Credit Constraints and Training Subsidies for Job-Seekers: Evidence from France

Todor Tochev*

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Abstract

This paper investigates the impact of increased subsidies for vocational training on job-seekers without unemployment benefits, focusing on a 2019 reform in France. The study employs a triple-differences methodology, exploiting geographic, time, and training subsidy eligibility variation. Higher subsidies lead to more training starts. This is driven by trainings that provide an officially recognised degree and by trainings that prepare for further training. Using a novel dataset, I find that missed training hours increased in regions that only increased the monthly training subsidy. This suggests that the marginal trainee brought in by the increase was less persistent in training. This effect is almost completely mitigated in a region that also introduced an unconditional upfront training grant, suggesting at least some job-seekers entering training are credit-constrained. Returns to training estimated using a regression discontinuity design are positive, but a returns-based IV test for credit constraints is inconclusive.

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1 Introduction

Active labour market policies (ALMPs) are an essential and widely used component of employment policy. Recent meta-reviews of ALMP impact evaluations have shown that across all ALMPs, some of the highest increases in employment and wages outcomes come from job training programmes for job-seekers (Card, Kluve, and Weber, 2018). Broadly defined, job training aims to deliver specialised vocational skills and make trainees immediately employable on the labour market. Job training for adults is seen as a promising response to multiple profound changes in the labour market, from automation (Feng and Graetz, 2020) to the green transition (Chen et al., 2020). Research has shown that policy can deliver a wage premium on par with that of four-year university degrees (Card, Kluve, and Weber, 2018; Katz et al., 2020; Yeyati, Montané, and Sartorio, 2021). Consequently, several large-scale job training policies have been enacted, such as the Canadian Work and Labour Market Development Agreements (CAN$ 3.5 billion), the EU Youth Guarantee (€7.1 billion), and the French Plan d'Investissement dans les Competences (€15 billion).

However, even in countries with well-developed training policies, trainees face a variety of participation costs. These include direct costs such as equipment, books, enrolment fees, transport and childcare. Attending adult vocational education also incurs opportunity costs leading to reduced earnings and employment probability in the short term. This "lock-in" effect is due to reduced job search effort and time spent working during training spells, and is an empirical regularity observed across policy evaluations of job training programmes (McCall, Smith, and Wunsch, 2016; Schochet, Burghardt, and McConnell, 2008). At the same time, job-seekers most likely to benefit from job training, such as young low-skilled first-time entrants, are likely to be credit constrained. Financial support offered to participants is thus a first-order issue in designing job training programmes.

In this paper, we ask whether financial barriers prevent young job-seekers from taking up training opportunities in a country with well-established job training policies targeting job-seekers. Using population-wide administrative data, we present three separate empirical exercises. First, we investigate whether increasing training subsidies for job-seekers improves training take-up. To answer this question, we evaluate the effects of a reform increasing subsidy amounts for job-seekers who do not have rights to unemployment benefits and implemented in three regions in France in 2019. Using a triple-difference approach, we find that the reform led to a substantial increase in training entries. Second, in a difference-in-differences setup, we examine how the reform affected training attendance by exploiting variation in reform components across treatment regions. We find that training attendance decreased when comparing control regions with treated regions that only increased the subsidy amount. Additionally, one treated region also added an upfront unconditional one-time starting grant. This improved attendance compared to other treatment regions, an effect consistent with credit constraints. Third, we examine the labour market returns to training. A necessary but not sufficient condition for the presence of credit constraints is to have positive labour market returns to training. Using a regression discontinuity design based on training-specific admission thresholds, we estimate a lower bound on the effects of training. We find that being accepted in a training improved wages and employment in the short-term. Finally, we implement an instrumental variables (IV) test similar to Cameron and Taber (2004) and look for evidence of heterogeneity in returns to training across the different margins of participation. This test does not show evidence of credit constraints, however it is likely inconclusive due to the instruments' weak predictive power.

This paper directly contributes to two main literatures. First, it adds to the extensive literature on vocational training (for detailed reviews see McCall, Smith, and Wunsch (2016) and McNally, Ventura, and Virtanen (2022)). Most of this literature has focused on evaluating the labour market effects of secondary and post-secondary vocational training. Meta-reviews conducted on the effects of ALMPs suggest programme effects are heterogeneous to a degree which is difficult to explain solely using the observable characteristics of participants (Card, Kluve, and Weber, 2018; Crépon and van den Berg, 2016; Yeyati, Montané, and Sartorio, 2021), suggesting that the specifics of a programme's design play an important role in its effects. One such component is the financial support offered to trainees. In this paper, we focus on how the financial support offered to job-seekers affects participation in vocational training. Second, our paper is related to work on credit constraints in access to post-secondary education (Aguirre, 2021; Anderson, 2020; Barr, 2016; Card and Solis, 2022; Deming and Dynarski, 2009; Denning, 2017; Dynarski, Page, and Scott-Clayton, 2022; Sun and Yannelis, 2016). This literature has mostly examined the effects of increased provision of grants and loans for university students in the United States and Latin America. In general, this body of work finds that increasing the availability of funds, through an increase in grants or by expanding access to loans, has a positive effect on persistence, academic results, and degree completion. The effect on post-higher education financial outcomes is however ambiguous, in particular in the case of vocational programmes and for-profit institutions, as enrolment may increase in programmes with lower average earnings. This paper focuses instead on a European-style programme oriented towards rapid acquisition of vocational skills by young job-seekers, many of who have dropped out of academic education.

More generally, we contribute to the literature documenting the effect of credit market frictions on labour market allocations. A large variety of mechanisms have been suggested: occupational choice (Banerjee and Newman, 1993), employer screening (Bos, Breza, and Liberman, 2018), recruitment application costs (Abebe, Caria, and Ortiz-Ospina, 2021), job mobility (Wang, 2012). Credit constraints in training for job-seekers are an additional mechanism preventing the efficient allocation of workers to occupations.

The paper proceeds as follows. In Section 2, we describe briefly the institutional background surrounding training policy in France. Each subsequent section presents one of the three empirical exercises outlined above. Section 3 examines training take-up, Section 4 focuses on attendance, and Section 5 on the returns to training. Section 6 concludes.

2 Institutional background

2.1 Training policies and reform context

Successive French governments have responded to persistent unemployment levels by implementing a series of policies aiming at facilitating matching between workers and firms and encouraging human capital accumulation. The latest of these, the *Plan d'Investissement dans les Compétences* (PIC), was deployed over the course of 2019. As part of the PIC, €15 billion were allocated towards policies facilitating access to training, piloting new courses and delivery methods, or promoting job training. A particular emphasis was placed on programmes aimed at young (less than 26 years old) first-time entrants in the job market, individuals with relatively low degree of formal education (high school degree or less), as well as those living in high-poverty urban or in rural areas. The policy argument for this targeting is that these groups have lower levels of marketable skills (e.g. IT skills), are likely to have difficulty planning a viable training path, and may have higher training entry costs (Ferry, 2012).

Adult job training policy in France is mainly operated by two types of actors: the devolved regional administrations (the Regions), and regional branches of the public employment service (PES) Pôle Emploi. Training allocations are bought in bulk through public procurement auctions at two to four year intervals. The types and volumes of training tendered are chosen in advance according to local job market needs by the regions and the PES, in consultation with local stakeholders. Training is delivered by private providers, who are paid according to the hours of training effectively delivered (i.e. taking into account attendance). Private providers are free to choose whether to accept a potential trainee's application, based on criteria such as admission test scores, training "alignment" with the applicant's previous work experience, motivation for and knowledge of the occupations targeted by the training programme.

In this paper, we consider three types of training undertaken by job-seekers. First, *degree-granting* courses are intended as terminal education options preparing for a well-defined occupation. To this end, they deliver a degree recognised by the Ministry of Labour, and hence require passing a certification examination. Second, *non-degree-granting* trainings may award a certificate of completion or other document certifying attendance, but are not officially recognised by the Ministry of Labour. These impart less specialised skills, which may be useful in several different occupations within the same industry. Third, *preparatory* training courses aim to provide a broader foundation of skills relevant to an industry (as opposed to specialised occupations) and facilitate entry into further training. Training attendance modes vary considerably across courses, however a typical structure for degree-granting trainings has trainees attend class- or workshop-based sessions on a specialised campus for an extended duration of time (several months to a year), followed by another two to three months of a firm-based internship. Non-degree-granting trainings and preparatory trainings are typically campus-based, but may involve a short job-shadowing component.

Jobseekers can fund the cost of training in two main ways. The first is through a referral by their counsellor at the PES, in which case the training tuition fee is bourne by the Regions and the PES. Prospective trainees can also decide to fund their training entirely on their own, for example if the course they are interested in is not deemed necessary for their career by their counsellor. In 2018, self-funded training represents approximately 10% of training starts, with the rest being funded by regions or the PES. Surveys of French job-seekers have consistently pointed towards financial constraints as one of the salient barriers to training (Aude and Pommier, 2013).

Jobseekers undertaking job training receive one of two types of ongoing financial support, depending on their eligibility for unemployment benefits. Those eligible for unemployment benefits continue to receive their monthly benefit amount, which is itself a fixed replacement rate function of their pre-unemployment wages. Jobseekers who are not eligible for unemployment benefits receive financial support under the form of monthly training support grants. The grant amount is *fixed in absolute terms* by administrative decree at the national level, depends on trainee characteristics, and is not automatically adjusted for inflation. The grant is similar to a conditional cash transfer, as it is paid out conditional on training attendance as verified by the training provider and reported to the relevant funding body (region or PES).

Qualitative research on financial barriers to job training take-up in France has repeatedly highlighted that job-seekers without rights to unemployment benefits face a sudden consumption drop immediately after entering training. This drop is created by two factors. First, there are large upfront costs associated with entering into training. Second, there is an administrative delay associated with the switchover from the minimum income benefit they receive prior to entering training. This delay occurs due to the administrative validation required for the receipt of the first training subsidy instalment. This validation requires having certified at least some training attendance, as well as a complete application file. The first requirement is subject to monthly data transmission schedules between the private training providers and the public funding bodies, whereas the second requires trainees to procure a list of documents such as evidence of previous employment history and bank account information. Consequently, job-seekers without a right to unemployment benefit keep receiving the possibly much lower minimum income benefit, while having to fund larger ongoing costs. This creates an incentive for trainees to prioritise income-generating activities such as part-time work over training attendance early on in the training process.

2.2 Training subsidy reforms

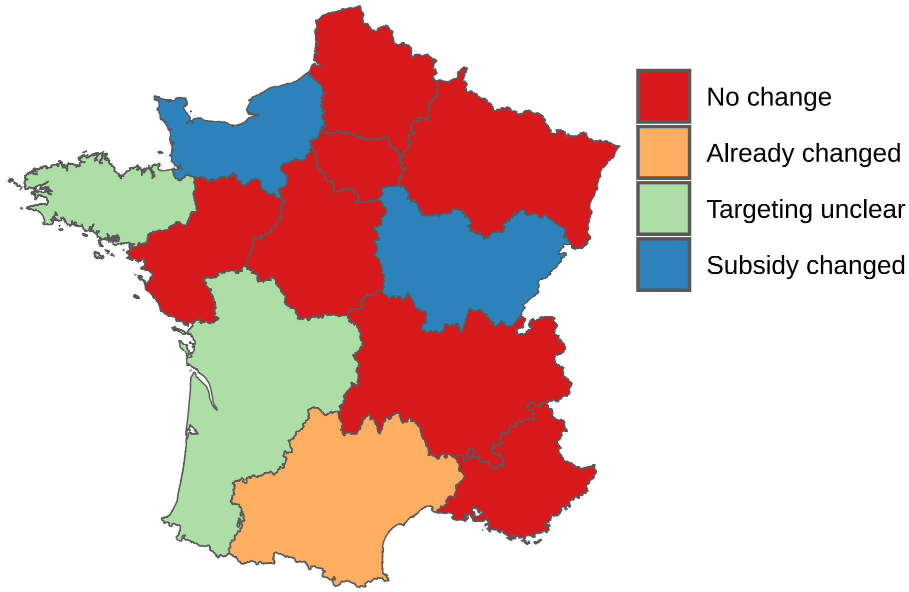
Subsidy amount changes An important specificity of the PIC policy was that the majority of allocated funding was channelled towards financing reforms and policy experiments proposed by the regions themselves. The *reform* regions Bourgogne-Franche-Comté and Normandie chose to use part of these funds to increase the fixed subsidy amounts given to job-seekers ineligible for unemployment benefits. The reform did not change other non-monetary benefits for trainees, such as social security or healthcare. The change was introduced in September 2019.

Appendix Table A1 presents the pre-reform subsidy amounts and the change in the subsidy for each category of job-seeker. Prior to 2019, the subsidy amount specified in the decree was last changed in 2002. At the time of the reform, trainees who received the fixed subsidy amount comprised approximately half of trainees. Overall the change brought fixed subsidy amounts for training to a minimum of €652 per month across all three implementing regions. The change was largest for young (16 to 25 year old) ineligible job-seekers, who were receiving the smallest subsidy amounts before the reform. For example, an individual aged 22 with less than six months of job experience and no dependents in full-time training would have experienced an increase from €339 per month (28% of the net minimum wage) to €652 per month (54% of the net minimum wage). In May 2021 the same subsidy reform was generalised by the central government to all trainees. Figure 1 shows the geographical distribution of training subsidy increases.

The *placebo* region Occitanie entered the study period with training subsidy levels already higher than the pre-reform national level, and these were targeted broadly to the same types of job seekers as the increase we are studying. Instead of opting for an increase in subsidy, Occitanie decided to use its allocated PIC funds for other policies, such as adding additional behavioural training modules to preparatory training courses. Furthermore, it is a region which historically has been strongly engaged in promoting job training policies. Given there was no differential increase in training subsidies in this region, we will use it as an alternative control in the estimations that follow.

Finally, we exclude two regions from the dataset. Nouvelle-Aquitaine increased subsidy, the

Figure 1: Subsidy change status by region, training subsidy reform in 2019



targeting of the reform is *unknown*, as we were unable to precisely determine the population and training courses to which the reform was applied. We thus drop this region from the study sample.

Upfront unconditional training grant As outlined in Subsection 2.1, the administrative delays associated with subsidy disbursement for job-seekers without unemployment benefit rights entering training create incentives for trainees to skip training sessions. In order to tackle this, the Bourgogne-Franche-Comté region implemented an additional reform component. As part of the training subsidy increase outlined in the previous subsection, new cohorts entering training from September 2019 onwards received a €200 unconditional transfer immediately after the start of their training spell. This transfer is *in addition* to the subsidy amount for the first month of training, and was not subject to claw-back in case of trainee drop-out.

2.3 Training admission procedure and admission thresholds

As part of our exploration of credit constraints in the subsequent sections of this paper, we will exploit the existence of selection tests administered by the Afpa to obtain an estimation of the labour market returns of training. In this subsection, we describe the framework of the admission procedure as it was implemented in the period covered by our training admission sample (Q2 2015 to Q4 2016).

Admission to training is determined by two broad components. First, potential applicants are required to pass a logic test and a problem-solving test, as well as a set of training-specific tests (e.g. English as a foreign language, knowledge of accounting procedures, a basic computer literacy test). Second, they are interviewed by a guidance councillor, with the aim of assessing the less quantifiable qualities of the applicant, such as motivation, the fit between the training and the applicant's previous work and educational background, and the extent to which the applicant understands the occupations the training will prepare them for. The final admission decision takes into account quantitative test results and the qualitative elements collected during the interview.

We focus on the logic and problem-solving tests. These have quantitative course-specific thresholds for admission, and are used across a wide array of trainings. All test questions are multiple choice, and there is a unique correct answer for each question. Logic tests are similar to general-purpose intelligence tests, while problem-solving tests aim to assess elementary (middle-school level) arithmetic and geometry skills. Each training has an associated logic and problem-solving threshold. Thresholds are calibrated according to the complexity of each curriculum, such that the dropout rate during the training is less than 20%. We consider that the thresholds are exogenous to the training applications, as the they were set at the national level more than three years before the earliest training applications in our sample were made.

3 The effect of increasing training subsidies on training start rates

In this section, we utilize the increase in training subsidy amounts for unemployed without a right to unemployment benefits implemented by the two regions in France described above to identify the effect of increases in financial support on training take-up.

3.1 Data

We use data from two main administrative databases. For information on unemployment registrations, individual characteristics, and entries into training, we rely on administrative data collected by the PES and the French Ministry of Labour.

