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// JÉRÉMY HERVELIN, PIERRE VILLEDIEU, AND ANTHONY TERRIAU

Active Labor Market Policy for Unemployed Youths

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Jeremy Hervelin† Pierre Villedieu‡ Anthony Terriau§

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Abstract

This paper evaluates a large-scale French program (PACEA) providing intensive job-search assistance and placement into training to unemployed youths. Guided by a job-search and matching model, we combine fine-grained administrative data with an instrumental-variables strategy that exploits the quasi-random assignment of youths to caseworkers and their heterogeneous propensity to enroll youths into the program, identifying the causal impact of participation over a four-year horizon. PACEA raises employment probabilities by about 10% after completion. These gains arise exclusively along the extensive margin and are not accompanied by higher wages, contract stability, or occupational upgrading. Analysis of mechanisms points to intensified interactions with caseworkers, more structured job-search trajectories, and short vocational training. Bringing the model to the data, we document negative spillovers on non-participants and highlight the importance of labor demand and program design for achieving higher aggregate employment gains and more favorable benefits-costs ratios.

Keywords: Job search assistance, Training, Active labor market policies, General equilibrium, Youth unemployment
JEL codes: J64, J68

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†ZEW, LERN-CIDER. Corresponding author, contact at jeremy.hervelin@zew.de.
‡DARES, French Ministry of Labor. Contact at pierre.villedieu@travail.gouv.fr.
§Le Mans University. Contact at anthony.terriau@univ-lemans.fr.

1 Introduction

Youth unemployment is one of the most persistent challenges facing advanced economies. In mid-2024, the unemployment rate of workers aged 16 to 24 stood at 9.8 percent in the United States and around 14.5 percent in the European Union – roughly two to three times the rate of prime-age adults.[1] These discrepancies are the results of frictions – unemployed workers and firms must search for one another under imperfect information, heterogeneous skills, and coordination constraints – which are particularly acute for disadvantaged youths, whose limited networks, weak signaling capacity, and uncertain productivity reduce matching efficiency (Kramarz and Skans, 2014; Neumark, 2018).

Governments respond with active labor market policies (ALMPs), which aim to mitigate these frictions by improving search efficiency through counseling and monitoring, and by enhancing productivity through training and skill acquisition (OECD, 2025; Le Barbanchon et al., 2024). ALMPs represent a sizable and uneven public investment: OECD countries spend on average about 0.4 percent of GDP on such programs, ranging from roughly 1.5 percent in Denmark to less than 0.1 percent in the United States, one of the lowest spenders.[2] Youths account for a substantial share of participants in training and subsidized employment schemes, yet existing empirical and theoretical work typically studies job-search assistance and training in isolation. We still lack a framework that jointly evaluates how these components interact, both at the individual level and in general equilibrium.

This paper asks whether intensive job-search assistance programs, possibly combined with vocational training, can durably improve labor market outcomes for unemployed youths, and through which channels. Beyond estimating average employment effects, we investigate whether such programs affect job quality, wages, and occupational trajectories, and whether micro-level gains translate into aggregate employment improvements once general-equilibrium effects are taken into account.

The answer is not straightforward *a priori*, because the two components of activation operate through opposing mechanisms. Job-search assistance increases effective search effort and may accelerate reemployment, but it can also intensify competition for a fixed number of jobs and displace non-participants (Cahuc and Barbanchon, 2010; Crépon et al., 2013). Training, by contrast, raises post-hire productivity and can stimulate vacancy creation, yet its effects materialize more slowly (Osikominu, 2013). To make this tension precise, we develop a stylized job-search and matching model with heterogeneous search efficiency and training-induced productivity. The model shows that individual employment gains need not translate

[1] Sources: U.S. Bureau of Labor Statistics, *Employment and Unemployment Among Youth* (youth aged 16–24, July 2024); Eurostat, *Unemployment statistics* (youth aged 15–24, EU, 2024).

[2] Source: OECD, *Public spending on labour markets* (2023). France spends about 0.7-0.8 percent of GDP on ALMPs, and its *missions locales* network alone mobilized around €910 million in 2021 (Union nationale des Missions Locales).

into higher aggregate employment: when job creation is costly or training intensity is low, expanding program coverage mainly reallocates existing jobs across workers and generates displacement, whereas sufficiently intensive training shifts labor demand outward and can create net jobs. Whether an activation program raises employment for all youths is therefore an empirical question that hinges on how search assistance is combined with training.

We study a large-scale French program, *Parcours contractualisé d'accompagnement vers l'emploi et l'autonomie* (PACEA), implemented nationwide in 2017 and delivered by the network of "missions locales" (French public agencies assisting young individuals). These youth centers provide individualized counseling, workshops, work placements, and conditional financial support to disadvantaged and out-of-school youths aged 16-25. PACEA is representative of modern "work-first" youth activation policies combining intensive caseworker support with flexible program content. Caseworkers, as primary intermediaries delivering services within these programs, play a crucial role (Rosholm, 2014; Schiprowski, 2020; Dromundo and Haramboure, 2021; Rasmussen, 2021; Cederlöf et al., 2024). Our analysis relies on newly assembled, fine-grained administrative data that match individual-level information from "missions locales" with exhaustive employment and training records from social security sources. The data allow us to follow nearly 300,000 youths over four years, observe detailed interactions with caseworkers, participation in workshops and training, and measure employment, wages, contract types, and occupations.

To identify causal effects, we exploit the quasi-random assignment of youths to caseworkers within centers and entry periods. There are no institutional rules that systematically match particular youths to specific caseworkers: upon arrival, youths meet the caseworker assigned to welcome new entrants that day, on a rotation planned several weeks in advance. Consistent with this setup, balance tests show no evidence of selection on observables between youths and caseworkers. At the same time, caseworkers display substantial and persistent heterogeneity in their propensity to enroll youths into PACEA, with enrollment rates ranging from 3 to 95 percent across the 2,269 caseworkers in our sample. We exploit this heterogeneity by constructing a leave-one-out measure of caseworker enrollment orientation as an instrument for participation. This strategy identifies local average treatment effects for youths whose participation is induced by caseworker assignment, following a growing literature that leverages judge- and caseworker-based instruments (Frandsen et al., 2023; Arni and Schiprowski, 2019).

Our IV estimates show that PACEA has no significant effect on employment in the first two years after registration – the period that broadly overlaps with participation in the program – but increases employment probabilities by about 4 to 5 percentage points three to four years after entry, roughly a 10 to 12 percent gain relative to the control mean. We find similar gains at the intensive margin: PACEA raises time in employment by about 9 to 12 additional days per year three to four years after registration, roughly an 8 to 9 percent increase relative to the control mean. Despite these sizable employment gains, PACEA does not raise wages, contract

stability, or occupational upgrading. Participants remain concentrated in low-paid jobs close to the minimum wage, with no detectable shift toward permanent contracts or higher-skilled occupations, and are employed primarily in low-skill service, industrial, and manual jobs that rely on basic operational skills rather than certified qualifications. Employment gains due to PACEA thus operate primarily at the extensive margin.

The analysis of mechanisms shows that PACEA substantially increases interactions with caseworkers, structures job-search trajectories, and raises participation in workshops and short vocational training. Youths experience about 50 events within their center instead of 12, including some 20 events with their referent caseworker instead of 5.[3] Participants typically move through sequential phases, from exploration to implementation, with a strong emphasis on career orientation and job-search assistance, and are more likely to enter intensive welfare-to-work schemes combining collective workshops, coaching, and income support. They are also more likely to complement their job search with vocational training: during participation, 20 percent experience external training versus 15 percent for non-participants, though training episodes are brief (about 30 days and 150 hours on average). Given the centrality of the referent caseworker in coordinating job search, monitoring progress, and adjusting objectives, these results point to a substantial strengthening of individualized support combined with modest human-capital investment.

Finally, we take the general-equilibrium predictions of the model to the data. Exploiting variation in program coverage across locations and time, we show that the employment prospects of non-participants is negatively correlated with the share of youths in PACEA. These negative spillovers can amount to 50% of the private micro-gain but are not significant under OLS specifications. We also bring the model to the data with a calibrated exercise and congestion effects indeed seem to represent about 50% of the micro-gain, indicating that expanding coverage without sufficiently intensive training generates displacement rather than net job creation. Consistent with the model, the sizable micro-level gains coexist with weak aggregate effects. We conclude by discussing policy scenarios under a fixed budget and highlight that concentrating resources on fewer participants with more intensive training may dominate broad but shallow activation strategies.

Related literature Our paper contributes to three strands of the literature. First, it relates to active labor market policies, and specifically to job-search assistance (JSA) and training. JSA programs typically generate modest short-run gains in reemployment, while training yields larger medium-term returns (Card et al., 2018). Causal evidence shows that design features – early engagement, credible conditionality, and structured caseworker protocols – significantly affect unemployment duration (Black et al., 2003; Graversen and van Ours, 2008; Le Barbanchon et al., 2024). General-equilibrium analyses caution, however, that local gains may reflect

[3]Events include individual meetings, information sessions, digital contacts, and administrative tasks.

displacement or congestion, with intensified placement services crowding out non-participants (Crépon et al., 2013; Gautier et al., 2018; Cheung et al., 2024). Effects for young job seekers are often smaller and more heterogeneous than for prime-age workers, reflecting weaker search capital and productivity (Caliendo and Schmidl, 2016; Kluve et al., 2019). We contribute by providing causal evidence on a nationwide youth-oriented ALMP that combines intensive JSA with limited but targeted placement in training. Despite temporary lock-in during participation, PACEA generates sizeable and persistent employment gains three to four years after entry – larger than those typically found for youth-focused JSA. These effects operate primarily along the extensive and intensive margins, without wage or occupational upgrading, suggesting improvements in search efficiency rather than substantial human-capital accumulation. By explicitly linking reduced-form estimates to general-equilibrium mechanisms, we further document negative spillovers in high-coverage areas, showing that the aggregate impact of youth JSA hinges on training intensity and program scale.

Second, we contribute to the literature on caseworkers as key intermediaries in labor market policies. Evidence shows that higher-value-added caseworkers significantly reduce unemployment duration and raise job-finding rates in Switzerland, Denmark, Sweden, and France (Schiprowski, 2020; Rasmussen, 2021; Cederlöf et al., 2024; Dromundo and Haramboure, 2021). Heterogeneity may reflect personality traits or interaction styles (Behncke et al., 2010b; van den Berg et al., 2020), but recent work emphasizes caseworker practices and monitoring intensity (Cederlöf et al., 2024). Tougher or less accommodating caseworkers often place job seekers more effectively (Behncke et al., 2010a; Huber et al., 2017), through mechanisms such as stricter application requirements (Arni and Schiprowski, 2019), vacancy transmission (Glover, 2021; Cheung et al., 2023), training assignment (Schmieder and Trenkle, 2020; Humlum et al., 2025), or the information exchanged during meetings (Schiprowski, 2020; Maibom et al., 2017). While this work focuses almost exclusively on adult job seekers in public employment services, we extend the analysis to a youth-oriented program delivered by *missions locales*, documenting substantial heterogeneity in caseworkers' PACEA placement rates and showing that sustained, structured interactions increase program participation and strengthen job-search planning among disadvantaged youths.

Finally, we contribute to the literature on school dropouts and their access to employment. Early school leaving is shaped by guidance failures and educational mismatch (Hoxby and Avery, 2013; Arcidiacono et al., 2025; Guyon and Huillery, 2021; Kirkeboen et al., 2016; Clemens et al., 2021; Dahl et al., 2022), and dropouts face persistently higher unemployment and inactivity risks due to limited labor-market skills and knowledge of job-search processes (Rumberger and Lamb, 2003). School dropouts also turn to public employment services only when private networks are already exhausted, reflecting limited access to firms and information (Kramarz and Skans, 2014; Lo Bello and Morchio, 2022). We show that caseworker-driven enrollment into activation programs improves job-search structure, workshop participation,

and vocational-training take-up, translating into higher employment probabilities, primarily at the extensive margin. Targeted activation combining close caseworker support with basic skill-building can therefore meaningfully facilitate labor-market entry for vulnerable school leavers, even absent wage or occupational upgrading.

The remainder of the paper is organized as follows. Section 2 develops the theoretical framework. Section 3 presents the institutional context, the data, and the population we study. Section 4 describes the empirical strategy to evaluate the effect of the program. Section 5 presents the main results on different labor market outcomes. Section 6 highlights the main mechanisms and discusses the macro-consequences. Section 7 establishes the benefit-cost ratio and debates the optimal policy-mix under a fixed budget. Finally, Section 8 concludes.

2 Theoretical framework

To guide the empirical analysis and interpret its results, we develop a stylized general-equilibrium job-search and matching model. The framework isolates the two margins through which an active labor market policy operates – *search assistance*, which raises search efficiency, and *training*, which raises post-hire productivity – and shows why individual employment gains need not translate into aggregate job creation.[4]

2.1 Job search and matching model

2.1.1 Environment

Time is continuous. The labor force is normalized to one and split between employment n and unemployment u, with $n + u = 1$. Jobs are destroyed exogenously at rate $\delta > 0$, and workers and firms discount the future at rate $r > 0$.

Let v denote vacancies and eu aggregate effective search.[5] Matches are produced by a matching function $m(eu, v)$ that is increasing, concave, and homogeneous of degree one. Labor market tightness is $\theta \equiv v/eu$, and the vacancy-filling and job-finding rates *per unit of effective search* are

$$q(\theta) = \frac{m(eu, v)}{v}, \qquad p(\theta) = \frac{m(eu, v)}{eu} = \theta q(\theta),$$

with $q'(\theta) < 0$ and $p'(\theta) > 0$.

2.1.2 Treatment: search assistance and training

A fraction $\pi \in [0, 1]$ of newly unemployed workers is assigned to the program. Treatment affects outcomes through two exogenous components. *Search assistance* raises search efficiency:

[4] We keep the exposition deliberately compact and relegate the heavier derivations to Appendix.

[5] Aggregate effective search is the efficiency-weighted sum of non-treated and treated unemployment, $eu \equiv e_0 u_0 + e_1 u_1$.

unemployed workers differ in efficiency $e_0 > 0$ (non-treated) and $e_1 > 0$ (treated), with $e_1 > e_0$, so that job-finding rates are $\lambda_i = e_i\, p(\theta)$.[6] *Training* raises post-hire productivity: conditional on being hired, workers operate with job-specific human capital $h_0 = 1$ (non-treated) and $h_1 = 1 + \Delta$ (treated), with $\Delta > 0$.[7] Thus $e_1 > e_0$ captures search assistance, whereas $h_1 > h_0$ captures training.

2.1.3 Steady-state employment

Treatment status is assigned upon entry into unemployment and applies throughout the spell. Let u_0 and u_1 denote non-treated and treated unemployment, with $u = u_0 + u_1 = 1 - n$. In steady state, flows out of unemployment equal flows in:

$$e_0 u_0 p(\theta) = (1 - \pi)\delta n, \tag{1}$$

$$e_1 u_1 p(\theta) = \pi \delta n. \tag{2}$$

Summing and using $u = 1 - n$ yields the Beveridge mapping in (n, θ)-space:

$$n = \frac{p(\theta)}{p(\theta) + \delta \left[\frac{1-\pi}{e_0} + \frac{\pi}{e_1}\right]}. \tag{3}$$

2.1.4 Microfoundation

Wage w is taken as given.[8] Let U_i denote the unemployment value of a type-i worker and E the common value of employment. Unemployed workers receive a flow payoff b and find jobs at rate $e_i p(\theta)$; employed workers earn w and, upon job destruction, re-enter unemployment and are reassigned to treatment with probability π. Hence

$$rU_i = b + e_i p(\theta)\,(E - U_i)\,, \qquad i \in \{0, 1\}, \tag{4}$$

$$rE = w - \delta\left(E - \bar{U}\right), \qquad \bar{U} \equiv (1 - \pi)U_0 + \pi U_1. \tag{5}$$

Because all matches pay the same wage and treatment status is redrawn on entry into unemployment, the employment value is common across types; what differs across types is only the value of being unemployed.

