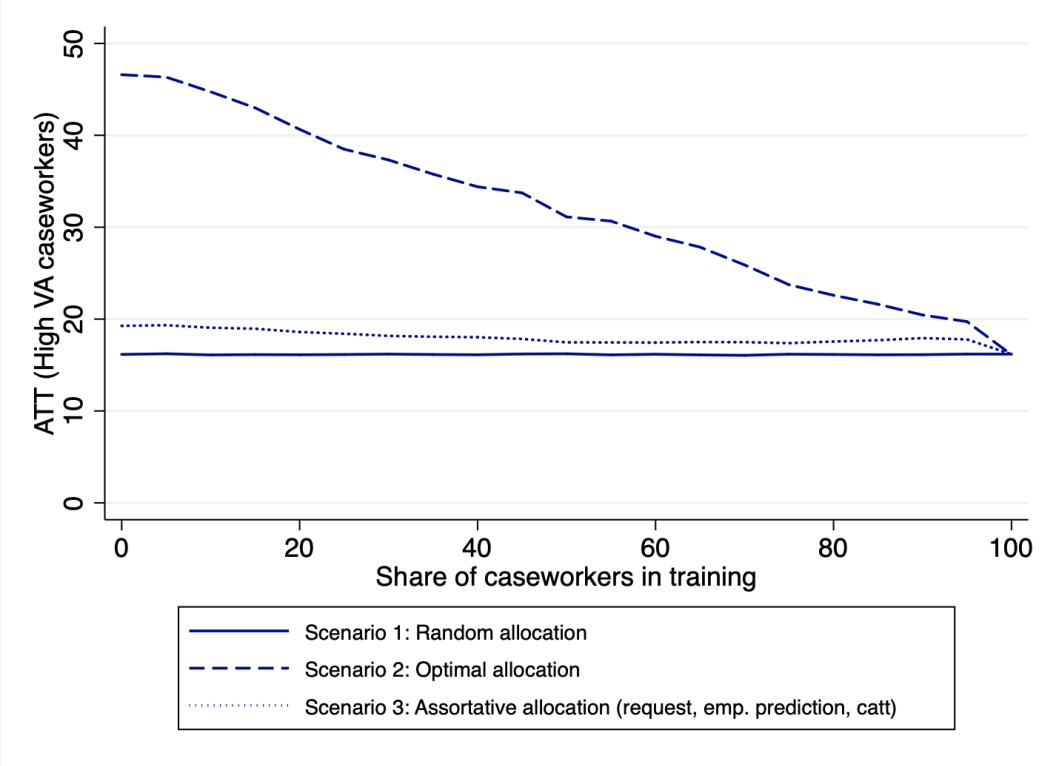
Nonetheless, we observe that payroll tax collection resulting from High employment VA caseworkers could potentially exceed the baseline scenario. In our optimal scenario, which assigns High VA caseworkers to young individuals with the highest conditional treatment effect, the average effect on employment days could increase from +16 days to +46 days within two years, consequently boosting payroll tax collection threefold. The assortative allocation scenario, which assigns the best caseworkers to the most responsive youths, results in an average increase of 19 employment days and an increment in payroll tax collection of about 38.2 million euros. Furthermore, in these two scenarios, the assignment of the best caseworkers to the most responsive youths reduces the negative effect on the loss of employment days compared to the random assignment case.

The logical next question is what can be done with the additional payroll taxes generated by High employment VA caseworkers. In a scenario where it's feasible to trace youths who secure employment due to "missions locales" caseworkers, one potential option would be to utilize the accumulated funds to train Middle and Low

[27]To introduce an element of randomness in the simulations, we grouped young people by terciles based on their conditional treatment effect and randomly ranked individuals within each group. This ensured that the youth with the highest conditional treatment effect was not always assigned first.

[28]We simulated this variable based on the observed variable in our analysis sample.

Figure 2.14: Impact of training Middle and Low VA caseworkers



Note: The 3 scenarios considered for training caseworkers correspond to the scenarios developed in Section 2.6.3 whose mean effects are described in Table 2.11.

VA caseworkers to elevate their performance to the High VA level.

Figure 2.14 demonstrates that the mean effect on employment days decreases when the share of trained caseworkers increases under the optimal and assortative allocation scenarios. This is because the most responsive youths are already assigned to high VA caseworkers. When the other caseworkers join the group of high VA after their training, they are assigned to young individuals who are less responsive to their treatment, thus limiting the effect of caseworker training[29].

2.7 Conclusion

Our study reveals that caseworkers can have a significant impact on the employment prospects of young individuals. When young people, specifically those not in employment, education, or training (NEETs), seek assistance from public agencies for job search support, being assigned to a caseworker whose value-added (VA) score is one standard deviation (SD) above the average in the respective distribution

[29]The mean effect on employment under random assignment is flat because for each additional caseworker who joins the high VA group, they are assigned to young individuals independently of their conditional treatment effect.

results in working 12 days more, equivalent to a 6% increase, over a 2-year period following their initial meeting. In terms of training, young individuals experience an additional 3 days, corresponding to a relative effect of 8.5%. While the relative magnitude of the employment impact aligns with recent studies on the influence of caseworkers [Cederlöf et al., 2021, Dromundo and Haramboure, 2021, Rasmussen, 2021] or that of school or college counselors [Canaan et al., 2022, Mulhern, 2023], it is lower than the effects observed for teachers in the United States based on students' performance in math and reading [Hanushek and Rivkin, 2010, Koedel et al., 2015].

Our dataset enables us to explore the sources of VA scores. We find that caseworkers tend to specialize in either employment or training placement. Caseworkers with high employment VA scores have a modest positive effect on securing permanent contracts, while those with high training VA scores have a greater relative impact on certified training. However, the absolute changes remain relatively small compared to caseworkers with middle or low VA scores. Moreover, we do not identify large differences in practices between caseworkers with higher and lower VA scores. Some subtle distinctions emerge, with caseworkers excelling in employment tending to reduce administrative tasks and outsource job search assistance. Conversely, caseworkers with expertise in training tend to facilitate more collective workshops and make additional training recommendations.

Notably, we observe persistence in the VA scores of caseworkers measured at different points in time. Additionally, there is evidence of competition effects among caseworkers. The presence of caseworkers with consistently high employment VA scores tends to keep their low VA counterparts in the low VA group, particularly in areas with high unemployment. Competition among caseworkers with high employment VA scores also impacts their group persistence, with increased competition leading to lower chances of remaining in the high VA group. Similar effects are observed for caseworkers with training VA expertise, where the persistence of caseworkers with low training VA scores increases in areas with low unemployment.

Finally, policy simulations based on alternative caseworker assignment mechanisms for young individuals yield substantial responses. Causal forest models allow us to estimate how the effect of caseworkers with higher added value varies according to the observable characteristics of young people. This approach enables us to assign caseworkers based on the expected conditional treatment effect to maximize the influence of caseworkers with high employment VA scores. This result is independent of what may be desirable for young individuals based on their specific characteristics. Our scenarios evaluate how the number of employment days would change, assuming that young individuals are indifferent between employment and training. These estimations represent an upper-bound scenario, as we assume that

employment occurs under non-subsidized contracts and do not consider potential general equilibrium effects [Crépon et al., 2013, Gautier et al., 2018]. Under these assumptions, we find an increase in employment days for young individuals and a greater payroll tax contribution resulting from reallocating caseworkers to young individuals. If traceable, this contribution could be utilized to provide additional training to the remaining caseworkers with middle or low employment VA scores, allowing them to join the high employment VA group.

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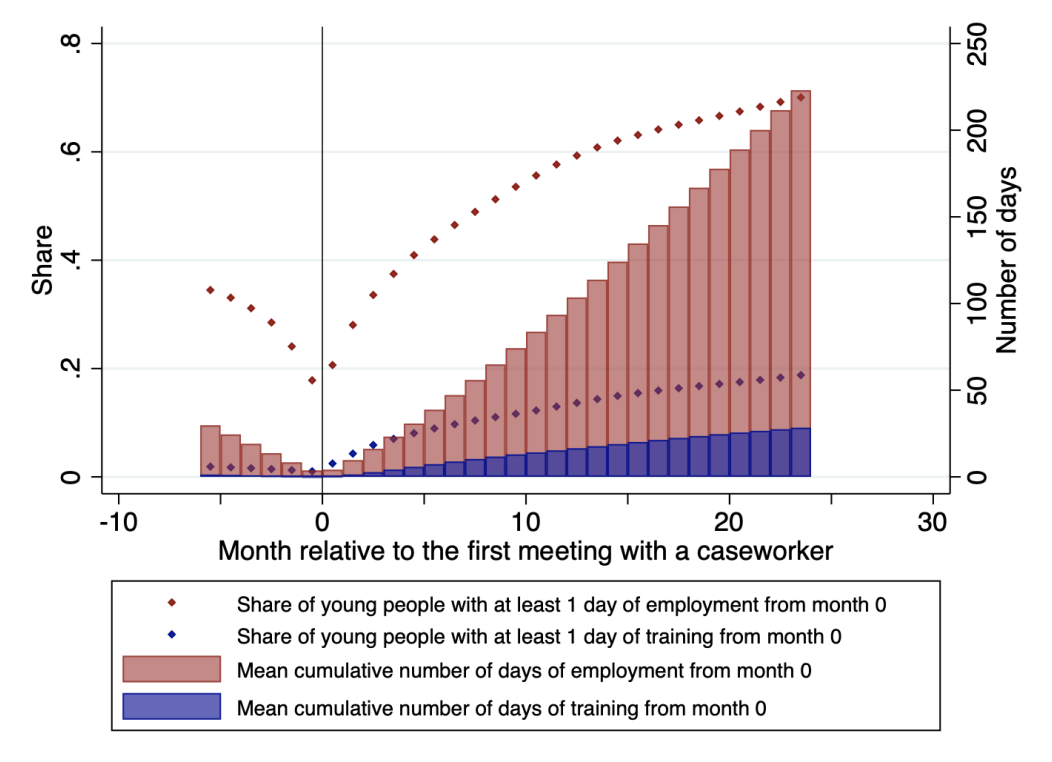
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Appendices

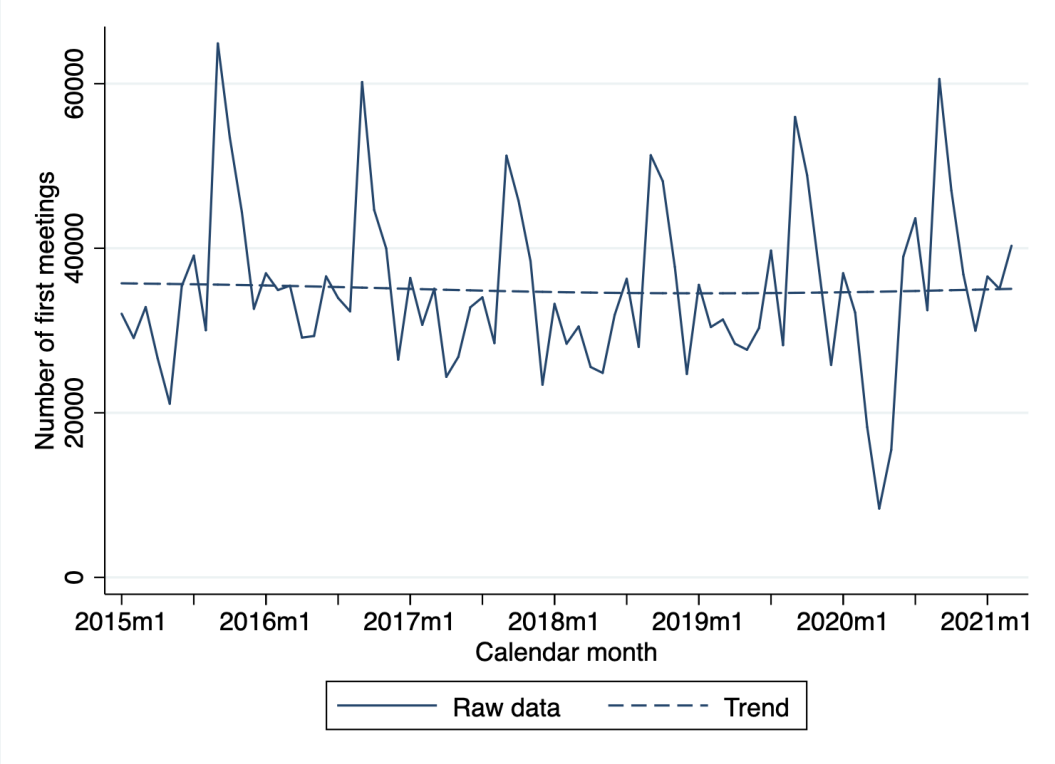
A General figures

Figure 2.15: Labor market situation of young people registered in a youth agency



Note: Month 0 is the month of the first meeting with a caseworker that occurred between July 2017 and December 2018 in any "mission locale" agency in France.
Source: IMILO-FORCE (2021), N=622,057, authors' calculations.

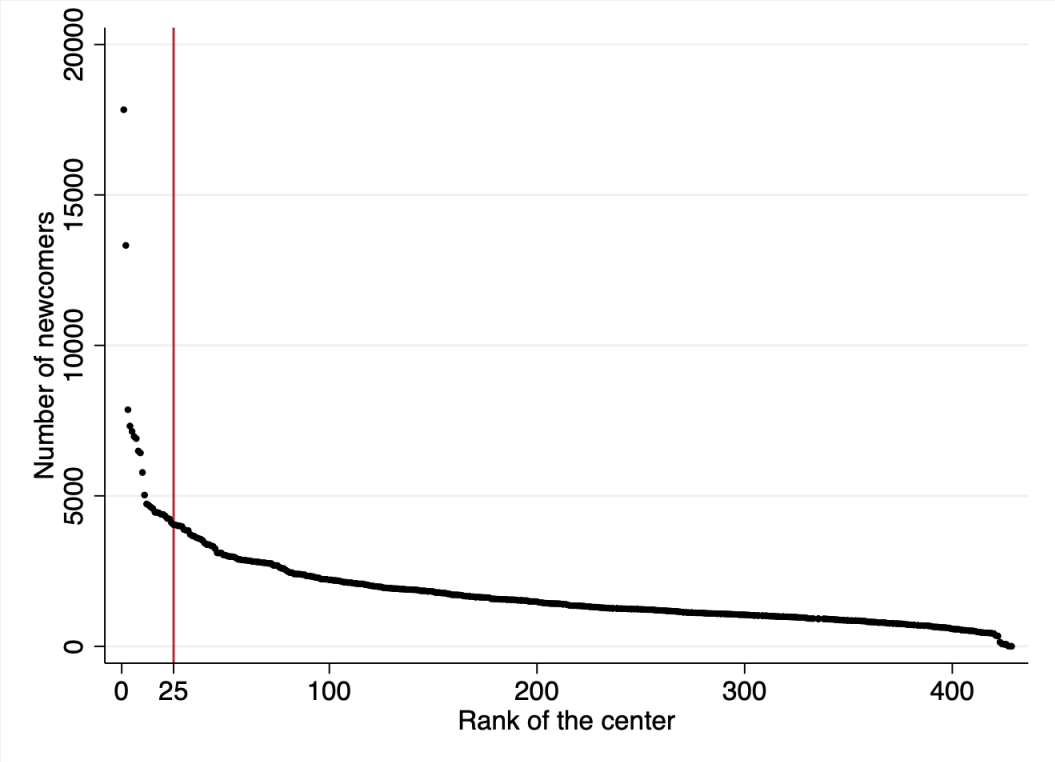
Figure 2.16: Evolution of the number of monthly first meetings from 2015 to 2021



Note: Number of monthly first meetings in "*Missions locales*" agencies.
Source: IMILO (extraction date: October 2021), authors' calculations.

B Sample Construction

Figure 2.17: Ranking of "missions locales" centers according to the number of first meetings between January 2017 and December 2018



Note: Each dot corresponds to a "mission locale" center. Paris is ranked first, Marseille second...
Source: IMILO (extraction date: October 2021), authors' calculations.

Table 2.12: Construction of the samples and related number of observations

Filters	Selected	Paris
	(1)	(2)
0. Initial number of observations	426,372	17,832
1. No missing value in characteristics	374,065	17,681
2. Only regular first meetings	269,091	15,560
3. Young people are NEETs	243,039	13,870
4. Econometric feasibility	60,384	6,862
Min date of the first meeting = 01/01/2017		
Max date of the first meeting = 12/31/2018		
Min number of youths by cell = 4		
Min number of caseworkers by cell = 2		
Min number of youth per caseworker by cell = 2		
Min number of years by caseworker = 2		
Min number of quarters by caseworker = 8		
Min number of quarters per year by caseworker = 4		
Min number of months by caseworker = 8		
Min number of months per year by caseworker = 4		
Min number of caseworkers by agency overall = 3		
Over 01/2017-12/2018		
Number of centers	54	1
Number of agencies	97	6
Number of caseworkers	445	36
Min number of years per caseworker	2	2
Min number of quarters per caseworker	8	8
Min number of months per caseworker	10	15
Min number of months per year per caseworker	5	7
Min number of quarters per year per caseworker	4	4
Min number of 1st meetings per caseworker	34	64
Min size of caseload per caseworker	44	65
Min number of caseworkers per agency	3	4
Per cell (Agency × Month)		
Min number of caseworkers per cell (Agency × Month)	2	9
Min number of youths per caseworker per cell (Agency × Month)	2	2

Note: This table reports the number of observations in our samples after each step of the construction for the econometric analysis and some statistics to check the consistency with the filters. A cell is defined as an agency × quarter intersection. The extended sample includes Paris center. Characteristics include all the characteristics listed in Table 2.13 in Appendix C and in Table 2.1 in Section 2.3.3. Regular first meetings are individual meetings with caseworkers that do job search assistance.

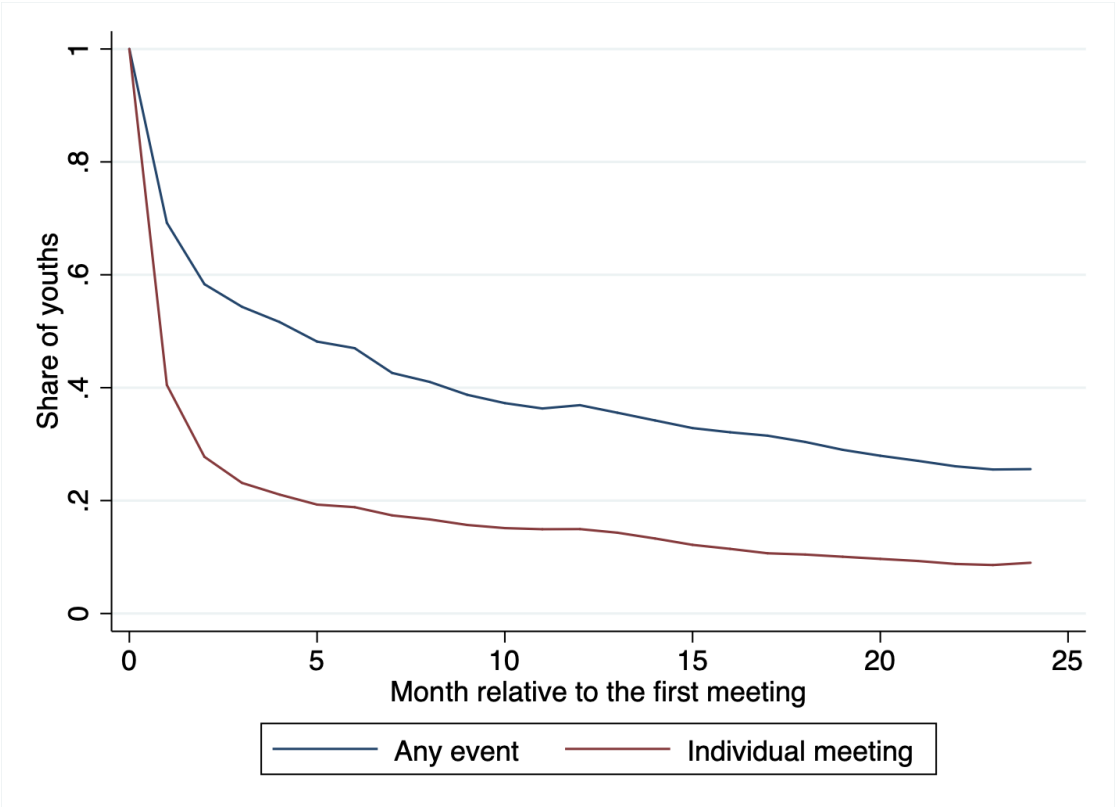
C Additional summary statistics

Table 2.13: Summary statistics on youth characteristics

	Complete sample	Selected sample	
		Full	Analysis
	(1)	(2)	(3)
Male	0.52	0.52	0.53
	(0.50)	(0.50)	(0.50)
Age	19.69	19.80	19.46
	(4.64)	(4.08)	(2.16)
Dropout	0.08	0.09	0.10
	(0.28)	(0.28)	(0.30)
Driving license	0.39	0.36	0.30
	(0.49)	(0.48)	(0.46)
Children	0.07	0.08	0.06
	(0.26)	(0.26)	(0.24)
Foreign nationality	0.12	0.14	0.17
	(0.33)	(0.35)	(0.37)
Northern African origin	0.13	0.14	0.18
	(0.33)	(0.35)	(0.38)
Sub-Saharan African origin	0.14	0.16	0.19
	(0.34)	(0.37)	(0.39)
Tenant	0.21	0.21	0.21
	(0.41)	(0.41)	(0.41)
Directed by relatives	0.55	0.56	0.59
	(0.50)	(0.50)	(0.49)
N	808,222	426,372	60,384

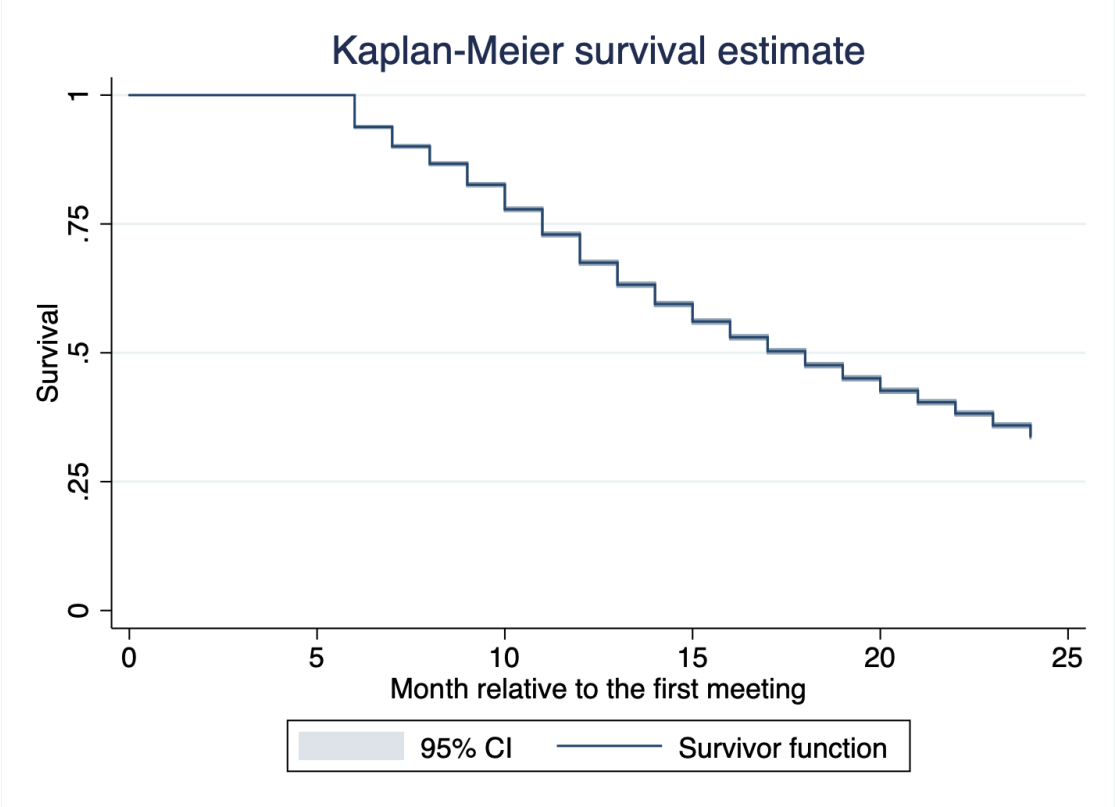
Note: Column (1) shows statistics for the complete sample. Columns (2) and (3) show statistics for the complete selected sample and the restricted selected sample for the econometric analysis as described in Section 2.3.2 respectively.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.18: Share of youths with at least one event per month



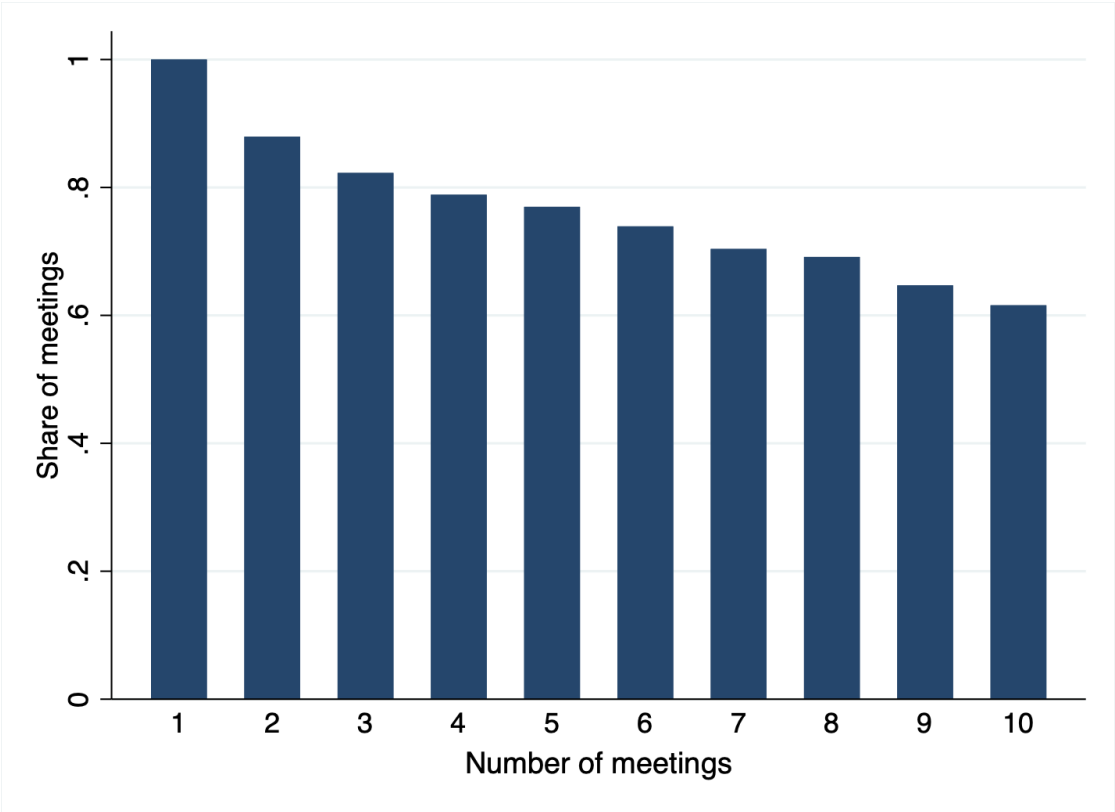
Note: Month 0 is the month of the first meeting with a caseworker that occurred between January 2017 and December 2018.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.19: Survival of youths within an agency