Unemployment spells and job-seeker characteristics The first dataset we use is the Fichier Historique Statistique (FHS). This dataset covers all unemployment spells registered with the PES since 2010. Unemployment registrations are mandatory for jobseekers who wish to receive unemployment benefits, as well as for those who wish to benefit from job search assistance. The data is reported at the spell level. For each spell, we have information on its start and end dates, the type of unemployment registration (whether the jobseeker is required to provide evidence of job search), and the type of unemployment benefit received (full unemployment benefits or social minimum). Spell records also contain information on job-seekers' socio-demographic characteristics: age, sex, number of children, matrimonial status, highest level of education attained, *commune* of residence, nationality, and whether the person is resident in a rural area or a high-poverty urban area. This individual-level data is updated at the start of each employment spell. We follow standard PES practice and correct for short term gaps[1] in unemployment spell registration by merging spells for the same individual whose respective end and start dates are less than 15 days apart.

Training starts We obtain information on training starts from the Base Régionalisée des Stagiaires de la Formation Professionnelle (BREST). This dataset lists all training spells in France that have begun and that have been at least partially financed by either a region or the PES. The unit of observation is the training spell. For each spell, we observe the start date, the expected end date, the type of training, and the planned number of hours. This database does not contain any information on training attendance nor completion, and indicated end dates are expectations based on usual course length at start but without further verification.

The data covers the period between May 2017 and April 2021 included. The geographical scope is mainland France excluding Corsica. We drop the Nouvelle Aquitaine region from the sample, as it was not possible to determine the selection of training courses for which the training subsidy increase was implemented. We focus on individuals aged 16 to 30 who were registered as full-time jobseekers with the PES.

The dataset is built as follows. We first create an individual by month panel of unemployment registrations, unemployment benefit eligibility, and entry into training. This panel is then aggregated to the month by cell level, where each cell corresponds to a distinct combination between département[2] of residence and socio-demographic characteristics. These characteristics are age, being a married female, being an unmarried female, being a foreign national, living in a rural area, living in high-poverty urban neighbourhood, highest educational attainment (high school diploma or less), pre-unemployment salary quartile, and right to unemployment benefits. Finally, we aggregate to a cell by admission cycle year, where the admission cycle year is from May to April included[3]. The reason for this final aggregation is the strongly pronounced seasonality of training starts, a large majority of which occur during September, October and November. Finally, we obtain a balanced panel by dropping cells which are not observed in all periods. We thus omit 5.16% of all cells.

[1] These gaps are due to changes to jobseekers' status, for example due to lapses in mandatory registration renewals, or very short term contracts.

[2] Administrative subdivision immediately below region.

[3] For example, the admission cycle year 2018 covers May 2018 to April 2019 included.

The main outcome of interest is the rate of jobseekers who start at least one training. We calculate this outcome in three steps. First, at the month by individual level, we calculate an indicator variable for whether that individual has started at least one training course during that month. Second, at the group i by month m level, we calculate the total number of jobseekers entering training (using the indicator variables from the previous step) and the total number of jobseekers, and take the ratio of the two. Finally, we take the mean of the ratio over the months in a given admission year t:

$$Y_{it} = \frac{10000}{12} \times \sum_{m \in t} \frac{\text{number of individuals entering at least one training}_{im}}{\text{number of jobseekers}_{im}}$$

The rescaling by a factor of 10 000 is done to improve the readability of the estimated coefficients. The final measure is thus the average number of individuals entering at least one training per 10 000 eligible jobseekers. The measure is calculated for all training entries as well as by the main subtypes of training (degree-granting, non-degree-granting, and preparatory). Finally, we use a similar procedure to calculate hours of training conditional on having started at least one new training of the corresponding type.

Table 1: Summary statistics for training starts data

Variable	Cell Count	Mean	SD
Socio-demographic variables			
Aged 16 to 25	368,528	0.23	0.42
No right to unemployment benefits	368,528	0.48	0.50
Married female	368,528	0.22	0.42
Single female	368,528	0.28	0.45
Male	368,528	0.49	0.50
Lives in high poverty urban area	368,528	0.13	0.34
Lives in rural area	368,528	0.10	0.31
High-school degree or less	368,528	0.70	0.46
Foreign national	368,528	0.15	0.36
Rate of new trainees per 10 000 job seekers			
Any training	368,528	19.47	34.17
Any degree-granting training	368,528	14.12	27.46
Any non-degree-granting training	368,528	3.02	10.76
Any preparatory training	368,528	2.37	14.69
Hours of training conditional on training entry			
Total hours of training	96,934	908.01	674.26
Total hours of degree-granting training	96,934	733.55	714.60
Total hours of non-degree-granting training	96,934	104.31	307.84
Total hours of preparatory training	96,934	70.14	186.86

Note: The data cover the period from May 2017 to April 2021. Summary statistics are weighted by average yearly cell size. Rates of training are monthly averages over admission years.

Table 1 shows the mean and standard deviation for the controls and main outcome variables in the constructed dataset. The sample is almost evenly split between individuals aged 16 to 25 and those aged 26 to 30, and it is also evenly split along gender lines. Approximately 40% of job-seekers have no right to unemployment benefits, and the majority have at most a high school degree. Training entry is relatively rare, with an average rate of 70 entrants per 10 000 registered job-seekers per month. Degree-granting training is by far the most popular form of training, with non-degree-granting and preparatory training being much rarer.

We also provide some contextual information on the differences between the three groups of regions we consider, calculated over the course of the pre-reform period. This is shown in Appendix Table A3, where we compare the mean differences between cells across the three groups of regions, weighted by the number of job-seekers in each cell. Treated and pure control regions had similar pre-reform unemployment rates and GDP per capita, but Occitanie had a higher unemployment rate than the two other groups. Occitanie and control regions had a higher proportion of job-seekers who are of foreign nationality. However, they also have a higher proportion of job-seekers who reside in high-poverty urban areas, and a lower proportion of job-seekers who reside in rural areas. There are no significant educational or gender differences across regional groups. Examining the outcome variables, it is clear that there are already important level differences, as training rates in reform regions are already 43% higher, driven by degree-granting and preparatory trainings. This further confirms the appropriateness of our chosen estimation strategy, as it employs treatment group by region fixed effects to correct for these baseline differences. Notwithstanding, we include the examine covariates in our estimations.

3.2 Estimation strategy

As outlined in Section 2.1, the change in training subsidy amounts that occurred in the reform regions targets individuals differentially depending on their demographic characteristics. The reform targeting naturally leads to a triple difference-in-differences (DDD) identification strategy, where we exploit three sources of variation. First, only two regions implemented the subsidy increase. Second, the subsidy amount was increased differentially depending on the demographic characteristics and unemployment benefit eligibility of job-seekers. Finally, the reform was implemented simultaneously in May 2019. The DDD estimation relies on a parallel trends assumption: within the set of treated regions, the average difference in untreated potential outcomes among groups with different subsidy increase amounts should be the same.

We estimate two specifications. The *static* specification, where the time variable is a binary indicator for the time periods after the start of the reform in May 2019, recovers the average effect of the reform on the targeted group in the treated regions. The *event-study* specification defines time as years relative to the reform start. The event study recovers the dynamic effects of the reform, and allows us to examine the timing of the effects and to what extent the parallel trends assumption holds prior to the reform. If our estimates show an effect which starts before the subsidy increase, this would suggest that the evolution of targeted and non-targeted groups was diverging across reform and non-reform regions prior to the subsidy change. This would be evidence against the parallel trends assumption underpinning the triple-difference estimation strategy.

We use a continuous treatment measure. At the individual level, this is the change in the subsidy amount, before compared to after the reform, had they been present in a reform region and conditional on individual characteristics. Since the cells in our dataset are formed based on demographic characteristics, the change in the subsidy amount will be the same for all individuals belonging to the same cell.

We estimate the following static DDD specification:

$$\begin{aligned} Y_{igrt} = & \beta_{DDD}\, \mathbf{1}_t^{\text{Post}} \times \mathbf{1}_r^{\text{Treated}} \times \Delta\text{subsidy}_g \\ & + \gamma_{gt}\, \mathbf{1}_t^{\text{Post}} \times \Delta\text{subsidy}_g + \gamma_{rt}\, \mathbf{1}_t^{\text{Post}} \times \mathbf{1}_r^{\text{Treated}} + \gamma_{rg}\, \mathbf{1}_r^{\text{Treated}} \times \Delta\text{subsidy}_g \\ & + \mathbf{X}'\boldsymbol{\beta} + \nu_g + \rho_r + \xi_t + \epsilon_{igrt} \end{aligned} \tag{1}$$

where Y_{igrt} denotes the outcome variable observed in socio-demographic cell i belonging to group g and located in region r during admission year t. The variable $\Delta\text{subsidy}_g$ denotes the training subsidy increase for individuals in group g. Note that there are relatively few values of $\Delta\text{subsidy}_g$, and multiple i cells are nested within a group g. $\mathbf{1}_t^{\text{Post}}$ is an indicator variable for admission years 2019 and onwards, and $\mathbf{1}_r^{\text{Treated}}$ denotes a group located in one of the regions that implemented the reform. We include time and region fixed effects, as well as separate fixed effects for each demographic group g affected by the increase. The coefficient of interest β_{DDD} estimates the average difference in the outcome variable between groups for a €1 increase in subsidy, in regions which enacted the reform compared to regions that did not, pre-reform vs post-reform.

We then examine the dynamic effects of the change by estimating the following event-study

DDD specification:

$$
\begin{aligned}
Y_{igrt} = & \sum_{k=-2,k\neq -1}^{1} \beta_k^{ES}\, \mathbf{1}_t[t-m=k] \times \mathbf{1}_r^{\text{Treated}} \times \Delta\text{subsidy}_g \\
& + \sum_{k=-2,k\neq -1}^{1} \sum_{c} \gamma_{kc}\, \mathbf{1}[t-m=k] \times \nu_g \\
& + \sum_{k=-2,k\neq -1}^{1} \sum_{r} \gamma_{kr}\, \mathbf{1}[t-m=k] \times \mathbf{1}[\text{Region}=r] \\
& + \mathbf{1}_r^{\text{Treated}} \times \Delta\text{subsidy}_g \\
& + \mathbf{X}'\boldsymbol{\beta} + \nu_g + \rho_r + \xi_t + \epsilon_{irt}
\end{aligned}
\tag{2}
$$

where k is relative event-time, the difference between admission year t and the start of the reform $m = 2019/2020$. The omitted value for k is the last period before the reform, as is convention in the literature. The coefficients of interest in this regression are the β_k^{ES}.

We first estimate the above specifications on the dataset containing all regions except Nouvelle-Aquitaine and Bretagny, which are dropped as outlined in section 2.2. We then switch to an estimation where we restrict the sample only to the reform regions and Occitanie, using Occitanie as the only control.

3.3 Effect of subsidy increase

3.3.1 Main estimates

Static specification We start by estimating a version of Equation 1 with only admission year, treatment group and reform region fixed effects, together with the triple interaction of interest. The estimated coefficient in Column (1) of Table 2 is 0.024, and is only statistically significant at the 10% level. Adding flexible fixed effects in Column 2 slightly alters the estimated effect, to 0.026, and the estimate is now statistically significant. At the average subsidy increase of approximately €300[4], this corresponds to an increase of 7.8 new trainees per 10 000 job seekers, or 28.6% of the pre-reform mean in reform regions. Finally, in Column (3), we add socio-demographic controls interacted with time fixed effects to control for possible composition changes over time. Their addition does not substantially affect the results, as the estimated coefficient remains 0.026. We nevertheless retain them in our preferred specification.

[4]Conditional on being in a demographic group which receives an increase in a reform region just before the reform.

Table 2: Estimates of the change in job training take-up due to training subsidy increases for French jobseekers

	No trends, no controls	No controls	Full specification
Reform region x Post x Subsidy change amount	0.024	0.026	0.026
	(0.013)*	(0.008)***	(0.008)***
Pre-reform mean outcome, reform regions	27.274	27.274	27.274
Observations	368 528	368 528	368 528
Admission year FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Subsidy change group FE	Yes	Yes	Yes
Post × Subsidy change group FE	No	Yes	Yes
Post × Region FE	No	Yes	Yes
Region × Subsidy change group FE	No	Yes	Yes
Socio-demographic controls × Admission year FE	No	No	Yes

Note:'*' = 0.1, '**' = 0.05, '***' = 0.01. This table presents the results from a triple difference-in-differences estimation of the effect of the change of training subsidy amount on training take-up by registered jobseekers in France. Numbers in parentheses indicate standard errors clustered at the region by treatment intensity group level.

Table 3: Estimates of the change in job training take-up due to training subsidy increases for French jobseekers by type of training

	All	Degree-granting	Non-degree granting	Preparatory
Panel A: Rate of new trainees per 10 000 jobseeekers				
Reform region x Post x Subsidy change amount	0.0260	0.0091	0.0014	0.0154
	(0.0078)***	(0.0052)*	(0.0010)	(0.0037)***
Treatment effect at mean subsidy change	7.79	2.72	0.43	4.62
Pre-reform mean outcome, reform regions	27.27	18.82	1.34	7.16
Observations	368528	368528	368528	368528
R2	0.133	0.115	0.057	0.080
Panel B: Subscribed hours of training per person conditional on any training entry				
Reform region x Post x Subsidy change amount	-0.2420	-0.3306	-0.0776	0.1663
	(0.0552)***	(0.1045)***	(0.0274)***	(0.0472)***
Treatment effect at mean subsidy change	-72.59	-99.19	-23.28	49.89
Pre-reform mean outcome, reform regions	880.83	719.14	46.93	114.76
Observations	96934	96934	96934	96934
R2	0.173	0.170	0.068	0.124

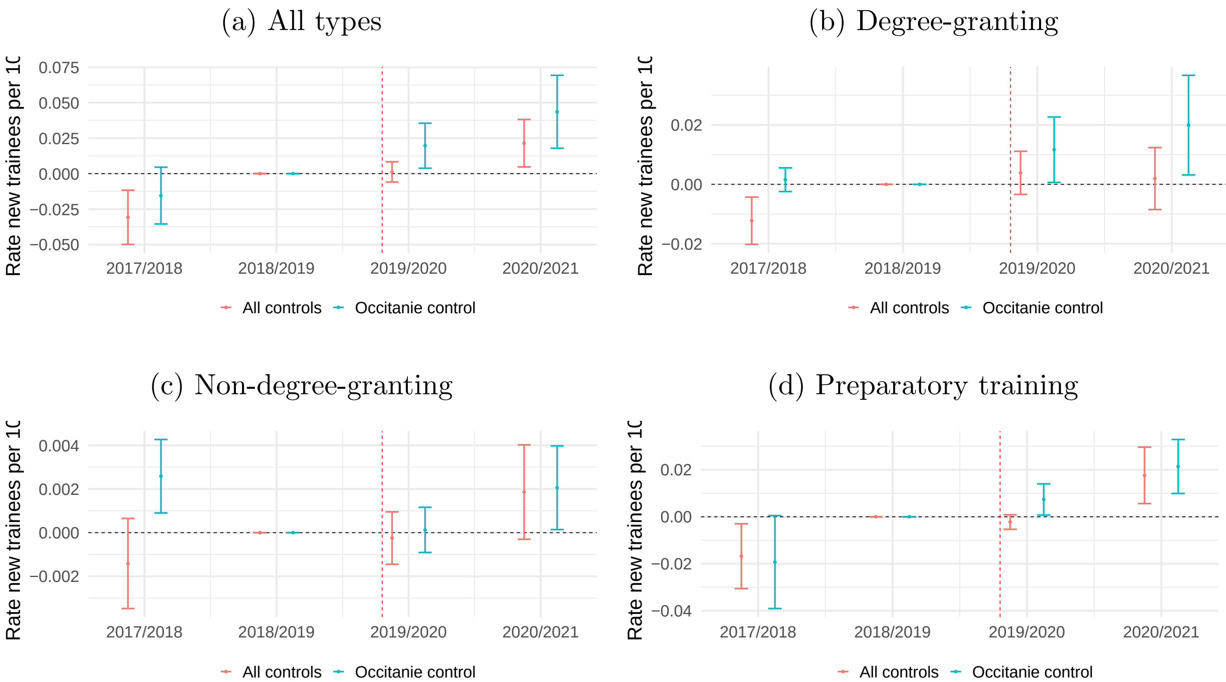
Note:'*' = 0.1, '**' = 0.05, '***' = 0.01. This table presents the results from a triple difference-in-differences estimation of the effect of the change of training subsidy amount on training take-up and planned hours of training by type of training. Predicted treatment effect calculated at mean subsidy increase in reform regions in period immediately before the reform. Numbers in parentheses indicate standard errors clustered at the region by treatment intensity group level. Stars next to each standard error calculation reflect the corresponding p-value.