[6] We take e to be exogenously determined by the treatment. Alternatively, e may be interpreted as endogenous search effort; endogenizing it would introduce an individual decision margin without altering the general-equilibrium congestion channel emphasized here.

[7] Training includes here formal vocational training where individuals learn general and/or specific skills.

[8] Allowing the wage to be determined endogenously (e.g., through bargaining) would introduce an additional wage-adjustment channel. The model's conclusions are robust as long as firms capture a strictly positive share of the match surplus. Moreover, wages earned by young individuals are close to the minimum wage and similar across treated and control youths as shown in Section 5.3.

Firms post vacancies before observing which worker type they match with, so job creation depends on the expected value of a filled job. In steady state the treated share among employed workers equals π,[9] so effective labor is $\tilde{n} \equiv (1-\pi)n + (1+\Delta)\pi n = (1+\pi\Delta)n$ and aggregate output is $Y = \tilde{n}^{\alpha} = (1+\pi\Delta)^{\alpha} n^{\alpha}$, with $\alpha \in (0,1)$ governing the curvature of labor demand. Because vacancies are posted before the worker type is known, firms evaluate an additional job at its expected flow output,

$$\bar{y} \equiv \frac{dY}{dn} = \alpha(1+\pi\Delta)^{\alpha} n^{\alpha-1}. \tag{6}$$

Letting J_i and V denote the values of a filled job and a vacancy, with vacancy cost c,

$$rJ_i = y_i - w - \delta\left(J_i - V\right), \tag{7}$$

$$rV = -c + q(\theta)\left(\bar{J} - V\right), \tag{8}$$

where $\bar{J}$ is the expected value of a filled job at posting. Proportional matching implies the treated share among hires also equals π,[10] so $\bar{J} = (1-\pi)J_0 + \pi J_1$. Free entry ($V = 0$) then gives $\bar{J} = c/q(\theta)$ and, using (7), $\bar{J} = (\bar{y} - w)/(r+\delta)$. Combining yields the job-creation condition in expected-output units:

$$\bar{y} = w + \frac{c(r+\delta)}{q(\theta)}. \tag{9}$$

2.1.5 Equilibrium

In a canonical DMP model with linear production, equilibrium is summarized by tightness alone. Here, productivity exhibits decreasing returns to effective labor and therefore depends on aggregate employment, so equilibrium is jointly determined by employment $n^{\star}$ and tightness $\theta^{\star}$.[11] From Equations (3), (6), and (9), the equilibrium $(n^{\star}, \theta^{\star})$ solves

$$n^{\star} = \frac{p(\theta^{\star})}{p(\theta^{\star}) + \delta\left[\frac{1-\pi}{e_0} + \frac{\pi}{e_1}\right]}, \tag{10}$$

$$\alpha(1+\pi\Delta)^{\alpha}(n^{\star})^{\alpha-1} = w + \frac{c(r+\delta)}{q(\theta^{\star})}. \tag{11}$$

Equation (10) is the Beveridge relation and Equation (11) the job-creation condition. Search assistance (e_1) primarily shifts the Beveridge relation by altering job-finding rates and employment composition, whereas training (Δ) shifts the job-creation condition by raising expected

[9] See Appendix A.9, Equation (A.24).

[10] See Appendix A.9, Equation (A.23).

[11] When $\alpha = 1$, production is linear in effective labor and the job-creation condition is independent of aggregate employment; the model then reduces to the standard DMP case, in which equilibrium is fully characterized by labor market tightness.

productivity. The equilibrium $(n^\star, \theta^\star)$ reflects the interaction between these two margins.

2.2 Policy mapping: search intensity vs training

We now trace how the program affects equilibrium. All comparative-statics results are local, evaluated at the interior steady state $(n^\star, \theta^\star)$. We proceed from the two pure channels to the mixed one: search assistance, which affects equilibrium only through the Beveridge relation; training, which affects it only through job creation; and program coverage, which combines both.

The steady state is implicitly defined by $F_1(n, \theta) = 0$ (Beveridge relation) and $F_2(n, \theta) = 0$ (job-creation condition), where

$$F_1(n, \theta) = n - \frac{p(\theta)}{p(\theta) + A(\pi, e_1)}, \qquad A(\pi, e_1) = \delta \left[\frac{1 - \pi}{e_0} + \frac{\pi}{e_1} \right], \tag{12}$$

and

$$F_2(n, \theta) = \alpha(1 + \pi\Delta)^\alpha n^{\alpha-1} - w - \frac{c(r + \delta)}{q(\theta)}. \tag{13}$$

The equilibrium is locally unique and differentiable in the parameters: the Jacobian $\mathcal{J}$ of (F_1, F_2) has $\det(\mathcal{J}) < 0$. Appendix A.10 reports the partial derivatives and sign structure.

2.2.1 Search assistance

We begin with search assistance. Holding all other parameters fixed, an increase in e_1 raises the search efficiency of treated unemployed workers and thereby shifts the Beveridge relation outward, but it does not enter firms' vacancy-posting condition: $F_{2e_1} = 0$.

Proposition 1 (Search assistance and congestion)**.** *Holding all other parameters fixed, an increase in search assistance e_1 raises equilibrium employment but lowers labor market tightness:*

$$\frac{\partial n^\star}{\partial e_1} > 0, \qquad \frac{\partial \theta^\star}{\partial e_1} < 0.$$

Proof. See Appendix A.11. □

Search assistance therefore works through improved matching efficiency and general-equilibrium congestion rather than through a direct shift in job creation: employment rises, but tightness falls because effective search expands faster than vacancy creation.

2.2.2 Training intensity

Training works through the opposite margin. Holding all other parameters fixed, an increase in Δ leaves the Beveridge relation untouched ($F_{1\Delta} = 0$) and raises expected post-hire productivity, shifting labor demand outward.

Proposition 2 (Training and job creation)**.** *Holding all other parameters fixed, an increase in training intensity Δ raises both equilibrium employment and labor market tightness:*

$$\frac{\partial n^\star}{\partial \Delta} > 0, \qquad \frac{\partial \theta^\star}{\partial \Delta} > 0.$$

Proof. See Appendix A.12. □

Unlike search assistance, which may merely reallocate existing opportunities, training generates genuine job creation in equilibrium.

2.2.3 Program coverage

Program coverage combines both margins: a larger π raises the share of workers receiving search assistance and training, and therefore shifts both equilibrium conditions. From (12) and (13),

$$F_{1\pi} = \frac{p(\theta)}{\big(p(\theta) + A(\pi, e_1)\big)^2}\, \delta \left(\frac{1}{e_1} - \frac{1}{e_0}\right) < 0, \qquad F_{2\pi} = \alpha^2 \Delta (1 + \pi\Delta)^{\alpha-1} n^{\alpha-1} > 0.$$

The term $F_{1\pi} < 0$ is the congestion effect of higher effective search; $F_{2\pi} > 0$ is the job-creation effect of higher expected productivity. Differentiating the equilibrium system and applying Cramer's rule,

$$\frac{d\theta^\star}{d\pi} = -\frac{F_{2\pi} - F_{2n} F_{1\pi}}{\det(\mathcal{J})}, \qquad \frac{dn^\star}{d\pi} = -\frac{F_{1\pi} F_{2\theta} - F_{2\pi} F_{1\theta}}{\det(\mathcal{J})}. \tag{14}$$

Proposition 3 (Coverage, employment, and displacement)**.** *Holding all other parameters fixed, an increase in program coverage π raises equilibrium employment, $dn^\star/d\pi > 0$, while its effect on labor market tightness is ambiguous, $d\theta^\star/d\pi \gtrless 0$.*

Proof. See Appendix A.13. □

Aggregate employment rises with coverage unambiguously because more workers occupy the high-efficiency search state – a composition effect that holds even absent any training. Tightness, by contrast, depends on which force dominates: congestion pushes it down, training-induced job creation pushes it up.

Because the job-finding rate of untreated workers is $\lambda_0 = e_0 p(\theta)$ with e_0 fixed, displacement operates solely through tightness:

$$\frac{d\theta^\star}{d\pi} < 0 \quad \Longleftrightarrow \quad \frac{d\lambda_0^\star}{d\pi} < 0.$$

When congestion dominates, expanding coverage reallocates job opportunities rather than creating enough vacancies to absorb the extra effective search: treated gains come at the expense of the non-treated – a "musical-chairs" effect. We call this region *displacement dominance*. Since $F_{2\pi}$ is zero at $\Delta = 0$ and rises with training intensity, displacement prevails when training is weak and recedes as it strengthens; but where the balance actually falls for a given program is a quantitative question. We answer it by calibration in Section 6.3 when discussing the impact of the program coverage at the macro-level.[12]

3 Institutional context

This section describes the setting in which we evaluate PACEA. We present the network of "missions locales" that delivers youth activation in France, our administrative data, the PACEA program and the population it serves, and – central to our identification strategy – the way caseworkers channel young people into the program.

3.1 The *missions locales* network

"*Missions locales pour l'insertion professionnelle et sociale des jeunes*", commonly known as "missions locales", are the public agencies in charge of activating young people in France. They assist a significant 10–15% of the entire 16–25 age population each year, focusing on those who are no longer in school and often face substantial barriers to entering the labor market. Beyond employment assistance, they provide comprehensive social support in areas such as health, housing, and administrative tasks. Established in 1982, these youth centers were integrated into the French public employment service (PES) in 2005. The network comprises around 430 centers spread across 6,800 affiliated agencies and employs approximately 15,000 workers, a third of whom are caseworkers.[13]

Each year, around 400,000 young people visit a youth center for the first time. Most come on their own initiative or on the recommendation of family or friends; agreements with other public institutions, including the Ministry of Education and the Ministry of Defense, also channel youths – particularly early school leavers – toward the nearest agency (Hervelin, 2022). Registration is the entry point of the activation process: it takes place through an in-person meeting with a caseworker who assesses the youth's situation and needs, and who

[12] We also show in Appendix A.14 how the curvature of the labor demand governs the micro-macro wedge.
[13] Source: Union nationale des Missions Locales (UNML), more details here.

then orients the young person toward the support measures and programs available at the center. The most important of these programs is PACEA, the contractual activation scheme we evaluate. Crucially, enrollment in PACEA is not automatic – it is proposed and formalized by the caseworker – so that the caseworker a youth happens to meet plays a decisive role in whether activation takes the form of PACEA.

3.2 Data sources

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

Our primary source is the national information system of "missions locales" (IMILO). It records socio-demographic information on young people – educational background, geographic location, housing conditions, and social security status, among others – and, importantly, tracks their entire trajectory within the agency. Because public funding is tied to recorded activity, caseworkers have a direct incentive to document it accurately.[14] We therefore observe every interaction with caseworkers – individual meetings, remote contacts (email, SMS, phone calls), workshops – and enrollment in specific programs such as PACEA.

Our secondary source (FORCE) is an administrative database of employment and vocational training records for individuals who have been in contact with the PES, including youth centers. It provides the universe of labor contracts signed and vocational training spells, together with contract type (permanent or temporary), wages, occupations, and training intensity (total hours). It covers private-sector employment but not civil-service or self-employment. Linking IMILO and FORCE lets us follow each youth's activation within the center and their subsequent labor-market outcomes. After applying restrictions to ensure the feasibility of the econometric analysis described in Section 4, we obtain a final sample of 298,142 youths, 2,269 caseworkers, and 317 youth centers.[15]

3.3 Population and activity in PACEA

PACEA is the main contractual activation program offered within "missions locales". It emerged from the *Garantie jeunes* (GJ), the French implementation of the European Youth Guarantee, which shifted youth assistance toward a *work-first* approach.[16] Building on GJ, PACEA generalized and formalized this support through a written contract establishing mutual commitments between the young participant and the center. It adopts a holistic approach,

[14] This is why IMILO reliably captures the interactions we exploit below.

[15] See Appendix A.2 for details about sample selection.

[16] The *Garantie jeunes* was introduced in October 2013 in experimental form in 10 territories (41 of the 437 centers in the network), extended from January 2015, and rolled out nationwide on January 1, 2017. Between the start of the experiment and the end of July 2018, 229,000 young people entered the program (Guillerm and Hilary, 2019).

Table 1: Summary statistics at registration

Socio-demographic characteristics	All	Non-PACEA	PACEA	Δ
	(1)	(2)	(3)	(4)
Male	0.521	0.533	0.508	-0.025***
	(0.500)	(0.499)	(0.500)	(0.000)
Adult	0.791	0.804	0.778	-0.025***
	(0.406)	(0.397)	(0.415)	(0.000)
Dropout	0.436	0.436	0.436	0.000
	(0.496)	(0.496)	(0.496)	(0.846)
Foreign birthplace	0.151	0.155	0.147	-0.008***
	(0.358)	(0.362)	(0.354)	(0.000)
Foreign nationality	0.118	0.125	0.109	-0.016***
	(0.322)	(0.331)	(0.312)	(0.000)
Village	0.185	0.195	0.175	-0.020***
	(0.388)	(0.396)	(0.380)	(0.000)
Parent house	0.560	0.537	0.583	0.046***
	(0.496)	(0.499)	(0.493)	(0.000)
Married	0.067	0.078	0.056	-0.021***
	(0.251)	(0.268)	(0.231)	(0.000)
Children	0.071	0.081	0.061	-0.021***
	(0.257)	(0.273)	(0.238)	(0.000)
Registered at PEA	0.637	0.564	0.714	0.150***
	(0.481)	(0.496)	(0.452)	(0.000)
Directed by self or relatives	0.576	0.552	0.602	0.050***
	(0.494)	(0.497)	(0.489)	(0.000)
Driving license	0.338	0.382	0.291	-0.091***
	(0.473)	(0.486)	(0.454)	(0.000)
N	298,142	153,689	144,453	

Note: Youths registered in "missions locales" in 2017 and 2018 in column (1), not enrolled in the PACEA program in column (2) and enrolled in PACEA in column (3). *** p<0.01, ** p<0.05, * p<0.1.
Source: IMILO (extraction date: November 2021), authors' calculations.

recognizing that barriers to employment often extend beyond job-related skills: participants may access vocational training, employment assistance, housing support, healthcare, mobility solutions, and financial aid, delivered through sequential phases with specific objectives. Its nominal duration is 24 months but flexible in practice, and participants may receive a monthly allowance of up to about €550 to stabilize their living conditions and sustain engagement.

Enrollment into PACEA is therefore a decision taken with the referent caseworker rather than a mechanical consequence of registering, and youths who enter the program differ systematically from those who do not. Table 1 documents the socio-demographic composition of the population and highlights these differences. PACEA participants are, on average, slightly less likely to be male and adult, and are less often married or parents. They are also less likely to hold a driving license or to live in rural areas, while being more likely to reside in the parental household. Educational disadvantage – measured by school dropout – is essentially identical across groups. PACEA youths are, however, significantly more likely to be registered at the public employment agency and to have been oriented toward the mission locale by themselves or their relatives, suggesting stronger attachment to employment services and a more proactive job-search stance.