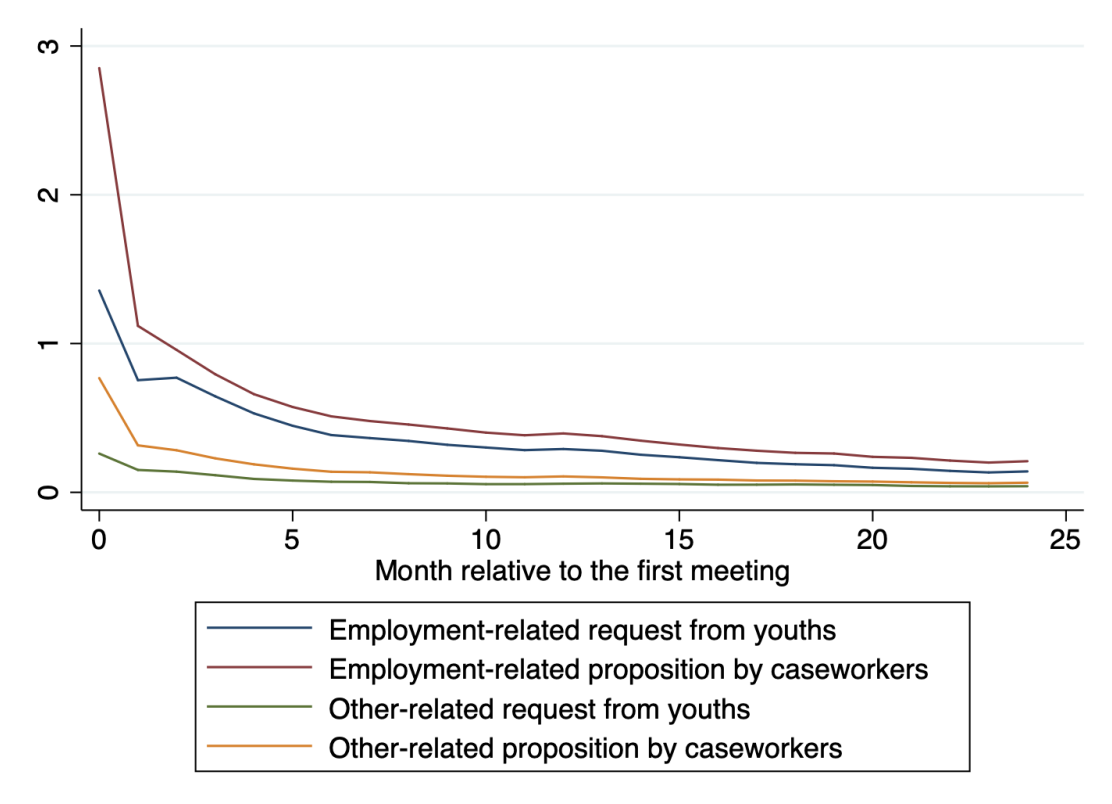
Note: Month 0 is the month of the first meeting with a caseworker that occurred between January 2017 and December 2018.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.20: Share of meetings with the referee caseworker



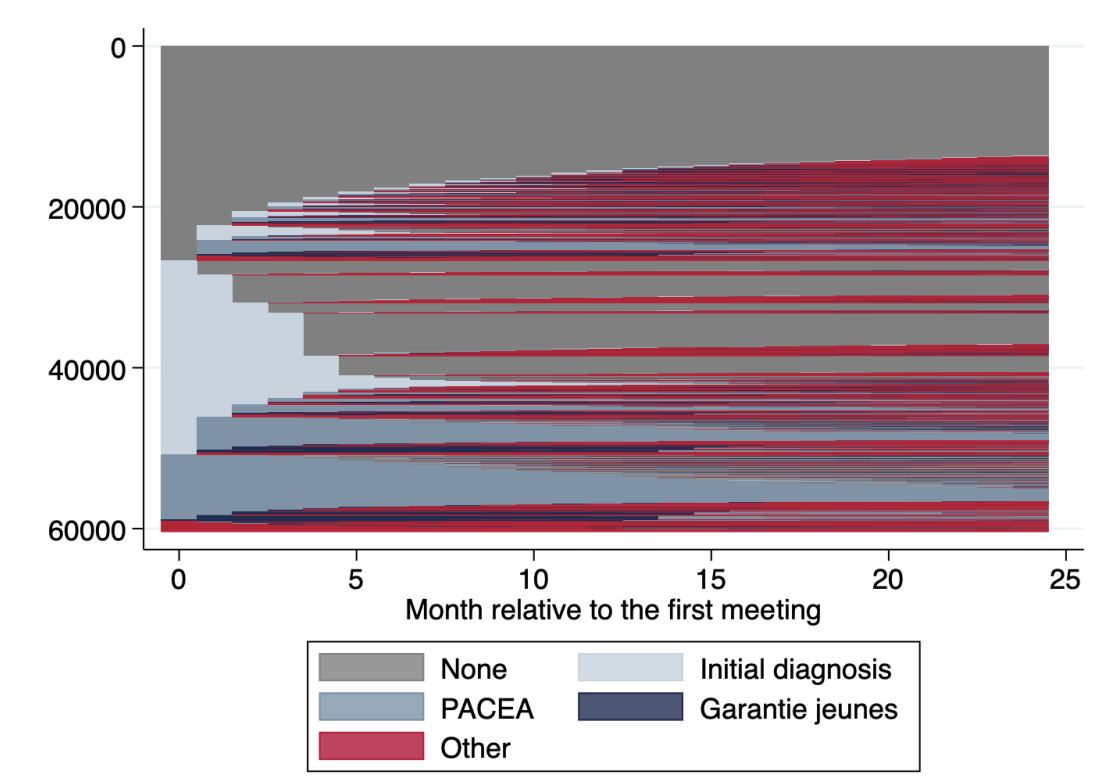
Note: The referee caseworker is the caseworker who did the first individual meeting with a youth for registration.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.21: Requests made by youths and propositions received from caseworkers



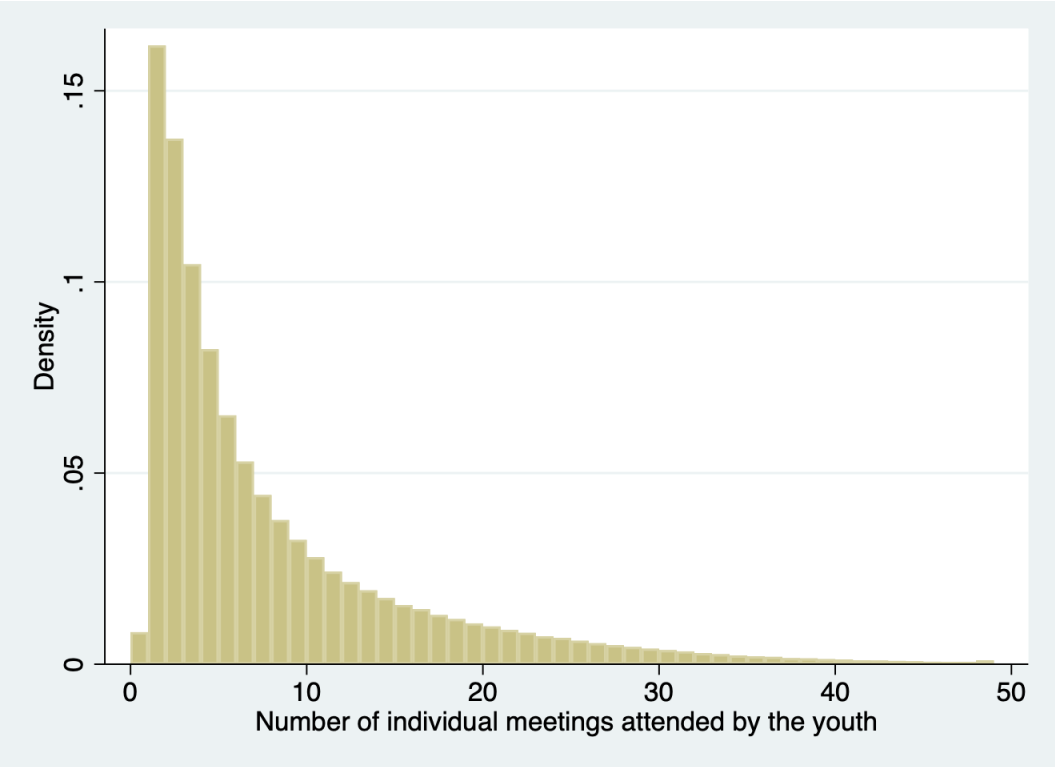
Note: Month 0 is the month of the first meeting with a caseworker that occurred between January 2017 and December 2018.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.22: Sequence index plot of youths within programs



Note: Month 0 is the month of the first meeting with a caseworker that occurred between January 2017 and December 2018.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.23: Total number of meetings with caseworkers



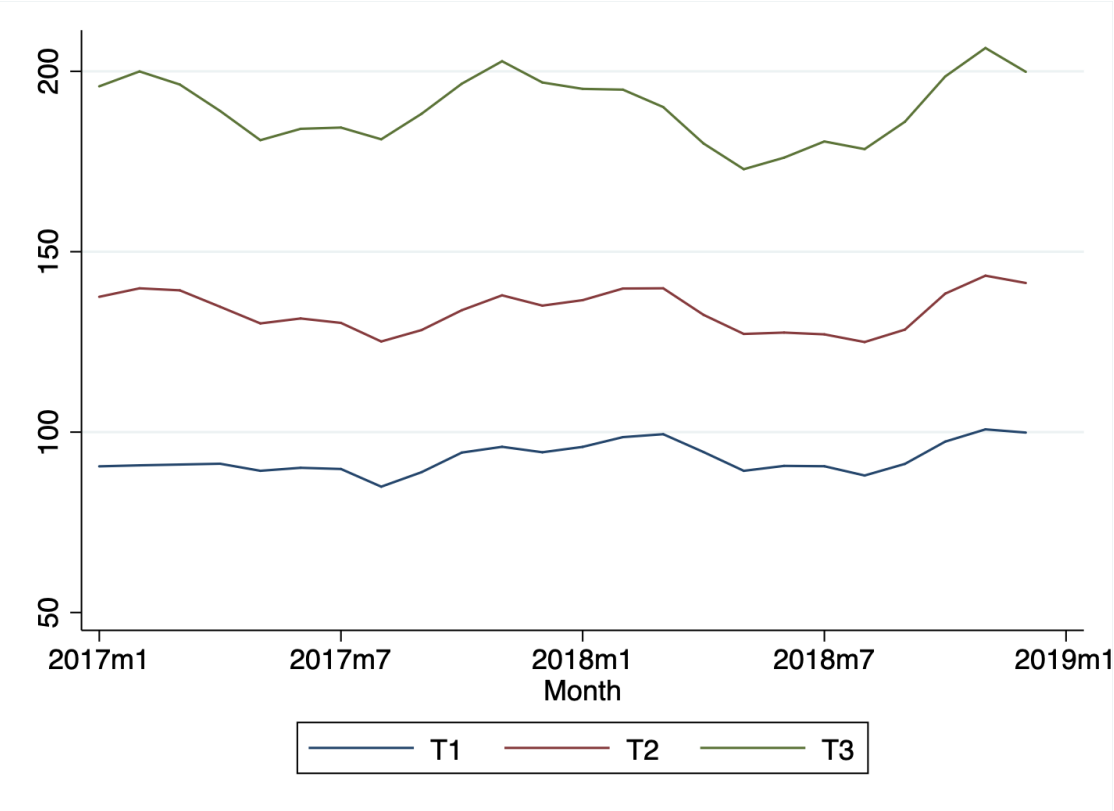
Note: This histogram depicts the distribution of the total number of meetings that young people have with caseworkers over during their follow-up at the Mission Locale.
Source: IMILO (extraction date: October 2021), authors' calculations.

Table 2.14: Summary statistics on caseworker characteristics

	Complete sample	Selected sample	
		Full	Analysis
	(1)	(2)	(3)
Male	0.20	0.22	0.25
	(0.40)	(0.41)	(0.43)
Age	41.47	41.00	44.46
	(11.50)	(11.32)	(8.64)
Northern African origin	0.10	0.11	0.12
	(0.30)	(0.31)	(0.33)
Sub-Saharan African origin	0.07	0.08	0.06
	(0.25)	(0.27)	(0.24)
Caseload at the first meeting	92.59	97.49	141.93
	(66.14)	(72.71)	(48.90)
N	10,321	5,227	445

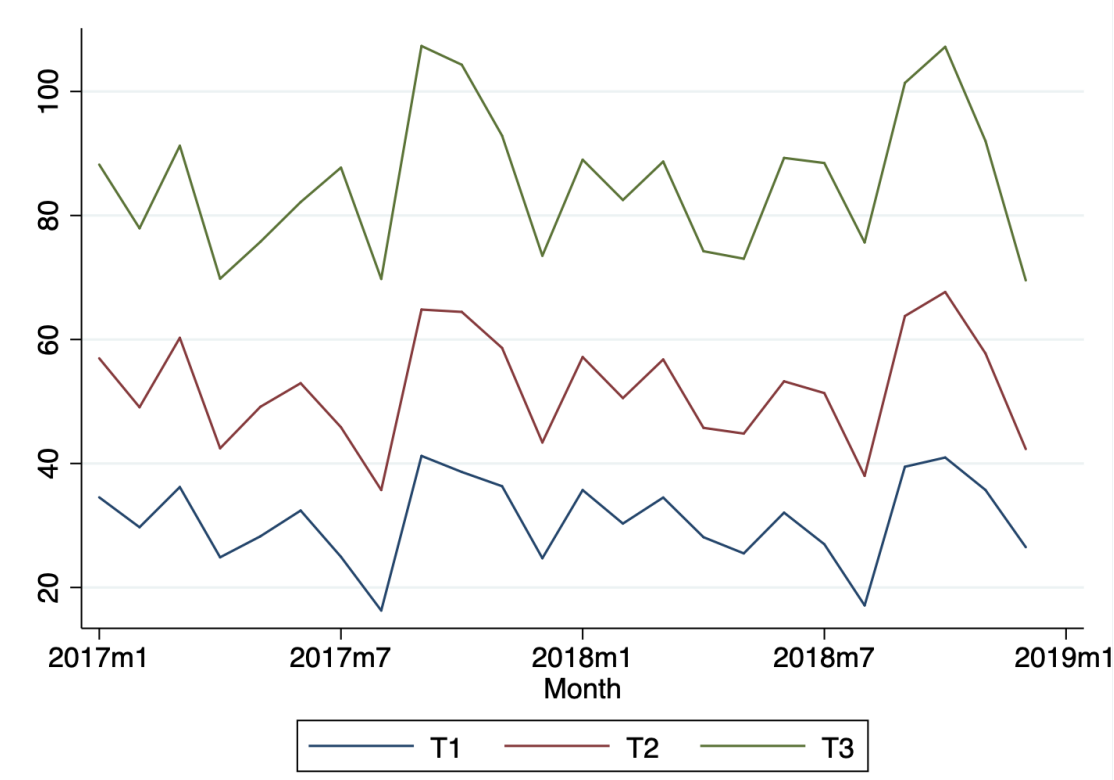
Note: Column (1) shows statistics for the complete sample. Columns (2) and (3) show statistics for the complete selected sample and the restricted selected sample for the econometric analysis as described in Section 2.3.2 respectively.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.24: Evolution of the caseload per caseworker



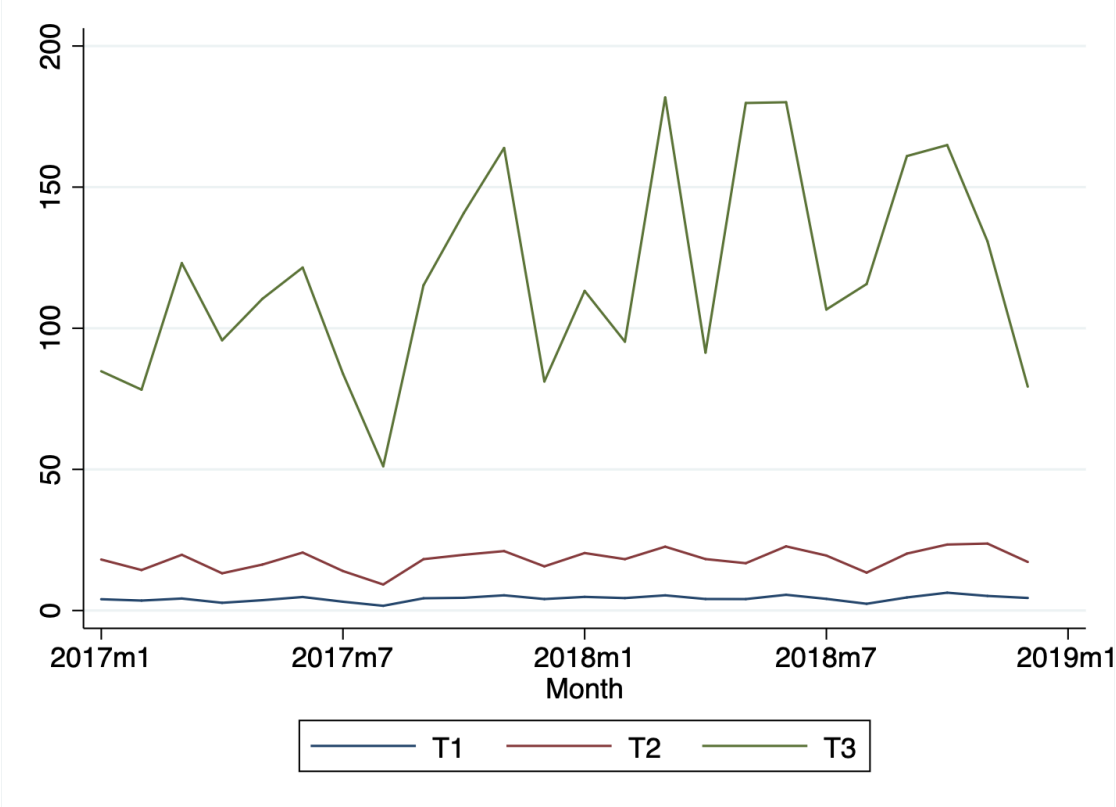
Note: Caseworkers are classified in terciles according to their monthly caseload from January 2017 to December 2018. The caseload of a caseworker is constituted by any youth they had met, at least once during a month for a period of a six months.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.25: Evolution of the number of individual meetings per caseworker



Note: Caseworkers are classified in terciles according to the number of individual meetings they performed each month from January 2017 to December 2018.
Source: IMILO (extraction date: October 2021), authors' calculations.

Figure 2.26: Evolution of the number of digital contacts per caseworker



Note: Caseworkers are classified in terciles according to the number of contacts they had each month from January 2017 to December 2018.
Source: IMILO (extraction date: October 2021), authors' calculations.

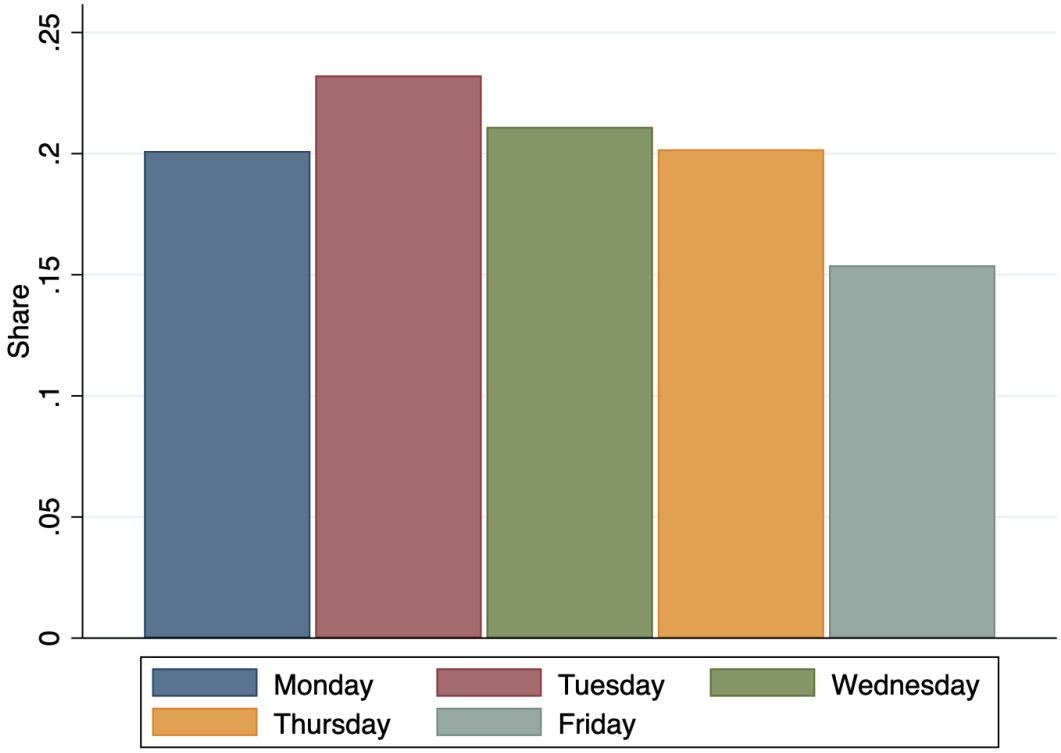
Table 2.15: Summary statistics on labor market characteristics

	Complete sample	Selected sample	
		Full	Analysis
	(1)	(2)	(3)
Unemployment rate	8.85	9.17	9.48
	(2.13)	(2.25)	(2.18)
Labor market tightness	2.45	2.59	2.87
	(1.09)	(1.08)	(1.17)
Share of permanent job vacancies	0.57	0.59	0.61
	(0.08)	(0.07)	(0.09)
Share of part-time job vacancies	0.14	0.14	0.14
	(0.03)	(0.02)	(0.02)
Share of difficult hiring projects	0.49	0.48	0.48
	(0.08)	(0.07)	(0.06)
Share of seasonal hiring projects	0.23	0.21	0.20
	(0.09)	(0.07)	(0.07)
N	287	126	50

Note: Column (1) shows statistics for the complete sample. Columns (2) and (3) show statistics for the complete selected sample and the restricted selected sample for the econometric analysis as described in Section 2.3.2 respectively. Labor market characteristics are the commuting zone level.
Source: IMILO (extraction date: October 2021), authors' calculations.

D Exogeneity of VA estimates

Figure 2.27: Histogram of the days of first meetings



Note: Days of the first meeting include days between January 1st, 2017 and December 31, 2018.
Source: IMILO (extraction date: October 2021), authors' calculations.

Table 2.16: Correlations between the day of the first meeting and youths' characteristics

	Monday	Tuesday	Wednesday	Thursday	Friday
	(1)	(2)	(3)	(4)	(5)
Employment before	-0.0002	-0.0034	-0.0064	0.0082*	0.0019
	(0.0043)	(0.0050)	(0.0044)	(0.0047)	(0.0041)
Training before	0.0195	0.0188	-0.0125	-0.0267*	0.0009
	(0.0147)	(0.0189)	(0.0163)	(0.0142)	(0.0136)
Male	0.0019	-0.0099**	0.0010	0.0055	0.0016
	(0.0037)	(0.0043)	(0.0040)	(0.0038)	(0.0034)
Age	0.0080	-0.0084	0.0052	-0.0084	0.0036
	(0.0058)	(0.0063)	(0.0049)	(0.0064)	(0.0039)
Dropout	0.0115	-0.0069	-0.0074	0.0071	-0.0043
	(0.0080)	(0.0080)	(0.0081)	(0.0071)	(0.0066)
Driving license	0.0013	0.0010	-0.0009	0.0041	-0.0055
	(0.0050)	(0.0049)	(0.0043)	(0.0048)	(0.0042)
Children	0.0053	-0.0041	0.0052	0.0030	-0.0094
	(0.0080)	(0.0077)	(0.0085)	(0.0101)	(0.0071)
Foreign nationality	-0.0054	0.0013	0.0109**	-0.0074	0.0006
	(0.0064)	(0.0048)	(0.0054)	(0.0052)	(0.0039)
Northern African origin	0.0039	0.0086*	-0.0018	-0.0040	-0.0067
	(0.0067)	(0.0050)	(0.0059)	(0.0048)	(0.0042)
Sub-Saharan African origin	0.0025	-0.0001	0.0019	-0.0008	-0.0035
	(0.0068)	(0.0061)	(0.0045)	(0.0050)	(0.0045)
Tenant	0.0057	-0.0048	-0.0092*	0.0046	0.0036
	(0.0054)	(0.0051)	(0.0054)	(0.0057)	(0.0041)
Directed by relatives	-0.0273*	0.0159**	0.0124**	-0.0037	0.0027
	(0.0143)	(0.0071)	(0.0052)	(0.0105)	(0.0059)
Observations	47,431	47,431	47,431	47,431	47,431
R-squared	0.0773	0.0784	0.0589	0.0949	0.0724
Agency x Month FE	Yes	Yes	Yes	Yes	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. 2.3.2.

Table 2.17: Correlations between the day of the first meeting and caseworkers' characteristics

	Monday	Tuesday	Wednesday	Thursday	Friday
	(1)	(2)	(3)	(4)	(5)
Male	-0.0032	-0.0102	0.0245	-0.0167	0.0057
	(0.0201)	(0.0168)	(0.0224)	(0.0152)	(0.0121)
Age	0.0001	-0.0013	-0.0008	0.0018**	0.0002
	(0.0010)	(0.0011)	(0.0014)	(0.0009)	(0.0010)
Northern-African origin	-0.0003	0.0011	0.0131	-0.0103	-0.0036
	(0.0176)	(0.0345)	(0.0429)	(0.0191)	(0.0213)
Sub-Saharan African origin	0.0176	-0.0683***	0.0703**	-0.0020	-0.0176
	(0.0277)	(0.0204)	(0.0301)	(0.0269)	(0.0226)
Caseload	-0.0003	0.0000	0.0006***	-0.0003	-0.0001
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Nb of appointments	-0.0000	0.0005	0.0001	-0.0001	-0.0005
	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0003)
Nb of first meetings	0.0013	-0.0009	-0.0009	-0.0006	0.0011
	(0.0016)	(0.0009)	(0.0007)	(0.0008)	(0.0007)
Nb of individual meetings	0.0000	-0.0001	-0.0002	0.0001	0.0001
	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Nb of collective workshops	-0.0002	0.0002	0.0000	-0.0000	-0.0000
	(0.0002)	(0.0003)	(0.0002)	(0.0004)	(0.0002)
Nb of information sessions	0.0013*	-0.0012**	-0.0007	0.0006	0.0001
	(0.0007)	(0.0005)	(0.0005)	(0.0009)	(0.0005)
Nb of administrative tasks	0.0002	-0.0001	-0.0001	-0.0000	0.0001
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Nb of digital contacts	0.0000	-0.0000	-0.0000	0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Observations	47,431	47,431	47,431	47,431	47,431
R-squared	0.0772	0.0805	0.0631	0.0969	0.0732
Agency x Month FE	Yes	Yes	Yes	Yes	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. 2.3.2.

Table 2.18: Exogeneity of caseworkers VA (Employment)

	Employment	Caseworker EB		
	(1)	(2)	(3)	(4)
Male	0.0007	0.0042	-0.0071	-0.0043
	(0.0084)	(0.0099)	(0.0045)	(0.0044)
Age	0.2748***	0.1105***	0.0196***	0.0090
	(0.0125)	(0.0187)	(0.0068)	(0.0059)
Dropout	-0.2462***	0.0200	0.0033	0.0008
	(0.0149)	(0.0220)	(0.0093)	(0.0096)
Driving license	0.3091***	-0.0794***	0.0091**	0.0095**
	(0.0101)	(0.0182)	(0.0044)	(0.0042)
Children	-0.3847***	-0.1819***	-0.0045	-0.0023
	(0.0169)	(0.0265)	(0.0085)	(0.0084)
Foreign nationality	0.0586***	0.1128***	0.0022	-0.0053
	(0.0130)	(0.0196)	(0.0067)	(0.0065)
Northern-African origin	-0.1457***	-0.0672*	-0.0086	-0.0074
	(0.0114)	(0.0346)	(0.0052)	(0.0052)
Sub-Saharan African origin	0.0117	0.0558**	0.0008	0.0024
	(0.0128)	(0.0222)	(0.0060)	(0.0061)
Rent shelter	0.1304***	0.0690***	0.0012	0.0010
	(0.0112)	(0.0229)	(0.0052)	(0.0051)
Directed by relatives	-0.0231***	-0.1012***	0.0117*	0.0044
	(0.0082)	(0.0174)	(0.0065)	(0.0063)
Observations	60,384	60,207	60,207	57,225
R-squared	0.0901	0.0109	0.7852	0.7466
Agency x Quarter FE	Yes	No	Yes	Yes
95% Winsorization of VA	No	No	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. FE and EB estimates are computed on the number of days of employment 24 months after the first meeting, according to equation (X) and (X) respectively.