We then turn to estimating this preferred specification for different types of training. The results are presented in Table 3. Panel A focuses on the extensive margin, and shows the estimated effect on the new training entrants to jobseekers ratio. Column (1) restates the estimates for all training types from Column (3) of Table 2. The globally positive result on all types of training is driven by the effects on degree-granting and preparatory training. The coefficient of 0.0091 in Column (2) implies an increase of 2.72 starts per 10 000 (22.4% of the pre-reform mean in reform regions) in degree-granting training. The estimated coefficient in Column (4) is 0.0154, corresponding to an increase in the take-up of preparatory training of 4.62 starts per 10 000 jobseekers, or 64.6%. The effect on non-degree granting training starts in Column (3), while large relative to baseline, is imprecisely estimated and not statistically significant, hence we do not see evidence of substitution away from the already relatively low level of non-degree granting training. Overall, it appears that there is an increase in demand for training following the reform, and that this extra demand is driven by degree-granting and preparatory trainings (which lead into degree-granting trainings). This is consistent with job-seekers recognising the important role formal training certifications play in alleviating information asymmetries in the job market, and accordingly demanding more of these types of courses (Carranza et al., 2020). Panel B looks at the intensive margin, with the dependent variable now being the subscribed (but not necessarily completed) number of hours of training per job seeker conditional on having entered any course of training. While overall the intensity of subscribed trainings has gone down, this varies by type of training. The subscribed duration of degree-granting and non-degree granting trainings has decreased, however the duration of preparatory training has increased.

Occitanie as alternative control As outlined in Section 2.1, the Occitanie region provides an opportunity for use as an alternative control. Appendix Table A4 presents the results of the static triple-differences specification which includes only the treated regions and Occitanie. The effect on overall take-up is still large and statistically significant, with an estimated increase of 11.56 per 10 000 (39.1% of the pre-reform mean in reform regions). Similarly to the main results, we find that the increase is driven by take-up of degree-granting and preparatory training. We find a statistically significant decrease in the hours of training, driven by a 13.5% decrease in hours of preparatory courses per person.

Event study Figure 2 shows the estimated coefficients from the event study specification described by Equation (2), estimated separately the sample with all non-reform regions as controls and for the sample where the only control is the Occitanie region. In the full sample, Panel (a) shows that the overall positive effect on the measure of all trainings take-up is driven by a statistically significant increase immediately following the reform during the 2019/2020 admission year. The increase continues in the subsequent 2020/2021 admission year. Panels (b) through (d) examine effects by type of training. Confirming the results from the static specification, Panels (b) and (d) show that the dynamic effects are driven by degree-granting and preparatory trainings. In Panel (c) the dynamic effects on non-degree-granting training

Figure 2: Event study graphs of changes in job training take-up by type of training



Note: These figures graph the coefficients β_{ES} from specification 2 for the triple interaction between an indicator for the periods after the subsidy change reform, an indicator for groups from regions which implemented the subsidy change, and a subsidy change exposure variable calculated as the predicted increase in training subsidy offered conditional on socio-demographic characteristics. Two different estimation samples are used. The main sample uses all available control regions, whereas the second contains only the reform regions and Occitanie. The vertical red line indicates the start of the reform at the beginning of the admission year 2019/2020. Following the usual convention in the literature, the coefficient in the last pre-reform period is normalised to 0. Regressions are weighted using the number of job seekers in each group. Confidence intervals are at the 95 percent level and account for clustering at the département level.

are small and imprecisely estimated, with a large decrease in 2018/2019. However, Panels (a), (b) and (d) exhibit pre-trends, as there appears to have been an increase in take-up in the year immediately preceding the reform start, and the magnitude is comparable to the estimates for the post-reform increases. In Panel (b), the pre-trend is in the opposite direction, as there appears to have been a large decrease in non-degree-granting training during 2018/2019.

Nonlinear effects of the subsidy increase and dose-response relationship The estimation strategy we have used thus far assumes that the effect of training subsidy change on take-up is linear in the subsidy increase. We now relax this assumption and instead estimate a specification where the effect varies by treatment discrete value groups:

$$\begin{aligned} Y_{igrt} &= \sum_{g=1}^{4} \beta_{DDD}^{g}\, \mathbf{1}_t^{\text{Post}} \times \mathbf{1}_r^{\text{Treated}} \times \nu_g \\ &+ \sum_{g=1}^{4} \gamma_{gt}\, \mathbf{1}_t^{\text{Post}} \times \nu_g + \gamma_{rt}\, \mathbf{1}_t^{\text{Post}} \times \mathbf{1}_r^{\text{Treated}} \\ &+ \mathbf{X}'\boldsymbol{\beta} + \nu_g + \rho_r + \xi_t + \epsilon_{igrt} \end{aligned} \tag{3}$$

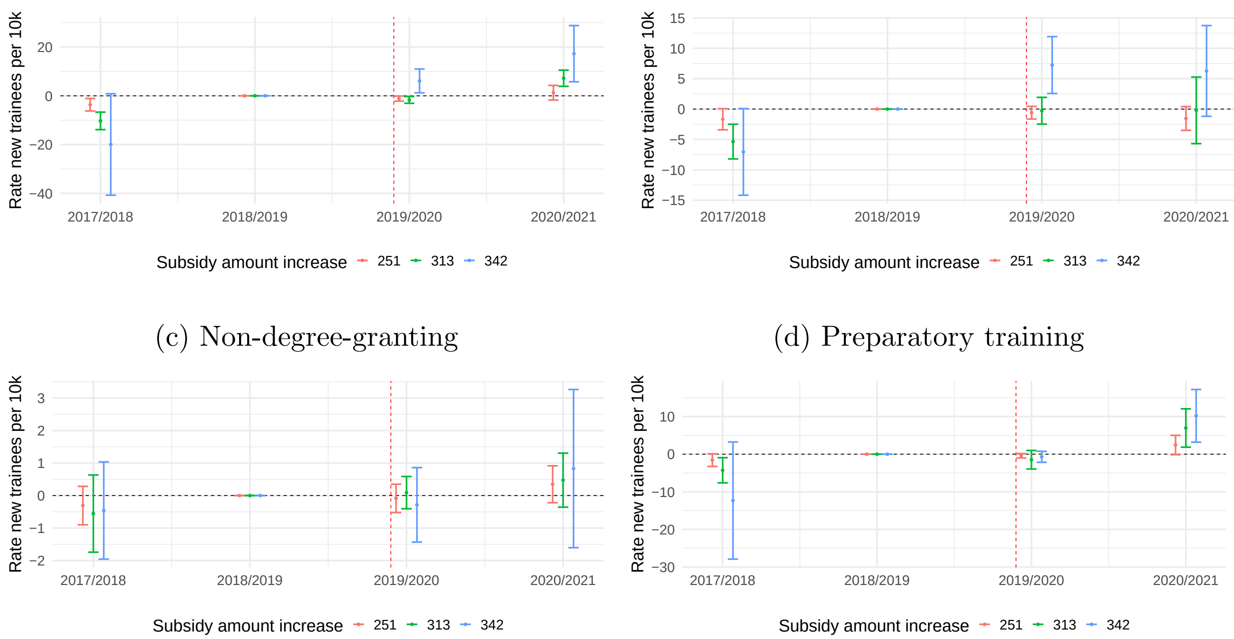
The results for the main sample are presented in Figure 3. The pre-trends observed in Figure 2 for the main sample are driven by the large increases in 2018/2019 in preparatory training take-up for the group which received a €522 increase. In addition, the effects for groups which experienced a larger increase are stronger, i.e., we observe a dose-response relationship. This is confirmed when we restrict the sample to the reform regions and Occitanie, as seen in Appendix Figure B1.

Heterogeneity in training starts We enrich the analysis by exploring how the effect varies across several demographic characteristics. We choose characteristics which are either correlated with difficulties in participating in the labour market, or have been found in previous research to be negatively correlated with job training take-up (Bucher et al., 2021; DARES, 2020; Gélot and Minni, 2006). These characteristics are: having a limited level of formal academic education (high school degree or less), being a married woman, living in a rural area, living in a high-poverty urban area, and being a foreign national. Table ?? presents the estimates from specification 1 for the two different samples examined previously, all controls and Occitanie as the only control region. In both samples, two demographic groups stand out. The first, individuals with at most a high school degree, are featured in Column (1). This group is both likely to have low levels of human capital and hence large potential returns from training, as well as be subject to credit constraints. Our results support this interpretation, as the estimated effect for this group is about two times larger

Figure 3: Event study graphs of changes in job training take-up by type of training

(a) All types (b) Degree-granting

(c) Non-degree-granting (d) Preparatory training



Note: These figures graph the coefficients β_{ES} from specification (3) for the triple interaction between an indicator for the periods after the subsidy change reform, an indicator for regions which implemented the subsidy change, and an indicator variable for each discrete predicted change amount. The vertical red line indicates the start of the reform at the beginning of the admission year 2019/2020. Following the usual convention in the literature, the coefficient in the last pre-reform period is normalised to 0. Regressions are weighted using the number of job seekers in each group. Confidence intervals are at the 95 percent level and account for clustering at the département level.

than for the rest of the population. The second is married women. Indeed, for this group the effect is halved compared to that of men, suggesting that either job training returns are lower, or that there are additional non-financial barriers to training take-up, such as disproportional childcare obligations. The result is specific to the combination between gender and matrimonial status, as unmarried women are not differentially affected compared to men (regression coefficient not reported).

Heterogeneity analysis

	High school or less	Married women	Aged 16 to 30	Rural	Urban poor	Foreigner
Reform region x Post x Subsidy Δ x Het. variable	0.0192	−0.0301	0.0377	0.0036	−0.0004	−0.0072
	(0.0148)	(0.0141)**	(0.0089)***	(0.0089)	(0.0111)	(0.0121)
Reform region x Post x Subsidy Δ	0.0109	0.0342	0.0033	0.0245	0.0257	0.0317
	(0.0088)	(0.0095)***	(0.0035)	(0.0077)***	(0.0083)***	(0.0104)***
Observations	368 528	368 528	368 528	368 528	368 528	368 528
R2	0.133	0.135	0.140	0.133	0.133	0.134

Note:'*' = 0.1, '**' = 0.05, '***' = 0.01. This table presents the results from a triple difference-in-differences estimation of the effect of the change of training subsidy amount on training take-up by type of training. Numbers in parentheses indicate standard errors clustered at the region by treatment intensity group level.

3.3.2 Robustness checks

Repeated training starts One possible concern with our measure of the training rate is that it may overstate take-up of training due to counting entries by individuals who do not intend to finish their course and who only enter training as a "holding pattern" in-between short-term job spells. While the administrative training spell data only measures training entries, we use the indicative training durations to generate a new measure. This measure is otherwise identical to our main one, except it does not include trainings which start during a time period that overlaps with a previous training spell that started within the last 12 months. The static triple-differences results, shown in Appendix Table A7, closely replicate the magnitude and pattern of the main results.

Synthetic difference-in-differences The event study graphs in Figure 2 exhibit signs of pre-trends. To correct for this, we estimate a synthetic difference-in-difference (Arkhangelsky et al., 2021) using cells in the control regions as the donor pool. This method improves on both difference-in-differences and synthetic controls by weakening the dependence on parallel trends assumptions, by being invariant to additive unit-level shifts, and by allowing for valid large-panel inference. This method estimates an average treatment effect $\hat{\tau}^{\text{sdid}}$, intercept μ, time β_t and unit α_i fixed effects such that

$$(\hat{\tau}^{\text{sdid}}, \hat{\mu}, \hat{\beta}_t, \hat{\omega}, \hat{\alpha}_i) = \underset{\tau,\mu,\beta_t,\omega,\alpha_i}{\arg\min} \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i^{\text{sdid}} \hat{\lambda}_t^{\text{sdid}} \tag{4}$$

where ω_i and λ_t are cross-sectional and time weights, and W_{it} is the treatment indicator (cell belongs to a treated region and time period is post-reform). The cross-sectional weights are chosen so that they align pre-treatment outcome trends in the treatment group with those of treated units, while the time weights are chosen so as to balance that the pre- and post-treatment outcomes of the control group.

The synthetic difference-in-differences requires a binary treatment indicator. In our context, the subsidy change brought on by the reform was largest for job-seekers aged 16 to 25 without rights to unemployment benefits. We thus define treated cells as those that are composed of individuals 16-to-25 without a right to unemployment benefit, in a treated region, and who are observed after the start of the reform. We also exploit the availability of an additional treatment variability dimension (eligibility for unemployment benefits) to mimic the main triple-difference estimation. Thus, in our case, we estimate instead

$$(\hat{\tau}^{\text{sddd}}, \hat{\mu}, \hat{\beta}_t, \hat{\omega}, \hat{\alpha}_i) = \underset{\tau,\mu,\beta_t,\omega,\alpha_i}{\arg\min} \sum_{i=1}^{N}\sum_{t=1}^{T}(\Delta Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i^{\text{sdid}} \hat{\lambda}_t^{\text{sdid}} \tag{5}$$

where ΔY_{it} is first-differenced across the unemployment benefit eligibility dimension[5]. The results are presented in Appendix Table A5. In this table, we also present for ease of comparison the results from the usual triple-difference specification, but with a similarly defined binary treatment variable. The estimates from the synthetic triple-differences are not significantly different from those computed with the usual triple-differences, and the qualitative pattern is the same. Appendix Figure B2 plots the dynamics of the synthetic control and the average of treated cells. We can see that the synthetic control closely tracks the average outcome of the treated cells in the two pre-treatment periods. A positive, significant and persistent treatment effect is estimated over all types of training. This is driven as before by the effect of degree-granting and preparatory trainings, whereas the effect of non-degree-granting trainings is not statistically different. Finally, the quantiles of the chosen cross-sectional weights are shown in Appendix Table A6, which shows that cells from all regions contribute to the synthetic control relatively equally.

4 Training attendance

The results from Section 3.3 suggest that the grant reform led to an uptake in training. However, bringing in new trainees does not necessarily imply that those trainees persisted in the programmes they entered. In this section, we explore two questions. First, how did the subsidy increase affect trainee attendance rates? The additional financial support may have induced more job-seekers to enter training, but these may be less attached to training and consequently miss out more hours of training. Second, did smoothing the training

[5] For a cell i located in département d, observed at time t and defined by covariates $\mathbf{x}$, we calculate $\Delta Y_{id\mathbf{x}t} = (Y_{id\mathbf{x}t}|\text{eligible}) - (Y_{id\mathbf{x}t}|\text{not eligible})$, where *eligible* denotes eligibility for unemployment benefits.

grant disbursement schedule improve training attendance? Indeed, as outlined in Section 2.1, individuals who do not receive unemployment benefits face a drop in their income stream when entering training. If trainees do not face credit constraints, unanticipated income shocks during training are simply smoothed out by additional borrowing, and trainees continue on their training trajectory. However, if trainees face substantial credit constraints, unanticipated income shocks during training force them to reduce participation in training.

4.1 Data

Tackling questions about training attendance requires us to marshal additional data. We obtain these from an operational dataset supplied by the Afpa, the largest training provider in France. The dataset covers the population of individuals who started a training course with the Afpa between the 1st of January 2017 and the 31st of July 2020. The data holds individual-by-course observations on the total hours of absence from training, as transcribed from attendance sheets signed by trainees. The information is highly accurate, since both trainees' and training providers' public funding amount is determined by training attendance, as outlined in Section 2.1. Further information includes the start and end dates of the training spell, the training course code, and the geographical location of the agency where the training occurred. We match this data to the job-seeker registry FHS described in Section 3.1 using fuzzy string matching methods using the name and date of birth of individuals in the two datasets[6]. The final matched sample is composed of 244 385 individuals. This match allows us to determine the unemployment benefit eligibility and socio-demographic characteristics of job-seekers. However, the training courses listed in this dataset lack a classification according to the types (degree-granting, non-degree granting, preparatory) discussed in Section 2.1. Furthermore, the rules for validating training attendance, as well as modes of attendance of training programmes, were substantially altered after the start of the Covid-19 epidemic in March 2020, and data after the start of the Covid-19 pandemic is not comparable with earlier periods.