Once enrolled, participation is intensive and long-lasting. Youths enter PACEA about 105 days after registering with a "mission locale" on average, and remain enrolled for roughly 494

days (median close to 500), typically moving through one to two successive phases of about nine months each. Participation is dense: participants accumulate more than 50 recorded events on average – meetings and contacts with the referent caseworker alongside activities linked to job-search assistance, training, and employment. A large share experience paid employment at some point during PACEA (62%), while 44% take part in another job-search assistance program and 17% enter vocational training; the mean total allowance received exceeds €1,600, with considerable dispersion. Table A.3.1 in Appendix A.3 details these trajectories, and Figure A.3.1 confirms the persistence of participation: whereas non-PACEA youths exit assistance relatively quickly, PACEA participants remain engaged well beyond one year. PACEA is thus a sustained, individualized pathway rather than a short activation spell.

Figure A.3.2 in Appendix A.3 plots OLS estimates relating monthly employment probability to PACEA enrollment, conditional on observed characteristics, as an event study around registration. It reveals a markedly negative differential at registration – close to −5 percentage points in the first six months – consistent with lock-in at the start of the program, followed by a gradual catching-up as the differential turns positive and stabilizes around 5 to 7 percentage points three to four years out. Because enrollment into PACEA is decided with the caseworker and correlated with hard-to-observe characteristics, however, these OLS estimates are likely biased and cannot be read as causal. This motivates the identification strategy developed in Section 4, which turns the caseworker's role in enrollment into a source of exogenous variation.

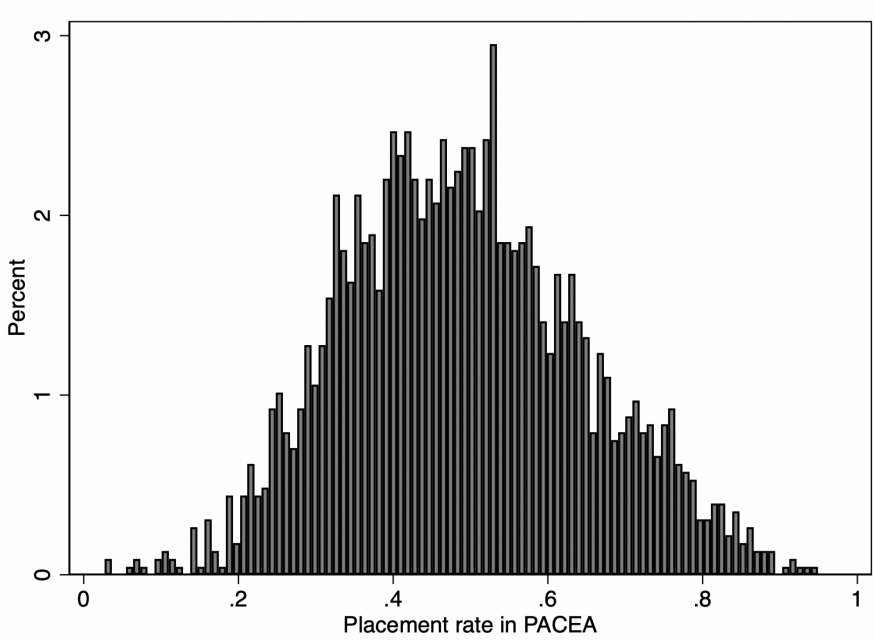
3.4 Caseworkers' intermediation

Caseworkers sit at the heart of the activation process, and understanding how they operate is essential to our design. To register with an agency, a young person must appear in person for an initial meeting, which typically lasts 45 minutes and during which the caseworker collects information on the youth's socio-demographic situation and needs. This first caseworker automatically becomes the youth's *referent* for the remainder of the follow-up: after the first meeting, about 83% of young people return for at least a second meeting with the same person. Referents deliver a wide range of assistance – individual meetings, workshops to improve oral and written skills, collective information sessions, and remote contacts by email, SMS, or phone – and, importantly, decide together with the youth whether to enter PACEA and which objectives to pursue.[17]

Two features of this process are central to our strategy. First, the assignment of youths to caseworkers is as good as random within a center and period. There are no rules matching particular youths to particular caseworkers: on the day a youth arrives, they meet whichever caseworker is scheduled to welcome new entrants that day, and caseworkers are assigned to

[17] A minimum of two years of higher education is usually required to be recruited as a caseworker, with a starting gross wage around €2,000. See Table A.1.1 in Appendix A.1 for details on occupations in "mission locale" centers.

Figure 1: Histogram of the placement rate in PACEA across caseworkers



Note: Share of youths enrolled in PACEA across caseworkers ($N = 2,269$).

calendar days – not to individuals – on a rotation planned several weeks in advance based on their workload.[18] Youths who contact the agency in advance are handled the same way, meeting one of the caseworkers on duty that day. Consistent with this institutional setup, Figure A.4.2 in Appendix A.4 regresses caseworker characteristics on the characteristics of their newly assigned youths within center × entry-quarter cells and finds overwhelmingly small and insignificant coefficients, indicating no systematic sorting of youths to caseworkers.

Second, caseworkers differ markedly in how they activate the youths they meet, and in particular in how often they enroll them into PACEA as shown by Figure 1. The distribution is broadly bell-shaped and centered around intermediate values, with most caseworkers exhibiting placement rates between roughly 30 and 70 percent, peaking around 40–50 percent. At the same time, it features long tails on both sides, with a non-negligible share of caseworkers displaying very low placement rates (below 20 percent) and others achieving rates above 80 percent. Because enrollment is at the caseworker's discretion, two observationally similar youths who register in the same center and quarter can be placed on different activation paths – one entering PACEA, the other not – simply because they happened to meet different caseworkers. This caseworker-driven variation in PACEA enrollment, orthogonal to youth characteristics once we condition on center × entry-quarter, is precisely what our identi-

[18] Figure A.4.1 in Appendix A.4 shows that fewer first meetings are conducted on Fridays, reflecting this scheduling.

fication strategy exploits. We document the heterogeneity in caseworkers' PACEA enrollment propensities and build our instrument from it in Section 4.

4 Empirical strategy

This section describes the empirical strategy developed to analyze the impact of the PACEA job search assistance program on labor market outcomes of unemployed youths.

4.1 Instrumental variable model

To identify the causal impact of PACEA participation on labor market outcomes, we rely on an instrumental variable (IV) approach estimated by two-stage least squares (TSLS). The strategy exploits the quasi-random variation in caseworker assignment at entry into the "mission locale" documented in Section 3.4 and, in particular, heterogeneity in caseworkers' propensity to enroll youths into PACEA.

The baseline specification is given by Equations (15) and (16):

$$\text{PACEA}_i = \alpha_1 + \gamma \widetilde{\text{CswkPACEA}}_{jt,-i} + \beta_1 X_i + \pi_{c,t} + v_i \tag{15}$$

$$y_i = \alpha_2 + \delta \widehat{\text{PACEA}}_i + \beta_2 X_i + \eta_{c,t} + u_i \tag{16}$$

In the first stage, individual participation in PACEA is regressed on an indicator capturing whether the assigned caseworker is pro-PACEA at the quarter of entry, controlling for a vector of individual characteristics X_i and for interacted center by entry-quarter fixed effects $\pi_{c,t}$. These fixed effects absorb all time-varying shocks and institutional differences at the local level, ensuring that identification stems from within-center, within-quarter variation in caseworker assignment.

The instrument, $\widetilde{\text{CswkPACEA}}_{jt}$, equals one if caseworker j exhibits a high propensity to enroll youths into PACEA at quarter t, leaving-out youth i and its cohort within center c and quarter t to avoid mechanical endogeneity, 0 otherwise. It captures persistent differences in enrollment practices across caseworkers. The first-stage coefficient γ therefore measures the extent to which assignment to a pro-PACEA caseworker increases the probability of entering the program. Conditional on individual covariates and local-time fixed effects, this variation reflects differences in caseworker behavior rather than systematic differences in youth characteristics. As shown by Figure 1, placement rates into PACEA vary widely across caseworkers, providing a strong and policy-relevant source of identifying variation.

In the second stage, the predicted value of PACEA participation is used to estimate its effect on labor market outcomes y_i. The coefficient δ can be interpreted as a local average treatment effect (LATE) for compliers, namely youths whose participation decision is affected

by assignment to a pro-PACEA caseworker.[19] Robust standard errors are clustered at the caseworker level to account for within-caseworker correlation in treatment assignment and outcomes. Under the identifying assumption that caseworker pro-PACEA orientation affects labor market outcomes only through PACEA participation, this IV strategy isolates the causal impact of the program net of endogenous selection.

4.2 Definition of the instrument

To construct our pro-PACEA caseworker instrument, we employ the methodology introduced by Chetty et al. (2014), which we adapt to our specific context. As caseworkers' assignments are expected to be quasi-random within a particular center and quarter[20], we condition our analysis on cohort fixed effects. These effects are defined as the group of individuals who had their initial meeting with a caseworker in the same quarter and within the same center.

Selection in the PACEA program is correlated with individual characteristics and caseworkers have different propensities to enroll young people in the PACEA program, we then start by computing PACEA enrollment residuals $\widehat{\text{PACEA}}_i$ by regressing youth outcome PACEA_i on cohort fixed effects $\gamma_{c,t}$, youth pre-determined characteristics X_i as well as caseworker fixed effects μ_j. Including caseworker fixed effects in the model allows us to control for potential selection in youth-caseworker assignment and estimate residuals relying on within caseworker variation.

We estimate the following regression:

$$\text{PACEA}_i = \mu_j + \beta X_i + \gamma_{a,t} + \epsilon_i \tag{17}$$

Youth PACEA enrollment residuals are then computed as

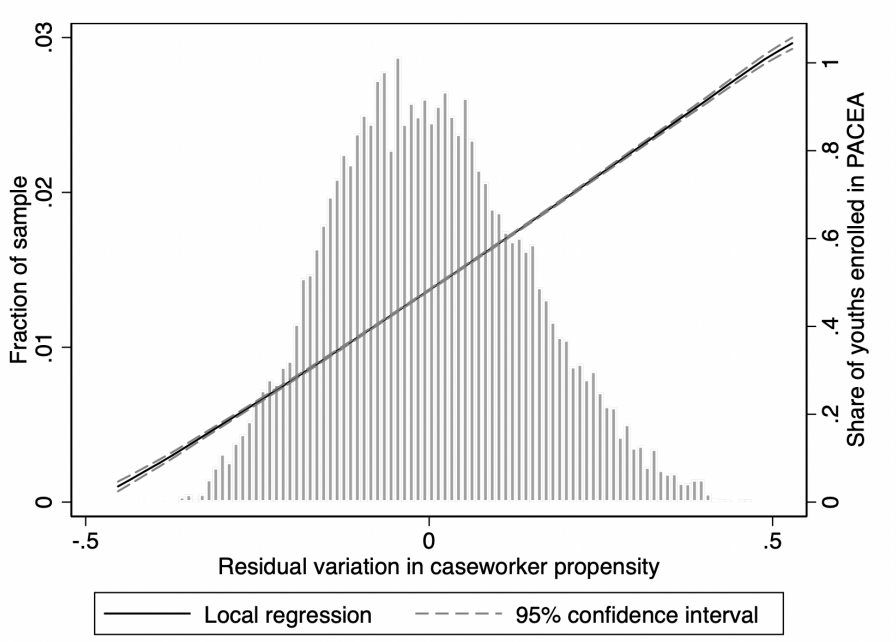
$$\widehat{\text{PACEA}}_i = \text{PACEA}_i - \hat{\beta} X_i - \hat{\gamma}_{a,t} \tag{18}$$

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

[19] Blandhol et al. (2026) show that TSLS with covariates entered non-saturatedly need not estimate a non-negatively weighted average of LATEs unless the specification has rich covariates, i.e. it reproduces the conditional mean of the instrument. Our design falls in the favorable case they discuss for conditionally randomly assigned instruments: because assignment is as good as random within center × entry-quarter cells that we absorb nonparametrically, this condition reduces to the requirement that individual covariates do not predict the instrument within cells, which we cannot reject (joint $F = 1.32$, $p = 0.20$). Consistently, our estimates are essentially unchanged when we omit the individual covariates – the specification that is saturated in the strata and thus has a *bona fide* LATE interpretation – and when we partial the covariates out with machine learning. See Appendix A.6.5.

[20] As a robustness check, we verified that our results remain consistent when considering different cohort definitions, such as center × month or semester. Results not shown in this paper.

Figure 2: Histogram of caseworker propensity instrument ($\overline{\text{CswkPACEA}}_{jt}$)



Note: Caseworker propensity instrument is computed from equation (19).

If our identifying assumption is verified, $\overline{\text{PACEA}}_{j,-t}$ is an unbiased estimate of the causal effect of caseworker j on cohort t youths' enrollment in PACEA. However, it may not be an optimal out of sample predictor of caseworker propensity. Thus, following Chetty et al. (2014) we construct our caseworker propensity estimate as the best linear predictor of $\overline{\text{PACEA}}_{j,t}$ based on the residuals of all other cohorts $\overline{\text{PACEA}}_{j,-t}$.

Specifically, we compute caseworker predicted propensity for cohort t as

$$\overline{\text{CswkPACEA}}_{jt} = \sum_{s=1\setminus\{t\}}^{T} \Psi_s \overline{\text{PACEA}}_{j,s} = \Psi' \overline{\text{PACEA}}_{j}^{-t} \tag{19}$$

where Ψ is the vector of coefficient that minimizes the mean-squared error of the forecasts of the residual outcomes.[21] It is estimated by regressing $\overline{\text{PACEA}}_{j,t}$ on $\overline{\text{PACEA}}_{j,-t}$.[22]

To illustrate the identifying variation underlying our IV strategy, Figure 2 plots a histogram of the residual variation in caseworker PACEA placement propensities computed from equation (19). The distribution shows substantial residual dispersion around zero, indicating

[21] $\Psi = \underset{\{\Psi_1,\ldots,\Psi_{t-1},\Psi_{t+1},\ldots,\Psi_T\}}{\arg\min} \sum_j \left(\bar{Y}_{j,t} - \sum_{s=1}^{T} \Psi_s \bar{Y}_{j,s}\right)^2$

[22] Figure A.5.1 in Appendix A.5 shows the autocorrelation vector of the propensity estimates at quarter t using other quarters. Using one lag implies a correlation of 60% and it monotonically decreases to 35% using 7 quarters. This rate is higher than the ones usually found for teachers which decrease from 45% using 1 lag to 20% using 7 lags as in Chetty et al. (2014).

pronounced heterogeneity in caseworkers' propensity to enroll youths into PACEA even after purging for center and quarter conditions and observable characteristics. Superimposed on the histogram, the solid line plots a local linear regression of the probability of PACEA enrollment on residual caseworker PACEA propensities, with the dashed lines denoting the 95 percent confidence interval. The relationship is strongly positive and close to linear across the support, documenting a clear and monotonic first-stage effect: youths assigned to caseworkers with higher PACEA propensities are substantially more likely to enter the program. The tight confidence bands further indicate that this relationship is precisely estimated throughout the distribution, providing visual confirmation of a strong and relevant first stage generated by caseworker heterogeneity in enrollment practices

To simplify interpretation in the remaining sections of the paper, we dichotomize our propensity instrument by defining an indicator equal to one if caseworker j's propensity to enroll youths in PACEA in quarter t is above the median, and zero otherwise.[23]

$$\rightarrow \widetilde{\text{CswkPACEA}}_{jt} = 1\left\{\overline{\text{CswkPACEA}}_{jt} > \text{med}\left(\overline{\text{CswkPACEA}}\right)\right\} \tag{20}$$

4.3 Assessment of the instrument

We develop and document the validity of our instrumental-variable strategy in Appendix A.6. First, the instrument is strongly relevant. Caseworkers exhibit substantial heterogeneity in their propensity to enroll youths into PACEA, and assignment to a more pro-PACEA caseworker strongly predicts individual participation. The first-stage coefficients are large and highly stable across specifications, with F-statistics far above conventional weak-instrument thresholds (see Table A.6.1 in Appendix A.6.1). This confirms that quasi-random assignment to caseworkers generates powerful within-center and entry-quarter variation in enrollment.