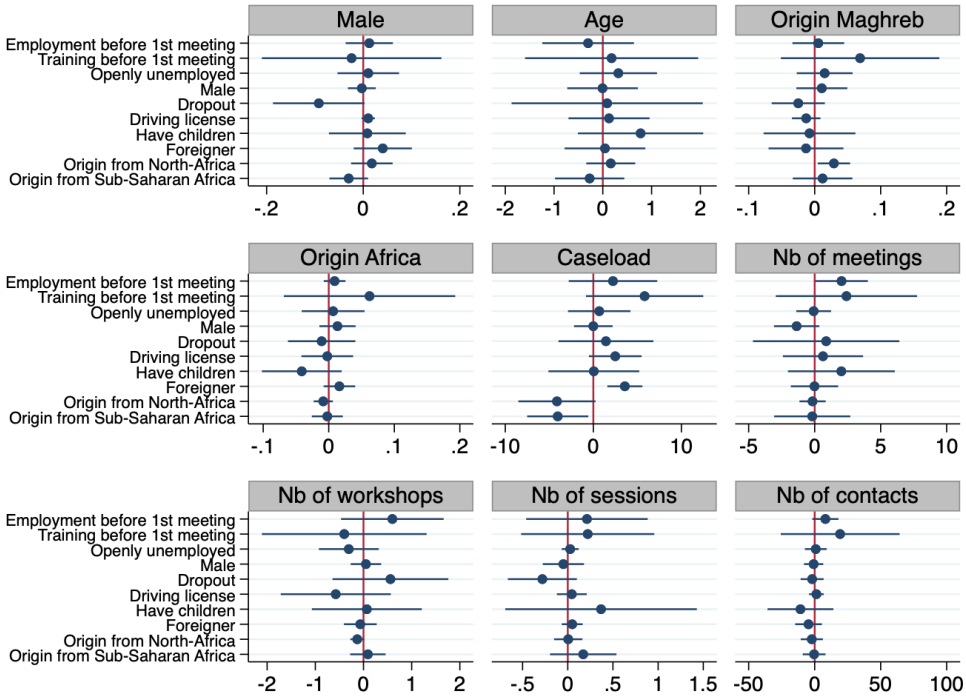
Table 2.19: Exogeneity of caseworkers VA (Training)

	Training	Caseworker EB		
	(1)	(2)	(3)	(4)
Male	-0.0082	-0.0236**	-0.0038	-0.0031
	(0.0087)	(0.0097)	(0.0046)	(0.0045)
Age	0.0201	-0.0068	-0.0025	-0.0020
	(0.0123)	(0.0181)	(0.0058)	(0.0057)
Dropout	0.1168***	0.0554**	0.0017	0.0105
	(0.0202)	(0.0215)	(0.0106)	(0.0096)
Driving license	0.0010	-0.0233	-0.0064	-0.0040
	(0.0100)	(0.0144)	(0.0049)	(0.0047)
Children	0.0024	-0.0083	-0.0038	-0.0030
	(0.0191)	(0.0244)	(0.0103)	(0.0101)
Foreign nationality	0.0309**	0.0138	0.0081	0.0056
	(0.0139)	(0.0169)	(0.0068)	(0.0066)
Northern-African origin	0.0084	0.1229***	-0.0096	-0.0033
	(0.0118)	(0.0275)	(0.0064)	(0.0061)
Sub-Saharan African origin	0.0748***	-0.0316	-0.0010	-0.0015
	(0.0131)	(0.0218)	(0.0061)	(0.0059)
Rent shelter	0.0166	-0.0393	0.0053	0.0066
	(0.0113)	(0.0264)	(0.0055)	(0.0055)
Directed by relatives	-0.0241***	0.0844***	0.0078	0.0091
	(0.0091)	(0.0185)	(0.0093)	(0.0094)
Observations	60,384	60,207	60,207	57,230
R-squared	0.0247	0.0054	0.7545	0.6797
Agency x Quarter FE	Yes	No	Yes	Yes
95% Winsorization of VA	No	No	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency $\times$ month level. FE and EB estimates are computed on the number of days of employment 24 months after the first meeting, according to equation (X) and (X) respectively.

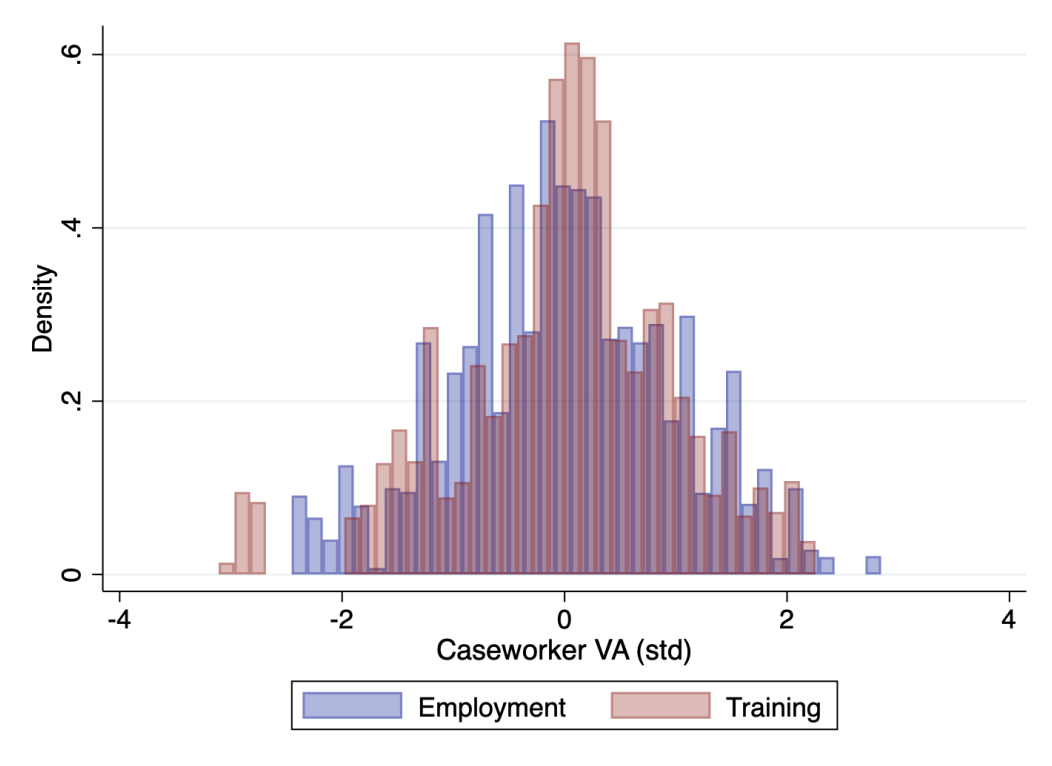
E Main results for Paris agencies

Figure 2.28: Correlation between youths' and caseworkers' characteristics



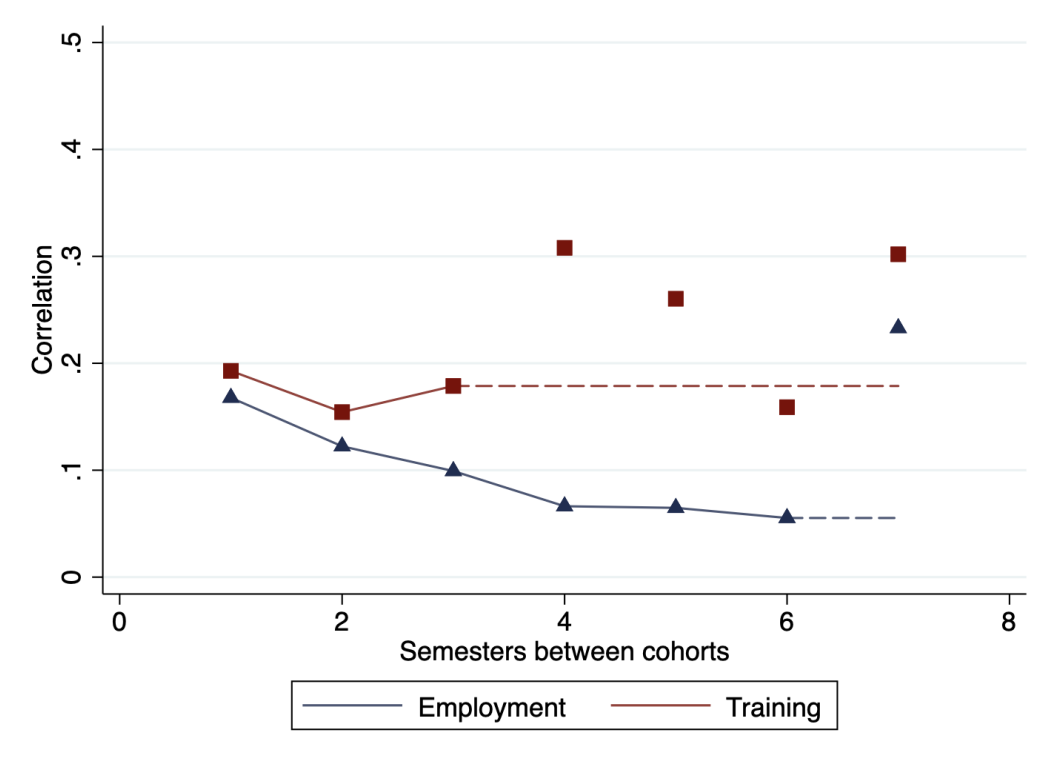
Note: Each square shows the estimates from an OLS regression of a specific caseworker characteristic on all its newcoming youths' characteristics.

Figure 2.29: Distribution of VA estimates



Note: Add description.

Figure 2.30: Auto-correlation vector



Note: Add description.

Table 2.20: Caseworkers impact on youth employment

Employment (std)	Before 1st meeting		After 1st meeting			
	(1)	(2)	(3)	(4)	(5)	(6)
Caseworker VA (std)	0.0109	0.0110	0.0488**	0.0521*	0.0464**	0.0497*
	(0.0192)	(0.0242)	(0.0168)	(0.0238)	(0.0153)	(0.0220)
Employment before 1st meeting					0.2150***	0.2146***
					(0.0190)	(0.0192)
Observations	5,252	4,969	5,252	4,969	5,252	4,969
R-squared	0.0653	0.0681	0.0504	0.0497	0.1000	0.0996
Outcome mean	16.6	16.6	201.6	201.6	201.6	201.6
Outcome standard deviation	39.8	39.8	225.5	225.5	225.5	225.5
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
95% Winsorization of VA	No	Yes	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the caseworker level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation (2.3). Individual characteristics include all the characteristics listed in Table 2.13 in Appendix C.

Table 2.21: Caseworkers impact on youth training

Training (std)	Before 1st meeting		After 1st meeting			
	(1)	(2)	(3)	(4)	(5)	(6)
Caseworker VA (std)	0.0078	-0.0003	0.0781***	0.0689***	0.0779***	0.0689***
	(0.0199)	(0.0215)	(0.0125)	(0.0161)	(0.0125)	(0.0160)
Training before 1st meeting					0.0151	0.0143
					(0.0079)	(0.0085)
Observations	5,252	5,010	5,252	5,010	5,252	5,010
R-squared	0.0117	0.0114	0.0405	0.0409	0.0408	0.0411
Outcome mean	.9	.9	31.6	31.6	31.6	31.6
Outcome standard deviation	9.8	9.8	85.5	85.5	85.5	85.5
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
95% Winsorization of VA	No	Yes	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the caseworker level. Caseworker VA is computed on the number of days of training 24 months after the first meeting according to equation 2.3. Individual characteristics include all the characteristics listed in Table 2.13 in Appendix C.

Table 2.22: Correlation of caseworkers VA with practices

High vs Low VA caseworker	Employment		Training	
	(1)	(2)	(3)	(4)
Nb of individual meetings	0.0021	0.0050	-0.0006	0.0010
	(0.0070)	(0.0074)	(0.0098)	(0.0091)
Nb of collective workshops	-0.0100	-0.0031	0.0015	0.0074
	(0.0422)	(0.0451)	(0.0359)	(0.0319)
Nb of information sessions	0.1057	0.0808	0.2247	0.2050
	(0.0696)	(0.0616)	(0.2711)	(0.2791)
Nb of administrative tasks	-0.0233	-0.0296	0.0008	0.0008
	(0.0296)	(0.0315)	(0.0319)	(0.0317)
Nb of digital contacts	0.0354**	0.0331***	0.0171	0.0126
	(0.0089)	(0.0079)	(0.0145)	(0.0136)
Nb of job propositions	0.0018	0.0018	0.0014	0.0010
	(0.0014)	(0.0014)	(0.0020)	(0.0020)
Nb of training propositions	-0.0162	-0.0166	0.0080	0.0096
	(0.0108)	(0.0122)	(0.0078)	(0.0078)
Nb of project propositions	-0.0062	-0.0077	-0.0148	-0.0135
	(0.0060)	(0.0064)	(0.0096)	(0.0099)
Nb of other propositions	-0.0064	-0.0071	-0.0164*	-0.0161*
	(0.0102)	(0.0106)	(0.0071)	(0.0065)
Observations	4,945	4,586	4,737	4,362
R-squared	0.1245	0.1265	0.0819	0.0718
Individual characteristics	Yes	Yes	Yes	Yes
Agency x Month FE	Yes	Yes	Yes	Yes
95% Winsorization of VA	No	Yes	No	Yes

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. Caseworker VA is computed on employment or training outcome 24 months after the first meeting according to equation 2.3.

F Robustness and sensitivity of VA estimates

Table 2.23: Impact of caseworkers VA in employment across samples

Period of reference	Month			Quarter			Semester		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Presence during a year	1/3	1/2	2/3	1/3	1/2	2/3	1/3	1/2	2/3
Nb of youths	47,383	46,913	39,043	47,297	46,827	38,957	47,209	46,739	38,957
$\bar{Y}$	210.3	210.5	210.8	210.3	210.5	210.8	210.3	210.5	210.8
$\hat{\delta}$	.0411**	.0639***	.0178***	.0481***	.0504***	.0446***	.0329***	.0324***	.0327***
$\hat{\sigma}_\delta$	.0125	.0202	.0035	.0121	.0128	.0151	.0105	.0101	.0120

Note: Estimates are obtained with OLS regressions. Standard errors are clustered at the caseworker level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation (2.3). Individual characteristics include all the characteristics listed in Table 2.13 in Appendix C.

Table 2.24: Impact of caseworkers VA in training across samples

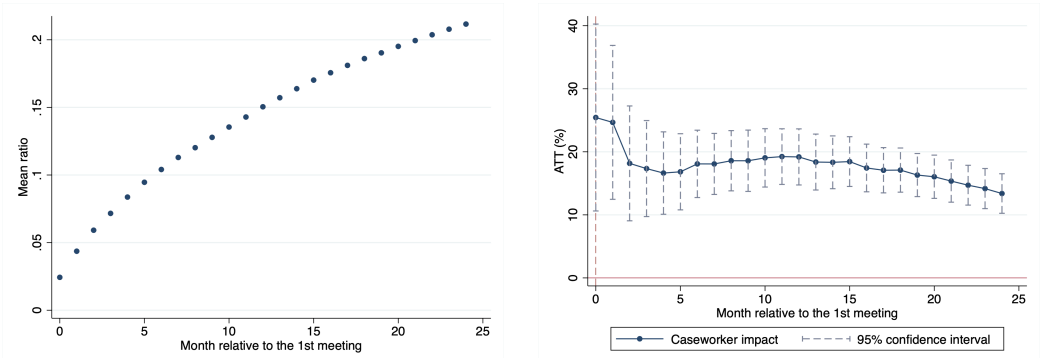
Period of reference	Month			Quarter			Semester		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Presence of caseworkers during a year	1/3	1/2	2/3	1/3	1/2	2/3	1/3	1/2	2/3
Nb of youths	47,383	46,913	39,043	47,297	46,827	38,957	47,209	46,739	38,957
$\bar{Y}$	79.9	80.1	80.3	79.9	80.1	80.3	79.9	80.1	80.3
$\hat{\delta}$	0.0024*	0.0007	0.0689**	0.0383***	0.0367***	0.0359***	0.0317***	0.0316***	0.0303***
$\hat{\sigma}_\delta$	0.0014	0.0033	0.0271	0.0119	0.0113	0.0121	0.0098	0.0102	0.0106

Note: Estimates are obtained with OLS regressions. Standard errors are clustered at the caseworker level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation (2.3). Individual characteristics include all the characteristics listed in Table 2.13 in Appendix C.

G High vs Low VA caseworkers

Figure 2.31: Effect of High vs Low VA caseworker (employment) on permanent employment

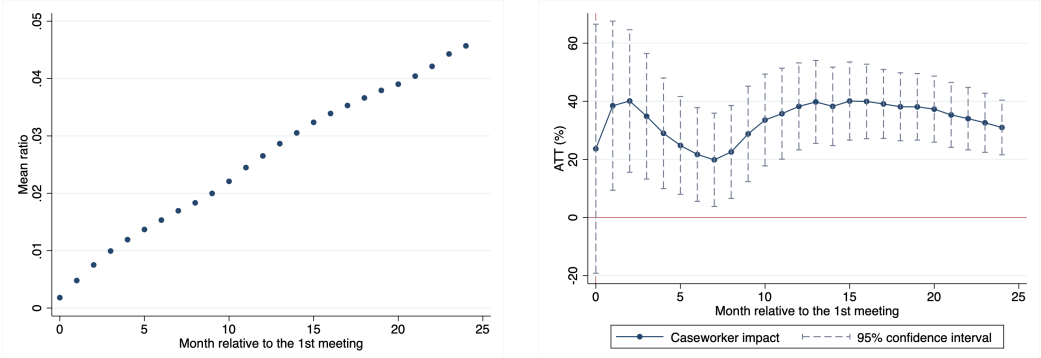
(a) Outcome mean (b) ATT



Note: The estimates are obtained from regressions equivalent to Column (6) in Table 2.2. The outcome is the ratio of the number of days of employment under permanent contract over the number ot days of employment under any contract.

Figure 2.32: Effect of High vs Low VA caseworker (training) on certified training

(a) Outcome mean (b) ATT

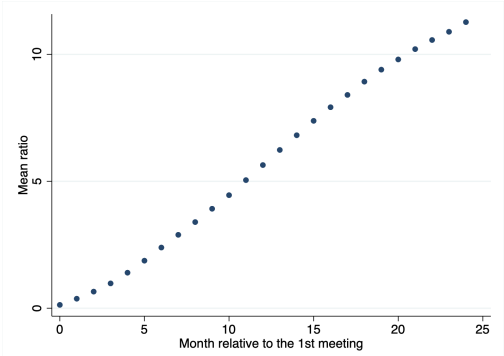


Note: The estimates are obtained from regressions equivalent to Column (6) in Table 2.2. The outcome is the ratio of the number of days of training under long-duration training over the number of days of training under any duration.

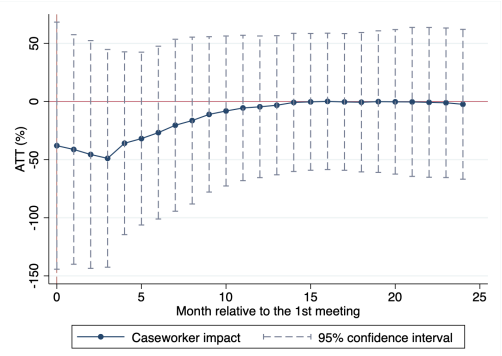
H Impact of training on employment

Figure 2.33: Effect of High vs Low and Middle VA caseworker (training) on certified training

(a) Outcome mean



(b) ATT



Note: The estimates are obtained from regressions equivalent to Column (6) in Table 2.2. The outcome is the ratio of the number of days of employment over the number of days of training.

Table 2.25: Impact of training, because of High VA caseworkers, on employment

1st-stage	Nb of training days	Training (0/1) within		
		2 years	1 year	6 months
	(1)	(2)	(3)	(4)
High Training VA caseworker	11.5720***	0.0474***	0.0383***	0.0253***
	(0.5445)	(0.0034)	(0.0033)	(0.0031)
Outcome mean	27.09	0.19	0.13	0.09
F-stat	32.58	22.84	18.12	9.49
2nd-stage	**Employment within 2 years (0/1)**			
	(1)	(2)	(3)	(4)
Nb of training days	0.0002			
	(0.0004)			
Training (0/1) within 2 years		0.0486		
		(0.0980)		
Training (0/1) within 1 year			0.0602	
			(0.1213)	
Training (0/1) within 6 months				0.0911
				(0.1835)
Observations	60,384	60,384	60,384	60,384
R-squared	0.053	0.053	0.053	0.053
Outcome mean	.71	.71	.71	.71
Individual characteristics	Yes	Yes	Yes	Yes
Agency x Quarter FE	Yes	Yes	Yes	Yes

Note: Estimates are obtained with 2-stage least square regressions. Standard errors are below coefficients in parentheses and clustered at the agency level. High VA caseworkers are caseworkers for whom value-added is in the third tercile of the training VA distribution. Caseworker VA is computed on the number of days of training 24 months after the first meeting according to equation (2.3). *** p<0.01, ** p<0.05, * p<0.1

I Persistence of High VA estimates

Table 2.26: Persistence of High VA and effect of other High VA caseworkers (employment)

High VA after 24 months (T_{jac}^{24m})	All	Unemployment rate		Job tightness	
	(1)	(2)	(3)	(4)	(5)
High VA after 6 months (T_{jac}^{6m})	0.3073***	0.4020***	0.1543	0.1534	0.3801***
	(0.0730)	(0.1258)	(0.1002)	(0.1118)	(0.1207)
Nb of permanent High VA caseworkers (Φ_{ac})	-0.2837***	-0.1920**	-0.5051***	-0.5355***	-0.1851**
	(0.0821)	(0.0874)	(0.1502)	(0.1721)	(0.0753)
Mean caseload ($\bar{C}_{jac}$)	-0.0006	-0.0001	-0.0007	-0.0004	-0.0003
	(0.0007)	(0.0014)	(0.0006)	(0.0006)	(0.0014)
Size of the agency (S_{ac})	0.0120	0.0139	0.0109	0.0108	0.0125
	(0.0088)	(0.0117)	(0.0120)	(0.0144)	(0.0099)
Constant	0.2992	0.0707	0.6358**	0.6362**	0.0640
	(0.2323)	(0.3188)	(0.2408)	(0.2909)	(0.2826)
Observations	484	233	229	234	228
R-squared	0.3150	0.3319	0.3879	0.4217	0.2991
Center FE (Ψ_c)	Yes	Yes	Yes	Yes	Yes
Sample restriction	None	Below median	Above median	Below median	Above median

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation 2.3.

Table 2.27: Persistence of High VA and effect of other High VA caseworkers (training)

High VA after 24 months (T_{jac}^{24m})	All	Unemployment rate		Job tightness	
	(1)	(2)	(3)	(4)	(5)
High VA after 6 months (T_{jac}^{6m})	0.2993***	0.3405***	0.0834	0.1467*	0.2576**
	(0.0700)	(0.1204)	(0.0807)	(0.0839)	(0.1093)
Nb of still High VA caseworkers (Φ_{ac})	-0.2928***	-0.2790**	-0.5456***	-0.5383***	-0.2708**
	(0.0763)	(0.1195)	(0.1024)	(0.0996)	(0.1030)
Mean caseload ($\bar{C}_{jac}$)	-0.0003	-0.0002	0.0000	0.0003	-0.0003
	(0.0007)	(0.0010)	(0.0011)	(0.0010)	(0.0009)
Size of the agency ($\S_{ac}$)	0.0160**	0.0284*	0.0068	0.0033	0.0256*
	(0.0080)	(0.0161)	(0.0076)	(0.0077)	(0.0129)
Constant	0.0274	-0.2295	0.4377**	0.4721**	-0.1260
	(0.2009)	(0.2871)	(0.1770)	(0.1910)	(0.1848)
Observations	474	227	225	231	221
R-squared	0.3028	0.3483	0.3373	0.4146	0.2489
Center FE (Ψ_c)	Yes	Yes	Yes	Yes	Yes
Sample restriction	None	Below median	Above median	Below median	Above median

Note: Estimates are obtained with OLS regressions. Standard errors are below coefficients in parentheses and clustered at the agency × month level. Caseworker VA is computed on the number of days of employment 24 months after the first meeting according to equation 2.3.

J Machine learning algorithms for policy simulations

Employment and wage predictions

Table 2.28: Predictions of employment days using machine learning algorithms

Employment days	Observed	Predicted				
		Linear Regression	Decision Tree	Random Forest	Support Vector	Neural Network
	(1)	(2)	(3)	(4)	(5)	(6)
Mean	212.32	298.33	216.54	214.5	157.86	213.77
Standard deviation	225.03	197.28	140.91	112.32	98.14	80.91
R^2 in training sample	–	.1135	.2884	.2760	.0019	.1399
R^2 in test sample	–	.1188	.0220	.0496	.0128	.1302
Observations	60,384	148,801	148,801	148,801	148,801	148,801

Note: Employment days shown in column (1) are the total number of employment days of young people from our analysis sample, purged from caseworker effects. The associated statistics are almost equivalent to those for young people who are assigned to a middle employment VA caseworker which serves as a control group as defined in Section 2.6.1. Employment days predicted via machine learning algorithms in columns (2) to (6) are based on a training sample which is a random draw of 80% of the observations from the analysis sample.

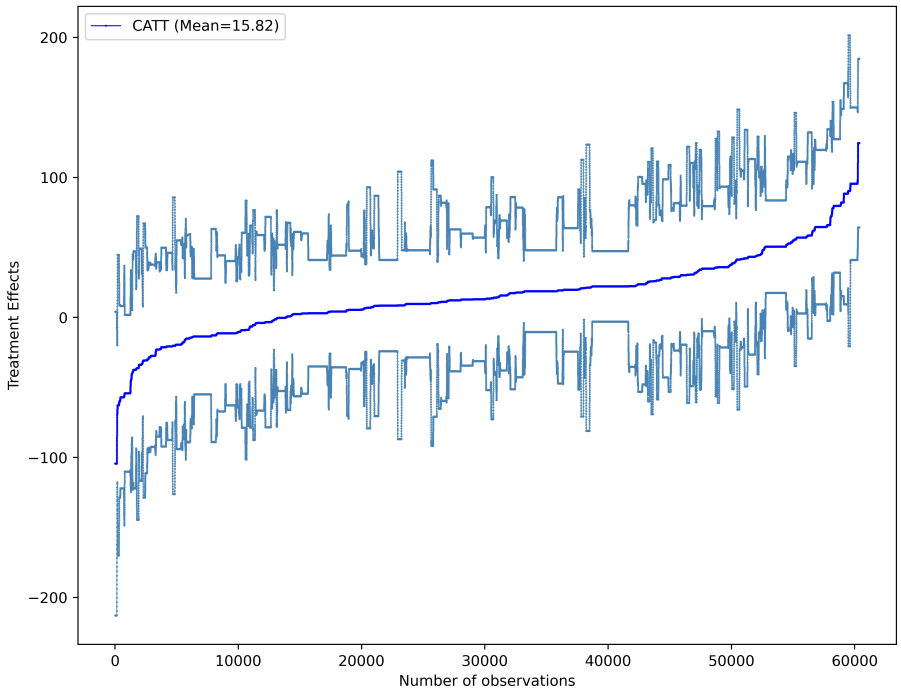
Table 2.29: Predictions of net wages using machine learning algorithms

Net wages	Linear Regression	Decision Tree	Random Forest	Support Vector	Neural Network
	(1)	(2)	(3)	(4)	(5)
Mean	1265.37	1273.17	1273.51	1163.62	1253.9
Standard deviation	322.44	329.91	329.26	259.83	282.75
R^2 in training sample	.1123	.1194	.1193	.0681	.1061
R^2 in test sample	.1284	.1288	.1285	.0557	.1175
Mean in analysis sample	1245.26	1250.57	1252.14	1150.29	1237.13
Observations	148,801	148,801	148,801	148,801	148,801

Note: Net wages shown in column (1) are the predicted net wages of young people from our analysis sample, based on their observable characteristics, using the pooled French labor force surveys from 2017 to 2020. Net wages predicted via machine learning algorithms in columns (2) to (6) are also based on the pooled French labor force surveys from 2017 to 2020. Net wages are expressed in euros base 2020 assuming that all wages between 2017 and 2019 grew at the same annual rate than the net minimum income.