Appendix Figure B3 shows the average hours of training missed by type of job-seeker and by region type. Target group job-seekers are ineligible for unemployment benefits, whereas control group job-seekers are eligible. The three types of regions are: Bourgogne-Franche-Comté, which increased the monthly training subsidy of job-seekers without a right to unemployment benefits *and* added an unconditional upfront training grant; Normandie, which only increased the monthly subsidy; and the control regions which did not change their training remuneration policies. The period on which we will focus starts with the introduction of

[6] This matching was done in a secure environment by the CASD, and in accordance with the relevant legislation. The final analysis was done on anonymised data where individuals were attributed a random identifier.

the reform in Q2 of the admission cycle year 2019-2020 (August-October 2019), and ends in Q4 of 2019/2020 (February to April 2020) with the onset of Covid-19. After this end date, attendance recording rules are considerably relaxed, and the missed hours data is no longer comparable. Hours of training missed are very similar across all groups and regions prior to the reform. In the admission year of the reform, there is an increase in training hours missed across all groups and regions. The control regions and the Bourgogne-Franche-Comté, the region that increased subsidies and introduced an upfront training grant, move together initially, but diverge three to six months after the start of the reform. Normandie, the region that only increased subsidies, saw a much larger increase in hours of training missed. Crucially, within each set of regions, the control group of job-seekers who are eligible for unemployment benefits and the reform target group of job-seekers ineligible for unemployment benefits behave similarly, justifying a choice of a triple-difference strategy to net out region-time shocks common to the two groups of job-seekers.

4.2 Estimation strategy

To answer whether the training grant reform impacted training attendance, we will use a triple-difference specification similar to the one used previously, where we will compare, within region types (subsidy increase only, subsidy increase and upfront training grant, no subsidy increase) trainees ineligible for unemployment benefits with trainees who are not eligible. We proceed in two steps. First, we compare Normandie, which only increased the monthly subsidy amount, with the control regions. This will give us an estimate of the effect of the subsidy increase only. We then compare Normandie with Bourgogne-Franche-Comte, the region which increased subsidies and added an unconditional grant paid out at the start of training. This comparison will yield the effect of the grant.

In both cases, we estimate a specification of the form:

$$
\begin{aligned}
Y_{igrt} = & \sum_{k=-9,k\neq-1}^{4} \beta_k^{ES}\, \mathbf{1}_t[t-m=k] \times \mathbf{1}_r^{\text{Treated}} \times \mathbf{1}_g^{\text{Ineligible}} \\
& + \sum_{k=-9,k\neq-1}^{4} \sum_c \gamma_{kc}\, \mathbf{1}[t-m=k] \times \mathbf{1}_g^{\text{Ineligible}} \\
& + \sum_{k=-9,k\neq-1}^{4} \sum_r \gamma_{kr}\, \mathbf{1}[t-m=k] \times \mathbf{1}_r^{\text{Treated}} \\
& + \mathbf{1}_r^{\text{Treated}} \times \mathbf{1}_g^{\text{Ineligible}} \\
& + \mathbf{X}'\boldsymbol{\beta} + \nu_g + \rho_r + \xi_t + \epsilon_{irt}
\end{aligned}
\tag{6}
$$

where i indexes trainee by course observations, r indexes regions, g indexes unemployment benefit eligibility, and t denotes admission year quarter of training start. The coefficients of interest are the β_k^{ES}. The indicator variable $\mathbf{1}_r^{\text{Treated}}$ denotes the region of interest (Normandie in the first specification, Bourgogne-Franche-Comté in the second), and the dummy variable $\mathbf{1}_g^{\text{Ineligible}}$ indicates whether the individual is ineligible for unemployment benefits and therefore the target group for the reform. The terms ρ_r, λ_c and ξ_t are region, unemployment benefit eligibility group, and quarter fixed effects respectively. We do not include individual fixed effects as there are relatively few repeat entries into training in this period. Standard errors are adjusted for two-way clustering at the region and course level.

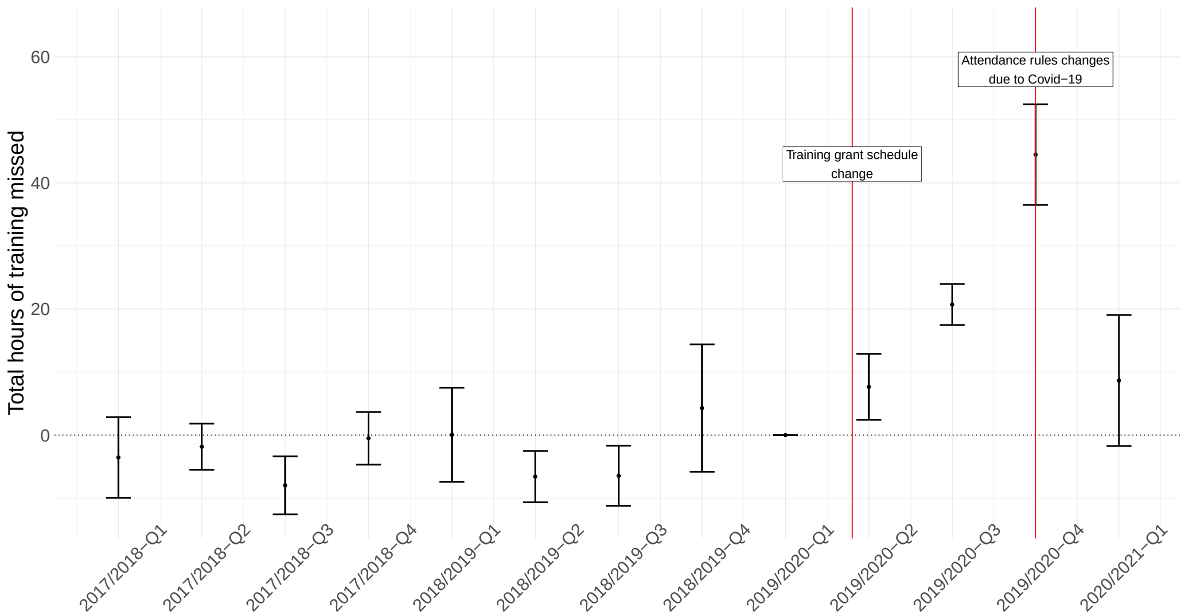
4.3 Results

Figure 4 presents the results of a triple-difference event-study estimation using the total number of hours of absence from training as the outcome. In Panel a, we compare Normandie (increased monthly subsidy) with control regions. In the third quarter of admission year 2019/2020, the effect of the subsidy was an increase of 18.6 hours, or 87% compared to the pre-reform mean in the reform region. This is consistent with new arrivals into training being credit-constrained, as trainees may be reducing their attendance in order to prioritise other (income-generating) activities. Panel b presents the results of the triple-difference estimation, but comparing Normandie and Bourgogne-Franche-Comté. Introducing the upfront unconditional grant caused a decrease in hours of missed attendance. The effect is largest for cohorts starting in Q3 2019/2020, with cohorts in the upfront grant region recording 17.3 less missed training hours, a 43% decrease compared to the average of 38.5 hours in the other subsidy reform region. This implies that introducing the upfront grant almost completely compensated the effect of the increased subsidy on attendance. In both panels, the parallel trends assumption is supported by the pre-treatment data, as we find little evidence of pre-trends in the pre-treatment period stretching back to Q1 2017/2018.

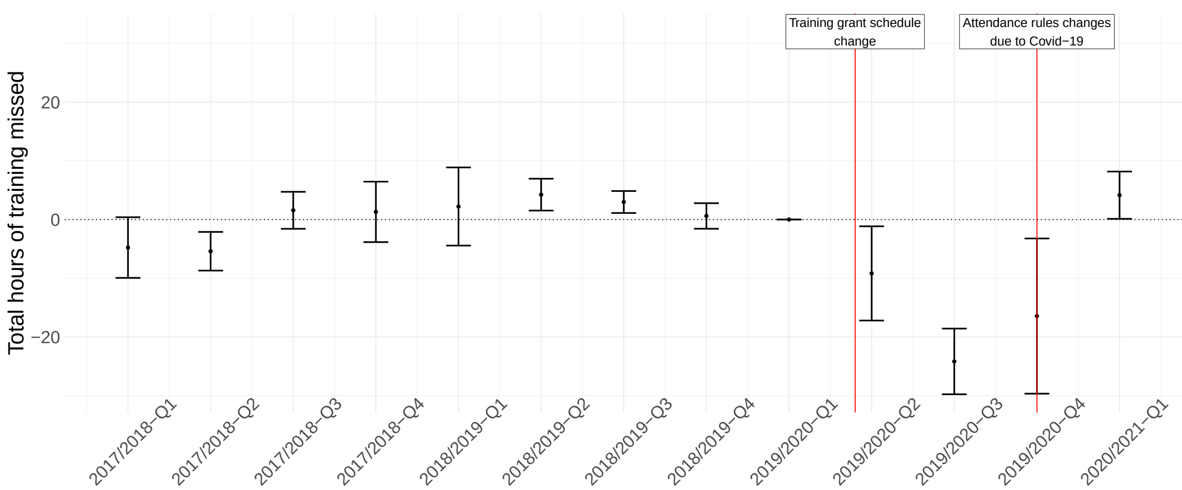
The estimates we review here, together with the results from Section 3.3, suggest that the grant increase and grant schedule change worked in opposite directions. While the increase in the amount paid brought more entrants into training, these were less persistent in their training attendance. The scheduling change worked in the opposite direction, suggesting that individuals who were credit constrained were able to pursue their training in the face of income shocks. We pursue further evidence of credit constraints in the next section.

Figure 4: Event study of effect of training financing reform components on average hours of training missed

(a) Effect of monthly subsidy increase



(b) Effect of upfront unconditional training grant



Note: These figure present the event-study coefficients of the interaction between an indicator for the cohort of training start and an indicator for individuals who registered as trainees in either Normandie (Panel a) or Bourgogne-Franche-Comté (Panel b). In Panel a, the comparison is between Normandie and control regions, whereas in Panel b it is between the two reform regions Bourgogne-Franche-Comté and Normandie. The first vertical line indicates the start of the reform in the first quarter of the admission year 2019/2020. The second vertical line indicates the start of the Covid-19 pandemic and the consequent change in training attendance rules in the last quarter of the 2019/2020 admission year. Following the usual convention in the literature, the coefficient in the last pre-reform period is normalised to 0. Confidence intervals are at the 95 percent level and account for clustering at the region by training course level.

5 Returns to training and IV test for credit constraints

In Section 3.3, we have shown evidence that an increase in training subsidy paid out to populations which are likely to be credit constrained in their demand for job training, namely job-seekers who are not eligible for unemployment benefits, leads to an increase in demand for training. Then, in Section 4, we documented evidence that trainees brought into training by increasing subsidies may be credit constrained and have reduced training attendance. However, an appeal to credit constraints must also examine the returns to training, for two main reasons. First, if returns to training are not positive, then individuals who do not attend training are choosing the rational option, rather than being credit constrained. Second, a simple model of training choice illustrates that, in the presence of credit constraints, returns to training must differ between individuals who enrol due to reductions in direct costs and individuals who enter training due to reductions in its opportunity cost. In this section, we address the first question by evaluating the labour market returns to training using a regression discontinuity design (RDD), exploiting the existence of admissions tests at a major French training provider. In the second part, we explore differences in wage returns to training along different margins of participation in training using an instrumental variables (IV) test in the spirit of Cameron and Taber (2004).

5.1 Data

Training admissions data The data we use to evaluate the labour market effects of training comes from a hand-collected and transcribed collection of paper applications for degree-granting training entry submitted to the Afpa between Q2 2015 and Q4 2016. We collected 31 900 individual files, of which 75.9% (28 645) are identifiable (contain a name and a date of birth) and hold at least one test score result sheet. Where there are instances of multiple attempts at the same test for the same application, we pick the oldest attempt. Where it is not possible to determine the chronological ordering, we pick a test score at random.

We match this sample to training acceptance records reported in the Afpa's databases described in section 4.1, and to employment data and socio-demographic characteristics contained in the datasets described in section 3.1. Matching is done on names and birth dates using probabilistic matching techniques: we match observations on a combination of the Jaro-Winkler (Jaro, 1989; Winkler, 1990) similarity between names and the edit distance (Levenshtein, 1966) between dates of birth[7]. Admission decisions were inferred by observing a match in Afpa training records, as the decision was often omitted from paper application folders. The final matched sample used in the estimations contains 24 219 observations, or

[7] Privacy rules prevent us from matching on national identification numbers (NIR), which are the only fully deterministic way to match observations across French administrative datasets.

75.9% of the total collected applications.

As described in Section 2.3, the test thresholds applied to applicants differ according to the training they applied for. We therefore normalise the raw logic and problem solving scores:

$$\text{normalised} = \frac{\text{raw} - \text{threshold}}{\text{max}}$$

where raw is the unadjusted score, threshold is the corresponding score threshold, and max is the maximum possible score obtainable on the test. Appendix Figure B4 presents the distribution of logic and problem solving scores. These do not indicate evidence of threshold manipulation.

Appendix Table A2 shows a set of descriptive statistics for the entire sample of applications, as well as for two subsamples (the construction of which is described more in detail in Section 5.2). Roughly half of trainee applications were accepted, with the majority of applications coming from men, and around 10% being from foreign nationals. Over the period covered by the outcome data (start of 2017 to end of 2019), on average 30% of applicants are employed in a month, and the average unconditional monthly base salary earnings is €600 per month.

The treatment variable we focus on is being accepted for training. We consider an individual to have been accepted if a training slot has been reserved for them in a at least one session corresponding to the training they applied for according to the Afpa's internal database. The courses targeted by the applications in our sample are training programmes in labour-intensive occupations experiencing a tight labour market locally, delivering a degree which is officially recognised by the Ministry of Labour. The top five most popular occupations are home care staff, electrician, facilities maintenance and cleaning, payroll accountant and plumber.

Employment and earnings We focus on two main types of outcomes: employment and earnings. We measure both using data from the Mouvements de Main d'Oeuvre (MMO) database, which we match to the training acceptance records and unemployment spells. The MMO database contains all private sector work contracts declared to Social Security by employers in France since 2017, excluding seasonal or short-term agricultural contracts. Each contract contains information on the location, duration, type of contract (fixed-term or permanent), and a detailed occupation code. Reported wages reflect only the base salary: additional remuneration such as tips, overtime, in-kind benefits and bonuses is not included and is not reported separately. We exclude contracts with a duration of less than one month, as the duration and wage information in these shorter contracts is likely to be misreported. Finally, the data does not contain information on hours worked.

For the RDD estimation, we calculate employment and earnings indicators at an yearly and a monthly frequency. For employment, we calculate the monthly probability of being employed, as well as the number of months of employment in each year. We consider a person to be employed in a month if they are attached to a contract covering at least five working days in that month.

For the IV estimation, we define an ad-hoc list of low-qualification occupations. We use as a starting point the official list of low-qualification occupations[8], but we remove those which are covered by job training courses offered to job-seekers. Based on this classification, we calculate mean wages for low-qualification occupations by month and by commune of worker residence.

5.2 Admission thresholds and RDD estimation of returns to training

Admission threshold compliance Our goal for this exercise is to estimate the labour market returns to job training. However, a simple OLS regression of a labour market outcome on an indicator for being accepted to training is likely to suffer from selection bias. To remedy this issue, we exploit the existence of pre-admission tests and of admission cutoffs at the Afpa and implement a regression discontinuity design (RDD) identification strategy. Since the test scores are only one factor taken into account in the admission process, the discontinuity is fuzzy. An initial examination of the change in the probability of being accepted into training as a function of the test score is shown in Appendix Figure B5. The figure suggests that overall, the cutoffs did not have a statistically significant effect on acceptance rates.