Second, conditional independence is supported by both institutional features and statistical evidence. Once we include center $\times$ entry-quarter fixed effects, assignment to a pro-PACEA caseworker is essentially orthogonal to observed socio-demographic characteristics and to youths' stated requests at registration (see Tables A.6.2 and A.6.3 in Appendix A.6.2). This suggests that within centers and entry cohorts, caseworker assignment is as good as random with respect to observed characteristics and requests at registration.

Most importantly, the credibility of our design rests on the exclusion restriction: assignment to a pro-PACEA caseworker must affect youths' labor market outcomes only through PACEA participation. The natural threat is that caseworkers who enroll more youths into PACEA might also differ on other, unobserved dimensions, that improve outcomes – offering more intensive counseling, transmitting more vacancy information, or exerting more pressure to search outside of the program. While the exclusion restriction cannot be tested directly,

[23] All results presented in the following sections are robust to using the continuous version of the instrument.

two placebo tests and the institutional design make this concern unlikely.

On the one hand, we implement a direct placebo test on youths who never enroll in PACEA. If pro-PACEA caseworkers improved outcomes through channels other than enrollment, non-participants assigned to them would still benefit. Instead, the reduced-form effect of assignment to a pro-PACEA caseworker on the employment of never-enrolled youths is small, statistically insignificant, and precisely estimated around zero at every horizon – six, twelve, eighteen, and twenty-four months after registration (Table A.6.4 in Appendix A.6.3). The precision of these nulls is what makes the test informative: it lets us rule out not only large but also economically modest effects operating outside the program. On the other hand, we verify that caseworker pro-PACEA orientation is unrelated to youths' employment and training histories in the six months preceding registration (Table A.6.5), which rules out sorting on unobserved pre-existing attachment that could simultaneously drive caseworker assignment and future outcomes.

These statistical results are, moreover, consistent with the institutional organization of "missions locales". Caseworkers' discretion operates primarily on the margin of program enrollment: access to other services – workshops, collective information sessions, or financial support – follows standardized, centrally defined rules, leaving little room for a pro-PACEA caseworker to systematically favor assigned youths through alternative channels. In addition, our instrument is a leave-out measure that captures a caseworker's persistent enrollment behavior toward other youths, purged of local labor-market conditions and observable composition through the center $\times$ entry-quarter fixed effects, so that it reflects enrollment practices rather than case-specific effort toward the focal youth. Taken together, the placebo evidence and the institutional design lend strong credibility to interpreting our IV estimates as the causal effect of PACEA participation.[24]

5 Main results

This section presents the impact of PACEA program on various labor market outcomes. We report the main employment table in the text and move the remaining econometric tables to Appendix A.7.

5.1 Impact on employment probability

We first present the main estimates, relying on equation (16), of the impact of PACEA participation on youths' employment probabilities over four years following registration at a mission locale. Table 2 reports both OLS and IV estimates at one-, two-, three-, and four-year horizons, while Figure A.7.1 in Appendix A.7 illustrates the dynamic profile of the effects at the

[24] We also show that our instrument satisfies the monotonicity assumption following the approach in Arni and Schiprowski (2019), see Table A.6.6 in Appendix A.6.4.

Table 2: Impact of PACEA on employment probability after registration in youth center

Employment (0/1)	Year 1		Year 2		Year 3		Year 4	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PACEA	-0.0095***	-0.0014	0.0244***	0.0078	0.0501***	0.0471***	0.0620***	0.0440**
	(0.0021)	(0.0170)	(0.0021)	(0.0177)	(0.0021)	(0.0182)	(0.0021)	(0.0183)
Observations	298,142	298,142	298,142	298,142	298,142	298,142	298,142	298,142
R-squared	0.0617	0.0386	0.0592	0.0359	0.0722	0.0390	0.0734	0.0425
Control mean	.3339	.3339	.3737	.3737	.4053	.4053	.4551	.4551
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with OLS and IV-2SLS regressions. Employment is equal to one if young people are in employment one year after registration in column (1) two years in column (2), three years in column (3), and four years in column (4), zero otherwise. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

semester level. At the one-year horizon, the OLS estimate is negative (–1 pp), which should reflect a lock-in mechanism, whereby participation in PACEA temporarily reduces job-finding rates as youths devote time to counseling sessions, administrative steps, training, and the definition of individualized projects rather than immediate job search.

From the second year onward, OLS estimates turn positive and increase steadily over time, reaching about 6 percentage points after four years. While these estimates suggest a gradual improvement in employment prospects for PACEA participants, they remain difficult to interpret causally given endogenous participation as discussed in the previous section. The IV estimates correct for this bias. At one and two years after registration, the IV coefficients are small and statistically insignificant, indicating that PACEA does not generate immediate employment gains once selection and endogeneity are addressed. This pattern reinforces the interpretation of the short-run negative OLS estimate as reflecting lock-in rather than lasting labor market penalties.

In contrast, medium-run IV effects are large and statistically significant. Three years after registration, which corresponds almost to one year after the end of PACEA, participation increases employment probability by about 4.7 percentage points, corresponding to an increase of roughly 12 percent relative to the mean employment rate at that horizon. This positive effect persists four years after registration – 2 years after PACEA –, with an estimated increase of 4.4 percentage points. Figure A.7.1 underscores the dynamic nature of these effects: employment gains emerge only after participants have progressed through the program's phases and accumulated human capital, work experience, or stronger labor market attachment. Overall, the results indicate that PACEA entails short-run lock-in effects but yields substantial and persistent improvements in employment outcomes in the medium run, highlighting the importance of evaluating youth activation programs over sufficiently long horizons.

These medium-run effects mask only limited heterogeneity across observable subgroups. Re-estimating the four-year employment impact separately by education, urban/rural location, prior registration with the public employment service, gender, and age (Table A.7.1 in

Table 3: Impact of PACEA on employment days after registration in youth center

Employment (days)	Year 1		Year 2		Year 3		Year 4	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PACEA	-12.7540***	1.4014	-1.4200**	1.8074	9.6627***	8.9930*	14.8252***	11.6379**
	(0.4614)	(3.8355)	(0.5832)	(4.9592)	(0.5880)	(5.1944)	(0.6132)	(5.3978)
Observations	298,142	298,142	298,142	298,142	298,142	298,142	298,142	298,142
R-squared	0.0855	0.0579	0.0661	0.0462	0.0668	0.0433	0.0721	0.0433
Control mean	74	74	102	102	111	111	126	126
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with OLS and IV-2SLS regressions. Employment is defined as the total number of days in employment during the first year after registration in column (1) during the second year in column (2), the third year in column (3), and fourth year in column (4), zero otherwise. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix A.7) yields point estimates clustered between about 4 and 5 percentage points for nearly every group. The effect is statistically significant, and somewhat larger, for youths with stronger baseline attachment – those who completed secondary school (4.7 pp), in urban areas (4.8 pp), men (5.2 pp), and adults aged 18 or older (5.4 pp) – and positive but imprecise for school dropouts, rural youths, and women. The one clear exception is minors: for youths under 18 the estimate is small and negative, consistent with weaker immediate labor-market readiness at that age. Because the subgroup estimates differ little relative to their standard errors, we read PACEA as delivering a broadly uniform medium-run employment gain, modestly concentrated among the more job-ready.

5.2 Impact on employment intensity

We now turn to the intensive margin, measured by the total number of days in employment during each of the four years following registration. Table 3 reports OLS and IV estimates, and the pattern mirrors the one obtained for employment probability. In the first two years – the period that broadly overlaps with participation – the IV estimates are small and statistically insignificant (about 1.4 and 1.8 additional days), while the OLS estimates are negative and significant, consistent with a lock-in mechanism whereby youths devote time to program-related activities rather than immediate employment. From the third year onward, the IV effects turn positive and significant: PACEA raises time in employment by about 9 days at the three-year horizon and 11.6 days at the four-year horizon, corresponding to increases of roughly 8 and 9 percent relative to the control means of 111 and 126 days. These gains persist through the fourth year, indicating that PACEA not only raises the probability of employment in the medium run but also leads to more sustained employment once participants have completed the program. Figure A.7.2 in Appendix A.7 traces the dynamic profile of these effects at the semester level, confirming the short-run lock-in and the positive medium-run effects.

To understand what drives these intensive-margin effects, Tables A.7.2 and A.7.3 in Ap-

pendix A.7 examine whether PACEA affects the number of labor contracts signed or the number of firms in which youths worked, conditional on having at least one employment spell. Across all horizons, the IV estimates are small, statistically insignificant, and show no systematic pattern. In particular, PACEA does not increase the number of contracts signed nor the number of distinct employers, either cumulatively over four years or within specific post-registration periods. This absence of effects on job fragmentation or employer mobility suggests that the higher employment intensity observed after program completion is mechanically driven by improvements at the extensive margin discussed in Section 5.1 – more frequent access to employment – rather than by changes in the structure of employment relationships among employed youths.

5.3 Impact on employment quality

We now turn to job quality, which we assess along three dimensions: contract stability, wages, and occupations.

A first dimension of job quality is the stability of employment contracts. Table A.7.4 in Appendix A.7 reports IV estimates of the impact of PACEA on the probability that a youth signs a permanent contract (CDI) over the four years following registration and within each specific year. Over the full four-year period, the point estimate on PACEA is close to zero and statistically insignificant, indicating no detectable difference in the likelihood of securing long-term employment between participants and comparable non-participants. In the first year after registration, the estimate is negative and statistically significant, which – given the absence of corresponding medium-run impacts – likely reflects the difficulty of the population to be attractive on the labor market and why caseworkers enroll them in the PACEA program. In years two through four, the estimated effects are small and not statistically significant, suggesting that participants caught up in finding permanent contracts after improving their labor market prospects. PACEA participants thus do not cycle through more contracts or employers; instead, they spend more time employed conditional on working, consistent with a stabilization of labor market trajectories rather than increased job turnover.

We next examine whether the positive medium-run employment effects of PACEA are followed by higher wages. As reported in Table A.7.5 in Appendix A.7, the mean monthly wage in the control group ranges from approximately €950 in the first year after registration to about €1,240 in the fourth year.[25] Similarly, the maximum wage observed within a year remains below €1,700 even four years after registration. These levels are low and lie close to the statutory minimum wage in France over the period under study, indicating that employment spells for this population are predominantly in low-paid jobs with limited scope for wage

[25] As wages are measured with errors in the administrative database, especially for temporary contracts, we run the same IV-regressions relying only on individuals who signed a permanent contract, for which wages are better filled. Table A.7.6 in Appendix A.7 leads to similar results.

progression. In this context, even sizable increases in employment probabilities or employment durations are unlikely to generate large gains in bargained wages. The absence of wage differentials conditional on working implies that participants are not employed in higher-paying jobs than comparable non-participants.

Beyond contract type and wages, the program could influence the types of occupations in which youths are employed, potentially channeling them into sectors with distinct career prospects or skill requirements. Table A.7.7 in Appendix A.7 examines this possibility by focusing on the most common occupational categories in the post-registration labor market, coded according to the PCS-ESE classification. Across the four years following registration, the main occupations in which youths are employed are concentrated in low-skill service, industrial, and manual jobs, reflecting the weak initial labor market attachment of the population served by "missions locales". In the first year, the most represented occupations include 676A (non-qualified industrial workers in processing and manufacturing), 551A (non-qualified employees in retail trade), 676E (non-qualified industrial workers, miscellaneous), 676C (non-qualified workers in handling, logistics, and packaging), and 674C (cleaning and basic maintenance workers). These occupations represent 40% of the total number of occupations where youths are employed and typically involve routine tasks, limited autonomy, and low formal skill requirements. They rely primarily on basic operational skills – such as punctuality, physical endurance, adherence to instructions, and teamwork – rather than certified qualifications.

The occupational structure is highly persistent across years, suggesting limited occupational mobility, and by the third and fourth years the overall skill content remains comparable even as some personal-services occupations (e.g., 525D) appear among the most frequent categories. The IV estimates indicate no systematic occupational reallocation for the vast majority of PCS-ESE categories across years three and four: coefficients are small and statistically insignificant. However, in both the first and second years after registration, PACEA participants are more likely to work in occupation 676E relative to non-participants, suggesting that the program may initially channel youths toward this broad category of non-qualified industrial workers.[26] A plausible interpretation is that PACEA facilitates early labor market access by increasing the likelihood of finding any employment, and such opportunities are more prevalent in large industrial sectors that demand flexible, low-skill labor.

Taken together, these results indicate that PACEA improves access to employment and employment duration without upgrading job quality: employment gains induced by the program operate primarily at the extensive margin and do not translate into more stable contracts, higher wages, or occupation-specific human capital. This pattern is consistent with a program that facilitates labor market entry and attachment for disadvantaged youths rather than

[26] Occupation 676E (Ouvriers non qualifiés divers de type industriel) includes non-qualified industrial workers such as laboratory aides and general industrial laborers that do not fall into more specific groupings. Jobs in this category are characterized by manual, low-skill tasks within industrial settings and typically carry limited prospects for rapid wage progression or career advancement compared with higher-skilled occupations.

raising job quality or productivity in the medium run.

6 Mechanisms

This section investigates the mechanisms through which PACEA affects labor market outcomes. Using detailed administrative data on activities, program content, workshops, and training participation, we show that PACEA primarily works by intensifying job-search support and structuring employment pathways. We then relate the micro-level evidence to the general-equilibrium predictions of the framework developed in Section 2.

6.1 Job search activities and planning

Table 4 shows that PACEA substantially increases youths' engagement with "missions locales". Participation leads to a sharp rise in the total number of recorded activities within centers: PACEA participants experience nearly 40 additional events on average, compared to a baseline mean of about 13. These increases span all types of activities, including meetings, contacts, and administrative tasks, indicating a broad intensification of follow-up rather than a narrow focus on one dimension. This result is consistent with the program's design as a comprehensive accompaniment framework, rather than a single intervention.

Importantly, a large share of this increase reflects intensified interactions with the referent caseworker, who plays a central role in implementing PACEA. Columns (5) to (8) of Table 4 show that PACEA raises the number of events involving the referent by more than 14, including five additional meetings and over seven additional contacts by phones or emails. Given the centrality of the referent in coordinating job-search activities, monitoring progress, and adjusting objectives, these results point to a substantial strengthening of individualized support. This increase in caseworker contact is a plausible mechanism behind the medium-run employment gains documented earlier, as sustained follow-up is known to improve job-search effectiveness among disadvantaged youths.