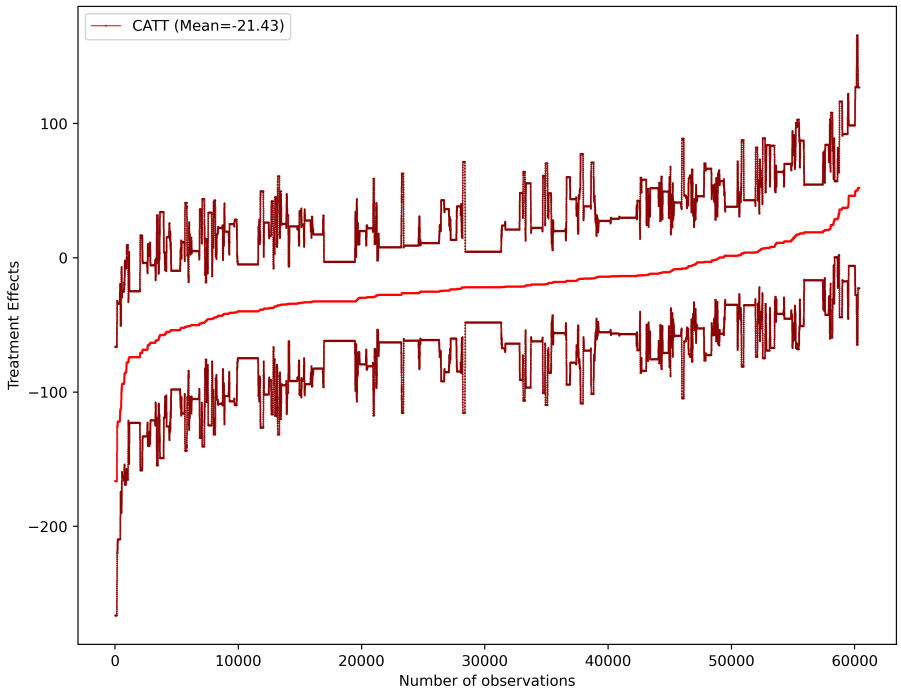
Causal forest and conditional treatment effects

Figure 2.34: Conditional average treatment effect on the treated of High employment VA caseworkers



Note: Conditional average treatment effect on the treated (CATT) is obtained with a causal forest model, grouping 1,000 trees with a minimum of 10 observations per leaf, using an honest approach.

Figure 2.35: Conditional average treatment effect on the treated of Low employment VA caseworkers



Note: Conditional average treatment effect on the treated (CATT) is obtained with a causal forest model, grouping 1,000 trees with a minimum of 10 observations per leaf, using an honest approach.

Chapter 3

The Benefits of Early Work Experience for School Dropouts: Evidence from a Field Experiment

This chapter is based on a joint work with Jérémy Hervelin (CY Cergy Paris University, THEMA)

Abstract: Employers do not necessarily view dropouts as completely lost causes, as they represent a heterogeneous group of young individuals actively seeking opportunities in the labor market. This raises the question of whether job-related work experience, especially in tight occupations, is sufficient to increase the employment opportunities of school dropouts in comparison with graduates. To answer this question, we conducted a correspondence study with 6,400 applications sent to real job offers and 8,200 unsolicited applications between January and December 2018. We found that dropouts have lower probabilities of being interviewed for job positions than graduates, but this gap is narrowed for dropouts who acquired job-related work experience and even more when skill certification is associated. Furthermore, sending unsolicited applications close the interview rate gap in slack labor markets for dropouts with job-related work experience.

3.1 Introduction

Young people who leave high school before graduation, known as school dropouts, are over-represented among those who are not in employment, education, or training (NEET). In 2021, the dropout rate among NEETs was 37% in the US, compared to 16% for high school graduates, and 36.9% in the

OECD countries, compared to 14.3% respectively.[1]

However, employers do not necessarily view dropouts as individuals who are completely adrift. A survey conducted in 2019 among French employers hiring young people in low-skilled occupations indicates that employers generally do not perceive dropouts negatively [Beffa and Broc, 2019]. They generally do not see them as individuals who have experienced significant misfortune or who have not put in enough effort at school or lack fundamental skills such as writing, listening, or mathematics. Instead, two-thirds of employers view dropouts as young people seeking opportunities in the labor market and pursuing specific work experiences. Beyond the various factors that contribute to their decision to leave school, such as inadequate guidance after middle school [Hoxby and Avery, 2013, Arcidiacono et al., 2016, Guyon and Huillery, 2021], or an unsuitable specialization during their education [Kirkeboen et al., 2016, Clemens et al., 2021, Dahl et al., 2022], dropouts represent a heterogeneous group with diverse individual and professional experiences.

In line with this perspective, dropouts face higher unemployment and inactivity probabilities and lower employment probabilities compared to other job seekers who possess adequate skills, particularly recent graduates [Rumberger and Lamb, 2003, Clemens et al., 2021]. This effect may be further exacerbated by the duration dependence of unemployment [Kroft et al., 2013, Eriksson and Rooth, 2014, Farber et al., 2019]. Yet, the situation may be less severe in occupations where employers struggle to find suitable candidates.

This study examines whether an early job-related work experience is sufficient to enhance the employment opportunities of school dropouts relative to graduates. Given the widespread existence of public programs designed to support disadvantaged youth, it is of interest to determine the effectiveness of such initiatives as potential stepping stones for early school leavers. Notably, our study focuses on recruiters' perceptions as one of the key mechanisms through which work experience can shape the employment trajectories of school dropouts.

To investigate this question, we conducted a correspondence study field experiment in France. Between January and July 2018, we submitted approximately 6,400 applications from fictitious candidates to actual job vacancies posted online. Additionally, between October and December 2018, we sent around 8,200 unsolicited applications to other firms employing individuals in the same occupations. The fictitious applicants were 18-year-old young men who had either graduated from a two-year vocational education program relevant to the targeted occupations or left school after middle school. While all dropout applicants were inactive during the first year after disengaging from school (at ages 16 to 17), their labor market experi-

[1] See the National Center for Education Statistics and Giret and Jongbloed [2021] for more details.

ence during the second year (ages 17 to 18) was randomly assigned. One group of dropouts remained inactive during this second year, while the other group acquired one year of work experience in the targeted occupations.

Our experiment focused on two occupations, namely cook and mason. These occupations were selected based on their notable prevalence among vocational students and school dropouts, not only in France but also in other European countries. Moreover, these occupations are among those with a substantial percentage of workers who receive wage subsidies. In our experiment, our fictitious applicants possessed job-related experience gained through a national program called "Emploi d'Avenir", which operated between 2012 and 2018. This program targeted young people aged 16 to 25 without diplomas or with low levels of qualifications. Firms and non-market structures were eligible for wage subsidies if they hired young people through this program.[2] They also had the option to provide vocational training to young people, leading to a diploma equivalent to the one obtained through vocational education. The certification status among dropouts with work experience was also randomized.

Both graduates and dropouts with job-related experience listed identical skills on their CVs, encompassing a blend of both soft and hard skills. This emphasis on diverse skills underscores their ability to function proficiently not only within their respective occupations but also in diverse professional environments. In their cover letters, applicants elaborated on the acquisition of these skills, whether through educational pursuits or job-related experience. Conversely, dropouts who remained inactive solely presented soft skills on their CVs and conveyed their enthusiasm to acquire hard skills through their first professional experience in their cover letters.

The results indicate that the average job interview rate for school graduates is approximately 25%, while it is 18% for school dropouts. However, there is heterogeneity among dropouts, with those who signal work experience having an interview rate of 20%, compared to only 8% for those who remained inactive. Furthermore, the interview rate significantly differs among dropouts with work experience, with a rate of 19% when there is no certification and 22% when dropouts signal a diploma. Consequently, the chances of being invited for an interview decrease by 67% when young people drop out of high school, remain inactive, and compete for jobs against peers who have completed vocational education. This gap narrows when young people gain job-related experience. Specifically, one year of job-related work experience reduces the interview penalty from 67% to 25%. Moreover, this penalty decreases to 12% when dropouts acquire a diploma to certify their skills.

[2]Regarding potential negative stigma associated with this subsidized employment program, Cahuc et al. [2019] found no difference from an equivalent non-subsidized work experience.

These differences in interview rates are not uniform across all settings. We utilize various indicators to assess opportunities in different local labor markets, such as the unemployment rate, the level of occupational tightness, and the proportion of dropouts with work experience among all workers. Our findings indicate that the situation for dropouts deteriorates in tight labor markets but improves in slack labor markets. This suggests that employers face more challenges in hiring qualified and experienced workers in tight labor markets, whereas the larger applicant pool in slack labor markets allows them to screen more new employees.

This explanation is particularly relevant for occupations where employers struggle to find suitable candidates and where skills can be acquired through on-the-job training. The link between acquired skills and integration into vocational occupations is indeed significant. By comparing the career paths of young novice workers in low-skilled occupations, some of whom have received more or less direct training in the field while others have not, Lainé [2005] demonstrates that the latter group is less likely to remain employed, more frequently engaged in precarious contracts, and experiences lower wage progression. Similarly, Girsberger et al. [2022] reveal that low-ability workers who have acquired specific manual skills in occupations similar to cook and mason have witnessed a 10% increase in their wages and a 50% reduction in their unemployment risk.

Lastly, we find that school dropouts with job-related work experience who send unsolicited applications to firms, without responding to specific job vacancies, achieve interview rates on par with graduates in slack labor markets. This positive effect applies regardless of whether skills are certified. However, we do not observe any effect of sending unsolicited applications in tight labor markets. We discuss the policy implications of our findings in the conclusion.

Our paper contributes to the extensive literature on audit studies that examine how employers respond to different characteristics of job candidates. Specifically, we contribute to the literature focusing on the role of applicants' work history. Previous studies have investigated the effect of current unemployment duration on callback probabilities, with mixed results. Oberholzer-Gee [2008] provided evidence from Switzerland suggesting that long unemployment spells (more than 2 years) negatively affect callback rates, while shorter spells (up to one year) tend to have a positive effect compared to individuals currently employed. Kroft et al. [2013], studying low- to middle-skill jobs in the United States, found a sharp decline in callback rates in the first year of unemployment spells, with duration dependence being stronger in tight labor markets. Eriksson and Rooth [2014] and Fremigacci et al. [2016] found similar results for Sweden and France, respectively, with a negative effect of unemployment spells lasting at least 9 to 12 months. However, Farber

et al. [2016] and Nunley et al. [2017] did not find a significant relationship between unemployment duration and callback rates when considering spells up to one year. Farber et al. [2019] provided evidence consistent with negative duration dependence of callback rates, but only after one year of unemployment.

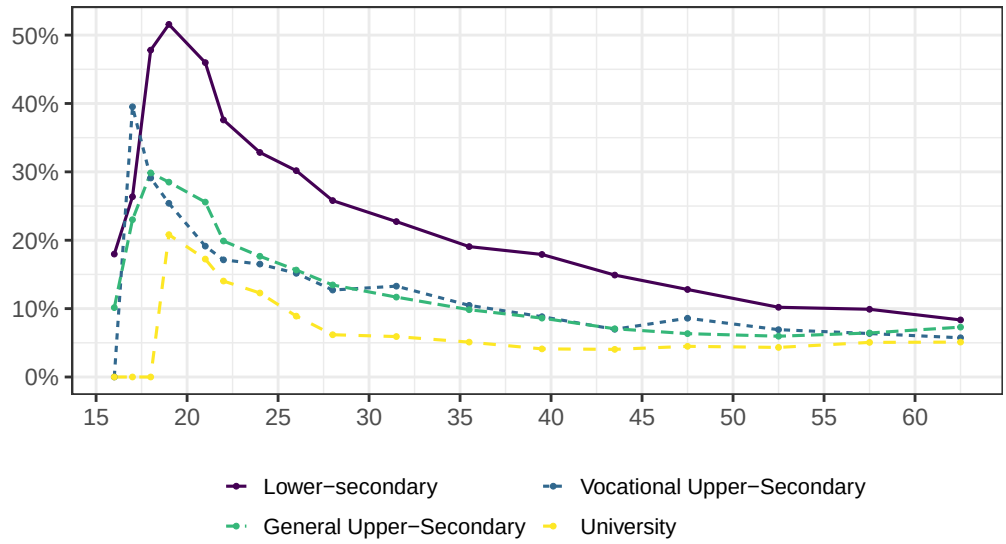
To date, there have been limited audit studies that have specifically examined the effect of work experience on recruiter interest using a dedicated experimental design. Eriksson and Rooth [2014] found that additional years of job-related experience increase callback rates, particularly for relatively high-skilled jobs. Baert et al. [2016], studying the effect of student work experience, found no significant effect on callback probabilities for former university students in Belgium. Conversely, Nunley et al. [2016] found that internship experience increased the interview rate by 14% for recent college graduates in the United States. Cahuc et al. [2019] analyzed the effect of formal job experience compared to part-time volunteering activities on recruiter callbacks for school dropouts aged 24 in France and found that only job-related experience with training leading to a national diploma had a positive effect on callback rates. This effect was primarily observed in areas with lower local unemployment rates. Our study adds to this existing body of evidence by investigating the effect of job-related work experience from a subsidized wage program targeting high school dropouts, a demographic crucial to public policy. Moreover, we concentrate on the marginal effect of having no experience versus some (i.e., one year), which is distinct from Eriksson and Rooth [2014] where all applicants have at least one year of experience. Contrary to the findings of Cahuc et al. [2019], our study uncovers that job-related experience has a substantial impact on callback rates, even without certification. This effect diminishes with local labor market tightness, but remains both economically and statistically significant across a range of specifications.

The remainder of the paper is structured as follows: Section 3.2 provides background information on the economic context. Section 3.3 describes the experimental design and its scope. Section 3.4 presents the main results and their sensitivity to various factors. Finally, Section 3.5 discusses the policy implications of our findings and concludes.

3.2 Background

Over the past 40 years, youth unemployment has been one of the most prominent features of the French labor market. In 2019, the unemployment rate for individuals aged between 15 and 24 years old was approximately 20% in France, systematically two to three times higher than the rest of the population. For comparison, the aver-

Figure 3.1: Evolution of the unemployment rate over the life cycle in France (2013Q1-2018Q1)



Note: This figure shows the evolution of the unemployment rate over the life cycle for individuals with lower-secondary educational level in purple, for individuals with 2- or 3-year vocational upper-secondary educational level in blue, with general upper-secondary education level in green, and with a university degree in yellow. Source: French Labor Force Survey, authors' calculations.

age youth unemployment rates were approximately 15% and 12% in EU and OECD countries, respectively, and about 8% in the United States. These figures rank France among the developed countries with the highest youth unemployment rate. However, the overall youth unemployment rate masks varied situations depending on the level of education.

Each year, around 820,000 pupils aged about six enter elementary schools in France. They learn the basics in several fields such as French, mathematics, history, geography, etc., until the 9th grade in middle school. At this stage, approximately 75% of pupils are about 15 years old, and 25% are aged 16 due to repeated years. Since the legal age to leave the education system in France is 16 years old, this is also the moment when some youth may decide to leave the education system altogether and enter the labor market. However, the majority of them continue their studies after middle school. In the last decade, around 63% of a generation pursued a three-year general upper-secondary diploma with the objective of pursuing higher education, while 27% ended up in a vocational upper-secondary track lasting 2 or 3 years. Consequently, about 10% of young people left school before completing their curricula.

School dropouts are more likely to come from a disadvantaged social background and to experience difficult situations during their education [Bouhia et al., 2011, Cayouette-Remblière and de Saint Pol, 2013]. Unsurprisingly, these difficulties have

Table 3.1: Correlations between labor market situations and being a dropout

OLS Estimates	Employment		Unemployment		Active Program	
	(1)	(2)	(3)	(4)	(5)	(6)
Dropout	-0.0394***	-0.0285**	0.0835***	0.0803***	0.0027**	0.0024*
	(0.0133)	(0.0134)	(0.0081)	(0.0080)	(0.0013)	(0.0014)
Constant	0.2340***	0.2338***	0.0221***	0.0222***	0.0016***	0.0016***
	(0.0023)	(0.0023)	(0.0007)	(0.0006)	(0.0001)	(0.0001)
Observations	487,041	487,041	487,041	487,041	487,041	487,041
R-squared	0.0002	0.0657	0.0078	0.0341	0.0001	0.0048
Control Variables	No	Yes	No	Yes	No	Yes

Note: This table reports OLS estimates, where the dependent variable is the number of days experienced in employment by school dropouts in columns (1) and (2); in open unemployment in columns (3) and (4); and, in an active program (vocational training or subsidized job) in columns (5) and (6). ≪Dropout≫ is a dummy variable equal to one if the individual was recognized as a school dropout by legal authorities at some date. Unreported control variables in columns (2), (4), and (6) include dummies for gender, year of birth, department of birth, school level, literacy level, department of residency, and month-year fixed effects. Robust standard errors are clustered at the individual level and reported below coefficients in parentheses. *** significant at 1 percent, ** significant at 5 percent, * significant at 10 percent.
Source: sample from TRAJAM (2015), authors' calculations.

often detrimental effects on their subsequent situations on the labor market. Figure 3.1 shows the evolution of the unemployment rate over the life cycle depending on the educational attainment. High school dropouts clearly stand out with a probability of being unemployed rising from 18% at age 16 to 50% at age 20 and steadily declining thereafter. The pattern is similar for the other educational groups (from vocational education to university degree) but at lower levels.

Due to the difficulties faced by dropouts in finding employment, successive French governments have implemented active labor market policies aimed at promoting vocational training and wage subsidies. Vocational training is provided by training centers and it is delivered through classroom and on-the-job experiences, or a combination of the two [Guillon, 2019]. Additionally, between 2012 and 2018, the *Emploi d'Avenir* program (EAV) was launched to reduce labor costs for companies when hiring unskilled youths aged between 16 and 25. The program subsidized between 35% to 75% of the gross minimum wage and allowed for a contract duration of one year, cumulative for up to three years. One notable innovation of the EAV compared to other subsidized contracts was that employers were required to provide complementary training. During the program's six-year operation, more than 360,000 contracts were signed and 60% of the young beneficiaries enrolled in a certified training, making it the primary subsidized contract program for youths [Borel and Pichavent, 2021].

Table 3.1 displays the correlations between school dropout status and various

labor market outcomes, including employment, open unemployment, and participation in an active program. The table shows that dropouts have a lower probability of being employed than non-dropouts in the following two years, and a higher probability of being in open unemployment or participating in an active program. To gain a deeper understanding of the specific challenges faced by school dropouts in successfully transitioning into employment, it is crucial to gather insights into employers' hiring preferences. In the upcoming section, we will introduce a correspondence study designed to provide valuable information in this regard.

3.3 Field experiment

This section describes the experimental protocol of our audit study: the treatment groups, the targeted occupations, the profiles of the applicants, the process of application, the collection of data, and the limits inherent to correspondence studies.

3.3.1 Treatment groups

Our study focuses on a sample of unemployed youths who completed lower-secondary school at age 16 in June 2015. However, these individuals faced diverse situations over the next two years, as depicted in Figure 3.2. Some of them continued with a two-year vocational education path, either school-based or workplace-based, and graduated in June 2017.[3] This group serves as the control group for young people facing academic difficulties since it follows the typical path within the education system. We refer to this first group as "Graduates".

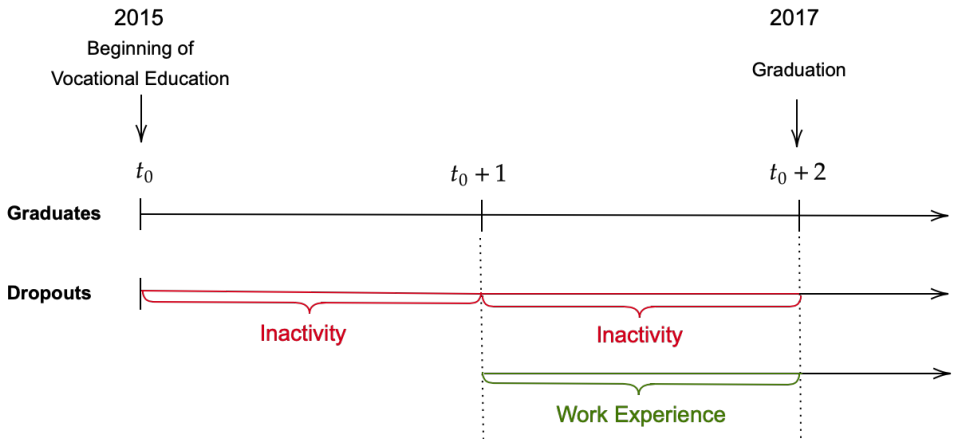
In contrast, the second group comprises a heterogeneous population of school dropouts who were subjected to different treatments. In the first year following their dropout, all participants were largely inactive, with two one-month temporary contracts unrelated to the occupations targeted in the audit study, and ten months of non-employment.[4] This period of inactivity serves as an indication to employers that the applicant has dropped out of school, as it signifies that the individual possesses a lower-secondary school diploma but lacks any upper-secondary diploma, which is the first qualification for entering the labor market.

Following the initial year of inactivity, school dropouts exhibit diverse situations

[3] Cahuc and Hervelin [2020] demonstrate that young people who compete for jobs after school-based education have equivalent chances to be called back for a job interview as those with the same diploma after workplace-based education but who were not retained by their training firm, which is the case here.

[4] Based on the French labor force survey (2013-2018Q1), more than 90% of young dropouts aged 17/18 worked for less than two months in the year prior to the survey.

Figure 3.2: Diagram of profiles in the experiment



Note: This figure shows the different profiles created in the field experiment described in Section 3.3.1. The control group is constituted of young people who stayed mainly *inactive* for two years after dropping out of school, i.e. they had two one-month temporary contracts with no link to the occupations targeted in the audit study and ten months of non-employment each year. The treatment group is constituted by young people who were *inactive* for one year and had job-related experience during the second year via subsidized contract.

in the subsequent year, as depicted in Figure 3.2. A first group of young dropouts continues to experience inactivity, with the same two one-month temporary contracts unrelated to the targeted occupations and ten months of non-employment. This group is referred to as the "Dropout with inactivity" group. In contrast, a second group of young dropouts gains professional experience in targeted occupations through the EAV subsidized wage program. This experience may be accompanied by the acquisition of a national diploma through complementary training, and it could have been attained in either the private or public sectors. This group is referred to as the "Dropout with job-related work experience".

To ensure that each group shared the same duration of current unemployment before applying to job vacancies, we stopped the last line of resumes in June 2017 for all applicants at age 18.

3.3.2 The occupations

The selection of occupations was guided by several criteria, including membership in different industries, the existence of a state certification for the required diploma, a sufficient proportion of school dropouts, presence in both market and non-market sectors, and a sufficient number of employees under subsidized contracts. These criteria led to a pool of five possible occupations, but due to financial and organizational constraints and a sufficient number of job vacancies, we ultimately chose the

occupations of cook and mason.[5]

Figures 3.3 and 3.4 in Appendix A provide evidence on the relevance of the chosen occupations with respect to the population of school dropouts, using pooled labor force surveys from 2011-2016.[6] These figures show how frequently cooks and masons are employed among dropouts in France and other European countries, with the former representing 1.5% and the latter 5% of employed dropouts in France. Furthermore, Figures 3.3 and 3.4 demonstrate that both occupations have relatively high dropout rates compared to other occupations in France and across most European countries.

The study also considers the prevalence of subsidized contracts in the chosen occupations. Figure 3.5 in Appendix A shows that EAV contracts (the one-year contract program mentioned in the motivation letter of the dropouts) are not uncommon in both occupations. Cooks have a lower proportion of EAV contracts than masons, but both occupations have higher EAV contract rates than most other occupations. Additionally, Figure 3.6 in Appendix A shows that the selected occupations are more common among subsidized contracts than most other occupations. In particular, among people aged 15 to 24 employed in EAV contract, about 4.4% are working as mason and 1.4% as cook. It makes these two occupations more frequent than 97% and 85% of other occupations among EAV contracts respectively.

3.3.3 The applicants

The profiles of applicants for the two occupations, namely cooks and masons, were designed based on descriptive evidence presented in Table 3.7 in Appendix A. The applicants were young men, aged 18 at the beginning of the application process and 19 at the end. We chose to focus on men as the majority of cooks and masons are male. Their first names, Théo and Alexis, were respectively the 9th and 13th most popular first names in 1999, as per the *Fichiers des prénoms* (INSEE). We selected these names randomly from the top 20 list. Similarly, we selected the surnames, Petit and Dubois, which were ranked 6th and 7th, respectively, according to the *Fichier patronymique* (INSEE). Thus, our applicants, *Alexis Dubois* and *Théo Petit*, have common names that prevent them from being easily identifiable on the internet.

The applicants' addresses were chosen to be located in the center of the adminis-

[5]We consulted various sources, including the French Labor Force Survey (Enquête emploi, INSEE), the Répertoire National des Certifications Professionelles (RNCP), and the Pôle emploi job search database to verify the existence of national diplomas and assess the number of job offers.

[6]See Section II of Cahuc and Hervelin [2020] for details and descriptive statistics about these two occupations in the French vocational education system. To sum-up, these two occupations include 20% to 25% of all vocational students and apprentices and they exhibit similar school-to-work transitions with all the others students and apprentices at the same educational level.

trative capital of the department where the job was posted. This was done to ensure that the candidates lived close enough to their potential future job and to prevent any geographic discrimination.[7] Since the diploma is national, there is no information about the specific training center, and we did not provide the name of the vocational school or the apprenticeship center where the diploma was obtained. We also did not provide the address of firms where graduates or dropouts worked during their job-related professional experience to avoid detection of fictitious applications.[8] The firms we selected were large and well-known in the private sector, and their addresses are not usually mentioned on résumés.[9] The job-related experience could also be acquired in the public sector, specifically in public administrations.

Furthermore, our applicants possess a mix of soft and hard skills. The occupation-related skills for cooks include developing and maintaining kitchen facilities, maintaining hygiene rules HACCP, and respecting recipes. For masons, these skills include plumbing and leveling, setting up frame elements, manufacturing and installing casings, and pouring concrete and breeze block installation. Firm-related skills are the same regardless of the position and are signaled by either "good team integration" or "good relational skill", depending on the layout.[10] For dropouts with "inactivity", who have never worked as a cook or mason, there were no occupation-related skills.

We did not emphasize the fact that the applicants dropped out of school after middle school, as advised by caseworkers helping this population. Recruiters deduce this information by examining the education block in the résumé, as explained in Section 3.3.1. We only mentioned in their cover letters that dropouts with job-related work experience did it through a subsidized contract (EAV). We note that Cahuc et al. [2019] found no stigma effect in signaling professional experience via the EAV contract, in comparison with a similar experience via a classical temporary non-subsidized contract for young people. Finally, we pre-submitted our fictitious applications in cook and mason positions to actual workers and caseworkers who confirmed their credibility.

[7] We collected and verified the addresses using *Google Maps*.

[8] This prevents us to capture any positive effect related to a potential recommendation by the previous employer, whether through a recommendation letter or simply by answering to the recruiter solicitation. From this perspective, our estimates of the effect of work experience on the chances to get an interview may be interpreted as a lower bound of the actual effect of work experience. We come back on this point in Section 3.3.6 which discusses the research limitations of our experiment.

[9] We made sure by looking at their website that these firms were present in all the French departments and that they were used to hiring young people as temporary workers, with certification or not, among others.

[10] We obtained these skills from the *fiches métiers Pôle emploi*. More details here for cooks and here for mason.

3.3.4 The applications

The application package consisted of a résumé, cover letter, and a brief email message. To avoid detection by potential employers and ensure that callbacks were not dependent on any specific presentation style, we created two templates each for résumés and cover letters. Examples of the résumés and cover letters can be found in Appendix B. The résumé templates were based on various samples obtained from the online library of the job center, a youth center sample, and Google searches.[11]

We mainly identified job offers for both occupations using the French job center online platform, with some additional help from private job search websites when the number of offers available on the job center platform was too low. Only job offers that allowed direct contact with the recruiter via email were considered. Offers issued by temporary work agencies or other intermediaries were excluded, and we did not send any applications to about 2/3 of mason job vacancies managed by temporary work agencies during our experiment. To improve the validity of our results, we then used the spontaneous applications channel, as discussed in Section 3.4.5, where we sent over 8,000 unsolicited applications.

For each job vacancy that met our criteria, we sent one pair of applications selected at random from the pool of {"*Graduate*"; "*Dropout with inactivity*"} and the pool {"*Dropout with job-related work experience*"} with different applicant names and profiles, and different layout types for the cover letters.[12] We also ensured that the same recruiter could not be contacted more than once, even if they posted different job positions in different French areas throughout the entire experiment period.

3.3.5 Data collection

The study involved sending a total of 6,453 applications from 22 January 2018 to 13 July 2018. This sample size was chosen based on power calculations, which are shown in Figure 3.9 in Appendix C, to detect a minimum effect of ± 2.5 percentage points between the baseline callback rate of applicants with "inactivity" and that of applicants with "graduate", at a 5% significance level and power of 80%.

Replies from recruiters were collected up to the last recorded phone call and email message on 10 October 2018. A reply from a recruiter stating that they did not select the application for the job vacancy is classified as a negative callback, along with the absence of callback. Positive callbacks are categorized as either "callbacks"

[11]The online library of the job center, namely *Pôle emploi CVthèque*, is available to help recruiters in selecting different available profiles. More details at here.

[12]The pool of applications that included "*Dropout with job-related work experience*" could be either certified or not, in the market sector or in the non-market sector. As a consequence, there was only one application of "dropout with job-related work experience" in each pair to avoid firm detection.

or "interviews". Callbacks include requests for further information and interview propositions. Requests for further information could be vague or ask for more precise information about the candidate's training or experience. Callbacks are interpreted as positive since they are likely motivated by the recruiter's potential interest in the candidate. Interviews are callbacks that offer a job interview proposition only. When recruiters provided a positive answer to an application, an email was sent back to thank them and inform them that the applicant declined the proposition.