However, the institutional context of the selection procedure suggests that local Afpa branches are allowed to run their selection process in the manner they see fit. We use this heterogeneity in admission procedures to select a sample in which the tests did in fact play an important role in admissions. To do this, we do a direct pre-test of the first stage. We split the sample into groups $g \in \{1, 2, \ldots G\}$, according to the local Afpa branch in which an application was submitted. We estimate the following regression separately for each group g:

$$\begin{aligned} \text{Accepted}_{ig} = \alpha + \beta_1 \mathbf{1}[\text{Above cutoff}]_{ig} \times \text{Test score}_{ig} \\ + \beta_2 \mathbf{1}[\text{Below cutoff}]_{ig} \times \text{Test score}_{ig} + \rho \mathbf{1}[\text{Above cutoff}]_{ig} + \epsilon_{ig} \end{aligned} \tag{7}$$

where i is an individual belonging to group g, $\mathbf{1}[\text{Above cutoff}]$ is an indicator variable for being above the relevant test score cutoff (vice versa for $\mathbf{1}[\text{Below cutoff}]$), and *Test score* is the normalised running variable. We select groups where the p-value of a one-sided t-test

[8] Available at `https://www.insee.fr/fr/metadonnees/definition/c1904`.

against the null of $\rho_g \leq 0$ is below 0.1. Figure 5 presents the first stage regression results when the sample has been pre-selected. In this sample, there is a clear and statistically significant jump at the cutoff. When using the logic score as the running variable, the jump in the acceptance probability is from 37.8% to 56.6%, whereas when using the problem solving test score the change is from 50.5% to 60.7%.

Labour market effects of acceptance into training We proceed with estimating the treatment effect of being accepted into training using the fuzzy regression discontinuity design described above. Given the limited number of discrete values forming the support of the test score distribution, we choose the estimation bandwidth manually. For the logic test score, we use a bandwidth of 0.3, whereas for the problem-solving score the bandwidth is 0.4.

Let Y be the outcome of interest, in our case employment probability or wages, T be an indicator variable for having been accepted into training, and c be the cutoff. We make two assumptions before we proceed with estimation. First, we assume local monotonicity: in the estimation neighbourhood crossing the cutoff weakly increases the probability of being accepted to treatment for all applications. We also assume that the test score only affects the outcome of interest by determining whether the application crosses the required cutoff for acceptance to training, and is not for example used as a separate hiring signal in the labour market.

The treatment effect we are estimating is:

$$\tau_{FD} = \frac{\lim_{\iota \downarrow 0} \mathbb{E}[Y|\text{Test score} = \text{cutoff} + \iota] - \lim_{\iota \uparrow 0} \mathbb{E}[Y|\text{Test score} = \text{cutoff} + \iota]}{\lim_{\iota \downarrow 0} \mathbb{E}[T|\text{Test score} = \text{cutoff} + \iota] - \lim_{\iota \uparrow 0} \mathbb{E}[T|\text{Test score} = \text{cutoff} + \iota]} \tag{8}$$

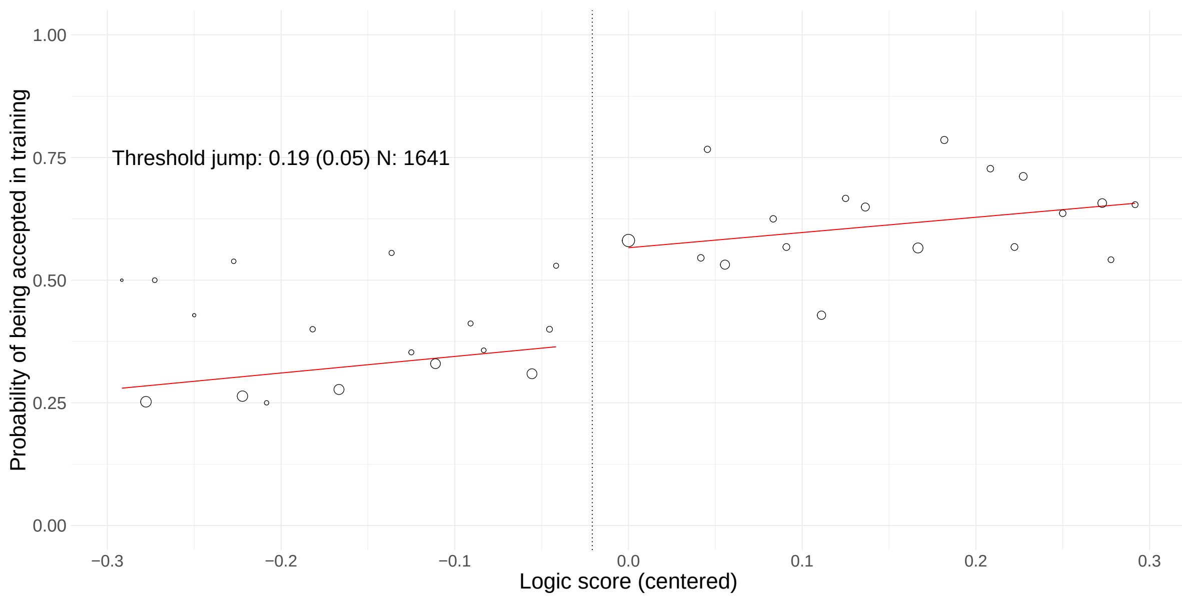
This is the average effect on the outcome for the proportion of applications who were accepted in a training due to having scored above the occupation-specific cutoff. Note that we are using being accepted to the training applied for as the treatment variable. Hence, the counterfactual includes all other options, including taking up a different training (or the same training but by a different provider), continuing the unemployment spell, or searching for a job.

We estimate τ_{FD} via a 2SLS local linear IV regression of the form:

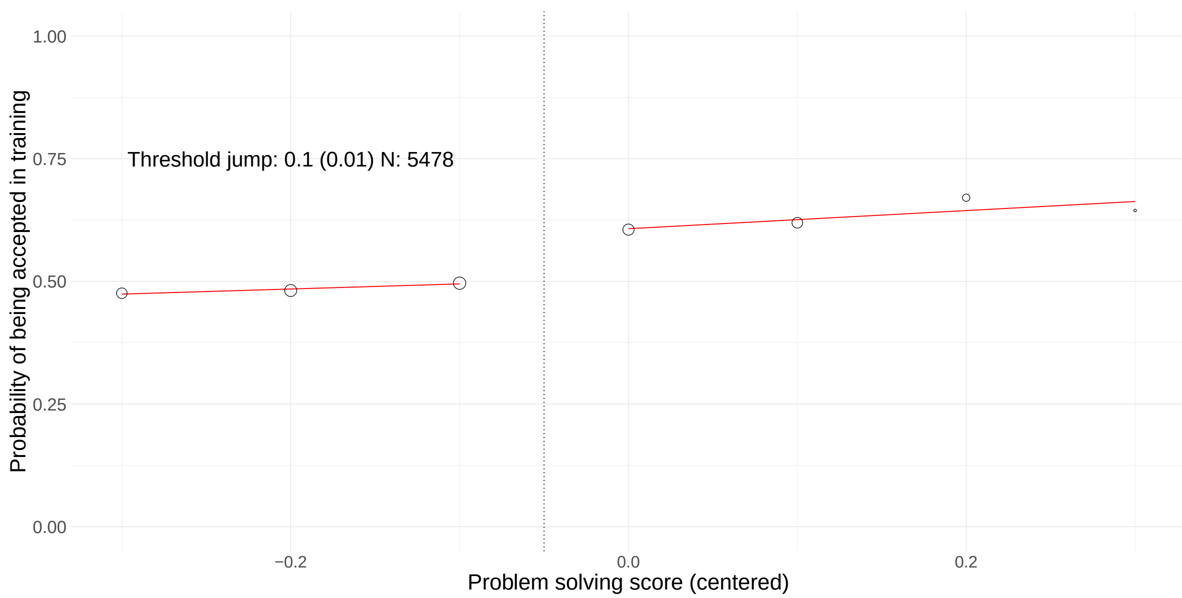
$$\begin{aligned} T_i &= \alpha + \delta_1 \mathbf{1}[\text{Above cutoff}]_i \times \text{Test score}_i + \delta_2 \mathbf{1}[\text{Below cutoff}]_i \times \text{Test score}_i \\ &\quad + \pi \mathbf{1}[\text{Above cutoff}]_i + \mathbf{X}'\zeta + \epsilon_i \\ Y_i &= \mu + \gamma_1 \mathbf{1}[\text{Above cutoff}]_i \times \text{Test score}_i + \gamma_2 \mathbf{1}[\text{Below cutoff}]_i \times \text{Test score}_i \\ &\quad + \tau_{FD} \hat{T}_i + \mathbf{X}'\beta + \nu_i \end{aligned} \tag{9}$$

where T_i and Y_i are defined as in the previous paragraph, and $\mathbf{X}$ is a set of control covariates

Figure 5: Pre-selected first stage training acceptance in the RDD estimation of returns to training



(a) Logic score test



(b) Problem solving score test

Note: This figure presents the proportion of individuals admitted into training as a function of the running variable, estimated on a sample which was selected using direct pre-selection at the first stage. In panel A, the running variable is the normalised logic test score, whereas in panel B it is the normalised problem solving test score. We plot the average acceptance rate for each discrete value of the running variable. Point sizes correspond to the number of applications at each running variable value. The vertical dotted line indicates the acceptance cutoff.

we add to improve precision. These covariates are the interaction between two indicator variables for being single and being female, an indicator variable for being a foreign national, an indicator for each five-year age bracket, indicator variables for living in a less affluent urban area and for living in a rural area. We cluster standard errors at the values of the running variable (Lee and Lemieux, 2010).

We begin first by examining the results of a simple benchmark OLS estimation, where we ignore the potential endogeneity of the training acceptance. The results for each month between January 2017 and December 2019 are presented in Appendix Figure B6. We estimate that throughout the study period, the difference in outcomes between applications that were accepted and those that were not is around 1.5 percentage points (or approximately 5% of the sample mean) for monthly employment, and approximately €40 (6.6% of the sample mean) for monthly wage income. This difference is in line with the results of training programmes reported by meta-analyses such as Card, Kluve, and Weber (2018).

We then turn to the results from the regression discontinuity estimation, presented in Figure 6. Using the problem solving score as a running variable results in more precise estimates, which is in agreement with the larger sample size retained after the pre-tests. The estimates obtained when using the logic score are almost never statistically significant. When using the problem solving score as a running variable, we find evidence of a short-run effect on the probability of being employed, which occurs around six months after the start of the study period. There is also a more pronounced effect on total wages earned. The point estimates are large, at the highest point reaching more than 100% of the sample mean over the period.

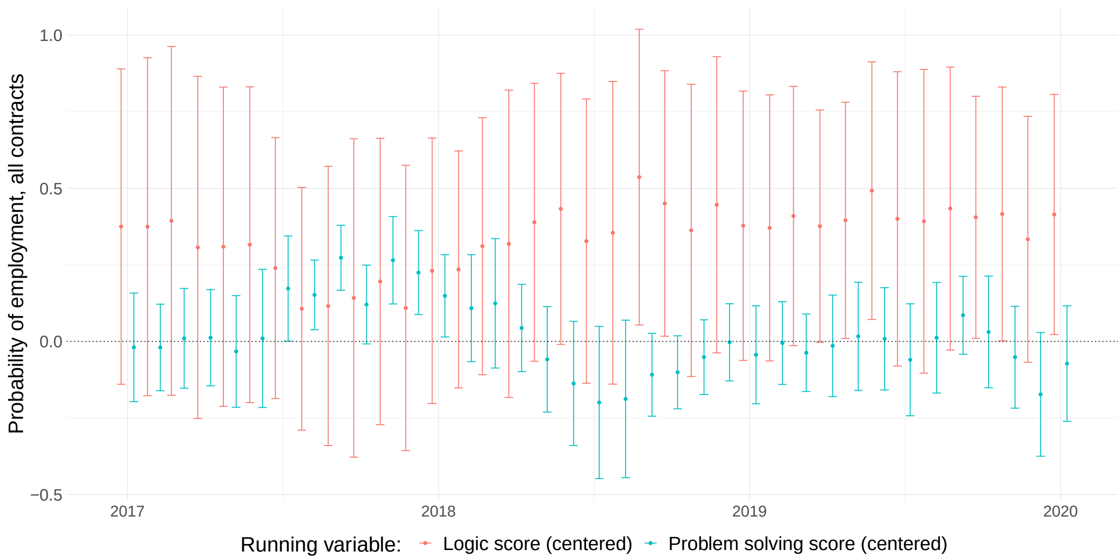
Overall, the results suggest that the returns to being accepted for training are high, but not persistent. One interpretation is that those who were not accepted "catch up" with their peers, either through starting another training course, or through on-the-job experience. This does seem likely, given that the treatment effect is estimated on the subset of test score marginal applicants. Furthermore, if the returns to training are this high, why are not more job-seekers entering training, even with low training subsidies? One possibility, which we investigate in the next section, is that they may be credit-constrained.

5.3 Heterogeneous returns to training as an indirect test for credit constraints

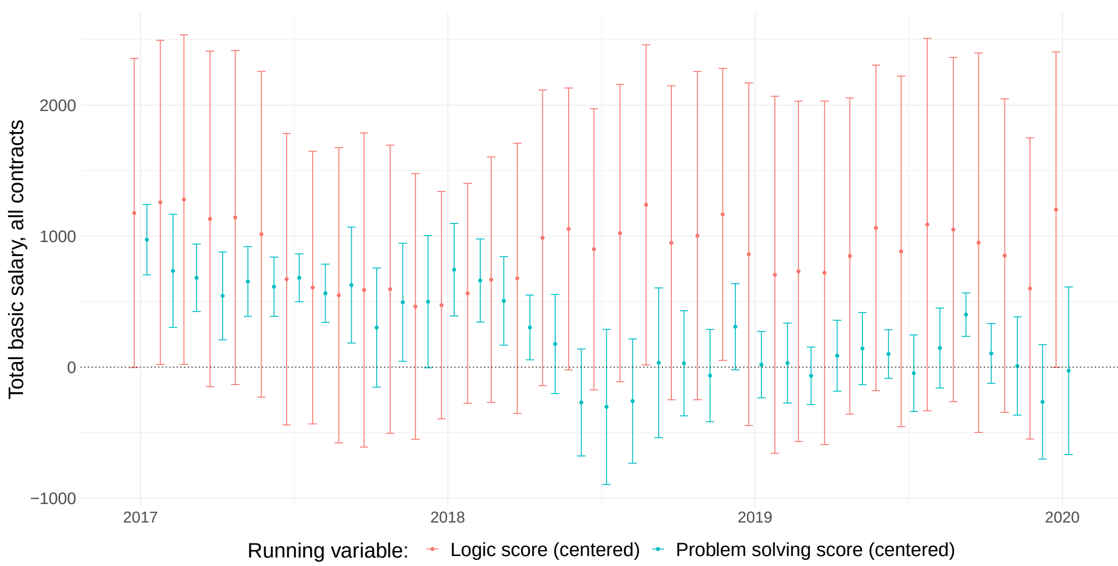
In this section, we test for the presence of credit constraints by exploring the possible heterogeneity in estimated returns to training across different margins of compliance in an instrumental variables (IV) approach, a strategy inspired by Cameron and Taber (2004). We formalise the intuition behind this estimation in an illustrative model, described in Appendix C. In that model, the degree of credit constraints is represented by a single parameter

Figure 6: Estimated returns to training by month

(a) Probability of being employed



(b) Total wages earned



Note: These figures graph the coefficients τ_{FD} from the specification described by equation 9 when using the logic score test and the problem solving score test as running variables. Confidence intervals are at the 95 percent level and account for clustering at the running variable level.

corresponding to the interest rate faced by job-seekers when deciding whether to apply for training. Individuals can be induced into training based on two margins of compliance. First, they can enter training because of a decrease in the direct costs of training. Second, they can decide to enter into training because of a decrease in the opportunity cost of training, namely wages in occupations which do not require training. In the model, we show that individuals react more to a unit change in the direct costs associated with training than to that of opportunity costs, and that this difference increases with the severity of credit constraints.