Beyond the volume of interactions, PACEA also structures job-search trajectories through clearly defined phases and objectives. Table A.8.1 in Appendix A.8 documents that participants typically move through sequential phases – defining, applying, and securing a project – with a strong emphasis on career orientation, job-search assistance, and mobility. These phases reflect a gradual shift from exploration to implementation, aligning with the delayed employment effects observed in the outcome analysis. Table A.8.2 in Appendix A.8 further shows that assignment to a pro-PACEA caseworker increases the likelihood that youths enter these key early phases and set objectives related to job orientation and mobility. Heterogeneity in caseworker practices therefore translates into meaningful differences in how structured and forward-looking youths' job-search plans are, consistent with the IV estimates: caseworker

Table 4: Effects of PACEA on activities within center

	Within center				Within center with referent caseworker			
	All Events	Meetings	Contacts	Admin. Tasks	All Events	Meetings	Contacts	Admin. Tasks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PACEA	38.4584***	8.2972***	16.4075***	6.9349***	14.0881***	5.0310***	7.3224***	1.3774***
	(2.9943)	(0.4461)	(2.4732)	(0.5108)	(2.3974)	(0.3471)	(2.2585)	(0.2633)
Observations	298,142	298,142	298,142	298,142	298,142	298,142	298,142	298,142
R-squared	0.2783	0.2926	0.1393	0.2043	0.0001	0.1755	-0.0772	0.0678
Control Mean	12.9	3.2	6.8	2.3	5.6	2.5	2.2	.8
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

orientation affects employment outcomes primarily by shaping participation and engagement within PACEA.

A related mechanism operates through access to intensive workshops and financial support, as documented in Table A.8.3 in Appendix A.8. PACEA substantially increases participation in local initiatives and in the *Garantie jeunes* (GJ), a highly intensive welfare-to-work program combining collective workshops, job-search coaching, and income support: enrollment in GJ rises by 25 percentage points among PACEA participants, highlighting strong complementarities between PACEA and more intensive activation schemes. PACEA also provides substantial monetary allowances, with participants receiving on average more than €1,100, which likely relax short-term financial constraints and allow youths to remain engaged in program activities and job search.

Overall, the evidence indicates that PACEA operates first and foremost by organizing job search, increasing the intensity of follow-up, and imposing a structured pathway toward employment, combining workshops, collective support, and financial assistance. In parallel, youths discover the labor market in easy-to-access occupations with very short-term contracts as described in Sections 5.2 and 5.3.

6.2 Enrollment in vocational training

PACEA also affects employment outcomes through vocational training. Table 5 shows that PACEA increases the probability of entering any vocational training by about 4 percentage points, relative to a baseline participation rate of 16 percent. This corresponds to a relative increase by 25%. Participants also accumulate significantly more training days and hours, indicating not only higher participation but also greater training intensity.

On the one hand, despite being statistically significant, the training followed by young individuals seems limited in scope and duration ($\sim$ 1 month on average), and opens the questions on whether they deliver productivity gains in the short or medium run. Instead, training may primarily serve as a complementary tool that supports employability and job readiness by providing the basic skills to young individuals in difficulty. The timing of training partici-

Table 5: Effects of PACEA on enrollment in vocational training

	Any	Days	Hours
	(1)	(2)	(3)
PACEA	0.0419***	9.1738***	45.1980***
	(0.0146)	(2.7229)	(12.1965)
Observations	298,142	298,142	298,142
R-squared	0.0126	0.0111	0.0116
Control Mean	.1591	22.6105	98.2892
Individual characteristics	Yes	Yes	Yes
Center × Entry Quarter Fixed Effects	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. Column (1) is the probability of having any vocational training during a 2-year period after registration (≈ during the time within PACEA for those enrolled). Column (2) is the total number of days experienced in training. Column (3) is the total number of hours experienced in training. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

pation also matters. Training occurs largely during the period of assistance and is embedded within the broader job-search strategy defined with the caseworker. As such, training likely contributes to employment outcomes indirectly – by improving confidence, signaling attachment to the labor market, or meeting minimum skill requirements – rather than by enabling access to higher-paid occupations.

On the other hand, young individuals who participated in the PACEA program face an initial lower probability to sign a permanent contract than non-participants during the first year after registration. However, this difference disappears thereafter, suggesting that PACEA participants improved their labor market prospects. This improvement could also be the consequence of improved specific skills due to vocational training.

Overall, the training evidence can reinforce the interpretation that PACEA is not a human-capital-intensive program,[27] but that access to vocational training may complement this limitation. In the language of the framework of Section 2, the limited scope of training corresponds to a small productivity gain Δ, a point we develop next.

6.3 Discussion on the macro-consequences

The empirical results leads to positive estimates at the individual level for the participants, but this may not translate into aggregate gains if it comes at the expense of non-participants. This is the pattern the comparative statics of Section 2.2 associate with treatment that operates primarily through search assistance rather than productivity-enhancing training (cf. Proposition 3): individual job-finding rates increase, but part of the equilibrium adjustment occurs through congestion in the matching process rather than through vacancy creation.

[27] Its main contribution lies in facilitating transitions into employment through coordination, guidance, and sustained engagement.

Table 6: Spillover effects of PACEA program on not-enrolled youths' employment

	Year 1	Year 2	Year 3	Year 4
	(1)	(2)	(3)	(4)
Share of peers in PACEA	-0.0170	-0.0294	-0.0295	-0.0117
	(0.0229)	(0.0247)	(0.0231)	(0.0271)
Observations	153,689	153,689	153,689	153,689
R-squared	0.0631	0.0634	0.0712	0.0742
Individual characteristics	Yes	Yes	Yes	Yes
ML Center FE	Yes	Yes	Yes	Yes
Entry Quarter FE	Yes	Yes	Yes	Yes

Note: Estimates obtained with OLS regressions. Outcome is the employment probability X years after registration for non-PACEA participants only. The share of peers in PACEA is computed at the cohort level (center × entry quarter), leaving-out the situation of the observation. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the center level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Direct evidence of this congestion mechanism is provided by the spillover estimates reported in Table 6, which exploit the variation in PACEA coverage across "mission locale" center × entry-quarter cells. A higher local share of youths enrolled in PACEA is negatively correlated with the employment probability of non-enrolled youths. Although these negative spillovers can be economically meaningful, at most around half the micro-gains, they are not statistically significant.

Importantly, these spillover effects may be confounded by events that occur in the local labor market so the estimates are biased and not precise. Moreover, one cannot test for specifications with varying training intensity. Indeed, the share of peers who enter into training, and its intensity, is directly impacted by the PACEA program as shown in Section 6.2 and, at the macro level, may also be confounded by local labor market conditions.

To circumvent this limit, a mediation analysis confirms that training is only a minor channel in the post-program employment situation. Decomposing the four-year employment effect into the shares operating through vocational training and through a direct component (Table A.8.4 in Appendix A.8), we find that only about 15% of the total effect runs through training. In the language of the model, increases in effective search raise matching congestion and reduce equilibrium labor-market tightness, thereby lowering job-finding prospects for non-treated workers. The absence of wage effects further reinforces this interpretation. If PACEA had substantially raised post-hire productivity, one would expect higher wages, occupational upgrading, or more stable contracts among treated youths. Instead, wages remained low and close to the minimum wage, and job quality indicators showed little differentiation between treated and non-treated workers. These empirical patterns suggest that the access to vocational training corresponds to a relatively small value of Δ in the model, insufficient to shift the job-creation condition outward in a meaningful way.

A calibrated exercise. To move from these qualitative patterns to magnitudes, we calibrate the model of Section 2 and use it as a laboratory for counterfactual coverage and training policies. Following the model's own logic, we fix $e_0 = 1$ and recover the two treatment parameters – i) the participant search-efficiency gain e_1 from the LATE estimates in Section 5.1, and ii) the training intensity Δ from the job-creation condition (11), by imposing one targeted moment (the labor-market tightness $\theta = 0.4$).[28]

Two findings follow. First, and centrally, expanding coverage displaces non-participants. Figure 3 plots the non-participant employment rate as coverage rises: at the calibrated training intensity (solid line) it falls steadily, from 0.464 with no program to 0.444 at PACEA's current coverage, and further as coverage expands. The mechanism is exactly the one the model describes – more assisted searchers crowd the same vacancies, tightness falls, and the non-participants left behind find jobs more slowly. The magnitude is similar to the one estimated with OLS in Table 6: raising coverage by one percentage points costs non-participants about 0.02 percentage points of employment. This corresponds to a displacement effect about 40% relative to the baseline estimate at the micro-level in Section 5.1.[29]

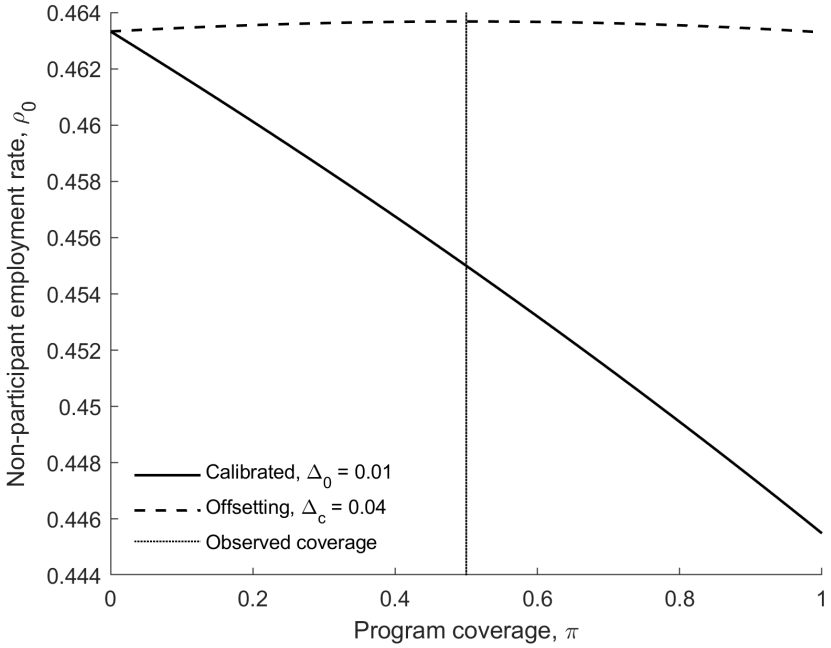
Second, the dashed line shows what the program would look like if training were intense enough: at an intensity of about 0.04 (i.e. training increased the job productivity of participants by 4%), training-induced job creation just offsets congestion and the non-participant line goes flat – displacement disappears. The program sits, in other words, well inside the region where coverage reallocates jobs, rather than creating them.

Taken together, the calibrated exercise comfort the reduced-form message. PACEA raises participants' employment; it displaces non-participants, by at least the congestion channel; and at its current shallow training intensity it lifts aggregate employment only by sorting youths into more effective job search, not by inducing firms to create jobs. Whether a fixed budget would buy more employment by reallocating from coverage toward training intensity is a question about costs as much as about mechanisms, and we take it up alongside the cost-benefit accounting in the following section.

[28] Appendix A.15 reports the calibration and details in Table A.15.1. Table A.15.2 shows how the aggregate employment rate and the labor market tightness vary with treatment variations.

[29] Table A.15.3 shows sensitivity tests of the macro-displacement estimates whose range varies from 26% to 56% when calibrating the model's parameters with alternative values.

Figure 3: Displacement effects of PACEA on non-participants



Note: Non-participant (control) employment rate as program coverage π varies from 0 to 1, at the calibration of Table A.15.1. At the calibrated training intensity ($\Delta \approx 0.01$, solid), expanding coverage lowers non-participant employment – displacement – because congestion reduces equilibrium tightness. At the offsetting intensity ($\Delta^\star \approx 0.04$, dashed), training-induced job creation just offsets congestion and the non-participant rate is flat. The vertical line marks the observed coverage $\pi = 0.5$.

7 Cost-benefit analysis and the optimal policy mix

We close by weighing PACEA's costs against its benefits and then using the calibrated model to ask which mix of coverage and training intensity a fixed budget should buy.

Costs. We approximate the full public cost of a PACEA participant, inclusive of the intensive schemes the program channels youths into. Three components add up. First, the PACEA allowance: the national envelope for the 2017–2018 cohorts was about €31 million for some 388,000 participants, or roughly €80 per participant on average (concentrated among the most precarious, whose mean allowance exceeds €1,600). Second, and much larger, the *Garantie jeunes* (GJ) – a highly intensive welfare-to-work track, with its own monthly allowance and collective coaching, that is embedded within PACEA. PACEA raises GJ enrollment by about 25 percentage points (Table A.8.3, column 1), and the GJ envelope for these cohorts was on the order of €880 million; spread over the PACEA population this is about €2,270 per participant. Third, caseworker time: at an average gross monthly caseworker wage of €2,270, a loaded hourly cost near €21, and the additional interactions PACEA generates (Table 4),

Table 7: Per-participant cost-benefit summary of PACEA

	Per participant
Panel A. Costs	
PACEA allowance	€80
Garantie jeunes (intensive track)	€2,270
Caseworker time	€250
Total cost	**€2,600**
Panel B. Benefits	
Additional employment (4 years)	24 days
Additional gross earnings	€1,200
present value (5%/yr)	€1,000
less: displacement of non-participants	−50%
Social benefit, present value	€500
Direct fiscal return (private base / social base)	**€400 / €200**
Panel C. Balance	
Net, fiscal private base - total cost	-€2,200
Net, fiscal social base - total cost	-€2,400
Benefit-Cost ratio (private / social)	**0.15 / 0.08**

Note: Note: Per-participant amounts for the 2017-2018 cohorts, rounded. Costs include the *Garantie jeunes* intensive envelope PACEA channels youths into (€880m over the population; Table A.8.3, column 1). The private benefit is the present value of the additional employment days of Table 3, valued at the control daily wage. The displacement adjustment applies the OLS estimates of spillovers (column (3) in Table 6) to the participant-non-participant employment gap (0.045); because the model's congestion channel understates the estimated spillover, 50% is a pessimistic view on the share displaced. Caseworker time relies on an unsourced hours assumption.

each participant absorbs on the order of €175-€250. The full per-participant cost is therefore about €2,600, of which the intensive GJ component accounts for the overwhelming share – a direct measure of how expensive program intensity is relative to light-touch coverage.

Benefits. The intensive-margin estimates in Table 3 imply roughly $1.4+1.8+9.0+11.6 \approx 24$ additional days of employment cumulated over four years. Valued at the control group's daily wage of about €50 (a monthly wage near €1,100 over some 22 working days), this is about €1,200 in additional gross earnings over four years. We value the employment-days effect at the observed control wage rather than relying on a direct earnings regression: as discussed in Section 5.3, administrative earnings are recorded with substantial measurement error for this population, so a direct estimate would be dominated by noise, and because the program leaves wages unchanged the well-identified employment gain maps into earnings at roughly the prevailing wage. Discounting at 5% per year, with the gains concentrated in years three and four, leaves a present value near €1,000.

Netting out displacement. That €1,000 is a private benefit: it is what a participant gains, and Section 6.3 showed that part of it is taken from someone else. The calibrated model aligns with the OLS estimates of displacement effects in magnitude at around half the micro-gains. In a conservative view from the public finance perspective, we then consider that about 50% of the private gain is reallocated rather than created, so the social benefit is roughly 0.5 times the private one – near €500 in present value. The associated direct fiscal return – contributions and income tax on the additional earnings, net of reduced transfers, at an effective rate of about 40% – is on the order of €400 per participant on the private base, or €200 on the social one.

Balance. Table 7 collects these figures. At the current program design the full cost (€2,600) exceeds the present value of the benefit on either reading – a benefit-cost ratio of 0.15 against the private gain and 0.08 once displacement is netted out. The imbalance is driven almost entirely by the intensive track of job search assistance when caseworkers and young people meet to plan and activate job search strategies.