We conducted randomization tests to ensure that job offer characteristics were not correlated with the different applicant profiles, and Table 3.8 in Appendix D presents the differences in means. The results indicate that the randomization was successful, and there were no significant correlations between any job characteristic and a specific applicant profile. This ensures that our treatment estimates are unbiased. Additionally, Table 3.9 in Appendix D reveals that none of the randomized application characteristics, including the name, layout, and order of sending, had any impact on the callback rate of the applicants, leaving aside any potential template bias [Lahey and Beasley, 2009].

3.3.6 Research limitations

We will now discuss the limitations of our experiment that should be considered when interpreting our results.

Firstly, similar to any correspondence study, our experiment only captures the impact of job-related work experience on the initial stage of the hiring process, specifically the likelihood of receiving a job interview invitation. We are unable to determine the subsequent probability of success, which is largely influenced by the skills and information employers value during the interview. However, we presume that, on average, applicants without prior work experience do not perform better than those with relevant job-related work experience during job interviews. Therefore, if anything, the effect of work experience on hiring outcomes should be even more significant after the initial callback stage.

Secondly, our study does not encompass all the effects that job-related work experience may have on the job application itself. For instance, work experience can provide opportunities to build professional networks and obtain recommendations from previous employers. Although we cannot quantify the prevalence or magnitude of these network effects, they are likely to have an overall positive impact on employment probability. Previous research has shown that letters of recommendation increase employment and earnings among youths who participated in summer employment programs. Hence, our estimates may be interpreted as a conservative

lower bound of the effect of previous work experience in a real-life setting. Furthermore, we do not consider aspects related to on-the-job search or wage bargaining. Nevertheless, our analysis focuses on young individuals who face challenging transitions from school to work, and on-the-job search and wage bargaining may be secondary concerns for this population, whose priority is to find employment.

Thirdly, our experiment targets firms that recruit through job postings. We do not include firms that utilize alternative channels, such as private networks, which could be significant for low-skilled positions. We attempted to mitigate this limitation by conducting a second audit study involving unsolicited applications to firms in late 2018. The findings from this second application channel support our primary results obtained from job offers.

Lastly, our results may be specific to the two occupations we analyzed (cooks and masons) and other features of our experimental design (time period, applicant profiles, etc.). However, our study demonstrates that the effect of job-related work experience is largely consistent across the two occupations and different specifications that we tested. Additionally, our results complement those of Cahuc et al. [2019], who focused on two different occupations (gardeners and receptionists) during another period (2016) and found some evidence supporting the value of formal work experience for school dropouts. We yet recognize that future research and dedicated experiments are necessary to understand why recruiters may assign different levels of importance to previous work experience across various occupations.

3.4 Results

This section presents the results of the field experiment, first by considering dropouts as one homogeneous group, and then by decomposing it into different relevant subgroups across alternative specifications.

3.4.1 The lower callback rate for school dropouts

The mean callback rates by the category of callback and by the profile of the applicant are presented in Table 3.2. The average callback rate for all applications is 25%, and the interview rate is only slightly lower at 21%. These callback rates are consistent with those reported in previous studies that examined similar occupations [Challe et al., 2020, Petit et al., 2016a, Fremigacci et al., 2015].[13]

[13]Specifically, Challe et al. [2020] found callback rates of approximately 25% for waiter applications in the restaurant industry in 2018-2019, which have profiles similar to our cooks, during a period when the French labor market's situation was comparable to that covered in our experiment. Petit et al. [2016b] reported an average callback rate between 19.3% and 26.2%, depending on the

Table 3.2: Callback rates descriptive statistics by profile

	All	Graduate	Dropout
	(1)	(2)	(3)
Callback	0.25	0.30	0.22
	(0.43)	(0.46)	(0.41)
Interview	0.21	0.25	0.18
	(0.41)	(0.43)	(0.39)
Observations	6,453	2,567	3,886

Note: This table reports the number of observations per profile and the mean value of the primary dependent variables. A callback is equal to one if the fictitious candidate received a demand for complementary information or a job interview. Interview is equal to one if the recruiter asks only for a job interview. Standard errors of the mean are reported in parentheses.

The mean callback rates conceal different outcomes according to the profile of interest. The callback rate is 30% for graduate applicants, whereas it is only 22% for dropout applicants. This difference of 8 percentage points is in favor of graduates and is upheld when only job interview propositions are considered, although the interview rates are lower, with 25% for graduates and 18% for dropouts. As the interview rate has a more straightforward interpretation than the callback rate and these two outcomes behave similarly regarding the profile or the occupation, we limit our analysis to job interview propositions in the remainder of this paper.[14]

In addition to the mean callback rate, Figure 3.10 in Appendix E shows the survival curves of the applications in our experiment. Almost all the applications of dropouts that received an interview proposition are called back by employers at most two weeks after the applications were sent, while graduates are called back during the four weeks following their application. Interestingly, more than half of the interview propositions are made within the first ten days after the applications were sent. The combination of the high callback rate levels and the quick delay for employer replies suggest that our occupations are in high demand, and graduate applications are favored. However, some differences exist across departments. Figure 3.11 in Appendix F shows the ratio of graduate over dropout callback rates by departments. It appears that graduate applicants are favored over dropout appli-

location, for cooks in France in 2011-2012. Fremigacci et al. [2015] discovered that young mason candidates aged 21 received an average callback rate of 19.3% in France in 2011, when the unemployment rate was increasing after the 2008-2009 crisis.

[14]Results are both quantitatively and qualitatively similar when using the larger definition of callback rate.

cants in 80% of the departments, leaving 20% of departments that favor dropout applicants.

Following the well-known critique of Heckman in the discrimination literature, it is possible that our results are biased due to employers' prior beliefs [Heckman, 1998]. Heckman argues that imposing the same set of observable characteristics on employers may cause discrimination to go unnoticed when it exists and highlight it when it does not. This issue arises because the two populations have different real-life characteristics that are not equivalent but are made equal in the audit study for the purposes of experiment. Consequently, the average characteristics of the two populations are equivalent, leaving aside potential differences in variance.

This potential difference in our study exists because school dropouts are likely to differ from graduates not only in job-related skills but also in other dimensions such as writing a CV or cover letter. This potential bias can be accounted for using the statistical approach proposed by Neumark [2012], which involves estimating a heteroskedastic Probit model that allows the variance of the error term to vary across groups. To identify the model, at least one characteristic related to the job vacancy that affects the callback rate of both groups in a similar way must be controlled.

In our experiment, required experience is a characteristic that varies substantially across the sample and impacts the callback rate of both graduates and dropouts in a similar way. Additionally, this variable is significant in the hiring process since it can be used as an impartial criterion to distinguish candidates. Furthermore, this feature negatively impacts the interview rate of all applicants, which is necessary for identification. Table 3.10 in Appendix G displays the interview rate difference between graduate and dropout applicants obtained through a Probit model in Panel A and a heteroskedastic Probit model in Panel B, for all applications in columns (1) and (2), restricted to cooks in columns (3) and (4), and to masons in columns (5) and (6).[15] Employers seem to view school dropouts as a more diverse group than graduates, with the marginal effect through variance being positive. This indicates that the difference in the interview rate is smaller than it should be, with a value of -7.5 percentage points instead of -11 percentage points on average. This result is not surprising as we decompose our group of school dropouts to determine whether their labor market experience impacted their callback rate.

[15]During our experiment, we found that a high percentage of open positions for masons were managed by temporary work agencies, accounting for up to 65% of the observations during certain weeks and leading to a lower number of observations in comparison with cook positions.

Table 3.3: Effects of job-related work experience on job interview probability

Interview (0/1)	All Applicants		
	(1)	(2)	(3)
Dropout with job-related work experience ($\hat{\beta}_W$)	-0.0491***	-0.0489***	-0.0495***
	(0.0091)	(0.0090)	(0.0090)
Dropout with inactivity ($\hat{\beta}_I$)	-0.1711***	-0.1703***	-0.1719***
	(0.0127)	(0.0125)	(0.0128)
Observations	6,453	6,453	6,453
R-squared	.0146	.0307	.0616
Graduate mean	.2517	.2517	.2517
Pval of $\beta_W = \beta_I$	0	0	0
Job offer characteristics	No	Yes	Yes
Month & Department FE	No	No	Yes

Note: This table reports OLS estimates of equation (3.1). The dependent variable is a dummy variable equal to one if the application gets a proposition for a job interview. Job offer characteristics include all the characteristics listed in Table 3.8 in Appendix D. Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

3.4.2 The benefit of early work experience

To analyze more extensively the interview rate differences across profiles, we estimate the following linear probability model with Ordinary Least Squares (OLS) estimators:[16]

$$y_{ij} = \alpha + \beta_W \cdot \text{Worker Dropout}_i + \beta_I \cdot \text{Inactive Dropout}_i + \gamma X_j + \varepsilon_{ij} \tag{3.1}$$

where y_{ij} is a dummy variable equal to one if applicant i gets an interview proposition for job j. Worker Dropout$_i$ is a dummy variable equal to one if applicant i is a school dropout with job-related work experience, zero if it remained in vocational education and graduated as depicted in Section 3.3.1. Similarly, Inactive Dropout$_i$ is a dummy variable equal to one if applicant i is a school dropout who remained in inactivity instead. X_i is a vector that control for the job offer characteristics, as well as month and department fixed effects. ε_{ij} is a residual term, which is by construction orthogonal to the treatment variables.

Results are shown in Table 3.3 by pooling all the applications together, introducing control variables progressively.[17] In accordance with Table 3.2, the mean interview rate for a job position is around 25% for graduate applicants and the difference

[16] Results are equivalent to non-linear model estimates since our treatment variables are exogenous by design.

[17] The results are quantitatively and qualitatively similar when disentangling the data by cook and mason positions. Although not shown here due to space constraints, they are available upon request.

with dropout applicant now differ given their labor market experience.

The interview rate of dropouts with inactivity decreases by 17 percentage points on average. In other words, the chances of being interviewed for a job position is 67% lower for dropout applicants when they remain inactive for two years after middle school than for graduate applicants.

The other result is promising as it demonstrates that indicating a one-year job-related experience in the relevant occupation more than doubles the probability of school dropouts receiving a job interview, with the average interview rate increasing from 8% to 20%. However, it is important to note that their interview rate remains significantly lower than that of vocational school graduates, with a difference of approximately 5 percentage points. We attribute this disparity to the fact that graduates not only acquire job-related experience through internships or apprenticeships during their two years of education but also signal to employers their successful completion of the curriculum with a diploma.

3.4.3 The premium associated to skill certification

As described in Section 3.3.1, school dropouts who obtain job-related work experience may choose to pursue additional training programs to obtain a national diploma and certify their skills, as was possible under the EAV program discussed in Section 3.2. In order to examine the impact of skill certification on the callback rate of school dropouts, we estimate equation (3.1) by disaggregating our treatment variable to account for the possibility that some applications display a certification in addition to one year of job-related experience.

Table 3.4 presents the results of certified and non-certified job-related work experience on the probability of job interviews. Overall, work experience *without certification* still significantly increases the probability of school dropouts receiving job interviews by more than two-fold (from 8% to 19%) compared to inactive school dropouts. Furthermore, the effect of skill certification further elevates the job interview probabilities of school dropouts to 22%, which is 3 percentage points higher than the interview rate of dropouts with job-related work experience but without certification. This increase corresponds to a treatment effects of skill certification of 14%.

Our findings align with the main result of the 2016 experiment analyzed by Cahuc et al. [2019], indicating that work experience gained through the EAV program has a stronger effect on the likelihood of obtaining a job interview when accompanied by a vocational training period leading to a diploma. The authors also find that the effect of the diploma obtained through training is greater when the ex-

Table 3.4: Effects of certified job-related work experience on job interview probability

Interview (0/1)	All Applicants		
	(1)	(2)	(3)
Dropout with job-related work experience certified	-0.0287**	-0.0294**	-0.0259*
	(0.0142)	(0.0141)	(0.0144)
Dropout with job-related work experience non-certified	-0.0590***	-0.0585***	-0.0612***
	(0.0099)	(0.0099)	(0.0101)
Dropout with inactivity	-0.1711***	-0.1703***	-0.1719***
	(0.0127)	(0.0125)	(0.0128)
Observations	6,453	6,453	6,453
R-squared	.0153	.0313	.0625
Graduate mean	.2517	.2517	.2517
Pval of $\beta_{W,C} = \beta_{W,NC}$	.0492	.0594	.0289
Pval of $\beta_{W,C} = \beta_I$	0	0	0
Pval of $\beta_{W,NC} = \beta_I$	0	0	0
Job offer characteristics	No	Yes	Yes
Month & Department FE	No	No	Yes

Note: This table reports OLS estimates of equation (3.1) by dis-aggregating the (≪Worker Dropout≫) variable according to whether job-related experience was certified with a national diploma or not. The dependent variable is a dummy variable equal to one if the application gets a proposition for a job interview. Job offer characteristics include all the characteristics listed in Table 3.8 in Appendix D. Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

perience was acquired in the non-market sector. They attribute this result to firms perceiving additional training in the non-market sector as more credible compared to the market sector. Notably, a recent study by the French Ministry of Labor, which primarily provides a descriptive overview of the EAV program, demonstrates that the training obligation imposed on employers was generally met and that access to training was indeed more prevalent in the non-market sector (89% of young people) compared to the market sector (65%) [Borel and Pichavent, 2021].

Consequently, we further investigate whether the work experience and skills acquired in the market sector yield different impacts compared to those acquired in the non-market sector. Table 3.11 in Appendix H presents the effect of certified and non-certified job-related work experience in both the market and non-market sectors. Across all specifications, including all applicants, the results remain consistent regardless of the sector in which school dropouts gained their experience. In our case, and given the specific occupations, skill certification proves to be a more influential factor in improving the job search process compared to the sector in which the experience was acquired.

Lastly, it should be noted that recruiters still exhibit a slight preference for school

graduates, despite the equivalent certification signaled by dropouts. In theory, both populations possess the same set of certified skills, but because dropouts acquired them within a shorter duration of one year instead of the two-year vocational education, employers may consider graduates to have a better mastery of these skills.

3.4.4 The impact of local labor market conditions

We proceed to examine the robustness of our main findings by considering various characteristics related to labor market conditions. Despite the overall tightness of the occupations to which we sent the applications, there are variations in tightness across territories, and employers' requirements may differ based on the pool of available candidates. Consequently, we explore the differences in interview rates within labor markets that vary in terms of unemployment rates, job tightness, and the proportion of dropouts working in low-skilled occupations.[18] This analysis is conducted at the commuting zone level.

The effects of certified job-related work experience on the probability of job interviews in different labor markets are presented in Table 3.5. To examine the impact of each labor market characteristic, we divide our sample into two groups based on whether the observations belong to commuting zones with values below or above the median value for each characteristic.[19]

Consistent with the findings of Cahuc et al. [2019], we anticipate that in areas with an excess labor supply, the positive effect of signaling job-related experience may decrease due to increased competition from an external pool of applicants. Although the occupations targeted in our study require no or low educational qualifications, all applicants are likely to face greater competition from other job seekers in areas with higher unemployment rates.

The results are displayed in Table 3.5.[20] Irrespective of the characteristic considered, we observe that the average interview rate of graduates declines in labor markets with a higher number of job seekers or a greater share of dropouts in employment. Furthermore, school dropouts who remained inactive are consistently given less consideration compared to graduates, regardless of the labor market conditions. The situation remains unchanged for dropouts who signal job-related work

[18] We calculate the dropout share by taking the ratio of the number of dropouts who have their first-ever work experience in occupations with the same educational level as cooks and masons to the total number of workers in those specific occupations.

[19] The median values in our sample are 8.7% for the unemployment rate, 1.7 for job tightness (defined as the ratio of job vacancies to job seekers in cook or mason positions), and 3.7% for the share of dropouts.

[20] As a robustness test, we also interact our treatment variables with these characteristics, as shown in Table 3.12 in Appendix I, yielding similar results.

experience without certification compared to the overall scenario. However, it differs significantly when they present an additional diploma certifying their skills.

Columns (1), (4), and (5), corresponding to areas with tight labor markets or a lower share of dropouts in employment, reveal that dropouts with certified job-related work experience receive fewer callbacks for job interviews than graduates, experiencing a decline of approximately 4 percentage points, corresponding to a 15% decrease. The premium associated with skill certification, compared to those with only work experience, also disappears. Conversely, in slack labor markets (columns (2), (3), and (6)), the premium related to skill certification increases to +25%, surpassing the overall situation where it was +14%. These findings may appear contradictory to those discussed in Cahuc et al. [2019], but they align with other correspondence studies analyzed by Cahuc and Hervelin [2020] and Kroft et al. [2013]. In tighter labor markets, employers face higher competition for qualified and experienced workers, which diminishes the positive signal associated with one-year job-related experience and the diploma obtained by the applicants.[21]

3.4.5 Results from unsolicited applications

A significant proportion of mason job vacancies were handled by temporary work agencies during the experiment, as mentioned in Section 3.4.1. Additionally, a characteristic of our chosen occupations is that workers are likely to be aware of a small yet notable number of job openings through networking or word of mouth. Consequently, we considered spontaneous applications as a second application channel. In other words, we submitted application profiles to firms operating in these two occupations without responding to specific job advertisements.

We compiled a list of firms engaged in these occupations from various online sources.[22] We then refined the list to ensure that no firms had received a candidate from our initial testing phase. Additionally, we excluded duplicate entries from the same parent company. Simultaneously, we utilized the same resumes and cover letters, with minor alterations to the cover letter and email content to suit the nature of spontaneous applications. We randomized the applicant's profile, template, and fictitious name for each firm. Furthermore, we randomly selected a date and time for submission.[23] Each firm received only one application, resulting in a total of

[21] According to the French employment agency, the share of recruiters who declare hiring difficulties was about 58% for masons and 61% for cooks in 2018, while the average share was 44% for all occupations in 2018 and 32% for all occupations in 2016.

[22] We extracted relevant information, such as the firm's national ID, department of location, zip code when available, phone number, and email address, from the *Qualibat* and *La Bonne Boite* websites, which provide details about the types of jobs firms are capable of hiring for.

[23] The date was randomly chosen from Monday to Friday, and the time was randomly selected

8,208 spontaneous applications sent to firms in October 2018 for mason positions and in November and December 2018 for cook positions.

Table 3.13 in Appendix J illustrates that both the callback rate and the interview rate are considerably lower when applications are sent spontaneously, as opposed to applying for online job vacancies. The average callback rate for graduates is 7.8%, with an average interview rate of 6.6%. The differences in interview rates between dropouts and graduates are smaller compared to the results presented in Sections 3.4.1 and 3.4.3. Nevertheless, it is evident that dropouts who remained inactive face a penalty of -3.5 percentage points, corresponding to a decline of 53%. This penalty is less severe for dropouts with job-related work experience, as their interview rate is only 1.5 percentage points lower than that of graduates. Notably, this represents a relative decline of 22%, which is comparable in magnitude to the findings of the main correspondence study described in Section 3.4.3. Only dropouts with job-related work experience and a certification diploma manage to catch up to the interview rate of graduates.

To supplement the analysis conducted in Section 3.4.4, we encountered limitations in our sample due to the unavailability of commuting zone data for half of the firms. Consequently, this introduces a potential sample bias, as indicated by the higher callback and interview rates observed in columns (2) and (4) of Table 3.13 compared to the rates obtained with the full samples in columns (1) and (3), respectively. Furthermore, the difference in interview rates between graduates and dropouts with non-certified work experience is no longer statistically significant, although the magnitude remains within the range of the results obtained with the full sample.

Given this limitation, we aggregated all the applications, including both unsolicited and directed applications to job vacancies, and examined the variations in interview rates across different labor markets. Table 3.6 presents the results in a format similar to Table 3.5. Notably, we observe that sending unsolicited applications reduces the callback rate for graduate applicants, irrespective of the labor market conditions although the reduction is higher in tight labor markets. Interestingly, unsolicited applications appear to bridge the gap between graduate applicants and dropouts with work experience in slack labor markets. We interpret this outcome as evidence that job-related experience on its own is sufficiently attractive to employers when the competition for qualified workers is less intense among firms, as they have the flexibility to callback alternative candidates in the future if the current applicant is deemed less suitable.

Overall, regardless of the occupation, specification, and sample, our results con-

between 8 am and 9 pm, consistent with the initial correspondence study.

sistently demonstrate that employers distinctly prioritize young applicant profiles as follows:

$$\underbrace{\text{Inactives} \prec \text{Workers} \prec \text{Workers with certification}}_{Dropouts} \preceq \text{Graduates}$$

Table 3.5: Effects of certified job-related work experience on job interview probability in different labor markets

Interview (0/1)	Unemployment Rate		Job Tightness		Share of dropouts	
	(1)	(2)	(3)	(4)	(5)	(6)
Dropout with job-related work experience certified	-0.0403*	-0.0061	-0.0061	-0.0460**	-0.0440**	-0.0074
	(0.0203)	(0.0200)	(0.0220)	(0.0192)	(0.0194)	(0.0214)
Dropout with job-related work experience non-certified	-0.0657***	-0.0568***	-0.0507***	-0.0708***	-0.0718***	-0.0495***
	(0.0140)	(0.0145)	(0.0138)	(0.0145)	(0.0154)	(0.0126)
Dropout with inactivity	-0.2081***	-0.1257***	-0.1410***	-0.2059***	-0.2079***	-0.1294***
	(0.0166)	(0.0162)	(0.0171)	(0.0199)	(0.0166)	(0.0166)
Observations	3,526	2,927	3,229	3,224	3,296	3,133
R-squared	0.0781	0.0596	0.0799	0.0828	0.0676	0.0527
Market mean	7.5315	10.7436	1.224	2.6158	.0272	.0596
Graduate mean	.2899	.2060	.2154	.2868	.2874	.2151
Pval of $\beta_{W,C} = \beta_{W,NC}$	.2845	.0216	.0534	.2735	.2155	.0768
Pval of $\beta_{W,C} = \beta_I$	0	0	0	0	0	0
Pval of $\beta_{W,NC} = \beta_I$	0	.0007	0	0	0	.0001
Job offer characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Month & Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample restriction (Below vs Above median)	Below	Above	Below	Above	Below	Above

Note: This table reports OLS estimates of equation (3.1) by dis-aggregating the (≪Worker Dropout≫) variable according to whether job-related experience was certified with a national diploma or not, and by splitting the sample according to local labor market characteristics. The dependent variable is a dummy variable equal to one if the application gets a positive callback to a job offer. Job offer characteristics include all the characteristics listed in Table 3.8 in Appendix D. Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Table 3.6: Effects of certified work experience on job interview probability in different labor markets with unsolicited applications

Interview (0/1)	Unemployment Rate		Job Tightness		Share of dropouts	
	(1)	(2)	(3)	(4)	(5)	(6)
Unsolicited application	-0.1508***	-0.1232***	-0.1121***	-0.1635***	-0.1572***	-0.0974***
	(0.0459)	(0.0343)	(0.0358)	(0.0492)	(0.0410)	(0.0355)
Dropout with job-related work experience certified	-0.0443**	-0.0074	-0.0036	-0.0477**	-0.0438**	-0.0080
	(0.0218)	(0.0194)	(0.0219)	(0.0189)	(0.0192)	(0.0218)
Dropout with work experience certified × Unsolicited	0.0122	0.0265	0.0047	0.0323	0.0299	0.0087
	(0.0286)	(0.0223)	(0.0251)	(0.0264)	(0.0244)	(0.0233)
Dropout with job-related work experience non-certified	-0.0636***	-0.0594***	-0.0527***	-0.0688***	-0.0715***	-0.0498***
	(0.0151)	(0.0133)	(0.0138)	(0.0140)	(0.0150)	(0.0126)
Dropout with work experience non-certified × Unsolicited	0.0442**	0.0577***	0.0498***	0.0511***	0.0593***	0.0391**
	(0.0188)	(0.0158)	(0.0170)	(0.0180)	(0.0180)	(0.0173)
Dropout with inactivity	-0.2078***	-0.1376***	-0.1416***	-0.2031***	-0.2074***	-0.1321***
	(0.0183)	(0.0147)	(0.0165)	(0.0197)	(0.0165)	(0.0170)
Dropout with inactivity × Unsolicited	0.1509***	0.0956***	0.1103***	0.1425***	0.1622***	0.0785***
	(0.0232)	(0.0192)	(0.0201)	(0.0236)	(0.0201)	(0.0210)
Observations	5,497	5,345	5,432	5,410	5,668	5,139
R-squared	0.0932	0.0629	0.0773	0.0921	0.0886	0.0625
Market mean	7.4067	10.5799	1.222	2.6259	.0278	.0595
Graduate mean	.2177	.1583	.1600	.2160	.2095	.1656
Pval of $\beta_{W,C} = \beta_{W,NC}$	.4455	.012	.0327	.3309	.197	.0804
Pval of $\beta_{W,C} = \beta_I$	0	0	0	0	0	0
Pval of $\beta_{W,NC} = \beta_I$	0	0	0	0	0	0
Job offer characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Month & Department FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample restriction (Below vs Above median)	Below	Above	Below	Above	Below	Above

Note: This table reports OLS estimates of equation (3.1) by dis-aggregating the (≪Worker Dropout≫) variable according to whether job-related experience was certified with a national diploma or not, and by splitting the sample according to local labor market characteristics. The dependent variable is a dummy variable equal to one if the application gets a proposition for a job interview. Job offer characteristics include all the characteristics listed in Table 3.8 in Appendix D. Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

3.5 Conclusion

This study investigates whether job-related work experience can enhance the employment opportunities of school dropouts relative to graduates, particularly in occupations facing labor market shortages. Through a correspondence study field experiment conducted in France, we submitted approximately 6,400 applications from fictitious candidates to actual job vacancies and sent around 8,200 unsolicited applications to other firms. The experiment focused on two low-skilled occupations with high proportions of vocational students and school dropouts: cook and mason.

Results show that, on average, school graduates have a job interview rate of 25%, while school dropouts with work experience have an interview rate of 20% and it is only 8% for those who remain inactive. Gaining one year of job-related work experience reduces the interview penalty for dropouts from 67% to 25%, and acquiring a diploma further decreases it to 12%. The study also reveals that differences in interview rates between dropouts and graduates vary based on labor market conditions. In tight labor markets, this difference increases, while in slack labor markets, it decreases. Lastly, dropouts with work experience who send unsolicited applications achieve interview rates on par with graduates in slack labor markets, regardless of certification. However, sending unsolicited applications does not close the interview rate gap with graduates in tight labor markets. These findings have several policy implications.

One policy implication to be drawn from the study is the importance of informing middle school students on the negative impact of dropping out of school, or conversely, providing better counseling on the benefits of vocational high-school education. By doing so, it is possible to reduce the number of dropouts and increase the number of students who are able to acquire the necessary skills to enter the job market [Cratty, 2012, van der Steeg et al., 2015, Bonilla, 2020]. Such counseling should be provided in collaboration with coaches, alumni, or caseworkers from job centers, local businesses and employers to ensure that students are aware of the available opportunities in the labor market.

Given that there will always be school dropouts, public authorities could also target occupations where employers have hiring difficulties and offer temporary wage subsidies to bring dropouts into employment. Although the evidence is mixed for an adult population, it seems more beneficial for young people in difficulty [Caliendo and Schmidl, 2016]. This could help bridge the skills gap between dropouts and graduates and provide opportunities for dropouts to acquire the necessary skills and work experience to become more competitive in the labor market. A complementary policy implication is to favor skill certification with a national diploma

[LaForest, 2023], either through complementary classroom training to subsidized wages or via a simplified system similar to"*validation des acquis de l'expérience*" (VAE) as in France.[24] This would help ensure that dropouts who do not have a diploma are still able to acquire skills that are recognized by employers, thereby increasing their employability and competitiveness in the labor market.

Finally, the study suggests that young people should look for job opportunities by reaching out to companies even if there are no vacancies posted online. By doing so, they can increase their chances of finding employment, especially if they have relevant job-related work experience or certifications. It is important for young people to be proactive in their job search and to explore all available options for gaining work experience and skills.

[24]The VAE program in France is a system that allows individuals, regardless of their age or level of education, to have their professional experience recognized and certified through a diploma or qualification. This can be done through a portfolio of evidence, an interview, or a combination of both. Once their skills and knowledge have been assessed, individuals can obtain a diploma or qualification that is recognized by employers and the education system. Yet, there are limitations to the program that should be considered. The program may not be well-known or accessible to everyone, it may not be available for all types of certifications or diplomas, and the process can be lengthy and require a significant amount of time and effort from the applicant.