The empirical estimation is implemented by estimating by 2SLS the following second stage and first stage equations:

$$Y_{i,t+12} = \beta_0 + \beta_1 X + \gamma \hat{T}_{i,t} + \mu_{r,t} + \chi_{\Delta\text{subsidy},t} + \lambda_{\Delta\text{subsidy},r} + \epsilon_{i,t} \tag{10}$$

$$T_{i,t} = \alpha_0 + \alpha_1 X + \pi Z + \phi_{r,t} + \psi_{\Delta\text{subsidy},t} + \kappa_{\Delta\text{subsidy},r} + \nu_{i,t} \tag{11}$$

The outcome of interest Y is wages for individual i one year after time t. Wages are measured in thousands of EUR, with the rescaling done for readability. The endogenous variable T is at least one entry into training of the given type during time t. In the direct cost IV, the instrument Z is the interaction between the simulated increase (measured in thousands of euros) in the training subsidy from the reform described in Section 2.1 (as if the individual were located in a treated region after the reform), interacted with a dummy variable for being located in a treated region and a dummy variable for the observation being from a time period after the training subsidy reform. The change in the training subsidy is simulated according to the demographic characteristics of the individual recorded as part of their job-seeker registration. In the indirect cost IV, Z is the average wage for low-qualification occupations in the commune of residence of the individual at time t, measured in thousands of euro. The vector of controls X includes age, age squared, indicator variables for being a married female, being an unmarried female, being a foreign national, living in a rural area, living in high-poverty urban neighbourhood, highest educational attainment (high school diploma or less), cumulative duration of unemployment spell (in days) and total base wage earnings across all work contracts in the previous twelve months (measured in thousands of euros). We also include flexible region by time, region by simulated training subsidy increase amount, and simulated training subsidy increase by time fixed effects.

The two first stages are presented in Appendix Table A8. Panel A presents the results for the indirect cost IV. The first stage estimate suggests that, as expected, local wages have a negative correlation with the probability of entering any training in any given month (Column 1). More precisely, a 1000 EUR increase in the local mean hourly wage in low-qualification occupations is associated with a 0.3 percentage point decrease in the probability of entering

Table 4: IV 2SLS coefficients

	Monthly wage 12 months later/1000			
	Any training	Degree-granting	Non-degree granting	Preparatory
Panel A: Indirect cost IV				
Training entry	-3.2508	-9.0190	-9.6944	-11.3558
	(1.6517)	(4.0986)	(6.4317)	(9.9997)
Kleibergen-Paap F-stat	3.4680	1.4770	1.3890	2.0660
Observations	1639149	1639149	1639149	1639149
R2	-1.972	-9.760	-4.708	-4.135
Region × Time FE	X	X	X	X
Region × Predicted subsidy change amount FE	X	X	X	X
Predicted subsidy change amount × Time FE	X	X	X	X
Panel B: Direct cost IV				
Training entry	-0.6131	-0.3303	1.9918	1.1718
	(2.1017)	(0.9175)	(4.9703)	(2.6812)
Kleibergen-Paap F-stat	1.6190	145.2560	0.7920	0.3260
Observations	1639149	1639149	1639149	1639149
R2	0.001	0.057	-0.137	0.020
Region × Time FE	X	X	X	X
Region × Predicted subsidy change amount FE	X	X	X	X
Predicted subsidy change amount × Time FE	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

training. The estimate correlation is small and not statistically significant regardless of training type. Overall, the first stage appears to be weak, as evidenced by the calculated Kleibergen-Paap (KP) F-statistic being less than 10 for all trainings. Panel B presents the first stage for the direct cost IV. The first stage coefficients have differing signs, but those for all trainings (Column 1), nor for preparatory (Column 3) and degree-granting trainings (Column 4) are not significant. However, the reform intensity is a very strong predictor of entry into degree-granting trainings (Column 2), with an increase of €1000 expected to increase degree-training take-up by 1.43 percentage points (+51%). The KP statistic is small (less than 10), except for the case of degree-granting training, where it is very large.

The second stage results are presented in Table 4. As discussed previously, we are primarily interested in the comparison between the estimated returns to training entry for each of the two sets of compliers. Panel A sets out the estimates for the indirect cost IV. Consistent with the issues noted when examining the first stage, the estimated coefficients for the wage returns to training are negative, implausibly large and not precisely estimated. Panel B presents the estimates from the direct cost IV. Similarly to Panel A, the coefficients are negative, implausibly large in magnitude and not statistically significant, except for degree-granting trainings (Column 2), where entry into degree-granting training is expected to lead to a €330 reduction in monthly wages. This appears to be evidence of a lock-in effect, which seems plausible as wages are measured only 12 months after the start of the training.

Given these results, the test for the presence of credit constraints is inconclusive. While numerically the estimated returns to training from the indirect cost IV are smaller than those from the direct cost IV, the estimates are not plausible and the correlation between the instruments used and training entry is weak. As explained previously, one possible explanation for the results is that the wage data we are using is missing important components of remuneration, and hence both the indirect cost instrument and the outcome variables are likely to suffer from extensive measurement error. We expect that future versions of the data used will contain more accurate measures of wages.

6 Conclusion

Job training is one of the most popular policy tools governments use to reduce unemployment. These programs may be able to deliver increases in the probability of employment and in earnings on par with university degrees. This paper presented several pieces of evidence supporting the existence of credit constraints in training. Using a training grant reform implemented in several regions in France in 2019, we find that increasing the amount of financial support for training increased entry, an effect which was mainly driven by degree-granting and preparatory trainings. However, this appears to have come at a cost with respect to attendance, as total missed hours training reported by a major provider of training increased when comparing regions with only grant increases with control regions that did not increase the grant. Interestingly, an additional grant reform component, namely smoothing the income of trainees by providing them with an unconditional upfront grant, improved training attendance substantially. This suggests that new entrants were less able to dedicate their time to training, as they may have had to dedicate their time to part-time work instead. We then use a discontinuity arising from the existence of an acceptance threshold for applications to training and find suggestive evidence of high returns to training. Finally, we test for evidence of credit constraints in the IV estimates of returns to training. However, this test is inconclusive due to the overall weak predictive power of the instruments.

The evidence presented in this paper suggests that there may be important financial barriers facing job-seekers who wish to enter training, and that the amount and timing of financial support matters even in countries with a well-developed training policy. The optimal design and targeting of such schemes in the context of adult job training for the unemployed is thus a first-order issue, and is a fruitful venue for further research.

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A Appendix Tables

Table A1: Job-seeker characteristics and predicted training grant change

Trainee type	**Pre-reform grant amount**	**Grant increase**
16-17 y.o.	€130	€552
18-20 y.o.	€310	€342
21-25 y.o.	€339	€313
Over 25 y.o	€401	€251
Worked at least six out of last twelve months Mother with at least three children Divorced, bereaved or separated less than three years ago Single parent or pregnant Disabled	€652	€0
Disabled, worked at least six out of previous 12 months	Avg. of previous 6 months salary (min €644, max €1932)	€0
Self-employed	€708	€0

Note: This table presents the monthly full-time equivalent grant amount for jobseekers not eligible for unemployment benefits, and the corresponding grant increase if the training occurred in one of the three reform regions (Bretagne, Normandie, Bourgone-France-Comté). For individuals who were disabled and worked at least six out of the last twelve months, the grant amount was equivalent to the average salary in the last six months subject to the minimum and maximum amounts indicated in this table.

Table A2: Summary statistics for training applications

	Sample		
	All applications	Logic direct pre-test	Problem solving direct pre-test
Accepted to training (percent)	50.73	48.63	55.79
Female (percent)	33.74	43.08	36.16
Foreigners (percent)	12.15	11.64	12.60
Probability of being employed in month (percent)	32.45	32.14	32.73
Earnings (EUR per month)	600.39	578.30	601.62
Logic score (centred)	0.04	0.01	0.03
Problem solving score (centred)	-0.06	-0.09	-0.03
Percent above threshold (logic)	63.13	57.53	60.23
Percent above threshold (problem solving)	48.29	44.76	50.07
Number of applications	24 219	1 641	5 478

Note: This table presents mean demographic and socioeconomic statistics for a set of different groups from the training application dataset. The first column is calculated based on the entire sample. The second and third column contain only applications from agencies which were selected using a direct first-stage testing procedure using the logic and problem-solving test scores respectively.

Table A3: Pre-reform economic and composition comparison across regions

Variable	Treated mean	Difference Occitanie	Difference Control
Regional economic indicators			
Unemployment (pct points)	7.88	2.05***	0.35
GDP per capita (euros, nominal)	28483.44	829.65**	9939.34
Job-seeker pool characteristics			
Female	0.52	0	-0.01
High-poverty urban area	0.10	0	0.04**
Rural area	0.17	0.04*	-0.1***
High School or less	0.77	-0.07***	-0.07***
Foreigner	0.09	0.04	0.07**
Regional training rates			
Rate of training entry per 10k, all trainings	28.56	-10.98**	-11.97***
Rate of training entry per 10k, degree-granting trainings	20.11	-7.47**	-8.12***
Rate of training entry per 10k, non-degree-granting trainings	1.31	1.12	1.68***
Rate of training entry per 10k, preparatory trainings	7.16	-4.65*	-5.52**

Note: '*' = 0.1, '**' = 0.05, '***' = 0.01. This table presents the results of a balance check regressing the pre-reform value of the row covariates on treatment status, where regressions are weighted by the number of job-seekers in each cell. The first column shows the mean for treated regions, whereas the second and third columns show the mean differences with Occitanie (a region which indicated willingness to change the grant amounts but ultimately did not proceed) and the regions that did not enact the change. Standard errors clustered at the département level.

Table A4: Estimates of the change in job training take-up due to training subsidy increases for French job-seekers by type of training, Occitanie as control region

	All	Degree-granting	Non-degree granting	Preparatory
Panel A: Rate of new trainees per 10 000 jobseeekers				
Reform region x Post x Subsidy change amount	0.0385	0.0148	-0.0002	0.0235
	(0.0130)***	(0.0068)**	(0.0007)	(0.0065)***
Treatment effect at mean subsidy change	11.56	4.43	-0.07	7.05
Pre-reform mean outcome, reform regions	29.56	20.93	1.95	6.76
Observations	136080	136080	136080	136080
R2	0.161	0.113	0.023	0.168
Panel B: Subscribed hours of training per person conditional on any training entry				
Reform region x Post x Subsidy change amount	-0.2621	-0.1809	-0.1656	0.0844
	(0.0680)***	(0.0883)*	(0.0321)***	(0.0414)*
Treatment effect at mean subsidy change	-78.64	-54.28	-49.68	25.32
Pre-reform mean outcome, reform regions	898.62	746.83	51.7	100.08
Observations	37242	37242	37242	37242
R2	0.121	0.127	0.087	0.206

Note:'*' = 0.1, '**' = 0.05, '***' = 0.01. This table presents the results from a triple difference-in-differences estimation of the effect of the change of training subsidy amount on training take-up and planned hours of training by type of training. Predicted treatment effect calculated at mean subsidy increase in reform regions in period immediately before the reform. Numbers in parentheses indicate standard errors clustered at the region by treatment intensity group level. Stars next to each standard error calculation reflect the corresponding p-value.

Table A5: ITT estimates of the change in job training take-up due to training grant increases for French jobseekers

	All	Degree-granting	Non degree-granting	Preparatory
Panel A: Synthetic triple-differences				
Average treatment effect	25.669***	13.201***	1.064	11.938***
	(4.149)	(2.959)	(0.976)	(3.514)
Panel B: Usual triple-difference				
Average treatment effect	16.931***	7.031***	0.893*	8.823***
	(3.339)	(2.213)	(0.458)	(1.661)
Mean pre-reform outcome in no change regions, targeted job seekers	35.952	25.095	2.125	8.775
Observations	130440	130440	130440	130440

Note:'*' = 0.1, '**' = 0.05, '***' = 0.01. This table presents the results from a synthetic triple-differences estimation (Panel A) and the usual triple-differences specification (Panel B) of the effect of the training grant reform on the number of unemployed individuals entering training per academic year. Standard errors in Panel A are calculated using a jackknife procedure, whereas in Panel B they are clustered at the region by treatment intensity group level.

Table A6: Control weights by dependent variable and by region for the synthetic difference-in-differences estimator

	Cross-sectional weights ω_i					
	Mean	SD	P25	P50	P75	P90
All						
Ile-De-France	0.000100802	0.000007091	0.000099825	0.000100671	0.000100768	0.000100774
Auvergne-Rhone-Alpes	0.000101299	0.000007900	0.000100580	0.000100729	0.000100769	0.000100777
Hauts-De-France	0.000100502	0.000000351	0.000100200	0.000100699	0.000100768	0.000100772
Provence-Alpes-Cote D'azur	0.000100675	0.000008566	0.000099630	0.000099770	0.000100725	0.000100770
Grand Est	0.000100501	0.000007693	0.000099532	0.000099687	0.000100729	0.000100772
Occitanie	0.000101591	0.000015647	0.000099662	0.000100711	0.000100768	0.000100781
Centre-Val De Loire	0.000101248	0.000013257	0.000100618	0.000100729	0.000100769	0.000100781
Pays De Loire	0.000101459	0.000008532	0.000100626	0.000100741	0.000100770	0.000100780
Degree-granting						
Ile-De-France	0.000100068	0.000000420	0.000100037	0.000100040	0.000100043	0.000100053
Auvergne-Rhone-Alpes	0.000100062	0.000000338	0.000100031	0.000100040	0.000100042	0.000100051
Hauts-De-France	0.000100045	0.000000053	0.000100037	0.000100040	0.000100044	0.000100056
Provence-Alpes-Cote D'azur	0.000100101	0.000001472	0.000100040	0.000100041	0.000100042	0.000100043
Grand Est	0.000100073	0.000000731	0.000100040	0.000100040	0.000100041	0.000100043
Occitanie	0.000100279	0.000004158	0.000100027	0.000100039	0.000100044	0.000100070
Centre-Val De Loire	0.000100054	0.000000101	0.000100029	0.000100040	0.000100046	0.000100071
Pays De Loire	0.000100090	0.000000663	0.000100026	0.000100039	0.000100044	0.000100067
Non degree-granting						
Ile-De-France	0.000100066	0.000000255	0.000100051	0.000100053	0.000100055	0.000100057
Auvergne-Rhone-Alpes	0.000100188	0.000000662	0.000100054	0.000100057	0.000100069	0.000100159
Hauts-De-France	0.000100083	0.000000162	0.000100054	0.000100056	0.000100063	0.000100077
Provence-Alpes-Cote D'azur	0.000100053	0.000000025	0.000100051	0.000100051	0.000100052	0.000100053
Grand Est	0.000100065	0.000000422	0.000100050	0.000100052	0.000100053	0.000100054
Occitanie	0.000100092	0.000001918	0.000100051	0.000100053	0.000100056	0.000100057
Centre-Val De Loire	0.000100074	0.000000034	0.000100055	0.000100060	0.000100081	0.000100110
Pays De Loire	0.000100668	0.000006003	0.000100054	0.000100059	0.000100081	0.000100193
Preparatory						
Ile-De-France	0.000103420	0.000017737	0.000101523	0.000101687	0.000102005	0.000102470
Auvergne-Rhone-Alpes	0.000108553	0.000035851	0.000101954	0.000102281	0.000102464	0.000102612
Hauts-De-France	0.000102159	0.000000320	0.000101925	0.000102129	0.000102402	0.000102490
Provence-Alpes-Cote D'azur	0.000102871	0.000020395	0.000101509	0.000101655	0.000101809	0.000102009
Grand Est	0.000102008	0.000006315	0.000101443	0.000101554	0.000101682	0.000101832
Occitanie	0.000103399	0.000024649	0.000101524	0.000101654	0.000101828	0.000102088
Centre-Val De Loire	0.000126065	0.000179216	0.000102148	0.000102394	0.000102509	0.000102610
Pays De Loire	0.000104996	0.000015715	0.000102084	0.000102306	0.000102502	0.000102704

Table A7: Estimates of the change in job training take-up due to training grant increases for French jobseekers by type of training, ignoring repeated training starts

	Rate new trainees per 10k jobseekers, all training types			
	All	Degree-granting	Non-degree granting	Preparatory
Reform region x Post x Subsidy change amount	0.0255***	0.0092*	0.0014	0.0154***
	(0.0076)	(0.0052)	(0.0010)	(0.0037)
Pre-reform mean outcome, reform regions	15.2190	10.7400	2.9390	1.7450
Observations	368 528	368 528	368 528	368 528
R2	0.131	0.115	0.057	0.080

Note:'*' = 0.1, '**' = 0.05, '***' = 0.01. This table presents the results from a triple difference-in-differences estimation of the effect of the change of training subsidy amount on entry into training, omitting repeated trainings (trainings starting before the indicated end date of a previous ending). Numbers in parentheses indicate standard errors clustered at the region by treatment intensity group level.