The optimal policy mix under a fixed budget. The accounting exercise evaluates the observed program. We next ask whether reallocating its budget between coverage and training could raise aggregate employment.[30] This is a distinct question: a small gain from redesigning an existing program need not imply a small gain from providing the program in the first place. We use the calibrated equilibrium model to choose coverage, π, and the productivity increment associated with training, Δ, while holding the search efficiencies of participants and non-participants fixed. The objective is steady-state aggregate employment, subject to a fixed budget per new unemployment entrant.

The expenditure categories reported above do not identify the cost of producing a given productivity increment. We therefore parameterize the cost of treating a participant as

$$\kappa(\Delta; s, \gamma) = (1 - s) + s \left(\frac{\Delta}{\Delta_0} \right)^{\gamma}, \qquad \Delta_0 = 0.01, \tag{21}$$

where s is the share of current participant cost attributed to the productivity-enhancing component and γ governs its curvature. Normalizing current participant cost to one gives $\kappa(\Delta_0; s, \gamma) = 1$ and a budget $B = \pi_0 = 0.50$ in every scenario. This normalization holds both the observed policy and its budget fixed across cost specifications. We vary s from 0.01 to 0.20 and consider $\gamma \in \{1, 1.5, 2, 3\}$. These parameters describe alternative cost technologies; they are not estimated from the expenditure accounting.

Table 8 reports selected solutions. With convex training costs, the employment gains from reallocation are often modest, even when the optimal policy differs appreciably from

[30] See Appendix A.16 for theoretical details and foundations.

Table 8: Employment-maximizing policy mixes under a fixed budget

	$\gamma = 1$ (linear)			$\gamma = 1.5$		
s	Δ^*	π^* (%)	Gain (pp)	Δ^*	π^* (%)	Gain (pp)
0.010	0.100†	45.9	2.23	0.100†	38.3	1.65
0.025	0.100†	40.8	1.84	0.100†	28.3	0.89
0.050	0.100†	34.5	1.36	0.054	31.6	0.34
0.100	0.100†	26.3	0.74	0.021	41.4	0.05
0.150	0.100†	21.3	0.36	0.011	49.5	0.00
0.200	0.100†	17.9	0.10	0.006	56.2	0.02
	$\gamma = 2$ (quadratic)			$\gamma = 3$ (cubic)		
s	Δ^*	π^* (%)	Gain (pp)	Δ^*	π^* (%)	Gain (pp)
0.010	0.069	34.2	0.75	0.025	43.4	0.22
0.025	0.036	38.3	0.29	0.017	45.6	0.07
0.050	0.021	42.6	0.09	0.012	47.9	0.01
0.100	0.012	48.5	0.00	0.009	51.7	0.01
0.150	0.008	53.3	0.01	0.007	55.4	0.04
0.200	0.006	58.0	0.05	0.006	59.3	0.10

Note: Δ^* and π^*: employment-maximizing training intensity and coverage. Observed policy: $\Delta_0 = 0.01$, $\pi_0 = 50\%$. Gains are $100\,[n^*(\pi^*, \Delta^*) - n(\pi_0, \Delta_0)]$, i.e. additional gains over the observed program, not program effects relative to no intervention. † Upper bound on training intensity ($\Delta \leq 0.10$).

the observed mix. For example, at $s = 0.01$ and $\gamma = 1.5$, optimal training intensity is 0.1, compared with 0.01 under the observed policy, while coverage falls from 50.0 to 38.3 percent. Equilibrium employment increases by 1.65 percentage points. At the same cost share with quadratic costs, the optimal intensity is 0.069, coverage is 34.2 percent, and the gain is 0.75 percentage points. With $s = 0.05$ and quadratic costs, the observed mix is already close to the employment-maximizing allocation: the gain from reallocation is approximately 0.09 percentage points.

These magnitudes reflect the interaction of costs, coverage, and job creation. A low training share at the observed intensity does not imply that much more intensive training is inexpensive. At $s = 0.01$ and $\gamma = 1.5$, the optimal intensity is ten times its observed level, raising participant cost to 1.837 times current cost. The budget then permits coverage of only 38.3 percent. Moreover, training enters job creation through $(1+\pi\Delta)^\alpha$: the participant productivity increment is weighted by coverage. In this example, $\pi\Delta$ rises from 0.005 to 0.0383, rather than to 0.1. Diminishing marginal productivity and the concavity of the job-finding rate in labor-market tightness further moderate the employment response.

The reduction in coverage also sacrifices the aggregate employment benefits of assisted search. To illustrate, raising intensity while retaining observed coverage would increase em-

ployment. Reducing coverage to finance that intensity subtracts percentage points, leaving the affordable gain at a lower value than the initial one. This sequential decomposition is illustrative: its intermediate policy exceeds the budget, and the allocation of the total effect between the two changes depends on their order. It nevertheless shows why a large increase in participant productivity can coexist with a modest net increase in aggregate employment. When training is more expensive, the preferred reallocation can increase coverage and reduce intensity. There is therefore no cost-independent ranking of broader assistance and deeper training.

The policy implication is conditional. The exercise establishes how the coverage-intensity trade-off depends on the cost of generating productivity improvements, but the available evidence does not identify that cost sufficiently to justify a large reallocation toward training. Where predicted gains are negligible, numerical differences between policy mixes provide little basis for redesign. Where gains are larger, their credibility depends on the assumed training technology and its validity away from the observed policy. Evidence on the cost and persistence of training-induced productivity gains is thus central to choosing between the two margins.

8 Conclusion

This paper asked whether intensive job-search assistance for unemployed youths, possibly combined with vocational training, can durably improve labor-market outcomes, through which channels, and whether individual gains translate into aggregate employment. We addressed these questions through the French *Parcours contractualisé d'accompagnement vers l'emploi et l'autonomie* (PACEA), using comprehensive administrative data and an instrumental-variables strategy based on the quasi-random assignment of youths to caseworkers and their heterogeneous propensity to enroll youths into the program. The empirical analysis is guided by a stylized general-equilibrium job-search and matching model that clarifies how individual-level gains interact with labor-market congestion, job creation, and policy scale.

Three main findings emerge. First, PACEA has limited short-run effects and generates temporary lock-in during the program period, reflecting the intensive nature of the intervention. Second, medium-run impacts are sizable and persistent: three to four years after registration, PACEA raises employment probabilities by about 4 to 5 percentage points and employment durations by roughly two days per month, indicating stronger attachment to the labor market once participants complete the program. Third, these gains operate almost entirely along the extensive margin. We find no robust effects on wages, contract stability, or occupational upgrading, suggesting that PACEA facilitates access to employment rather than improving job quality or productivity.

The analysis of mechanisms sheds light on these patterns. PACEA substantially increases

interactions with caseworkers, structures job-search trajectories, and raises participation in workshops and vocational training. These components enhance search efficiency and persistence, consistent with the delayed but durable employment gains observed in the data. At the same time, the brevity of training and the absence of wage and productivity effects indicate that training intensity is insufficient to generate sustained improvements in job quality, especially in a labor market with a high prevalence of low-wage jobs for young workers.

Embedding these results in a general-equilibrium framework highlights an important micro-macro distinction. While search assistance improves job-finding rates for treated individuals, scaling up such programs raises aggregate effective search and congestion in the matching process. When training intensity is limited, equilibrium adjustment occurs mainly through the reallocation of job opportunities rather than net job creation, generating displacement effects for non-participants. Consistent with this mechanism, we document negative spillovers so that around 50% of the gain induced at the micro-level is done at the expense of non-participants. Training in which some participants are enrolled need much higher intensity to raise expected post-hire productivity and stimulating vacancy creation.

These findings have implications for the design of youth labor-market policies. Under a fixed budget, increasing training intensity requires reducing coverage, trading potential productivity gains against the employment benefits of assisted search. Our calibrated exercise shows that the preferred allocation depends critically on the cost of generating productivity improvements. With convex training costs, the additional aggregate employment gains from reallocation are often modest, even when the preferred policy mix differs substantially from the observed program. Larger gains arise under favorable cost assumptions but can depend on allowing training intensities far beyond the observed level. The results therefore do not support an unconditional recommendation to concentrate resources on fewer participants. Rather, they highlight the need to identify the cost and persistence of training-induced productivity gains and to evaluate employment effects at the labor-market level, including displacement. PACEA illustrates how assistance can improve disadvantaged youths' employment prospects while leaving the aggregate benefits of alternative program designs uncertain.

Several directions for future research follow. First, evaluating how specific combinations of search assistance, training content, and employer involvement affect productivity would help identify the training threshold beyond which coverage generates net job creation rather than displacement. Second, richer data on the content and certification of training – unavailable in our administrative sources – would sharpen the interpretation of the productivity channel. Finally, extending the general-equilibrium analysis to account for the interaction between youth activation and broader labor-demand policies would clarify how to improve both individual outcomes and aggregate employment.

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A Appendix

A.1 Classification of occupations within missions locales

Table A.1.1 reproduces the occupational classification grid of the "missions locales" network, which we use to situate caseworkers within the broader staff structure of youth centers. Occupations are grouped into four categories – job search and social assistance, information and communication, management, and direction – each associated with a skill code and a corresponding gross monthly wage. Caseworkers, who conduct the first meetings and accompany young people throughout their pathway, belong to the "job search and social assistance" category and are classified at skill levels 11 and 12, with starting gross wages of about €1,996 and €2,326, respectively. These figures confirm that caseworkers are mid-qualified staff, typically recruited with at least two years of higher education, and that the recruitment and pay of the intermediaries whose behavior we exploit are governed by a standardized national grid rather than by local discretion.

Table A.1.1: Classification system of occupations within "missions locales"

Category	Occupation	Skill code	Wage (€)
Job search and social assistance	Receptionist	6	1,811
	Caseworker level 1	11	**1,996**
	Caseworker level 2	12	**2,326**
	Project Manager	14	2,644
Information and communication	Animation Coordinator	6	1,774
	Documentation officer	8	1,966
	Communication Officer	10	2,098
Management	Administrative Assistant	4	1,731
	Management Assistant	7	2,018
	Executive Assistant	10	2,337
	Financial Assistant	10	2,413
	IT Assistant	11	2,167
Direction	Area Manager	15	3,165
	Director	16	4,230

Note: This table reports the list of occupations within youth job centers and the associated raw wage.
Source: "*Le modèle économique des missions locales pour l'insertion professionnelle et sociale des jeunes*", IGAS, 2016

A.2 Sample selection

Employment and training data (FORCE) starts in 2017, we thus restricted our sample to cohorts of young people who registered with public centers after January 1, 2017. In order to observe labor market trajectories over a sufficiently long period after their first meeting, we exclude individuals who visited an agency after December 31, 2018.[31] This results in approximately 800,000 young people for whom we have information on both their follow-up at the agency and their subsequent labor market situations until December 2022. However, we had to impose a number of restrictions to construct our final sample.

First, we drop observations with missing values in socio-demographic characteristics. Second, we excluded young people who did not have a regular one-to-one first meeting with a caseworker when they initially registered with an agency.[32] As we focus on young people seeking for job related assistance, we only keep young people who were NEET at the time of their first meeting (about 90%).

Finally, we imposed additional restrictions to ensure econometric feasibility of our analysis. This included narrowing the sample to young people who registered with a "mission locale" center between January 2017 and December 2018. We also excluded center $\times$ month cells with fewer than four youths, two caseworkers, and two youths per caseworker to ensure that each cell had a minimum of four observations (2 youths $\times$ 2 caseworkers). Finally, we considered caseworkers who conducted first meetings for at least one month per quarter over the two-year period. This condition guaranteed that caseworkers were present throughout the period.

Table A.2.1 provides a comprehensive list of the restrictions we imposed and the resulting number of observations. This process yields a final sample of 298,142 youths, 2,269 caseworkers, and 317 youth centers.

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

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

Table A.2.1: Steps to select final sample

Filters	N
	(1)
Initial number of observations/youths	808,222
1. No missing value in characteristics	
2. Only regular first meetings	
3. Econometric feasibility	
(Min, Max) date of the first meeting = (01/01/2017, 12/31/2018)	
Min number of youths by cell = 4	
Min number of caseworkers by cell = 2	
Min number of youth per caseworker by cell = 2	
Min number of years by caseworker = 2	
Min number of quarters by caseworker = 8	
Min number of caseworkers by center overall = 3	
Final number of observations/youths	298,142
Final number of caseworkers	2,269
Final number of centers	317

Note: This table reports the number of observations in our selected sample after filtering the raw database. All individual characteristics are listed in Table 1. Regular meetings corresponds to individual meetings with a caseworker in a registration agency during workweek. A cell corresponds to an ML center × entry quarter.

A.3 Additional descriptive evidence on PACEA participation

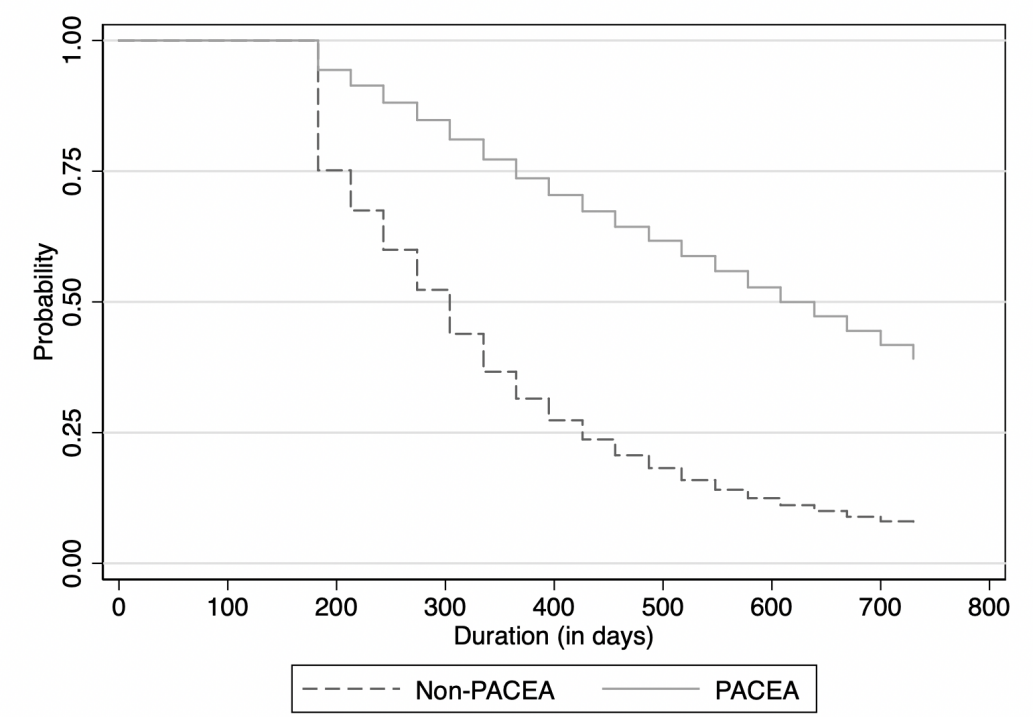
Table A.3.1 details activities and trajectories during PACEA, Figure A.3.1 documents the persistence of participation in youth centers, and Figure A.3.2 reports the descriptive OLS event study of employment probability discussed in Section 3.3.