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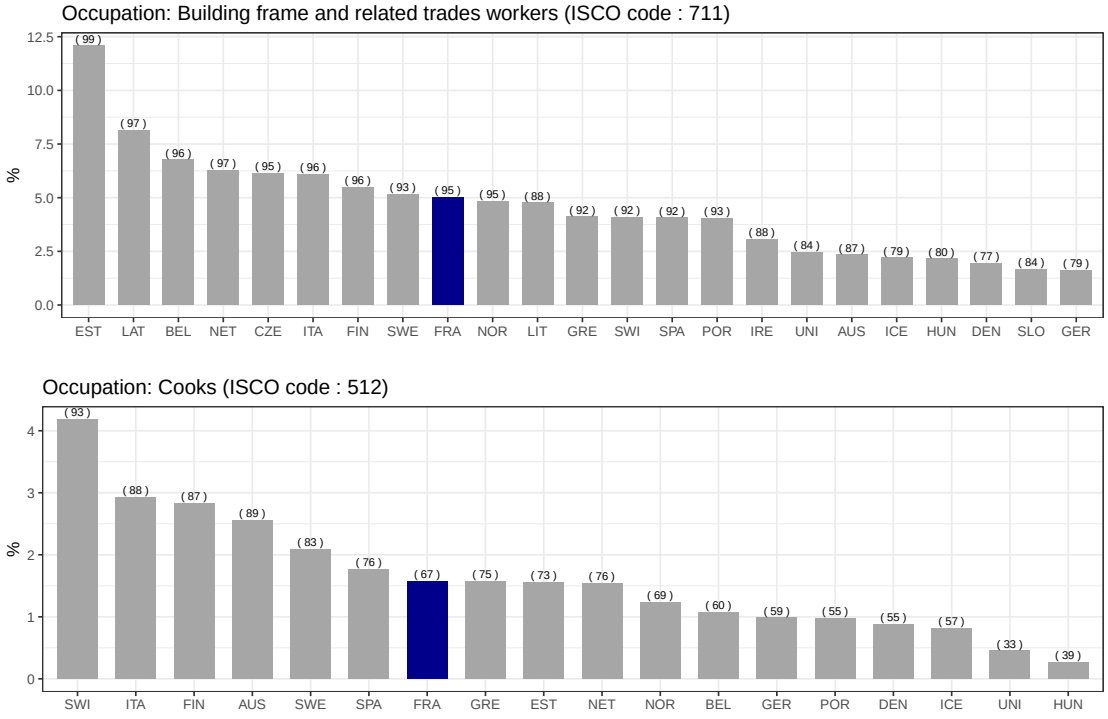
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Appendices

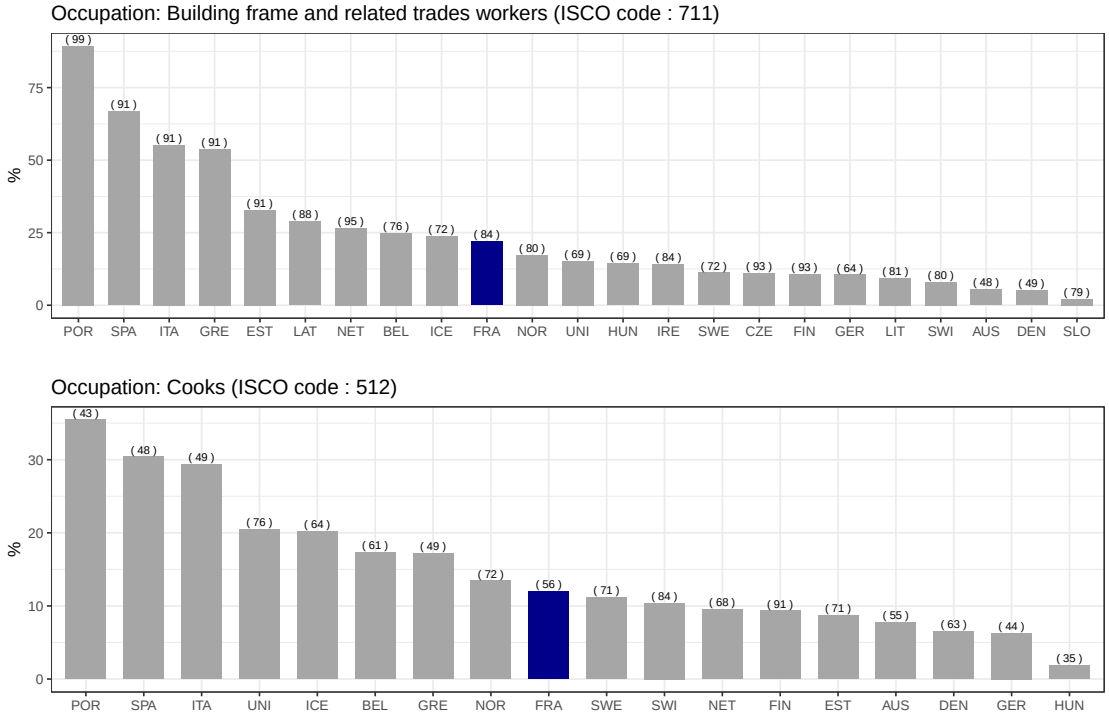
A The choice of occupations

Figure 3.3: Share of masons and cooks among early leavers from education and training who are employed in Europe



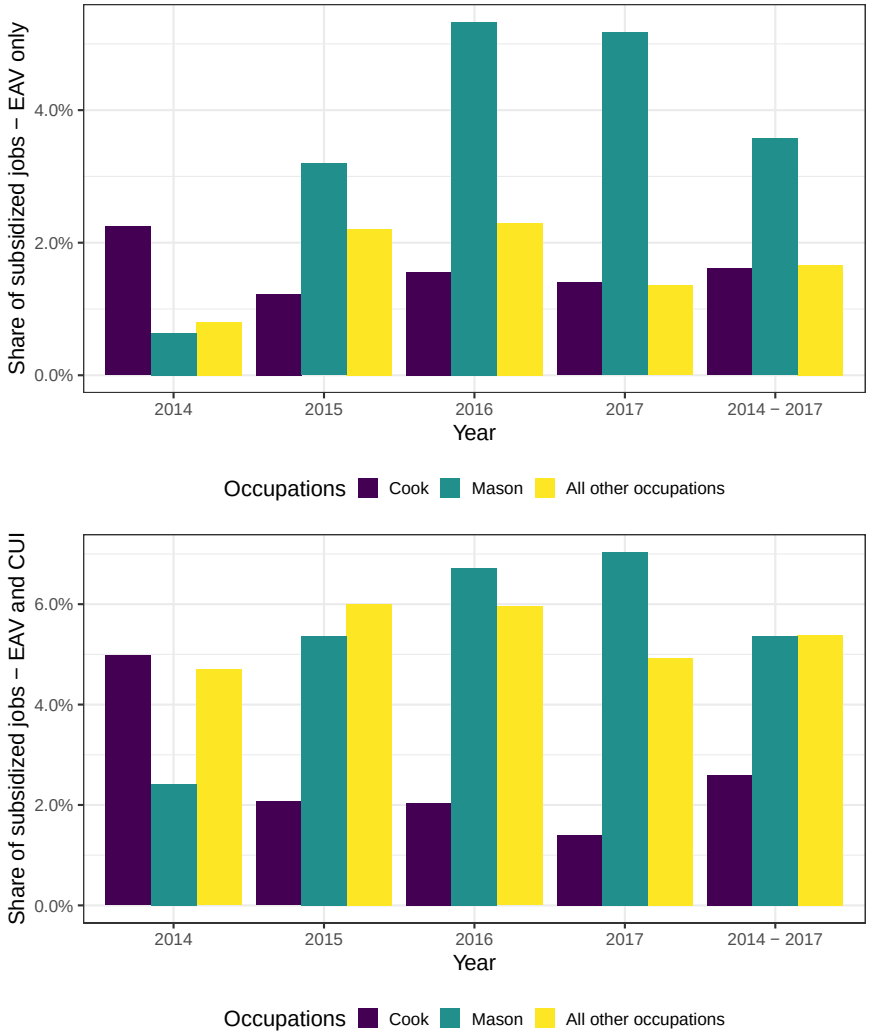
Note: In France, ≪building frame and related trades workers≫ represents about 5% of all youths who are early leavers from education and training, this occupation is more frequent among this population than 95 percent of other occupations. Youth are defined as individuals aged 15–29 years old. Shares are calculated on pooled 2011–2016 data. For Germany, they are calculated on pooled 2011–2013 data. Countries for which there are less than 20 observations are not reported.
Source: OECD calculations based on the European Union Labour Force Survey (EU-LFS) microdata

Figure 3.4: Share of early leavers from education and training among masons and cooks



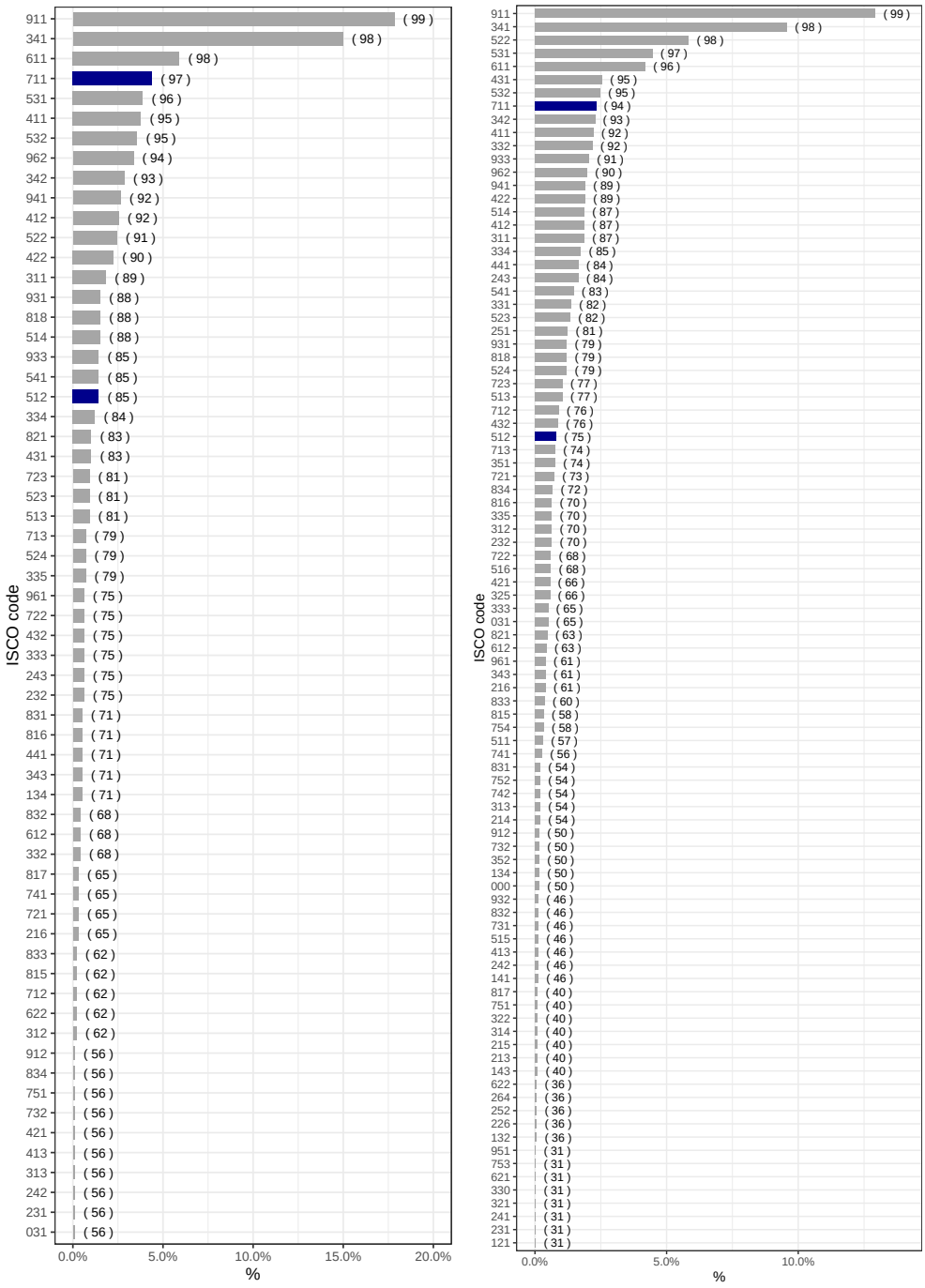
Note: In France, early leavers from education and training represents about 22% of ≪building frame and related trades workers≫, the share of dropouts in this occupation is higher than 84 percent of the other occupations. Youth are defined as individuals aged 15–29 years old. Shares are calculated on pooled 2011–2016 data. For Germany, they are calculated on pooled 2011–2013 data. Countries for which there are less than 20 observations are not reported.
Source: OECD calculations based on the European Union Labour Force Survey (EU-LFS) microdata

Figure 3.5: Share of subsidized jobs in the two selected occupations compared to others



Note: In France, on average over the period 2014 to 2017, about 3.6% of people working as masons are employed through an Emploi d'Avenir (EAv), and about 5.4% through either an Emploi d'Avenir or Contrat Unique d'Insertion (CUI). Youth are defined as individuals aged 15–24 years old. The occupation of ≪Masons≫ is defined by the ISCO code 711 and ≪cooks≫ corresponds to the ISCO code 512.
Source: Labour Force Survey (Enquête Emploi)

Figure 3.6: Share of each occupations among EAV (left) and all subsidized (right) contracts



Note: In France, ≪building frame and related trades workers≫ (ISCO code 711) represents about 4.4% of all Emplois d'Avenir (EAv), EAv jobs are more frequent in this occupation than 97 percent of the other occupations. Youth are defined as individuals aged 15–24 years old. Data are pooled over the period 2014-2017.
Source: Labour Force Survey (Enquête Emploi)

Table 3.7: Descriptive statistics on EAV subsidized jobs

Characteristics	All	Under 18 yo (1.26%)		
		All	Cook	Mason
	(1)	(2)	(3)	(4)
Sex (*Male*)	50.11%	62.60%	74.26%	100.0%
Nationality (*French*)	95.07%	97.61%	97.79%	95.83%
School level				
Lower-secondary	27.39%	59.89%	38.24%	60.42%
2-year vocational upper secondary	47.39%	37.44%	61.76%	39.58%
3-year upper secondary	20.59%	02.63%	00.00%	00.00%
University	04.63%	00.03%	00.00%	00.00%
Youth center registration	100%	100%	100%	100%
Job center registration	69.73%	35.21%	36.03%	37.50%
Last duration in unemployment				
Less than 6 months	30.06%	60.42%	53.06%	83.33%
From 6 to 11 months	28.71%	27.91%	32.65%	16.67%
From 12 to 23 months	27.81%	10.95%	14.29%	00.00%
More than 23 months	13.42%	00.67%	00.00%	00.00%
Mean age (at entry)	21.6 yo	16.9 yo	16.9 yo	16.8 yo
Temporary contract	75.60%	65.06%	35.29%	50.00%
Contract duration				
$\leq$ *1 year*	57.23%	57.17%	52.08%	70.83%
$\leq$ *3 years*	42.77%	42.83%	47.92%	29.17%
Hours of work (per week)	33.6 h	33.2 h	34.2 h	35.1 h
Market sector	29.25%	49.41%	80.88%	87.50%
Firm size				
Small	34.48%	53.93%	72.97%	80.00%
Medium	45.36%	39.55%	25.23%	17.50%
Large	20.16%	06.52%	01.80%	02.50%
W/ certified training	30.07%	32.55%	30.15%	35.42%
In center	74.04%	78.45%	82.93%	94.12%

Note: This table reports descriptive statistics on the French "EAV" subsidized jobs program described in Section 3.2. Column (1) reports the statistics for the whole population in subsidized employment (234,910 young people), while columns (2) to (4) restrict the sample for individuals aged below 18 years-old (2,965 young people, i.e 1.26%). Column (3) further restricts the sample to youths who worked as cook and column (4) as masons.
Source: IMILO (2013-2017), 234,910 observations, authors' calculations.

B Examples of documents for applications

Application email messages (by layout)

For type 1 applications, the email message was the following:

Object: Application job offer n°XXX
Attached files: Curriculum_Vitae.pdf, Lettre_Motivation.pdf

Dear Madam, Sir,

With reference to your advertisement XXX for the position of YYY, I wish to submit my application.
Please find enclosed my cover letter and my resume.
May I assure you, Madam, Sir, of my sincere gratitude.

First name, Last name
Phone number

For type 2 applications, the email message was the following:

Object: Application (job ads XXX)
Attached files: CV.pdf, LM.pdf

Dear Madam, Sir,

I am pleased to submit my application for the position of YYY following your advertisement XXX published on the website Pôle emploi.
I am sending you in the attachment my resume and my cover letter.
May I assure you, Madam, Sir, that I remain faithfully yours.

First name, Last name
Phone number

Application reply email messages (by candidate)

For Alexis Dubois application reply, the email message was the following:

Greetings,

Thank you for your consideration of my application. However, I am unable to respond favorably. Indeed, I have accepted another offer.

With kind regards,
Alexis Dubois

For Théo Petit application reply, the email message was the following:

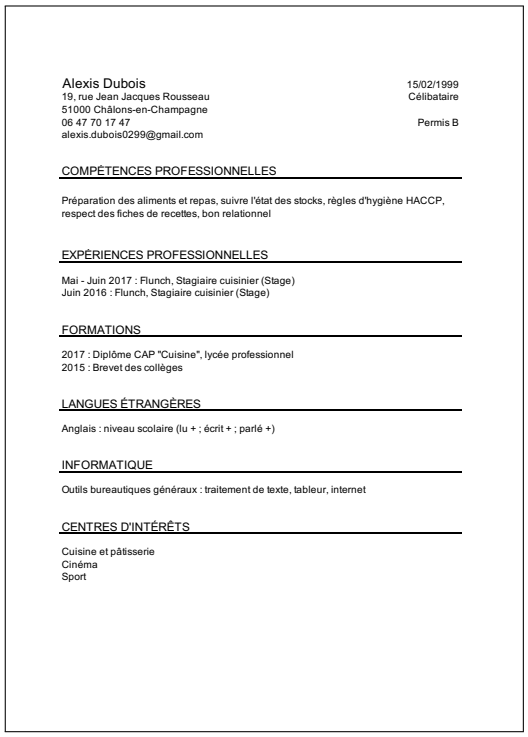
Good morning,

I thank you for your answer regarding my application. Nevertheless, I have just accepted another offer.

Sincerely,
Théo Petit

Figure 3.7: Sample of résumés from graduate and dropout applicants

(a) Graduate



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

15/02/1999
Célibataire
Permis B

COMPÉTENCES PROFESSIONNELLES

Préparation des aliments et repas, suivre l'état des stocks, règles d'hygiène HACCP, respect des fiches de recettes, bon relationnel

EXPÉRIENCES PROFESSIONNELLES

Mai - Juin 2017 : Flunch, Stagiaire cuisinier (Stage)
Juin 2016 : Flunch, Stagiaire cuisinier (Stage)

FORMATIONS

2017 : Diplôme CAP "Cuisine", lycée professionnel
2015 : Brevet des collèges

LANGUES ÉTRANGÈRES

Anglais : niveau scolaire (lu + ; écrit + ; parlé +)

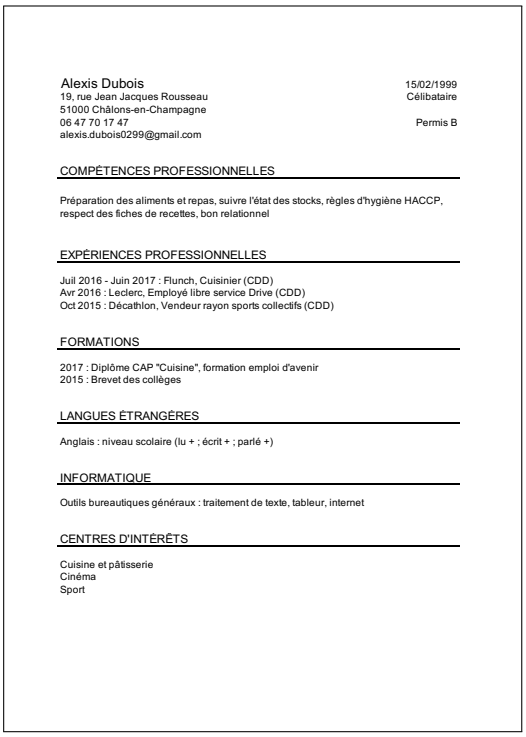
INFORMATIQUE

Outils bureautiques généraux : traitement de texte, tableur, internet

CENTRES D'INTÉRÊTS

Cuisine et pâtisserie
Cinéma
Sport

(b) Dropout, Work, Certified



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

15/02/1999
Célibataire
Permis B

COMPÉTENCES PROFESSIONNELLES

Préparation des aliments et repas, suivre l'état des stocks, règles d'hygiène HACCP, respect des fiches de recettes, bon relationnel

EXPÉRIENCES PROFESSIONNELLES

Juil 2016 - Juin 2017 : Flunch, Cuisinier (CDD)
Avr 2016 : Leclerc, Employé libre service Drive (CDD)
Oct 2015 : Décathlon, Vendeur rayon sports collectifs (CDD)

FORMATIONS

2017 : Diplôme CAP "Cuisine", formation emploi d'avenir
2015 : Brevet des collèges

LANGUES ÉTRANGÈRES

Anglais : niveau scolaire (lu + ; écrit + ; parlé +)

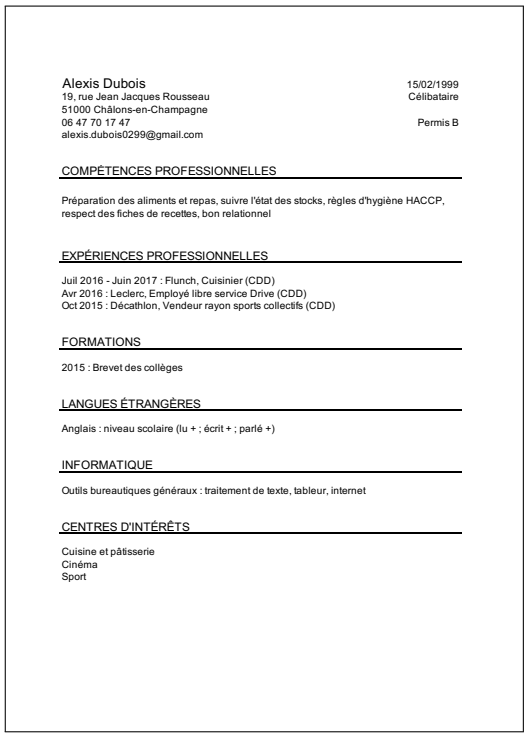
INFORMATIQUE

Outils bureautiques généraux : traitement de texte, tableur, internet

CENTRES D'INTÉRÊTS

Cuisine et pâtisserie
Cinéma
Sport

(c) Dropout, Work



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

15/02/1999
Célibataire
Permis B

COMPÉTENCES PROFESSIONNELLES

Préparation des aliments et repas, suivre l'état des stocks, règles d'hygiène HACCP, respect des fiches de recettes, bon relationnel

EXPÉRIENCES PROFESSIONNELLES

Juil 2016 - Juin 2017 : Flunch, Cuisinier (CDD)
Avr 2016 : Leclerc, Employé libre service Drive (CDD)
Oct 2015 : Décathlon, Vendeur rayon sports collectifs (CDD)

FORMATIONS

2015 : Brevet des collèges

LANGUES ÉTRANGÈRES

Anglais : niveau scolaire (lu + ; écrit + ; parlé +)

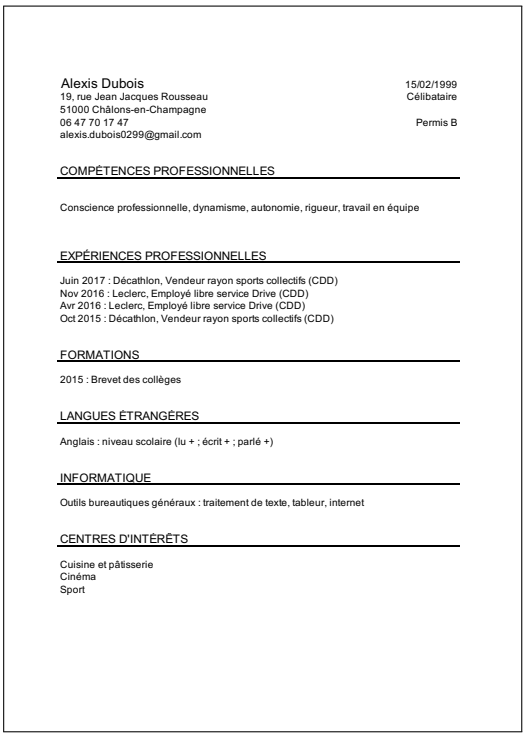
INFORMATIQUE

Outils bureautiques généraux : traitement de texte, tableur, internet

CENTRES D'INTÉRÊTS

Cuisine et pâtisserie
Cinéma
Sport

(d) Dropout, Inactivity



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

15/02/1999
Célibataire
Permis B

COMPÉTENCES PROFESSIONNELLES

Conscience professionnelle, dynamisme, autonomie, rigueur, travail en équipe

EXPÉRIENCES PROFESSIONNELLES

Juin 2017 : Décathlon, Vendeur rayon sports collectifs (CDD)
Nov 2016 : Leclerc, Employé libre service Drive (CDD)
Avr 2016 : Leclerc, Employé libre service Drive (CDD)
Oct 2015 : Décathlon, Vendeur rayon sports collectifs (CDD)

FORMATIONS

2015 : Brevet des collèges

LANGUES ÉTRANGÈRES

Anglais : niveau scolaire (lu + ; écrit + ; parlé +)

INFORMATIQUE

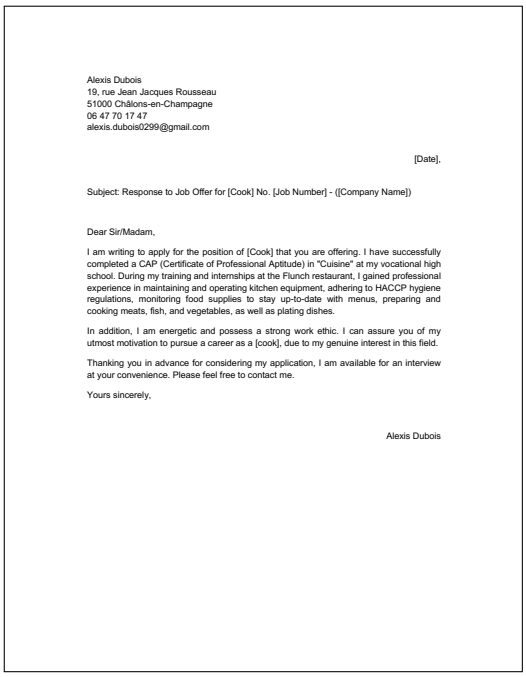
Outils bureautiques généraux : traitement de texte, tableur, internet

CENTRES D'INTÉRÊTS

Cuisine et pâtisserie
Cinéma
Sport

Figure 3.8: Sample of cover letters from graduate and dropout applicants

(a) Graduate



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

[Date],

Subject: Response to Job Offer for [Cook] No. [Job Number] - ([Company Name])

Dear Sir/Madam,

I am writing to apply for the position of [Cook] that you are offering. I have successfully completed a CAP (Certificate of Professional Aptitude) in "Cuisine" at my vocational high school. During my training and internships at the Flunch restaurant, I gained professional experience in maintaining and operating kitchen equipment, adhering to HACCP hygiene regulations, monitoring food supplies to stay up-to-date with menus, preparing and cooking meats, fish, and vegetables, as well as plating dishes.

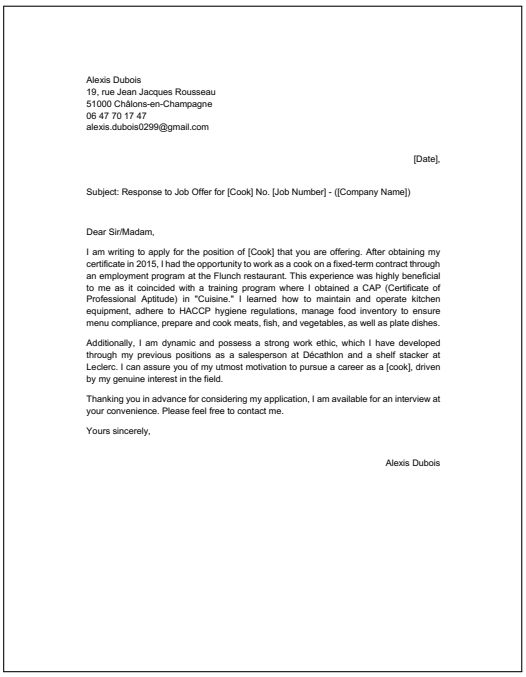
In addition, I am energetic and possess a strong work ethic. I can assure you of my utmost motivation to pursue a career as a [cook], due to my genuine interest in this field.