Table A8: IV first stage coefficients

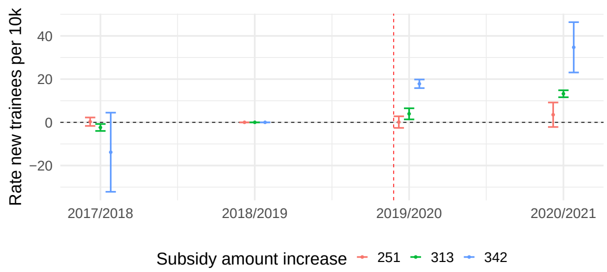
	Training entry			
	Any training	Degree-granting	Non-degree granting	Preparatory
Panel A: Indirect cost IV				
Mean base wage in low-qualification occupations/1000	-0.0031	-0.0011	-0.0010	-0.0009
	(0.0014)	(0.0009)	(0.0007)	(0.0006)
Age	-0.0249*	-0.0073	-0.0020	-0.0157
	(0.0084)	(0.0041)	(0.0013)	(0.0089)
Age^2	0.0005*	0.0001	0.0000	0.0003
	(0.0002)	(0.0001)	(0.0000)	(0.0002)
Married female	-0.0155***	-0.0097**	-0.0055***	-0.0003
	(0.0022)	(0.0017)	(0.0008)	(0.0003)
Single female	-0.0060**	-0.0031*	-0.0037**	0.0008
	(0.0017)	(0.0012)	(0.0009)	(0.0006)
High School or less: High School	0.0118**	0.0072***	0.0024*	0.0023
	(0.0021)	(0.0008)	(0.0009)	(0.0013)
High School or less: Some higher ed	0.0023	0.0016	0.0014	-0.0006
	(0.0014)	(0.0010)	(0.0010)	(0.0019)
Rural area	-0.0058**	-0.0025*	-0.0022	-0.0011
	(0.0015)	(0.0010)	(0.0011)	(0.0009)
High-poverty urban area: Less affluent urban area	0.0084**	0.0053*	0.0020	0.0009
	(0.0024)	(0.0020)	(0.0009)	(0.0011)
High-poverty urban area: Not High-poverty urban area	-0.0021	0.0004	-0.0011	-0.0014
	(0.0015)	(0.0008)	(0.0011)	(0.0012)
Foreign national: Foreigner	-0.0097	-0.0069**	-0.0051	0.0011
	(0.0063)	(0.0021)	(0.0049)	(0.0030)
Foreign national: French	-0.0328***	-0.0154***	-0.0148*	-0.0038
	(0.0051)	(0.0021)	(0.0054)	(0.0019)
Cumulative unemployment spell duration (days)	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Total earnings over last 12 months/1000	-2.4121***	-1.3782***	-0.6408***	-0.3945*
	(0.3768)	(0.2130)	(0.0751)	(0.1476)
Kleibergen-Paap F-stat	3.4680	1.4770	1.3890	2.0660
Mean pre-reform training take-up	0.0467	0.0277	0.0107	0.0084
Observations	1639149	1639149	1639149	1639149
R2	0.034	0.021	0.006	0.046
Region × Time FE	X	X	X	X
Region × Predicted subsidy change amount FE	X	X	X	X
Predicted subsidy change/1000 × Time FE	X	X	X	X
Panel B: Direct cost IV				
Predicted subsidy change/1000 × Treated region × Post	0.0077	0.0143***	-0.0024	-0.0040
	(0.0054)	(0.0012)	(0.0020)	(0.0071)
Age	-0.0249*	-0.0073	-0.0020	-0.0157
	(0.0084)	(0.0043)	(0.0013)	(0.0094)
Age^2	0.0005*	0.0001	0.0000	0.0003
	(0.0002)	(0.0001)	(0.0000)	(0.0002)
Married female	-0.0155***	-0.0097**	-0.0055***	-0.0003
	(0.0022)	(0.0017)	(0.0008)	(0.0003)
Single female	-0.0060**	-0.0031	-0.0037**	0.0008
	(0.0017)	(0.0014)	(0.0009)	(0.0006)
High School or less: High School	0.0118**	0.0072***	0.0024*	0.0023
	(0.0021)	(0.0009)	(0.0009)	(0.0011)
High School or less: Some higher ed	0.0023	0.0016	0.0014	-0.0007
	(0.0014)	(0.0010)	(0.0010)	(0.0015)
Rural area	-0.0058**	-0.0025*	-0.0022	-0.0011
	(0.0015)	(0.0010)	(0.0011)	(0.0009)
High-poverty urban area: Less affluent urban area	0.0084**	0.0054*	0.0020	0.0009
	(0.0024)	(0.0021)	(0.0009)	(0.0011)
High-poverty urban area: Not High-poverty urban area	-0.0021	0.0004	-0.0011	-0.0014
	(0.0015)	(0.0011)	(0.0011)	(0.0013)
Foreign national: Foreigner	-0.0097	-0.0069**	-0.0051	0.0011
	(0.0063)	(0.0022)	(0.0049)	(0.0038)
Foreign national: French	-0.0328***	-0.0154***	-0.0148*	-0.0038
	(0.0051)	(0.0021)	(0.0054)	(0.0028)
Cumulative unemployment spell duration (days)	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Total earnings over last 12 months/1000	-2.4126***	-1.3783***	-0.6410***	-0.3946*
	(0.3769)	(0.2129)	(0.0753)	(0.1477)
Kleibergen-Paap F-stat	1.6190	145.2560	0.7920	0.3260
Mean pre-reform training take-up	0.0467	0.0277	0.0107	0.0084
Observations	1639149	1639149	1639149	1639149
R2	0.034	0.021	0.006	0.046
Region × Time FE	X	X	X	X
Region × Predicted subsidy change amount FE	X	X	X	X
Predicted subsidy change/1000 × Time FE	X	X	X	X

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

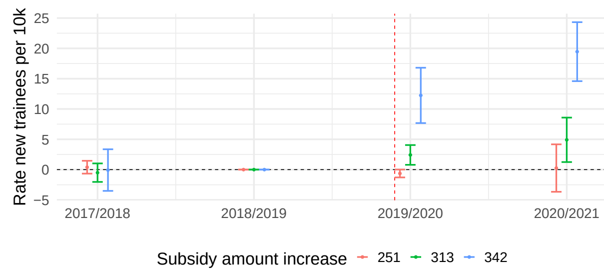
B Appendix Figures

Figure B1: Event study graphs of changes in job training take-up by type of training, Occitanie as control region

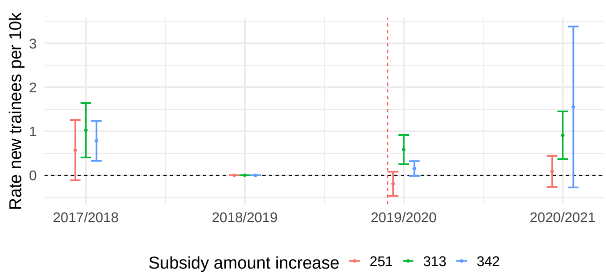
(a) All types



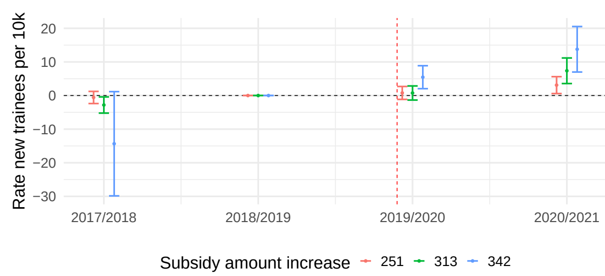
(b) Degree-granting



(c) Non-degree-granting



(d) Preparatory training



Note: These figures graph the coefficients β_{ES} from specification (3) for the triple interaction between an indicator for the periods after the grant change reform, an indicator for regions which implemented the grant change, and an indicator variable for each discrete predicted change amount. The vertical red line indicates the start of the reform at the beginning of the admission year 2019/2020. Following the usual convention in the literature, the coefficient in the last pre-reform period is normalised to 0. Regressions are weighted using the number of job seekers in each group. Confidence intervals are at the 95 percent level and account for clustering at the département level.

Figure B2: Synthetic triple-differences treated and synthetic control cell outcomes

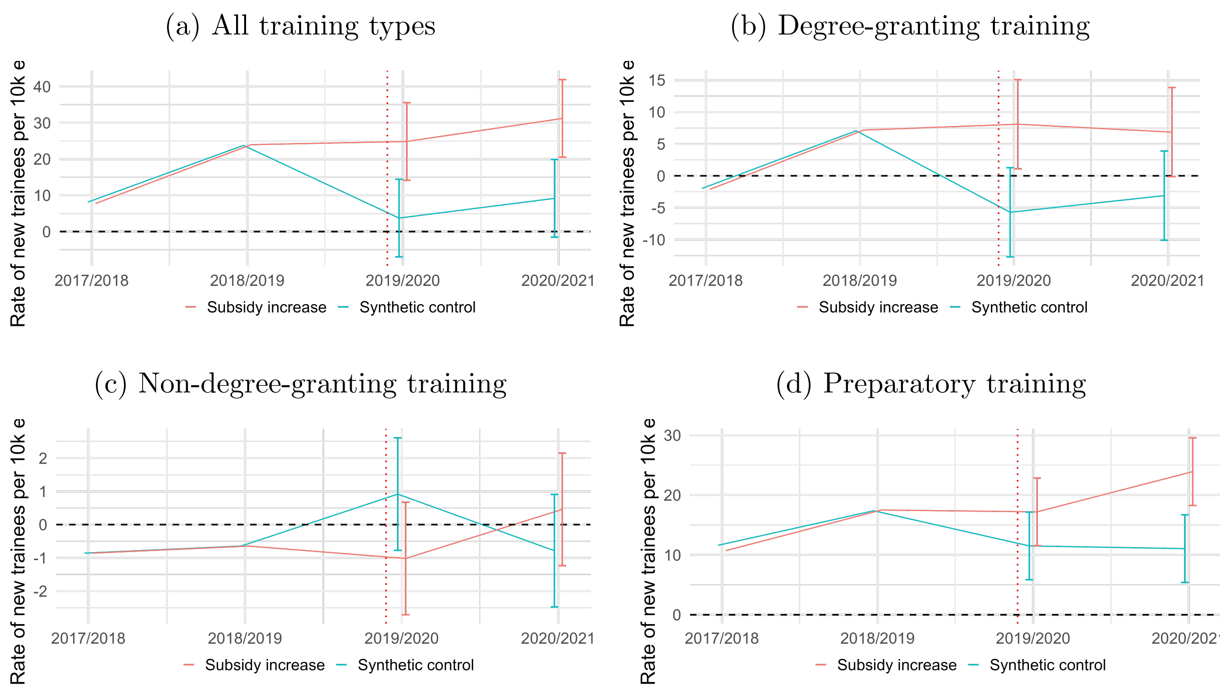
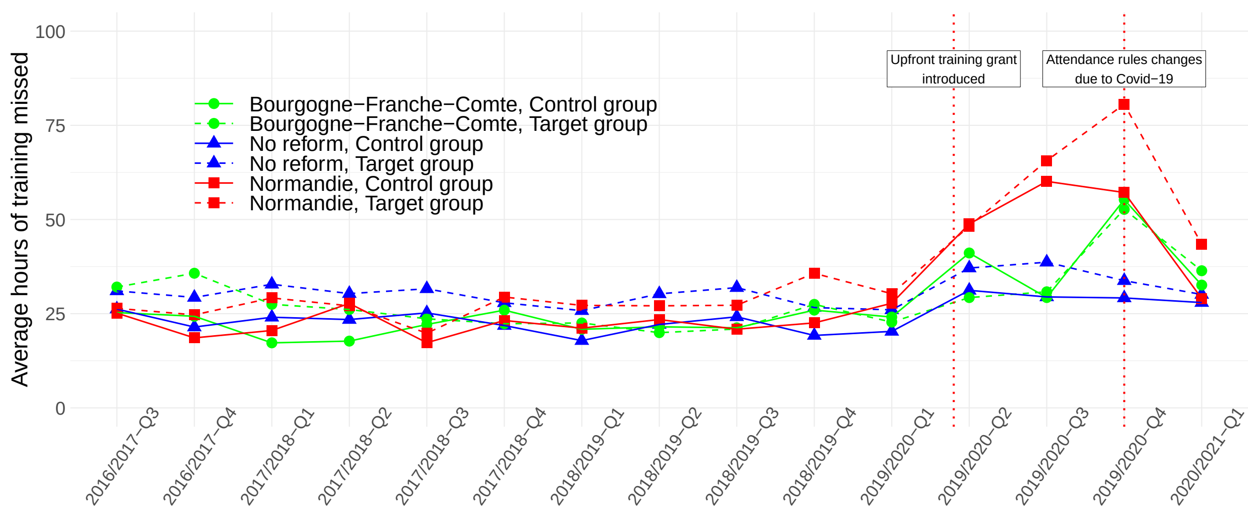
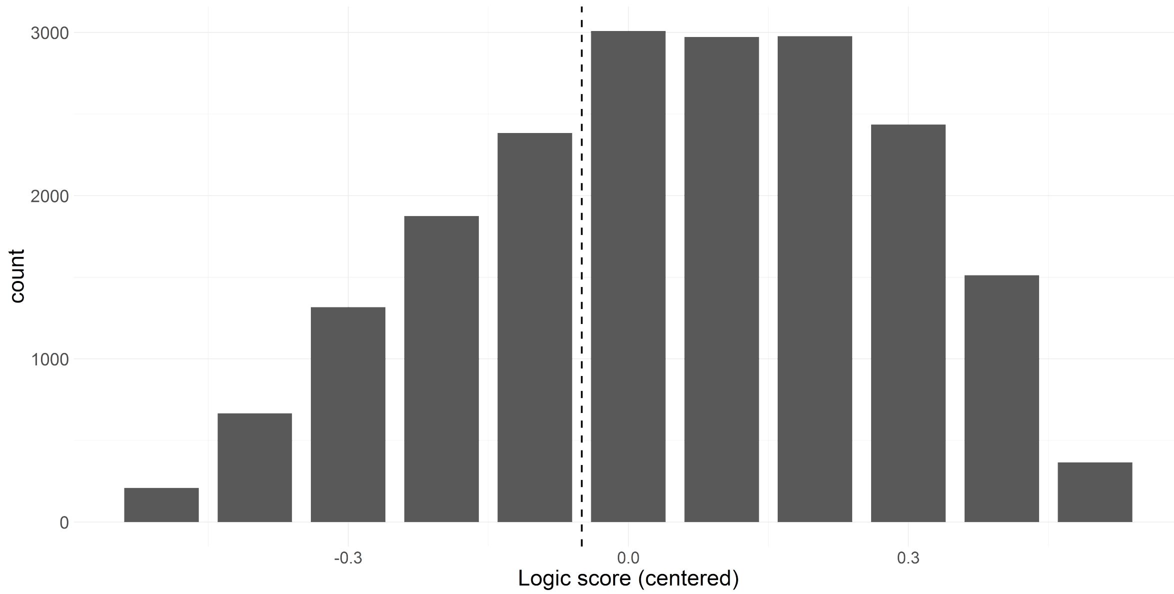


Figure B3: Average hours of training missed across reform and control regions

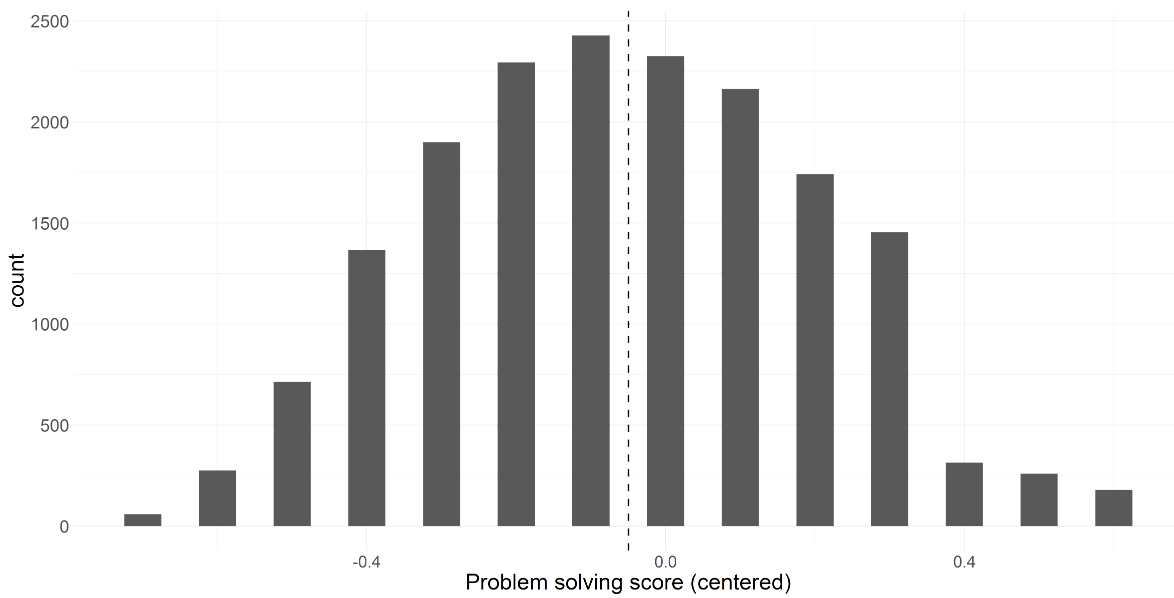


Note: This figure presents the average number of hours missed from campus-based training, all absence types included, for training courses offered by the Afpa. Each line denotes a combination of reform target group and region reform type. The reform target group is job-seekers without unemployment benefits, and the control group is job-seekers eligible for unemployment benefits. The different regions according to reform type are Bourgogne-Franche-Comté (which both increased the monthly subsidy and added an unconditional cash grant at the start of training), Normandie (which only increased monthly subsidies) and the control regions, which did not change their reform amounts. The first vertical line indicates the start of the grant reform, while the second indicates the onset of the Covid-19 pandemic and the subsequent change in rules governing the recording of missed training sessions. Time (the x-axis) is the quarter of start of the training spell, and is labelled with reference to the yearly cycle of training admissions, with Q1 starting in May and Q4 ending in April of the following calendar year. The first vertical line indicates the start of the grant reform, while the second indicates the onset of the Covid-19 pandemic and the subsequent change in rules governing the recording of missed training sessions.