Table A.3.1: Activities during PACEA

	Mean	Sd	Min	Q1	Q2	Q3	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Duration before PACEA (in days)	105.10	159.62	0	9	30	121	730
Duration in PACEA (in days)	493.79	241.88	0	301	495	729	1,761
Nb of events	52.33	51.98	0	13	31	82	817
Including meetings with referent caseworker	4.15	4.73	0	1	3	6	80
Including contacts with referent caseworker	3.39	9.92	0	0	1	4	559
Nb of defined phases	1.51	0.86	0	1	1	2	9
Duration of a phase (in days)	274.18	170.32	0	164	234	364	1,649
Duration before 1st phase (in days)	6.41	42.39	0	0	0	0	1,020
Duration between each phase (in days)	30.40	75.95	0	1	1	11	769
Nb of defined objectives per phase	1.48	1.46	0	1	1	2	34
Share who experienced intensive workshops	0.44	0.50	0	0	0	1	1
Share who experienced vocational training	0.17	0.37	0	0	0	0	1
Share who experienced paid employment	0.62	0.48	0	0	1	1	1
Total monetary allowance (in €)	1,651.89	2,301.43	0.00	0.00	175.00	3,502.26	14,765.60

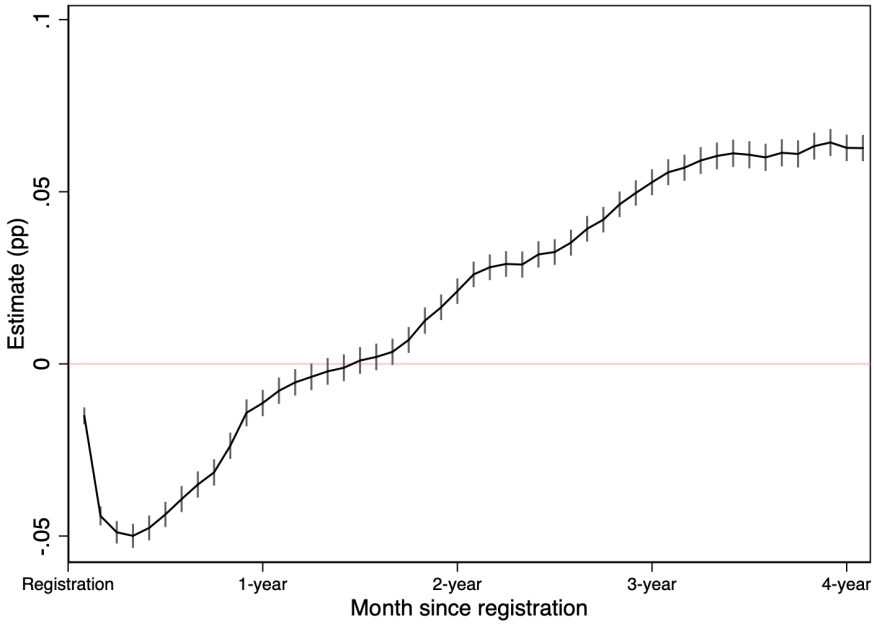
Note: List of activities made by youths before and during the PACEA program ($N = 144,453$).

Figure A.3.1: Survival in youth center (2-year window)



Note: Survival is defined as having at least one event (meeting, workshop, information session, or contact) with a caseworker within a period of five consecutive months. Probabilities obtained with non-parametric Kaplan-Meier estimates.

Figure A.3.2: Difference in employment probability over time



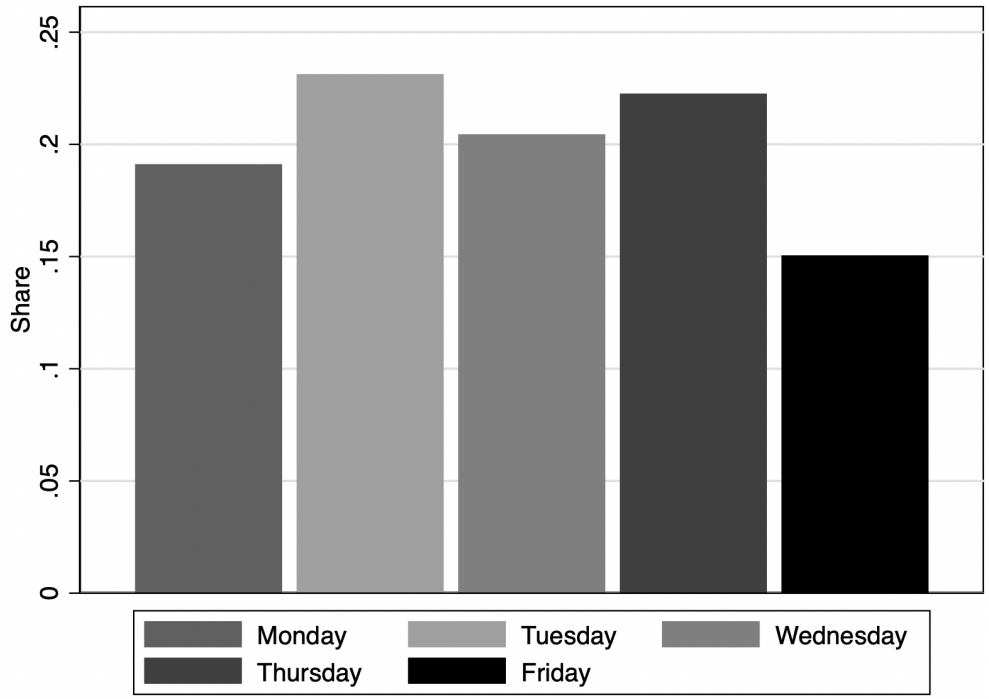
Note: OLS estimates of PACEA on monthly employment probability, controlling for individual characteristics listed in Table 1 and Center × Entry quarter fixed effects.

A.4 Assignment of caseworkers

This appendix provides supporting evidence for the quasi-random assignment of youths to caseworkers discussed in Section 3.4. Figure A.4.1 plots the distribution of first meetings across weekdays. Meetings are spread fairly evenly from Monday to Thursday and are less frequent on Fridays, consistent with the organization of centers, where caseworkers are scheduled to welcome new entrants on a rotation planned several weeks in advance and where team meetings typically take place at the end of the week. Because caseworkers are assigned to calendar days rather than to individuals, the day on which a youth happens to register determines which caseworker they meet.

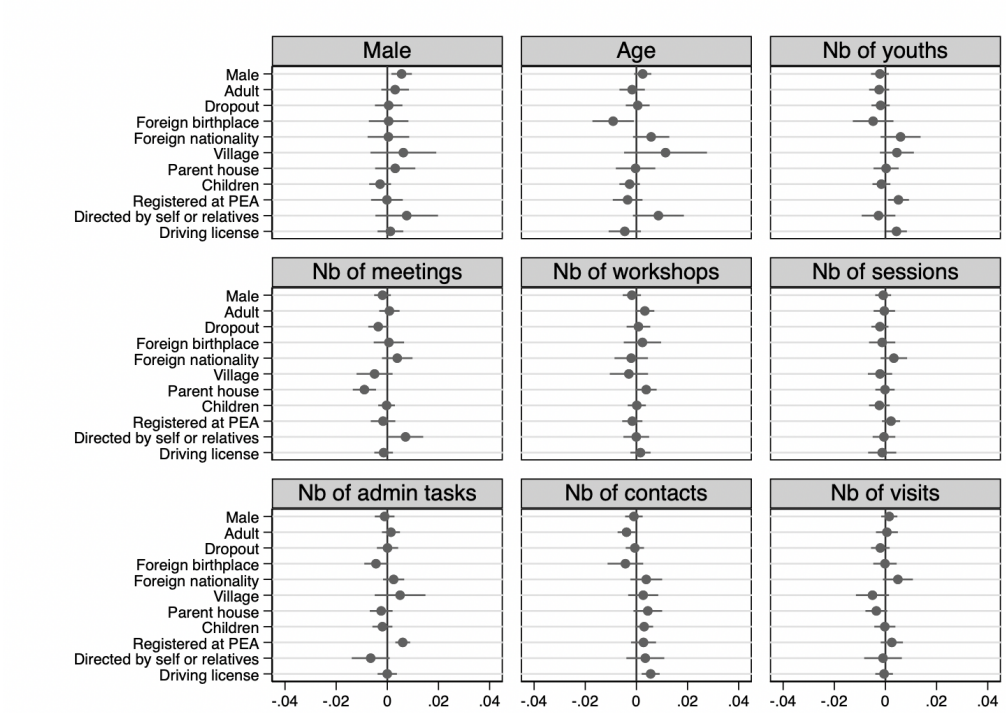
Figure A.4.2 then assesses whether this assignment is orthogonal to youth characteristics. Each square reports the coefficient from an OLS regression of a given (standardized) caseworker characteristic on the (standardized) characteristics of their newly assigned youths, within center × entry-quarter cells. The vast majority of coefficients are small and statistically insignificant, indicating that, conditional on the center and entry quarter, youths are not systematically sorted toward caseworkers with particular characteristics. This balance is what underlies the identifying assumption of our instrumental-variable strategy.

Figure A.4.1: Histogram of the days of first meetings



Note: Days of the first meeting include days between January 1st, 2017 and December 31, 2018.

Figure A.4.2: Correlation between youths' and caseworkers' characteristics

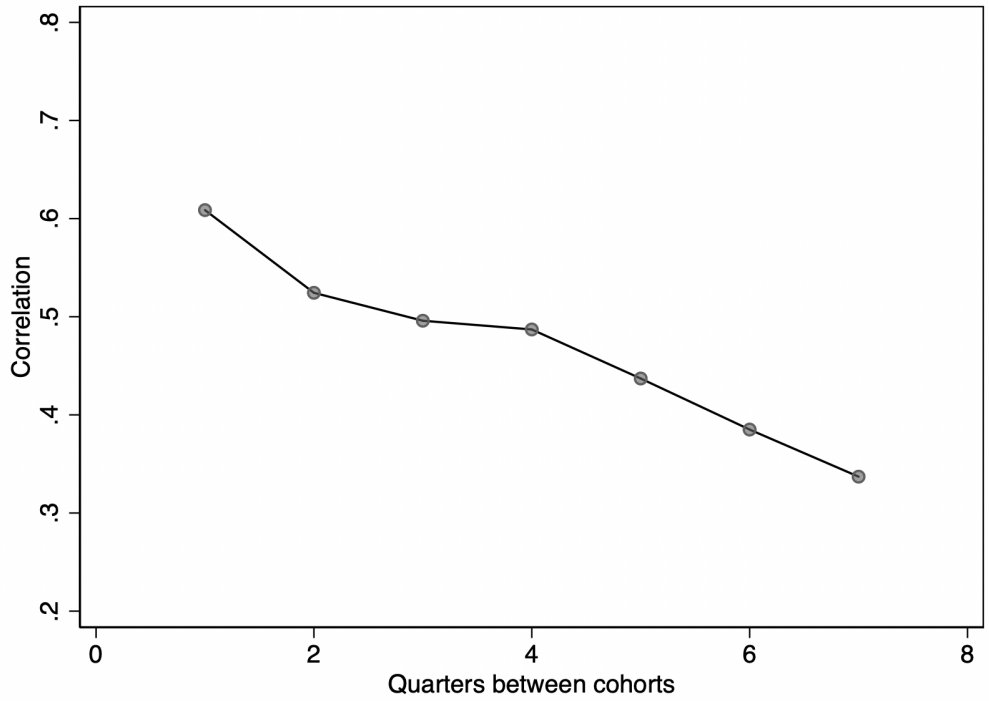


Note: Each square shows the estimates from an OLS regression of a specific caseworker characteristic on all its newcoming youths' characteristics listed in Table 1 and Center × Entry Quarter fixed effects. All variables are standardized and standard errors are clustered at the caseworker level.

A.5 Assessment of caseworkers propensity instrument

This appendix documents the persistence over time of caseworkers' propensity to enroll youths into PACEA, which underlies the construction of our instrument (Section 4.2). Following Chetty et al. (2014), the instrument is built as the best linear predictor of a caseworker's PACEA enrollment residual in a given quarter using their residuals in all other quarters, so its informativeness hinges on how stable this propensity is across cohorts. Figure A.5.1 plots the autocorrelation of the mean PACEA placement propensity across cohorts handled by the same caseworker in different quarters. The correlation is about 0.60 between adjacent quarters and declines monotonically to around 0.35 at a seven-quarter lag. This persistence is more pronounced than the analogous rates typically found for teacher value-added, which decline from about 0.45 to 0.20 over the same range (Chetty et al., 2014). It indicates that enrollment practices reflect a stable caseworker trait rather than transitory noise, so that leaving out the focal cohort still yields an informative predictor of a caseworker's PACEA orientation.

Figure A.5.1: Autocorrelation vector in youth center for PACEA enrollment



Note: Correlation between mean PACEA placement propensity across cohorts assisted by the same caseworker in different quarters.

A.6 Assessment of the instrument

This appendix develops the assessment of our instrumental-variable strategy summarized in Section 4.3. We examine in turn the conditions required for a causal interpretation of the IV estimates – the relevance of the first stage, the conditional independence of the instrument, the exclusion restriction, and monotonicity – and then discuss what our specification identifies in light of recent work on the LATE interpretation of two-stage least squares.

A.6.1 Relevance of the first stage

The identification strategy relies on the existence of substantial and systematic heterogeneity in caseworkers' propensity to enroll youths into PACEA. Table A.6.1 provides evidence on the strength of the first-stage relationship between caseworker-level PACEA orientation and individual participation. Columns (1) and (2) use the leave-out mean share of PACEA enrollments among other youths handled by the same caseworker, $\overline{\text{CswkPACEA}}_{jt}$, as an instrument. The estimated coefficients indicate a strong first-stage relationship: when the caseworker's average PACEA enrollment rate increases by one unit, the probability that an assigned youth enters PACEA increases by about 0.82. This estimate is highly stable to the inclusion of individual covariates, suggesting that the first-stage relationship is not driven by observable differences in youth composition across caseworkers. The corresponding first-stage F-statistics exceed 3,700, far above conventional thresholds, documenting an exceptionally strong instrument.

Table A.6.1: First stage regressions

	PACEA_i			
	(1)	(2)	(3)	(4)
$\overline{\text{CswkPACEA}}_{jt}$	0.8200***	0.8183***	–	–
	(0.0135)	(0.0124)	–	–
$\widetilde{\text{CswkPACEA}}_{jt}$	–	–	0.1387***	0.1378***
	–	–	(0.0050)	(0.0048)
Observations	298,142	298,142	298,142	298,142
R-squared	0.1189	0.1609	0.1076	0.1495
Individual characteristics	No	Yes	No	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes
F-stat for underidentification	3717	4324	780	819

Note: $\overline{\text{CswkPACEA}}_{jt}$ is defined in equation (19) and $\widetilde{\text{CswkPACEA}}_{jt}$ is defined in equation (20).