Thanking you in advance for considering my application, I am available for an interview at your convenience. Please feel free to contact me.

Yours sincerely,

Alexis Dubois

(b) Dropout, Work, Certified



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

[Date],

Subject: Response to Job Offer for [Cook] No. [Job Number] - ([Company Name])

Dear Sir/Madam,

I am writing to apply for the position of [Cook] that you are offering. After obtaining my certificate in 2015, I had the opportunity to work as a cook on a fixed-term contract through an employment program at the Flunch restaurant. This experience was highly beneficial to me as it coincided with a training program where I obtained a CAP (Certificate of Professional Aptitude) in "Cuisine." I learned how to maintain and operate kitchen equipment, adhere to HACCP hygiene regulations, manage food inventory to ensure menu compliance, prepare and cook meats, fish, and vegetables, as well as plate dishes.

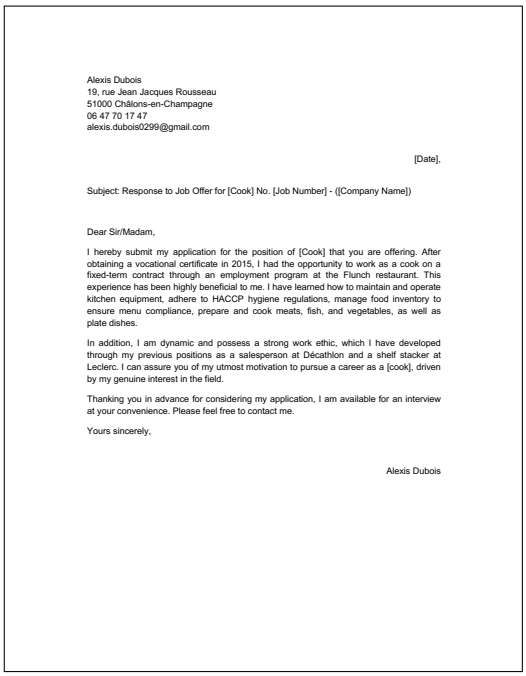
Additionally, I am dynamic and possess a strong work ethic, which I have developed through my previous positions as a salesperson at Décathlon and a shelf stacker at Leclerc. I can assure you of my utmost motivation to pursue a career as a [cook], driven by my genuine interest in the field.

Thanking you in advance for considering my application, I am available for an interview at your convenience. Please feel free to contact me.

Yours sincerely,

Alexis Dubois

(c) Dropout, Work



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

[Date],

Subject: Response to Job Offer for [Cook] No. [Job Number] - ([Company Name])

Dear Sir/Madam,

I hereby submit my application for the position of [Cook] that you are offering. After obtaining a vocational certificate in 2015, I had the opportunity to work as a cook on a fixed-term contract through an employment program at the Flunch restaurant. This experience has been highly beneficial to me. I have learned how to maintain and operate kitchen equipment, adhere to HACCP hygiene regulations, manage food inventory to ensure menu compliance, prepare and cook meats, fish, and vegetables, as well as plate dishes.

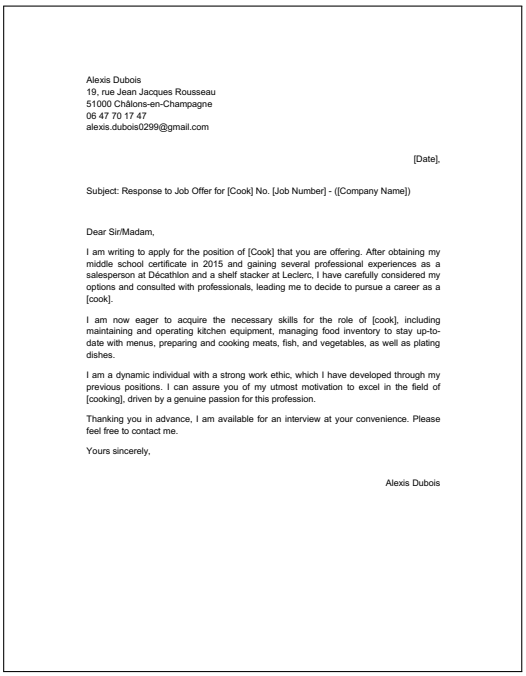
In addition, I am dynamic and possess a strong work ethic, which I have developed through my previous positions as a salesperson at Décathlon and a shelf stacker at Leclerc. I can assure you of my utmost motivation to pursue a career as a [cook], driven by my genuine interest in the field.

Thanking you in advance for considering my application, I am available for an interview at your convenience. Please feel free to contact me.

Yours sincerely,

Alexis Dubois

(d) Dropout, Inactivity



Alexis Dubois
19, rue Jean Jacques Rousseau
51000 Châlons-en-Champagne
06 47 70 17 47
alexis.dubois0299@gmail.com

[Date],

Subject: Response to Job Offer for [Cook] No. [Job Number] - ([Company Name])

Dear Sir/Madam,

I am writing to apply for the position of [Cook] that you are offering. After obtaining my middle school certificate in 2015 and gaining several professional experiences as a salesperson at Décathlon and a shelf stacker at Leclerc, I have carefully considered my options and consulted with professionals, leading me to decide to pursue a career as a [cook].

I am now eager to acquire the necessary skills for the role of [cook], including maintaining and operating kitchen equipment, managing food inventory to stay up-to-date with menus, preparing and cooking meats, fish, and vegetables, as well as plating dishes.

I am a dynamic individual with a strong work ethic, which I have developed through my previous positions. I can assure you of my utmost motivation to excel in the field of [cooking], driven by a genuine passion for this profession.

Thanking you in advance, I am available for an interview at your convenience. Please feel free to contact me.

Yours sincerely,

Alexis Dubois

C Power tests

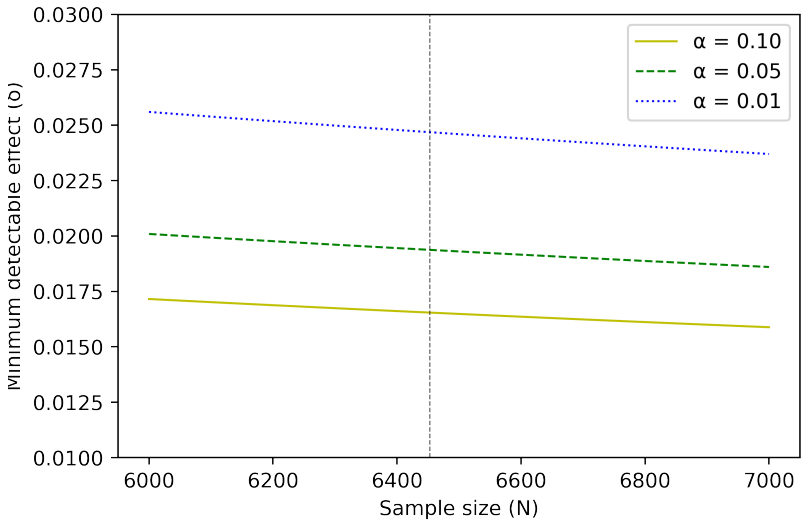
We use the single-level trials with binary outcomes formula from Djimeu and Houndolo [2016] to compute the minimum detectable effect of our experiment:

$$\delta = (t_1(\alpha) + t_2(\beta)) \times \sqrt{\left(\frac{P(1-P)}{T(1-T)N}\right)} \tag{3.2}$$

where:

Parameter	Definition	Value
δ	Minimum detectable effect	?
α	Desired significance level	0.05
β	Desired power of the design	0.80
P	Proportion of control group with outcome=1	0.18
T	Proportion randomly assigned to the treatment group	0.40
N	Total sample size	6,400

Figure 3.9: Minimum detectable effect given the sample size



Note: This figure reports the minimum detectable effect from equation (3.2) by comparing the outcomes of the «graduate» group vs the «dropout» group as described in Section 3.3.1.
Lecture: The minimum detectable effect (without covariate) is ±2 pp when the total sample size is 6,450 at the 95% significance level.

D Randomization tests

Table 3.8: Randomization tests

	Graduate	Dropout					
		All		w/ inactivity		w/ work experience	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sample mean	Sample mean	Difference (2)-(1)	Sample mean	Difference (4)-(1)	Sample mean	Difference (6)-(1)
Cook	0.822	0.813	-0.009	0.795	-0.027	0.817	-0.005
	(0.383)	(0.390)	(0.372)	(0.404)	(0.109)	(0.387)	(0.619)
Market	0.927	0.926	-0.001	0.924	-0.003	0.926	-0.001
	(0.261)	(0.262)	(0.865)	(0.265)	(0.810)	(0.262)	(0.908)
Permanent contract	0.425	0.425	-0.000	0.427	0.002	0.424	-0.001
	(0.494)	(0.494)	(0.974)	(0.495)	(0.925)	(0.494)	(0.945)
Full time	0.938	0.940	0.002	0.944	0.006	0.939	0.001
	(0.241)	(0.238)	(0.777)	(0.231)	(0.585)	(0.239)	(0.886)
Required experience	0.426	0.426	0.000	0.427	0.001	0.426	0.000
	(0.495)	(0.495)	(0.977)	(0.495)	(0.953)	(0.495)	(0.990)
Male recruiter	0.621	0.624	0.003	0.628	0.007	0.623	0.002
	(0.485)	(0.484)	(0.803)	(0.484)	(0.752)	(0.485)	(0.855)
Observations	2,567	3,886	6,453	658	3,225	3,228	5,795

Note: This Table reports means across subsamples of the experimental sample as described in Section 3.3.1 and presents randomization tests based on comparing the associated means. Column (3), (5) and (7) present mean differences with respect to column (1) and the standard error of the mean below them in parentheses. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

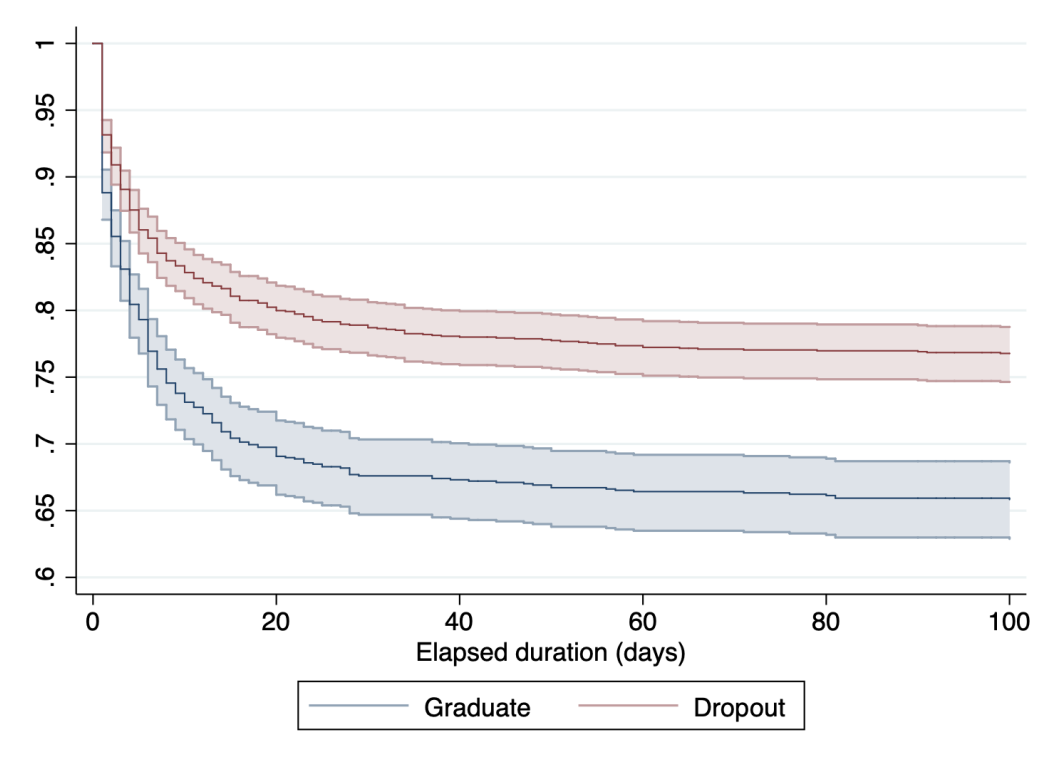
Table 3.9: Interactions between the treatment profile and features of the applications

	Callback	Interview
	(1)	(2)
Dropout	-0.1044***	-0.0877***
	(0.0352)	(0.0328)
Name	-0.0175	-0.0389
	(0.0357)	(0.0313)
Dropout × Name	0.0676	0.0598
	(0.0520)	(0.0456)
Layout	0.0112	0.0127
	(0.0357)	(0.0348)
Dropout × Layout	-0.0157	-0.0287
	(0.0462)	(0.0448)
Name × Layout	-0.0689	-0.0468
	(0.0474)	(0.0423)
Dropout × Name × Layout	0.0291	0.0130
	(0.0697)	(0.0604)
Order	0.0114	-0.0137
	(0.0358)	(0.0326)
Dropout × Order	0.0360	0.0430
	(0.0464)	(0.0391)
Name × Order	-0.0157	0.0039
	(0.0571)	(0.0508)
Dropout × Name × Order	-0.0564	-0.0563
	(0.0699)	(0.0579)
Layout × Order	0.0211	0.0324
	(0.0551)	(0.0528)
Dropout × Layout × Order	-0.0377	-0.0253
	(0.0679)	(0.0602)
Name × Layout × Order	0.0396	0.0141
	(0.0754)	(0.0706)
Dropout × Name × Layout × Order	-0.0300	-0.0198
	(0.1073)	(0.0925)
Constant	0.3085***	0.2730***
	(0.0277)	(0.0263)
Observations	6,453	6,453
R-squared	0.0124	0.0119

Note: This table reports OLS estimates of equation (3.1) by interacting the "dropout" variable as defined in Section 3.3.1 with randomized features of applications: the name of the applicant ("Name"=1 if the name was Alexis Dubois, 0 if Théo Petit), the layout of the application ("layout"=1 if the template was the first, 0 if the second), and the order of sending ("Order"=1 if the application was sent in the first position, 0 if second). The dependent variable is a dummy variable equal to one if the application gets a positive callback to a job offer in column (1) and only a job interview in column (2). Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

E Survival of applications

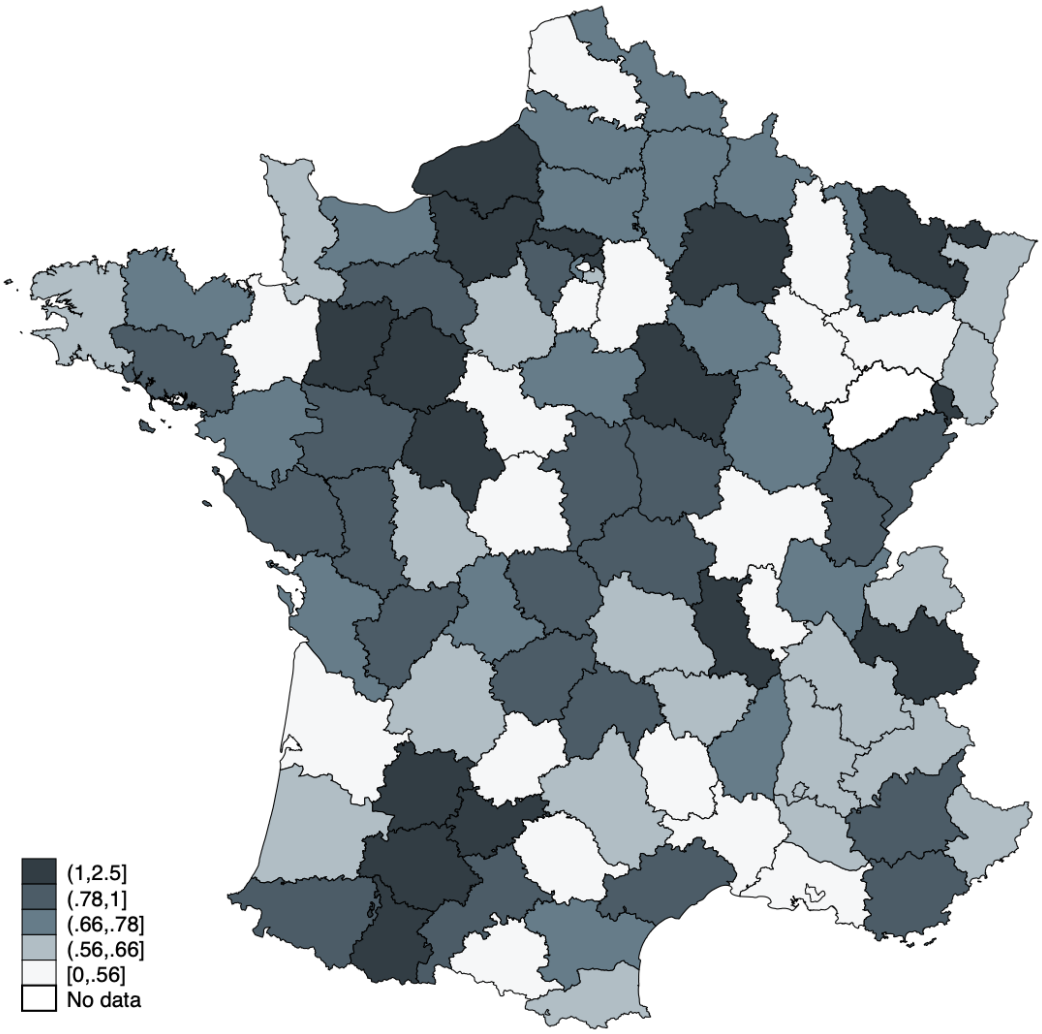
Figure 3.10: Survival of applications



Note: The event of non-survival is being called back for a job position.

F Geographic distribution of callback rate ratios

Figure 3.11: Ratio of graduate over dropout callback rates by department



Note: Each department received at least one application of graduate and one of dropout.

G Probit models and Neumark decomposition

Table 3.10: Probit and Heteroskedastic Probit estimates

	All		Cook		Mason	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Probit model					
Dropout (vs Graduate)	-0.0817***	-0.0703***	-0.0868***	-0.0738***	-0.0594***	-0.0545***
	(0.0086)	(0.0086)	(0.0102)	(0.0097)	(0.0227)	(0.0218)
	Panel B: Heteroskedastic Probit model (required experience)					
Dropout (vs Graduate)	-0.0749***	-0.0616***	-0.0820***	-0.0652***	-0.0519***	-0.0492***
	(0.0102)	(0.0112)	(0.0109)	(0.0107)	(0.0273)	(0.0274)
Marginal effect through level	-0.1116	-0.1140	-0.1055	-0.1168	-0.1022	-0.0853
Marginal effect through variance	0.0366	0.0523	0.0234	0.0516	0.0502	0.0361
Standard deviation of unobservables	1.1823	1.2482	1.1160	1.2476	1.2400	1.1613
Wald test statistic (p-value)	.3237	.2875	.4793	.2438	.6416	.7726
Observations	6,453	6,453	5,270	5,270	1,183	1,183
Outcome	Callback	Interview	Callback	Interview	Callback	Interview

Note: This table reports marginal effects from Probit and heteroskedastic Probit regressions of equation (3.1) in Panel A and Panel B respectively. The dependent variable is a dummy variable equal to one if the application gets a positive callback for a job position in columns (1), (3) and (5), while only interview propositions are considered in columns (2), (4) and (6). The marginal effects are evaluated at sample means. Regressions in Panel A control for the required professional experience (in years). Regression in Panel B allows for the variance of unobserved characteristics to change between «Graduate» and «Dropout» as developed in Neumark [2012]. Robust standard errors are clustered at the department level and reported below the coefficients in parentheses. The null hypothesis associated to the Wald test statistic poses that ratio of standard deviations equals one. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

H The effect of the sector of the training firm

Table 3.11: Effects of certified work experience in different training firms on job interview probability

Interview (0/1)	All Applicants		
	(1)	(2)	(3)
Dropout with work experience certified in market sector	-0.0296	-0.0329	-0.0264
	(0.0217)	(0.0218)	(0.0225)
Dropout with work experience certified in non-market sector	-0.0278	-0.0261	-0.0255
	(0.0195)	(0.0194)	(0.0192)
Dropout with work experience non-certified in market sector	-0.0509***	-0.0493***	-0.0520***
	(0.0149)	(0.0150)	(0.0154)
Dropout with work experience non-certified in non-market sector	-0.0670***	-0.0675***	-0.0701***
	(0.0122)	(0.0121)	(0.0121)
Dropout with inactivity	-0.1711***	-0.1703***	-0.1719***
	(0.0127)	(0.0125)	(0.0128)
Observations	6,453	6,453	6,453
R-squared	0.0154	0.0314	0.0626
Graduate mean	.2517	.2517	.2517
Pval of $\beta_{W,C,M} = \beta_{W,C,NM}$	.9510	.8218	.9766
Pval of $\beta_{W,C,M} = \beta_{NC,M}$	.4001	.5215	.3361
Pval of $\beta_{W,C,M} = \beta_{W,NC,NM}$	.1305	.1659	.0893
Pval of $\beta_{W,C,M} = \beta_I$	0	0	0
Pval of $\beta_{W,C,NM} = \beta_{NC,M}$	.2921	.2868	.2239
Pval of $\beta_{W,C,NM} = \beta_{W,NC,NM}$	.0744	.0592	.043
Pval of $\beta_{W,C,NM} = \beta_I$	0	0	0
Pval of $\beta_{W,NC,M} = \beta_{W,NC,NM}$	.3887	.3323	.3391
Pval of $\beta_{W,NC,M} = \beta_I$	0	0	0
Pval of $\beta_{W,NC,NM} = \beta_I$	0	0	0
Job offer characteristics	No	Yes	Yes
Month & Department FE	No	No	Yes

Note: This table reports OLS estimates of equation (3.1) by dis-aggregating the (≪Worker Dropout≫) variable according to whether job-related experience was certified with a national diploma or not and by the sector in which the training firm operated (market sector vs non-market sector). The dependent variable is a dummy variable equal to one if the application gets a proposition for a job interview. Job offer characteristics include all the characteristics listed in Table 3.8 in Appendix D. Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

I Robustness on the effect of labor market characteristics

Table 3.12: Effects of certified work experience on job interview probability in different labor markets

Interview (0/1)	Unemployment rate	Job tightness	Share of dropouts
	(1)	(2)	(3)
Dropout with job-related work experience certified	-0.0396*	-0.0055	-0.0438**
	(0.0203)	(0.0218)	(0.0191)
Dropout with work experience certified × Above Median	0.0297	-0.0399	0.0361
	(0.0282)	(0.0293)	(0.0286)
Dropout with job-related work experience non-certified	-0.0656***	-0.0488***	-0.0717***
	(0.0138)	(0.0139)	(0.0155)
Dropout with work experience non-certified × Above Median	0.0095	-0.0243	0.0221
	(0.0199)	(0.0195)	(0.0199)
Dropout with inactivity	-0.2073***	-0.1347***	-0.2073***
	(0.0160)	(0.0166)	(0.0167)
Dropout with inactivity × Above Median	0.0792***	-0.0764***	0.0766***
	(0.0224)	(0.0264)	(0.0234)
Observations	6,453	6,453	6,429
R-squared	0.0640	0.0635	0.0631
Graduate mean	.2517	.2517	.2517
Pval of $\beta_{W,C} = \beta_{W,NC}$	.2705	.0588	.208
Pval of $\beta_{W,C} = \beta_I$	0	0	0
Pval of $\beta_{W,NC} = \beta_I$	0	0	0
Job offer characteristics	Yes	Yes	Yes
Month & Department FE	Yes	Yes	Yes

Note: This table reports OLS estimates of equation (3.1) by dis-aggregating the («Worker Dropout») variable according to whether job-related experience was certified with a national diploma or not, and by interacting those variable with local labor market characteristics separately for the unemployment rate in column (1), the level of job tightness in column (2), and the share of dropouts among workers in column (3). These characteristics are defined identically to Table 3.5 in Section 3.4.4. The dependent variable is a dummy variable equal to one if the application gets a positive callback to a job offer. Job offer characteristics include all the characteristics listed in Table 3.8 in Appendix D. Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

J Unsolicited applications

Table 3.13: Effects of certified job-related work experience on job interview probability with unsolicited applications

Outcome	Callback		Interview	
	(1)	(2)	(3)	(4)
Dropout with job-related work experience certified	-0.0054	-0.0152	-0.0025	-0.0076
	(0.0071)	(0.0114)	(0.0069)	(0.0117)
Dropout with job-related work experience non-certified	-0.0199***	-0.0176*	-0.0149**	-0.0091
	(0.0062)	(0.0091)	(0.0057)	(0.0088)
Dropout with inactivity	-0.0329***	-0.0483***	-0.0354***	-0.0498***
	(0.0070)	(0.0106)	(0.0065)	(0.0095)
Observations	8,208	4,424	8,208	4,424
R-squared	0.0242	0.0429	0.0242	0.0426
Graduate mean	.0780	.0934	.0661	.0786
Pval of $\beta_{W,C} = \beta_{W,NC}$	.0495	.8173	.0881	.8991
Pval of $\beta_{W,C} = \beta_I$	.0009	.0064	0	.0004
Pval of $\beta_{W,NC} = \beta_I$	.0747	.0026	.0022	0
Month & Department FE	Yes	Yes	Yes	Yes
Sample restriction	No	Yes	No	Yes

Note: This table reports OLS estimates of equation (3.1) by dis-aggregating the (≪Worker Dropout≫) variable according to whether job-related experience was certified with a national diploma or not. It further includes a dummy variable indicating if the application was sent spontaneously to firms. The dependent variable is a dummy variable equal to one if the application gets a positive callback in columns (1) and (2), or a proposition for a job interview only in columns (3) and (4). Applications were sent spontaneously to firms as depicted in Section 3.4.5. Robust standard errors are clustered at the department level and reported below the coefficients. * significant at 10 percent, ** significant at 5 percent, *** significant at 1 percent.

Appendices References

Eric W. Djimeu and Deo-Gracias Houndolo. Power calculation for causal inference in social science: sample size and minimum detectable effect determination. Journal of Development Effectiveness, 8(4):125–149, 2016.

David Neumark. Detecting Discrimination in Audit and Correspondence Studies. The Journal of Human Resources, 47(4):1128–1157, 2012.

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Programme doctoral d'Économie
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Doctorat en Économie

Essais en Économie du Travail

Assurance Chômage, Aide à la Recherche d'Emploi et Contrats Aidés

Pierre Villedieu

Thèse dirigée par
Pierre Cahuc, Profesor of Economics, Sciences Po

Soutenance le 11 *Octobre 2024*

Jury

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Résumé en français

L'interaction entre les dynamiques du marché du travail et les politiques publiques constitue un axe central de recherche en économie du travail. Cette thèse explore trois des principaux leviers d'action des politiques publiques sur le marché du travail : l'assurance chômage, l'accompagnement des demandeurs d'emploi, et les dispositifs d'emplois aidés. Le premier chapitre se concentre sur l'analyse du dispositif d'« activité réduite », qui permet aux bénéficiaires de l'assurance chômage de cumuler des revenus d'activité avec une partie de leur allocation, prolongeant ainsi la durée de leurs droits. Le deuxième chapitre examine l'impact de l'accompagnement des jeunes demandeurs d'emploi au sein des missions locales en France. Nous mesurons en particulier l'influence des conseillers sur le parcours des jeunes qu'ils suivent et étudions les mécanismes à l'oeuvre. Enfin, le troisième chapitre présente une étude par correspondance (ou *testing*), dans laquelle nous évaluons comment l'expérience professionnelle acquise grâce à un emploi aidé peut améliorer les chances de jeunes décrocheurs scolaires d'obtenir un entretien de la part des recruteurs.

Ci-dessous, je décris chaque chapitre plus en détail.