Figure B4: Running variable distributions



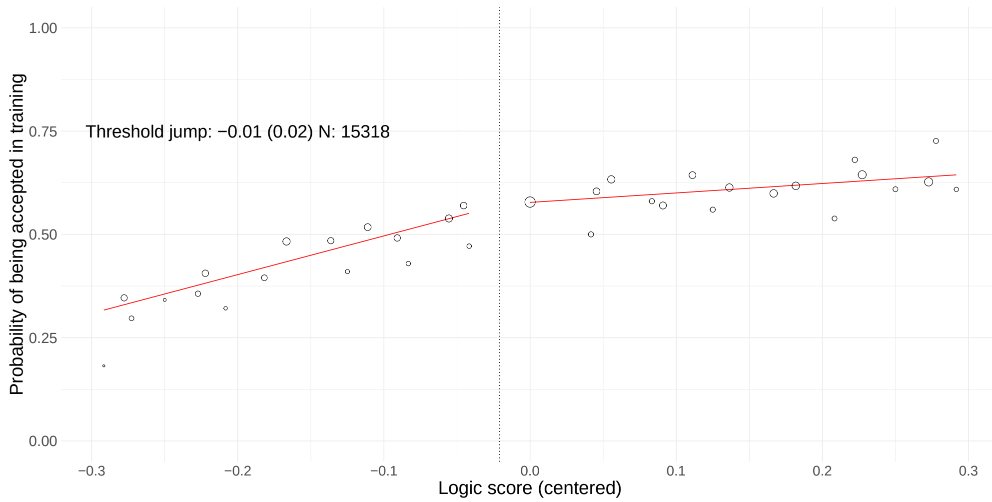
(a) Logic test score



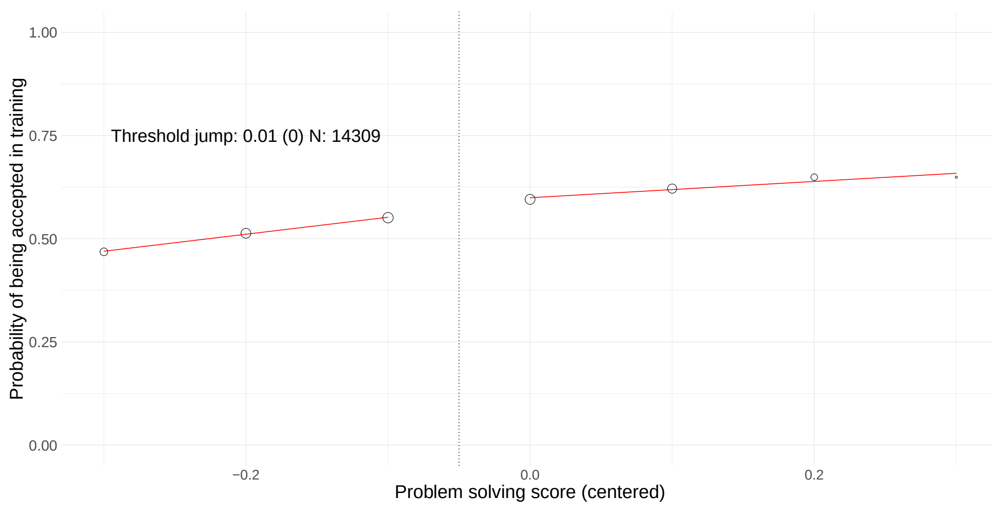
(b) Problem solving test score

*N*ote: These figures plot the distribution of logic and problem solving test scores from admissions tests administered to job training applicants who applied to the Afpa over the course of Q2 2015 to Q4 2016. The logic score distribution was plotted using bins of width 0.10, with the value under each bar corresponding to the left limit of the bin. The probability score distribution has a small number of discrete support points, hence we plot the frequencies for each possible discrete value.

Figure B5: Pooled first stage training acceptance in the RDD estimation of returns to training



(a) Logic score test

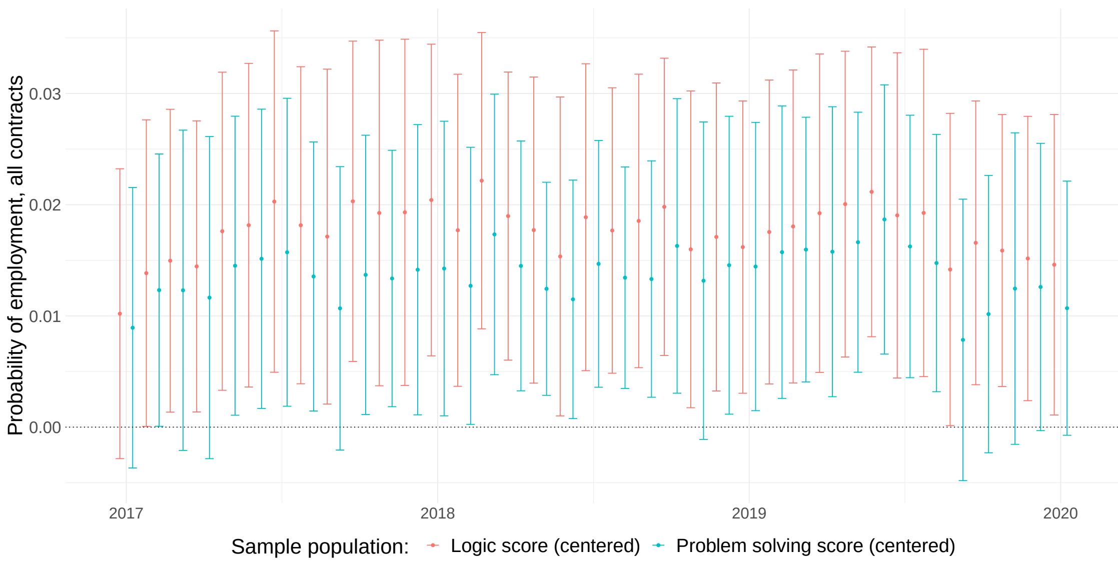


(b) Problem solving score test

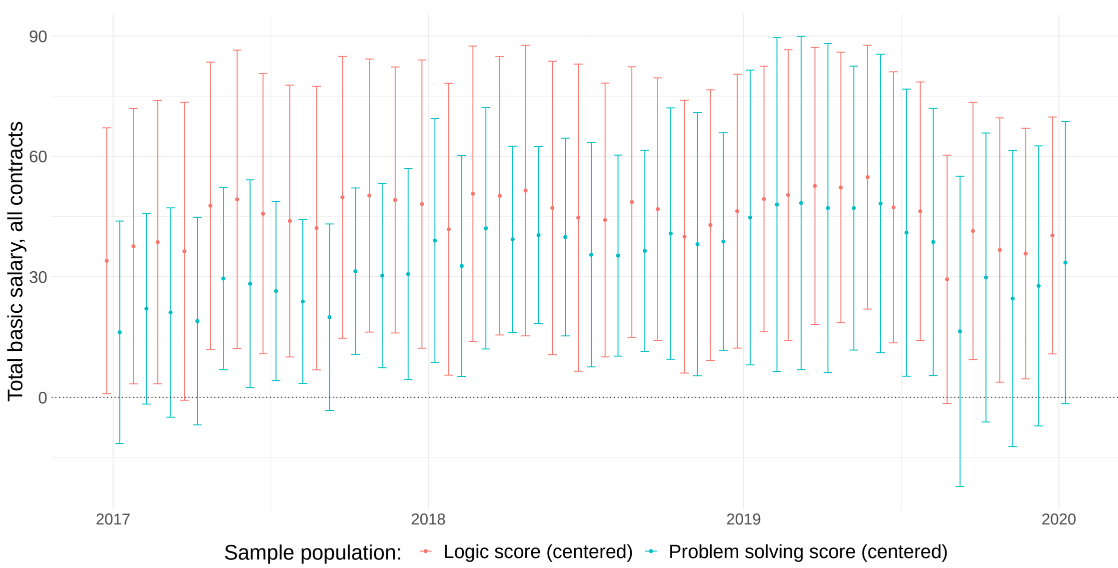
Note: This figure presents the proportion of individuals admitted into training as a function of the running variable, estimated on a pooled sample of applications with no pre-selection at the first stage. In panel A, the running variable is the normalised logic test score, whereas in panel B it is the normalised problem solving test score. We plot the average acceptance rate for each discrete value of the running variable. Point sizes correspond to the number of applications at each running variable value. The vertical dotted line indicates the acceptance threshold.

Figure B6: Estimated returns to training by month

(a) Probability of being employed



(b) Total wages earned



Note: These figures graph the estimated effect of training from a direct OLS regressing the outcome variable on an indicator variable for being accepted to training, together with the same controls as used for the regression discontinuity design and controlling for either logic or problem-solving test score. Confidence intervals are at the 95 percent level and account for clustering at the logic score level in order to be comparable with the results from the regression discontinuity estimation.

C Model of training choice under credit constraints

Model setup

Timing:

The model has two periods:

- In period 0, job-seekers make a decision on whether to train and on their consumption schedules. If they train, they do so at a net cost τ. If not, they receive a payoff of 0.
- In period 1, job-seekers receive a continuation value of employment/wages depending on their training decision. If they trained in the first period, they receive W_H and W_L if they didn't.

Preferences:

Let $T \in \{0, 1\}$ be the training decision indicator. The associated value function is

$$V_T = ln(c_0) + \delta\, ln(c_1) + \mu(T) \tag{C1}$$

where c_0 and c_1 are consumption choices in periods 0 and 1[9], δ is a time preference parameter, and $\mu(T)$ represents unobserved taste for training. Note that in what follows, unobserved taste for training is independent of potential wages.

Budget constraint:

The individual must finance consumption, and has access to credit markets at an interest rate R which can vary.

We assume that individuals know the continuation values of training W_H and not training W_L with certainty, and that these also include future labour supply decisions. Their income stream depends on their training choice and is

$$I_T = \begin{cases} I_0 = W_L/R \text{ if } T = 0 \\ I_1 = W_H/R - \tau \text{ if } T = 1 \end{cases} \tag{C2}$$

The individual's optimisation problem is to choose training participation and a consumption schedule such that utility is maximised subject to the budget constraint.

[9]Note that we can't use linear utility here as the consumption choices would be undefined. This is because both the level curves and the budget constraint are linear.

Model analysis

Outline of the argument The parameter R controls the degree of credit constraints that potential trainees are exposed to. First, we will show that the probability of training is decreasing in R. Then, we will examine how changes in direct costs τ and opportunity costs W_L affect the training salary W_H demanded by the marginal participant in training when R increases.

Indirect utility functions

The Lagrangian for this problem is:

$$\mathcal{L}_T = V_T + \lambda\Big[I_T - c_0 - \frac{c_1}{R}\Big] \tag{C3}$$

From the first order conditions, we have that:

$$\frac{1}{c_0} = \lambda$$
$$c_1 = R\delta\, c_0$$

Substituting in the budget constraint and solving for c_0, we have two consumption paths and hence two value functions depending on the training choice.

Case 1: T = 0

$$c_0 = \frac{W_L/R}{1+R\delta}$$
$$V_0 = ln\Big(\frac{W_L/R}{1+R\delta}\Big) + \delta ln\Big(R\delta\,\frac{W_L/R}{1+R\delta}\Big) + \mu(0)$$

Case 2: T = 1

$$c_0 = \frac{W_H/R - \tau}{1+R\delta}$$
$$V_1 = ln\Big(\frac{W_H/R - \tau}{1+R\delta}\Big) + \delta ln\Big(R\delta\,\frac{W_H/R - \tau}{1+R\delta}\Big) + \mu(1)$$

Conditional on wages, training costs, and time preference, individual will choose to train if $V_1 > V_0$, that is if

$$D > \mu(0) - \mu(1)$$

where

$$D \equiv \left[ln\Big(\frac{W_H/R-\tau}{1+R\delta}\Big)+\delta ln\Big(R\delta\,\frac{W_H/R-\tau}{1+R\delta}\Big)\right]-\left[ln\Big(\frac{W_L/R}{1+R\delta}\Big)+\delta ln\Big(R\delta\,\frac{W_L/R}{1+R\delta}\Big)\right]$$
$$= (1+\delta)ln\Big(\frac{W_H/R-\tau}{W_L/R}\Big)$$

From this expression, we can see that the probability of participating in training increases with the degree of time preference δ and the trained potential wage W_H, and decreases in direct training costs τ and the untrained potential wage W_L.

The effect of an increase in credit constraints can be thought of as an increase in the interest rate facing the individual. The change in the probability of training induced by a change in the interest rate is given by:

$$\frac{\partial D}{\partial R} = -(1+\delta)\frac{\tau}{R(W_H/R-\tau)}$$

which is negative as long as the net present value of training $\frac{W_H}{R}-\tau$ is positive. Hence, the more an individual is credit constrained, the smaller the probability they will participate in training.

Effects of direct vs opportunity costs on the LATE

We know from the work on marginal treatment effects (MTEs) that the estimated LATE will be a weighted average of the individual MTEs (Björklund and Moffitt, 1987; Heckman and Vytlacil, 2005). Since we don't have heterogeneity in this model, the MTE (conditional on the parameters) will be constant. Therefore, to see how the LATE changes we need to look at how the MTE changes. The MTE is the treatment effect for individuals who are indifferent between taking and not taking up the treatment, i.e.

$$\text{LATE} = \text{MTE} \equiv E(W_H - W_L|D=0) \tag{C4}$$

In our case, we have that individuals are indifferent if the net present value of the two choices is the same:

$$(1+\delta)ln\Big(\frac{W_H/R-\tau}{W_L/R}\Big) = 0, \text{ i.e. if}$$
$$\frac{W_H}{R}-\tau = \frac{W_L}{R}$$

Given this simple relationship between potential wages for the marginal entrant into training, we can define the training wage W_H^* demanded by the marginal entrant into training as a function of the untrained wage and direct costs of training:

$$W_H^*(\tau, W_L) = R\tau + W_L$$

Since $\dfrac{\partial W_H^*}{\partial \tau} = R$ and $\dfrac{\partial W_H^*}{\partial W_L} = 1$, the wage demanded by the marginal entrant into training will react more to a unit change in direct costs τ than to a unit change in the opportunity cost W_L. Furthermore, since $\dfrac{\partial^2 W_H^*}{\partial R \partial \tau} = 1$ and $\dfrac{\partial^2 W_H^*}{\partial R \partial W_L} = 0$, the difference between these effects will increase with the severity of credit constraints. Hence, we expect the LATE estimated using an instrument for direct costs to be higher than the LATE estimated using an instrument for opportunity costs, and this difference to increase with the severity of credit constraints.