Chapitre 1 : Les effets d'enfermement de l'information sur le dispositif d'activité réduite

Les dispositifs d'activité réduite destinés aux personnes occupant des emplois non réguliers jouent un rôle de plus en plus important dans les systèmes d'assurance chômage.[25] L'augmentation de l'incidence de ces formes d'emploi alternatives, telles que le travail temporaire, le travail à temps partiel, le travail indépendant, et les nouvelles formes de relations de travail émergentes sur les plateformes de « l'économie à la tâche »(ou online *gig economy*) a entraîné une hausse du recours au chômage partiel dans plusieurs pays. En France, près d'un bénéficiaire d'allocation chômage sur deux travaille pendant sa période d'indemnisation. Les

[25]Les emplois réguliers comprennent généralement les emplois permanents à temps plein ou les emplois temporaires à temps plein de longue durée.

dispositif d'activité réduite sont également répandues en Belgique, en Finlande, en Autriche et en Allemagne.[26]

En principe, les allocations perçues dans le cadre d'une activité réduite visent à inciter les demandeurs d'emploi à accepter des emplois non réguliers en attendant de trouver un emploi régulier. Cela peut augmenter l'emploi global et réduire la durée des périodes de chômage si les emplois non réguliers servent de tremplins vers des emplois réguliers. Cependant, ce dispositif peut également induire des effets d'enfermement en décourageant les demandeurs d'emploi de rechercher des emplois réguliers. Connaître l'importance relative des effets de tremplin et d'enfermement, qui conditionnent l'accès à un emploi régulier, est essentiel pour évaluer l'impact des allocations de chômage partiel sur l'offre de travail et les dépenses d'assurance chômage. Malheureusement, la sélection potentielle dans le chômage partiel des individus ayant des caractéristiques non observables corrélées à leur taux de sortie du chômage rend l'évaluation du dispositif d'activité réduite très difficile.

Pour mieux comprendre l'impact de l'activité réduite, nous avons mené une expérience contrôlée randomisée à grande échelle auprès d'environ 115 000 bénéficiaires d'allocations chômage en France, au cours de laquelle nous leur avons fourni des informations sur ce dispositif. Nous pouvons ensuite déduire l'impact de l'activité réduite sur le comportement des demandeurs d'emploi à partir du changement de leur comportement induit par la fourniture d'informations. Le choix de cette stratégie est justifié par le manque de connaissance sur le dispositif d'activité réduite parmi les demandeurs d'emploi. Une enquête réalisée par l'agence pour l'emploi (Unédic [2012]) a montré que 41,2% des demandeurs d'emploi ne connaissent pas l'existence du programme et que 33,6% en connaissent l'existence mais pas les règles. Dans notre expérience, nous avons sélectionné des individus qui sont indemnisés pour la toute première fois et qui sont au chômage depuis moins de 7 mois au début de l'expérience, afin de sélectionner des personnes moins susceptibles de connaître les règles de l'indemnisation en activité réduite.[27] Ils ont été aléatoirement répartis entre un groupe traité et un groupe témoin. Les individus affectés au groupe traité ont reçu des e-mails contenant une description du dispositif d'activité réduite. Les individus du groupe témoin n'ont reçu aucun message, tout en faisant face aux mêmes conditions en termes d'accompagnement vers l'emploi. Pour examiner comment le traitement affecte le comportement des demandeurs d'emploi, nous combinons des données administratives issues de France Travail (ex Pôle emploi) et des données d'intentions d'embauche déclarées par les employeurs[28], ce qui nous per-

[26] Voir Ek Spector [2015] et Boeri and Cahuc [2023].

[27] Les conséquences de cette sélection sur l'impact du traitement sont analysées à la lumière de nos résultats sur l'hétérogénéité de l'effet du traitement.

[28] La DPAE (Déclaration préalable à l'embauche) doit obligatoirement être adressée, au plus tôt 8

met de savoir si les individus trouvent effectivement un emploi après leur sortie du chômage. La comparaison des résultats entre les individus traités et non traités nous permet d'identifier les effets causaux de la fourniture d'informations sur le dispositif d'activité réduite.

Pour interpréter les conséquences de notre traitement, nous proposons un modèle simple de recherche d'emploi dans lequel l'activité réduite permet de combiner les revenus du travail avec une partie des allocations chômage et de prolonger la durée potentielle de l'indemnisation grâce à la partie non consommée pendant les périodes d'emploi. Ce modèle montre que la propension à travailler pendant l'indemnisation diminue avec le taux marginal d'imposition sur les revenus du travail, qui dépend non seulement de l'imposition des revenus du travail actuels, mais aussi de la durée anticipée de l'épisode de chômage et donc de la probabilité de percevoir les indemnités non consommées lors des périodes d'activité réduite. Nous montrons qu'un effet positif de notre traitement informationnel sur le recours à l'activité réduite peut être interprété comme une révision des croyances selon laquelle le taux marginal d'imposition diminue. Or, les effets d'une baisse du taux marginal d'imposition sur la recherche d'emplois réguliers peuvent transiter par deux canaux. Le premier est un effet d'*anticipation* illustrant l'impact du dispositif d'activité réduite sur le montant des allocations qu'un individu peut espérer percevoir sur l'ensemble de son épisode de chômage. Dans la mesure où la situation relative d'une personne au chômage s'améliore avec la baisse du taux marginal d'imposition - ce qui augmente son salaire de réserve et réduit l'effort de recherche d'emploi - ce canal réduit nécessairement le taux de sortie du chômage vers des emplois réguliers. De plus, le modèle montre que cet effet s'intensifie à mesure que la date initiale d'épuisement des droits approche. En effet, la prolongation de la durée des allocations chômage induite par l'activité réduite retarde l'augmentation de l'effort de recherche qui précède habituellement la date d'épuisement des droits. L'autre canal provient de l'*effet direct de l'activité réduite* sur le taux de sortie du chômage vers l'emploi régulier. Travailler durant l'indemnisation peut générer plus d'opportunités d'emploi que de rester au chômage. Mais travailler durant l'indemnisation peut aussi laisser moins de temps pour rechercher des emplois réguliers. Par conséquent, ce second canal peut soit augmenter, soit diminuer le taux de sortie du chômage vers des emplois réguliers lorsque l'activité réduite devient plus rémunératrice.

Nous constatons que la fourniture d'informations a un impact significatif et positif sur la propension à travailler durant l'indemnisation : la probabilité que les individus traités travaillent durant l'indemnisation augmente d'environ 6% trois mois après avoir reçu l'information, par rapport aux individus non traités. Nous

jours avant l'embauche (et avant la mise au travail effective du salarié) à l'Urssaf.

explorons l'hétérogénéité potentielle des effets du traitement pour voir si la fourniture d'informations a des effets de signes différents sur la propension à accepter un emploi non régulier. En suivant l'approche développée par Chernozhukov et al. [2018], nous ne trouvons aucune preuve que la fourniture d'informations ait des effets négatifs sur la propension à travailler durant l'indemnisation. Pour tous les groupes, les effets sont soit non significatifs, soit positifs, ce qui suggère que le traitement a changé les croyances des demandeurs d'emploi dont l'inattention comportementale (Gabaix [2019]) les avait conduits à sous-estimer les gains liés à l'activité réduite (i.e. sur-estimer le taux marginal d'imposition).

L'augmentation de la propension à travailler durant l'indemnisation est associée à une diminution du taux de sortie du chômage vers l'emploi autour de la date initiale d'épuisement des allocations.[29] Conformément aux prédictions du modèle de recherche d'emploi, nous constatons que la différence de taux de sortie du chômage vers l'emploi entre les personnes traitées et non traitées est la plus marquée près de la date initiale d'épuisement des droits, puis diminue.[30] L'effet est en effet significatif : une augmentation de 6% de la probabilité que les demandeurs d'emploi acceptent de travailler durant l'indemnisation 3 mois après le début du traitement est associée à une baisse de 1,5% de la probabilité qu'ils aient quitté le chômage pour un emploi régulier le dernier mois avant la date initiale d'épuisement des droits. Il est donc clair que les effets d'enfermement associés à l'activité réduite dominent dans notre contexte.[31]

Il est possible que l'activité réduite diminue la sortie vers l'emploi régulier en général, mais augmente la sortie vers des emplois réguliers de bonne qualité, caractérisés par des périodes d'emploi plus longues. Cependant, nous constatons que les personnes traitées sont moins susceptibles de connaître des périodes d'emploi longues (c'est-à-dire plus de 3 ou 6 mois) que les personnes non traitées.

En ce qui concerne le nombre total de jours indemnisés par l'assurance chômage, nous constatons que ces effets d'enfermement contrebalancent l'augmentation du travail durant l'indemnisation. Plus précisément, d'une part, le dispositif d'activité

[29] Notez que le canal d'anticipation mis en évidence par le modèle implique qu'il n'est pas possible d'utiliser le traitement informatif comme instrument pour étudier directement l'effet de l'activité réduite sur le taux de sortie du chômage, car le traitement peut modifier le taux de sortie du chômage même pour les individus qui ne travaillent pas pendant leur épisode de chômage.

[30] Comme expliqué ci-dessous, travailler durant l'indemnisation décale la date d'épuisement des allocations chômage prévue à la date d'entrée au chômage. Tout au long de cet article, par date initiale d'épuisement des droits, nous entendons la date d'épuisement prévue au début de la période de chômage.

[31] Dans notre analyse empirique, l'effet d'« enfermement »fait référence au fait que le traitement a conduit les demandeurs d'emploi à travailler plus fréquemment durant l'indemnisation (c'est-à-dire dans des emplois non réguliers, selon notre définition, présentée en Annexe C) que ceux du groupe de traitement *et* à rester plus longtemps au chômage en tant que bénéficiaires d'allocations chômage.

réduite incite les demandeurs d'emploi à travailler durant l'indemnisation, ce qui réduit le nombre de jours indemnisés pendant la période de chômage, mais, d'autre part, elles retardent la sortie vers l'emploi régulier.[32] Au final, les dépenses nettes d'assurance chômage sont restées inchangées sur l'ensemble de la période d'analyse.

Bien qu'il soit possible que la fourniture d'informations sur l'activité réduite ait amélioré le bien-être des individus traités en leur permettant de faire des choix mieux informés, nous montrons, sur la base du modèle théorique, que son impact sur le bien-être est ambigu. Cela dépend de l'impact des paramètres du dispositif d'activité réduite sur les anticipations des demandeurs d'emploi concernant l'état du marché du travail. Par exemple, le modèle montre que des règles d'indemnisation de l'activité réduite plus généreuses peuvent réduire l'utilité anticipée des demandeurs d'emploi et entraîner plus de travail durant l'indemnisation si cela est interprété comme un signe de rareté des emplois réguliers. Par conséquent, en l'absence d'informations précises sur les anticipations des demandeurs d'emploi, l'évaluation plus approfondie de l'impact de l'assurance chômage sur leur bien-être devra faire l'objet des recherches futures.

Chapitre 2 : Le rôle des conseillers dans l'insertion professionnelle des jeunes demandeurs d'emploi : le cas de la France

Les politiques actives du marché du travail ont suscité une attention considérable au cours des 40 dernières années, avec des résultats mitigés, en particulier pour les jeunes [Caliendo and Schmidl, 2016, Card et al., 2018, Kluve et al., 2019]. Plus que la nature des politiques elles-mêmes, ce sont leur conception et leur mise en oeuvre qui semblent déterminantes. Les conseillers, en tant qu'intermédiaires essentiels dans le cadre de ces programmes, jouent un rôle crucial [Rosholm, 2014]. Des études récentes ont démontré une forte hétérogénéité dans l'impact des conseillers sur le retour à l'emploi [Schiprowski, 2020, Cederlöf et al., 2021, Dromundo and Haramboure, 2021, Rasmussen, 2021].

En 2022, dans l'Union européenne, seuls 20% des jeunes demandeurs d'emploi déclaraient avoir reçu un soutien utile de la part des services publics de l'emploi (SPE).[33] Cependant, le contexte français est potentiellement différent : entre 66% et

[32] Notez encore une fois que ce résultat est conforme à la prédiction du modèle de recherche d'emploi.

[33] Voir *Statistics on young people neither in employment nor in education or training* d'Eurostat.

80% des jeunes NEET (ni en emploi, ni en éducation, ni en formation) cherchent de l'aide auprès des SPE pour bénéficier de l'accompagnement des conseillers [Borel, 2022, Arnoult and Ghrairie, 2022]. En France, des organismes dédiés spécifiquement aux jeunes sont connus sous le nom de « missions locales ». Elles comprennent environ 437 centres principaux et 7 000 agences affiliées, avec un effectif total de 10 000 agents répartis à travers la France. Ces centres ciblent les NEET âgés de 16 à 25 ans, fournissant un accompagnement dans la recherche d'emploi à travers des entretiens individuels et des ateliers collectifs dirigés par des conseillers. Ces structures offrent également un soutien plus global sur des problématiques liées à la santé, le transport, le logement, etc. Chaque année, plus d'un million de jeunes sont en contact au moins une fois avec un conseiller de mission locale, et 80% des jeunes déclarent recevoir un soutien utile lors de leur accompagnement [UNML, 2021].

Le manque de connaissance sur les entreprises et les opportunités de carrière, en particulier pour les jeunes peu qualifiés et sans réseau personnel, constitue un obstacle majeur à l'insertion professionnelle [Kramarz and Skans, 2014, Lo Bello and Morchio, 2022]. Les analyses empiriques montrent que les jeunes NEET recherchent généralement de l'aide lorsque leurs perspectives sur le marché du travail se dégradent.[34] Toutefois, après leur inscription dans une mission locale, leur situation semble s'améliorer nettement, comme en témoigne l'augmentation du nombre total de jours d'emploi ou de formation.

Cette étude s'intéresse à deux questions centrales : L'insertion professionnelle des jeunes suivis par une mission locale dépend-elle du conseiller attribué ? Si oui, par quels mécanismes cette influence s'exerce-t-elle ?

Nous nous appuyons sur des données administratives très détaillées permettant d'analyser l'impact de l'accompagnement fourni par les conseillers sur l'intégration des jeunes sur le marché du travail. Afin d'identifier cet effet, nous tirons parti de l'affectation quasi-aléatoire des conseillers aux jeunes dans les agences des missions locales les plus importantes. Les conseillers se relaient selon un planning établi des semaines ou des mois à l'avance pour accueillir les jeunes lors de leur premier accueil à l'agence. Par défaut, le conseiller rencontré lors de ce premier entretien devient le conseiller *référent* du jeune, en charge de la supervision et de l'accompagnement du jeune tout au long de son parcours de recherche d'emploi.

Les informations de terrain ainsi que les tests quantitatifs suggèrent que l'affectation entre les conseillers et les jeunes est aléatoire dans les missions locales les plus importantes. En effet, nous ne détectons aucun schéma discernable dans la répartition des premiers entretiens effectués par les conseillers au sein d'une agence. Nous montrons également que les caractéristiques des conseillers ne sont

[34] Voir la Figure 2.15 en Annexe A.

pas corrélées avec celles des jeunes au sein d'une agence et d'une période donnée. Des réunions avec des personnes travaillant au sein de la mission locale de Paris nous ont permis de préciser l'organisation des premiers accueils : les conseillers établissent plusieurs semaines en avance le calendrier des permanences de premiers accueils en fonction des horaires de leurs activités déjà planifiées et du nombre total de jeunes qu'ils assistent déjà (leur charge de travail). Bien que certains conseillers puissent éventuellement préférer certains jours plutôt que d'autres, en fonction des caractéristiques des jeunes qu'ils s'attendent à rencontrer, ils ne choisissent pas parmi les jeunes qui viennent réellement ces jours-là.

Comme Adjeoda [2021] a pu le noter dans le contexte de la mission locale de Paris, les pratiques et activités des conseillers varient considérablement, même au sein de la même agence. Simultanément, nous observons également des différences dans l'accès des jeunes en emploi ou en formation. Ainsi, nous profitons de l'affectation quasi-aléatoire des conseillers pour étudier leur impact causal sur les trajectoires professionnelles des jeunes qu'ils accompagnent.

Concrètement, nous mesurons la capacité des conseillers à faciliter l'entrée des jeunes sur le marché du travail ou en formation en évaluant leur *valeur ajoutée* en termes de jours d'emploi ou de formation sur une période de deux ans suivant le premier accueil dans une mission locale. En supposant que les jeunes ont des résultats potentiels similaires sur le marché du travail, en fonction de l'agence et de la période de la première rencontre, nous pouvons estimer des scores de valeur ajoutée non biaisés pour chaque conseiller en matière d'emploi ou de formation. Pour confirmer que nos résultats ne sont pas biaisés par des différences *a priori* dans les chances qu'ont les jeunes de trouver un emploi ou d'entrer en formation, nous effectuons des tests placebo dans lesquels nous mesurons « l'effet »des conseillers sur le parcours des jeunes *avant* qu'ils ne se présentent à la mission locale. De manière rassurante, nous ne détectons aucun lien significatif entre la valeur ajoutée des conseiller et les expériences de leurs jeunes en emploi ou en formation avant le premier accueil.

Nos résultats montrent que le fait d'être assigné à un conseiller avec une valeur ajoutée un écart-type au-dessus de la moyenne augmente le nombre total de jours d'emploi de 0,05 écart-type en moyenne sur les deux années suivantes. Cela représente une augmentation de 12 jours en termes absolus, soit une augmentation de 6% par rapport aux jeunes suivis par un conseiller qui se situe dans la moyenne de la distribution des valeurs ajoutées. Cet effet est cohérent avec les résultats de la littérature sur l'impact des conseillers sur l'insertion professionnelle des demandeurs d'emploi plus âgés. L'effet absolu est néanmoins plus faible, ce qui illustrent les nombreux obstacles auxquels les jeunes NEET doivent faire face pour trouver un

emploi [Jellab, 1998, Babcock et al., 2012].

L'accompagnement des conseillers ne se limitant pas à la seule recherche d'emploi, nous examinons également l'impact des conseillers sur les entrées en formation, souvent considérées comme un tremplin pour les populations les plus éloignées du marché du travail [Escudero and Mourelo, 2017]. Nous mesurons que le fait d'être assigné à un conseiller se situant un écart-type au-dessus de la moyenne dans la distribution de la valeur ajoutée sur la formation entraîne une augmentation de 0,03 écart-type du nombre total de jours de formation, soit une hausse d'environ 8,5%. Nous constatons en outre que les conseillers ayant le plus fort impact sur les entrées en emploi n'affectent pas particulièrement l'entrée en formation, et vice versa. Nos résultats suggèrent ainsi que les conseillers semblent favoriser *a priori* l'une ou l'autre de ces approches dans l'accompagnement des jeunes, confirmant ainsi les observations de l'approche qualitative menée par Adjeoda [2021].

Nous examinons également l'hétérogénéité des effets des conseillers afin d'identifier les sous-population pour lesquelles l'influence des conseillers est la plus notable. Les hommes bénéficient principalement des conseillers ayant une forte valeur ajoutée sur l'emploi, tandis que les femmes profitent davantage des conseillers ayant un impact accru sur l'entrée en formation. De plus, nous constatons que les jeunes sans diplôme ne semblent pas bénéficier de l'effet des conseillers ayant une forte valeur ajoutée sur l'emploi mais qu'ils sont les plus affectés par les effets sur la formation. Concernant la situation du marché du travail local, les effets des conseillers qui améliorent l'accès à l'emploi se manifeste principalement dans les zones ou le taux de chômage est relativement élevé.

En ce qui concerne les mécanismes sous-jacents, nous examinons d'abord comment les conseillers diffèrent dans leurs pratiques avec les jeunes qu'ils accompagnent. Globalement, nos résultats suggèrent que c'est le contenu des interactions qui semble déterminant, plus que leur quantité. En effet, on n'observe aucune différences notables dans le nombre d'entretiens réalisés ou le nombre de contacts digitaux (mails, SMS) entre les conseillers selon leur valeur ajoutée. Nous constatons que les jeunes suivis par les conseillers ayant le plus fort impact sur l'emploi ont légèrement plus de chances d'être embauché en CDI. Ils reçoivent également plus de propositions directes d'emploi sans que cette différence ne suffise pas à expliquer le différentiel avec les conseillers à plus faible valeur ajoutée. Concernant le placement en programme, la principale différence concerne les entrées en *Accompagnement Intensif des Jeunes* (AIJ), en partenariat France Travail (ex *Pôle emploi*).

En ce qui concerne la formation, nous constatons que les conseillers à plus forte valeur ajoutée affecte la probabilité de s'inscrire à des programmes de formation menant à une certification nationale, même si les différences restent faibles en termes

absolus. Ils proposent également plus d'opportunités de formation et ont un impact accru sur la contractualisation du parcours des jeunes au sein de la mission locale. Enfin, les jeunes suivis par les conseillers à plus forte valeur ajoutée participent à plus d'ateliers collectifs, favorisant l'assimilation des informations ainsi que des compétences de bases.

Enfin, nous explorons l'impact de différents scénarios de réaffectation des conseillers sur l'insertion des jeunes ainsi que les recettes fiscales de l'État. Afin de mesurer des effets de traitement conditionnels sur l'ensemble des caractéristiques observées des jeunes, nous utilisons l'algorithme d'apprentissage automatique développé par [Wager and Athey, 2018]. Les résultats montrent une hétérogénéité très importante dans l'effet des conseillers allant de -100 jours à +120 jours d'emploi. Sur la base de ces effets de traitement conditionnels, nous avons simulé deux scénarios, en comparaison de la situation où l'affectation est aléatoire. Dans le premier scénario, nous avons attribué des conseillers à forte valeur ajoutée aux jeunes ayant l'effet du traitement conditionnel le plus élevé pour maximiser le nombre de jours d'emploi. Nous trouvons qu'en moyenne le nombre total de jours en emploi passerait de 16 à 46 selon ce scénario, triplant ainsi l'effet moyen des conseillers à forte valeur ajoutée. La mise en oeuvre d'un tel processus d'assignation étant difficile dans la réalité, nous avons considéré un deuxième scénario où les conseillers à forte valeur ajoutée sont attribués aux jeunes qui i) déclarent explicitement un accompagnement pour l'emploi, ii) ont une faible probabilité de trouve de l'emploi et iii) présentent l'effet de traitement conditionnel le plus élevé. Cette réaffectation n'augmenterait l'effet moyen des conseillers à forte valeur ajoutée que de 18% par rapport au scénario de base (de 16 à 19 jours).

Chapitre 3 : Les bénéfices de l'expérience professionnelle pour les jeunes décrocheurs scolaires : Résultats d'une étude par correspondance

Les jeunes quittant le lycée sans diplôme, appelés décrocheurs, sont largement surreprésentés parmi ceux qui ne sont ni en emploi, ni en éducation, ni en formation (NEET). En 2021, le taux de décrocheurs parmi les NEETs était de 37% aux États-Unis, contre 16% pour les diplômés du secondaire. Dans les pays de l'OCDE, ces chiffres étaient respectivement de 36,9% et 14,3%.[35]

Cependant, les employeurs ne perçoivent pas nécessairement les décrocheurs

[35]Voir le National Center for Education Statistics et Giret and Jongbloed [2021] pour plus de détails.

comme étant complètement en marge du marché du travail. Une enquête menée en 2019 auprès d'employeurs français recrutant des jeunes pour des emplois peu qualifiés montre que la perception des décrocheurs n'est généralement pas négative [Beffa and Broc, 2019]. Les employeurs ne les considèrent pas nécessairement comme des individus ayant échoué ou manquant de compétences fondamentales telles que l'écriture, la compréhension orale ou les mathématiques. Au contraire, deux tiers des employeurs les voient comme des jeunes cherchant des opportunités sur le marché du travail et désireux d'acquérir des expériences professionnelles spécifiques. Plusieurs études montrent enfin que les raisons de quitter l'école sont variées [Hoxby and Avery, 2013, Arcidiacono et al., 2016, Guyon and Huillery, 2021, Kirkeboen et al., 2016, Clemens et al., 2021, Dahl et al., 2022], ce qui fait des décrocheurs un groupe hétérogène en termes d'expériences personnelles et professionnelles.

Cette étude explore si une expérience professionnelle en lien avec l'emploi ciblé peut améliorer les perspectives d'emploi des décrocheurs. Compte tenu de l'existence de nombreux programmes publics conçus pour soutenir les jeunes en difficulté, il est essentiel de mesurer l'efficacité de ces initiatives en tant que tremplins pour les jeunes quittant l'école. Nous nous intéressons en particulier à la perception des recruteurs comme l'un des mécanismes clés par lequel l'expérience professionnelle peut influencer les trajectoires professionnelles des décrocheurs.

Pour étudier cette question, nous nous appuyons sur une étude par correspondance (ou *testing* menée en France. Entre janvier et juillet 2018, nous avons soumis environ 6 400 candidatures de candidats fictifs à des offres d'emploi réelles publiées en ligne. De plus, entre octobre et décembre 2018, nous avons envoyé environ 8 200 candidatures spontanées à d'autres entreprises employant des personnes dans les mêmes professions. Les candidats fictifs étaient des jeunes hommes de 18 ans ayant soit obtenu un diplôme d'enseignement professionnel de deux ans pertinent pour les métiers ciblés, soit quitté l'école après le collège. Tous les décrocheurs ont été inactifs pendant la première année après leur sortie de l'école (âgés de 16 à 17 ans), mais leur expérience professionnelle au cours de la deuxième année (âgés de 17 à 18 ans) a été déterminée aléatoirement : un groupe est resté inactif, tandis que l'autre a acquis une année d'expérience professionnelle dans les métiers ciblés.

Notre expérience a porté sur deux métiers, cuisinier et maçon, choisis pour leur importance parmi les jeunes en formation professionnelle et les décrocheurs, à la fois en France et en Europe. Ces métiers comptent également parmi ceux où la part de travailleurs bénéficiant d'emplois aidés est relativement grande. Les candidats fictifs ayant acquis une expérience professionnelle l'ont fait dans le cadre du programme national « Emplois d'Avenir », en vigueur entre 2012 et 2018. Ce programme ciblait les jeunes peu diplômés de 16 à 25 ans sans diplôme ou peu qualifiés. En contre-

partie de subventions salariales, les employeurs devaient proposer aux jeunes une formation professionnelle qui pouvait faire l'objet d'une certification.[36] Cette certification a également été randomisée au sein des profils de décrocheurs ayant acquis une expérience professionnelle.

Les diplômés et les décrocheurs ayant une expérience professionnelle ont signalé des compétences identiques sur leur CV, couvrant à la fois des aptitudes générales mais aussi techniques. Dans leurs lettres de motivation, les candidats ont détaillé l'acquisition de ces compétences, soit par le biais de leurs études, soit de leur expérience professionnelle. En revanche, les décrocheurs restés principalement inactifs et sans expérience professionnelle directement reliée à l'emploi ciblé n'ont pas présenté de compétences spécifiques sur leur CV et ont simplement exprimé leur envie d'acquérir des compétences techniques à travers leur première expérience professionnelle.

Les résultats indiquent que le taux moyen d'entretiens proposés par les recruteurs pour les diplômés est d'environ 25%, contre 18% pour les décrocheurs. Cependant, il existe des variations significatives parmi les profils de décrocheurs : ceux ayant une expérience professionnelle ont un taux d'entretien de 20%, contre seulement 8% pour ceux restés inactifs. Le taux d'entretiens proposés diffère également parmi les décrocheurs ayant une expérience professionnelle, avec un taux de 19% en l'absence de certification et de 22% lorsque les décrocheurs signalent une certification. Dans le contexte de notre étude, les chances d'être invité à un entretien d'embauche sont ainsi divisées par trois pour des jeunes décrocheurs sans expérience professionnelle significative par rapport à des pairs ayant validé une formation professionnelle. Cet écart se réduit lorsque les jeunes acquièrent une expérience professionnelle en lien avec l'emploi ciblé. Plus précisément, une année d'expérience professionnelle réduit la pénalité d'entretien de 67% à 25%. De plus, cette pénalité se réduit encore, à 12%, lorsque les décrocheurs ont pu certifier les compétences acquises lors de cette expérience professionnelle.

Ces différences dans les taux d'entretien ne sont pas uniformes dans tous les contextes. Pour rendre compte des différences d'opportunités sur les différents marchés du travail locaux, nous croisons nos résultats avec divers indicateurs tels que le taux de chômage, le niveau de tension sur les métiers et la proportion de décrocheurs ayant une expérience professionnelle. Nos résultats indiquent que l'effet pour les décrocheurs d'avoir acquis une expérience professionnelle est d'autant plus grand que le marché du travail est tendu.

Ce résultat fait écho à plusieurs études [Lainé, 2005, Girsberger et al., 2022] qui

[36] Cahuc et al. [2019] n'ont trouvé aucune preuves de stigmatisation entre l'expérience acquise via ce programme et celle obtenue par un emploi non subventionné.

soulignent l'importance du lien entre les compétences acquises via l'expérience professionnelle et les trajectoires professionnelles des jeunes. Elles montrent notamment que pour les jeunes occupant des postes peu qualifiés, le fait d'avoir bénéficier d'une formation directement sur le terrain augmente leurs chances de rester durablement en emploi et d'avoir une progression salariale.

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