Columns (3) and (4) report first-stage results using the alternative binary instrument $\widetilde{\text{CswkPACEA}}_{jt}$, which captures whether the assigned caseworker is classified as pro-PACEA at the time of entry. Assignment to a pro-PACEA caseworker increases the probability of PACEA enrollment by around 14 percentage points, again with virtually identical estimates when individual characteristics are included. Although the magnitude of the coefficient is mechanically smaller due to the binary nature of the instrument, the corresponding F-statistics – between 780 and 819 – remain well above any weak-instrument concern. Taken together, these results provide strong evidence that caseworker heterogeneity generates

Table A.6.2: Relation of youth characteristics to PACEA and assignment to caseworker

	PACEA		Caseworker Pro-PACEA	
	(1)	(2)	(3)	(4)
Male	-0.0340***	-0.0327***	-0.0040	-0.0047***
	(0.0020)	(0.0019)	(0.0025)	(0.0016)
Adult	-0.0200***	-0.0117***	-0.0125**	-0.0029
	(0.0033)	(0.0030)	(0.0049)	(0.0025)
Dropout	-0.0054**	0.0015	-0.0014	0.0004
	(0.0025)	(0.0024)	(0.0033)	(0.0019)
Foreign birthplace	0.0444***	0.0507***	-0.0349***	-0.0003
	(0.0050)	(0.0046)	(0.0077)	(0.0040)
Foreign nationality	-0.0483***	-0.0536***	0.0123*	0.0017
	(0.0055)	(0.0052)	(0.0069)	(0.0043)
Village	-0.0298***	-0.0353***	0.0236*	0.0111**
	(0.0051)	(0.0033)	(0.0125)	(0.0057)
Parent house	0.0297***	0.0141***	0.0341***	0.0012
	(0.0029)	(0.0025)	(0.0054)	(0.0027)
Married	-0.0539***	-0.0469***	-0.0115	-0.0053
	(0.0043)	(0.0039)	(0.0071)	(0.0039)
Children	-0.0617***	-0.0633***	0.0131**	0.0009
	(0.0047)	(0.0042)	(0.0063)	(0.0033)
Registered at PEA	0.1866***	0.1874***	0.0101*	0.0040*
	(0.0033)	(0.0029)	(0.0053)	(0.0024)
Directed by self or relatives	0.0618***	0.0491***	0.0284***	0.0022
	(0.0031)	(0.0025)	(0.0065)	(0.0039)
Driving license	-0.1174***	-0.1101***	-0.0043	0.0023
	(0.0027)	(0.0025)	(0.0039)	(0.0019)
Joint F-test (covariates = 0)				1.32
Joint p-value				0.198
Observations	298,142	298,142	298,142	298,142
R-squared	0.0478	0.1397	0.0036	0.4850
Center × Entry Quarter FE	No	Yes	No	Yes

Note: Estimates obtained with OLS regressions. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

large and precisely estimated variation in PACEA participation within "mission locale" centers and entry quarters.

A.6.2 Conditional independence

A key identifying assumption underlying our IV strategy is that assignment to a pro-PACEA caseworker is conditionally independent of youths' observable characteristics at entry. Table A.6.2 assesses this assumption by relating both actual participation in PACEA and assignment to a pro-PACEA caseworker to the set of individual characteristics. Columns (1) and (2) document strong correlations between youth characteristics and PACEA participation as discussed in Section 3.3.

In contrast, Columns (3) and (4) focus on the assignment to a pro-PACEA caseworker. Without fixed effects (Column 3), several individual characteristics are significantly correlated with caseworker type, indicating non-random assignment across centers or periods. However, once center × entry-quarter fixed effects are included (Column 4), the vast majority of coefficients shrink sharply in magnitude and lose statistical significance. The remaining statistically significant coefficients, such as those for gender or rural residence, are economically very small, typically below half a percentage point. This suggests that most of the raw correlation between youth characteristics and caseworker type is driven by between-center or between-period variation rather than selective assignment within local offices and entry cohorts. Conditional on those, assignment to a pro-PACEA caseworker appears

Table A.6.3: Relation of instrument with youth requests at registration

	Caseworker Pro-PACEA				
	(1)	(2)	(3)	(4)	(5)
Request: Employment	-0.0007				0.0055
	(0.0034)				(0.0067)
Request: Training		0.0041			0.0086
		(0.0047)			(0.0072)
Request: Apprenticeship			0.0028		0.0088
			(0.0042)		(0.0072)
Request: Professional Project				0.0037	0.0088
				(0.0048)	(0.0077)
Observations	210,882	210,882	210,882	210,882	210,882
R-squared	0.5026	0.5026	0.5026	0.5026	0.5027
Individual characteristics	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with OLS regressions. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

largely orthogonal to observed socio-demographic characteristics.

Table A.6.3 provides an additional test of conditional independence by examining whether youths' stated requests at registration (employment, training, apprenticeship, or a professional project) predict assignment to a pro-PACEA caseworker. Across all specifications, none of these requests significantly affects assignment, either individually or jointly. This result is particularly informative, as these requests directly capture youths' expressed preferences and intentions at entry, which could plausibly influence caseworker assignment or behavior. Their absence of predictive power provides further support for the assumption that conditional on individual characteristics and center × entry-quarter fixed effects, assignment to a pro-PACEA caseworker is as good as random.

A.6.3 Exclusion restriction

The exclusion restriction requires that assignment to a pro-PACEA caseworker affects youths' labor market outcomes only through participation in PACEA and not through alternative channels of caseworker assistance or pre-existing differences across youths. Table A.6.4 provides an indirect test of this assumption by estimating the reduced-form effect of assignment to a pro-PACEA caseworker on employment outcomes for youths who never enroll in PACEA. Across all horizons – six, twelve, eighteen, and twenty-four months after registration – the estimated coefficients are small in magnitude, statistically insignificant, and precisely estimated. Point estimates are close to zero and show no systematic pattern over time. This absence of reduced-form effects among non-participants suggests that pro-PACEA caseworkers do not improve labor market outcomes differently through channels other than PACEA enrollment itself, such as more intensive counseling, vacancy referrals, or informal placement support.

Table A.6.5 further supports the exclusion restriction by examining whether assignment to a pro-PACEA caseworker predicts youths' labor market histories prior to registration in a "mission locale"

Table A.6.4: Impact of Caseworker Pro-PACEA on labor market outcomes for youths not enrolled in PACEA

Employment	6-months	12-months	18-months	24-months
	(1)	(2)	(3)	(4)
Cswk Pro-PACEA	0.0005	-0.0036	-0.0021	-0.0045
	(0.0035)	(0.0035)	(0.0035)	(0.0035)
Observations	153,689	153,689	153,689	153,689
R-squared	0.1191	0.1222	0.1172	0.1138
Individual characteristics	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes

Note: Estimates obtained with OLS regressions using only young individual who did not enroll in PACEA. Employment is equal to one if young people are in employment six months after registration in column (1), twelve months in column (2), eighteen months in column (3), and twenty-four months in column (4), zero otherwise. Caseworker Pro-PACEA is computed from (20). Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

center. Reassuringly, the instrument is unrelated to both pre-registration employment and training participation over the six months preceding entry. These placebo tests rule out sorting on unobserved past labor market attachment that could simultaneously affect assignment to a given caseworker and future employment outcomes. Conditional on individual characteristics and center × entry-quarter fixed effects, youths assigned to pro-PACEA and non-pro-PACEA caseworkers exhibit indistinguishable pre-registration trajectories.

Taken together, the evidence from these two tables complements the institutional arguments regarding the organization of "missions locales" and the bundled nature of PACEA. Caseworkers' discretion primarily operates along the margin of program enrollment, while access to other forms of assistance is governed by standardized rules and centralized constraints. Although not testable directly, the absence of reduced-form effects on non-participants and on pre-treatment outcomes provides empirical support for the exclusion restriction. This lends credibility to the interpretation of the IV estimates as capturing the causal effect of PACEA participation on labor market outcomes.

A.6.4 Monotonicity

A final identifying assumption of the IV strategy is monotonicity, namely that assignment to a more pro-PACEA caseworker increases the probability of PACEA participation for all youths, and does not induce some individuals to systematically opt out of the program. Following the approach in Arni and Schiprowski (2019), we assess this assumption by examining whether the first-stage relationship between caseworker pro-PACEA orientation and individual enrollment is non-negative across a wide range of subsamples. Column (1) of Table A.6.6 reports first-stage estimates obtained using the baseline instrument for different groups defined by gender, age, education, and mobility constraints. Across all subsamples, the estimated coefficients are positive, precisely estimated, and of similar magnitude, ranging from about 0.12 to 0.16. This uniformity indicates that assignment to a pro-PACEA caseworker increases enrollment probabilities for all observed groups, providing strong support for monotonicity.

As a more demanding test, Column (2) reports estimates based on a reverse-sample instrument. For each subsample, the instrument is constructed using only individuals outside the subgroup – for example, using female youths to compute the caseworker pro-PACEA measure and estimating its

Table A.6.5: Impact of Caseworker Pro-PACEA on pre-registration labor market histories

	Employment	Training
	(1)	(2)
Cswk Pro-PACEA	0.0035	-0.0001
	(0.0021)	(0.0005)
Observations	298,142	298,142
R-squared	0.1408	0.0146
Individual characteristics	Yes	Yes
Center × Entry Quarter FE	Yes	Yes

Note: Estimates obtained with OLS regressions. Employment is equal to one if young people experience at least one day of employment experience within a period of six months before registration in a "mission locale" center in column (1), and similarly for training in column (2), zero otherwise. Caseworker Pro-PACEA is computed from (20). Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

effect on male enrollment. This procedure addresses concerns that the instrument might be driven by subgroup-specific sorting or differential caseworker behavior. Reassuringly, the estimated coefficients remain positive and highly significant across all subsamples, although their magnitude is mechanically smaller than in the baseline specification.

Taken together, the results provide no evidence of violations of the monotonicity assumption. The fact that the first-stage relationship is positive for all subgroups and remains so under reverse-sample constructions implies that there are no "defiers" for whom assignment to a pro-PACEA caseworker would systematically reduce the likelihood of participation. Consequently, the IV estimates can be interpreted as local average treatment effects for compliers, defined as youths whose PACEA participation is influenced by caseworker orientation, in line with the standard interpretation of judge- or caseworker-based instruments (Dobbie et al., 2018; Arni and Schiprowski, 2019).

Table A.6.6: Testing monotonicity

	Baseline instrument	Reverse sample instrument
	(1)	(2)
A. Gender		
1. Subsample: Male	0.1383***	0.1170***
	(0.0056)	(0.0053)
2. Subsample: Female	0.1366***	0.1172***
	(0.0052)	(0.0051)
B. Age		
1. Subsample: Adult	0.1429***	0.0847***
	(0.0051)	(0.0048)
2. Subsample: Non-adult	0.1164***	0.1114***
	(0.0068)	(0.0066)
C. Education		
1. Subsample: Dropout	0.1212***	0.1055***
	(0.0053)	(0.0050)
2. Subsample: Non-dropout	0.1490***	0.1075***
	(0.0056)	(0.0051)
D. Birthplace		
1. Subsample: France	0.1396***	0.0704***
	(0.0050)	(0.0048)
2. Subsample: Abroad	0.1221***	0.1103***
	0.2647***	0.2716***
E. Nationality		
1. Subsample: French	0.1396***	0.0704***
	(0.0050)	(0.0048)
2. Subsample: Foreign	0.1207***	0.1051***
	(0.0083)	(0.0085)
F. Location		
1. Subsample: City	0.1352***	0.0854***
	(0.0051)	(0.0057)
2. Subsample: Village	0.1453***	0.1234***
	(0.0087)	(0.0076)
G. Residency		
1. Subsample: Parent house	0.1396***	0.1084***
	(0.0056)	(0.0050)
2. Subsample: Other	0.1330***	0.1110***
	(0.0053)	(0.0053)
H. Marital status		
1. Subsample: Married	0.1302***	0.1259***
	(0.0109)	(0.0105)
2. Subsample: Non-married	0.1381***	0.0601***
	(0.0049)	(0.0049)
I. Parental status		
1. Subsample: Children	0.1431***	0.1369***
	(0.0098)	(0.0099)
2. Subsample: No child	0.1369***	0.0628***
	(0.0049)	(0.0048)
J. Registered at PEA		
1. Subsample: Yes	0.1423***	0.0959***
	(0.0056)	(0.0053)
2. Subsample: No	0.1288***	0.1082***
	(0.0059)	(0.0054)
K. Directed by		
1. Subsample: Self or relatives	0.1326***	0.1037***
	(0.0055)	(0.0050)
2. Subsample: Others	0.1438***	0.1114***
	(0.0059)	(0.0058)
L. Driving licence		
1. Subsample: Yes	0.1559***	0.1203***
	(0.0064)	(0.0063)
2. Subsample: No	0.1279***	0.0998***
	(0.0049)	(0.0046)
Individual characteristics	Yes	Yes
Center × Entry Quarter FE	Yes	Yes

Note: Estimates of caseworker pro-PACEA on PACEA enrollment for different sub-samples using baseline instrument in Column (1) and reverse sample (i.e. using female subsample to compute caseworker pro-PACEA instrument and analyse its effects on male enrollment) in column (2). Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.6.5 TSLS and the LATE interpretation

Blandhol et al. (2026) show that, when covariates enter a linear IV specification non-saturatedly, the two-stage least squares (TSLS) estimand does not in general recover a non-negatively weighted average of local average treatment effects (LATEs): a causal interpretation requires the specification to have rich covariates, i.e. to reproduce the conditional mean of the instrument. Our design falls in the favorable case they discuss for conditionally randomly assigned instruments: assignment to a pro-PACEA caseworker is as good as random within center × entry-quarter cells, which we absorb nonparametrically through fixed effects. In this case the rich-covariates condition reduces to the requirement that, within cells, the individual covariates do not predict the instrument. We assess this condition and, more directly, the sensitivity of our estimates to how covariates enter.

First, column (4) of Table A.6.2 regresses the binary instrument on the individual covariates within center × entry-quarter cells. The covariates are jointly insignificant ($F = 1.32$, $p = 0.20$) and individually negligible.

Second, Tables A.6.7 and A.6.8 report our estimates under four covariate specifications. Panel A omits the individual covariates entirely and identifies the effect purely from within-cell variation; because it is saturated in the only covariates on which assignment depends (the cells), it has a bona fide LATE interpretation. Panel B is our baseline linear specification. Panels C and D partial the covariates out by double/debiased machine learning (DML partially-linear IV, Chernozhukov et al., 2018), using linear learners (Panel C) and gradient boosting (Panel D), with cross-fitting by caseworker and standard errors clustered at the caseworker level. The point estimates are remarkably stable across all four specifications. For employment probability, the three- and four-year effects lie between 4.2 and 5.6 percentage points, and the flexible machine-learning estimates in Panel D are virtually identical to the baseline; the same stability holds for employment duration. Together with the rich-covariates test, this confirms that the linear covariate adjustment does not distort the estimand and supports interpreting our IV estimates as a convex-weighted average of caseworker-induced LATEs.

Table A.6.7: Robustness of the LATE to covariate specification: employment probability

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4
Panel A. Baseline (linear covariates)				
PACEA	-0.0014	0.0078	0.0471***	0.0440**
	(0.0170)	(0.0177)	(0.0182)	(0.0183)
First-stage F	818.8	818.8	818.8	818.8
Panel B. No individual covariates (saturated in strata)				
PACEA	0.0049	0.0159	0.0563***	0.0546***
	(0.0187)	(0.0195)	(0.0202)	(0.0204)
First-stage F	779.9	779.9	779.9	779.9
Panel C. DML–PLIV, linear learners				
PACEA	-0.0016	0.0080	0.0474***	0.0440**
	(0.0169)	(0.0176)	(0.0181)	(0.0182)
First-stage F	828.9	828.9	828.9	828.9
Panel D. DML–PLIV, gradient boosting				
PACEA	-0.0028	0.0064	0.0470***	0.0419**
	(0.0168)	(0.0173)	(0.0179)	(0.0181)
First-stage F	813.2	813.2	813.2	813.2
Observations	298,142	298,142	298,142	298,142

Note: IV estimates of the impact of PACEA on employment probability. Panel A adds the individual covariates linearly as the baseline specification in Table 2; Panel B includes only center × entry-quarter fixed effects; Panels C and D partial the covariates out by DML partially-linear IV with linear learners and gradient boosting, respectively, cross-fitting by caseworker. Standard errors clustered at the caseworker level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6.8: Robustness of the LATE to covariate specification: employment days

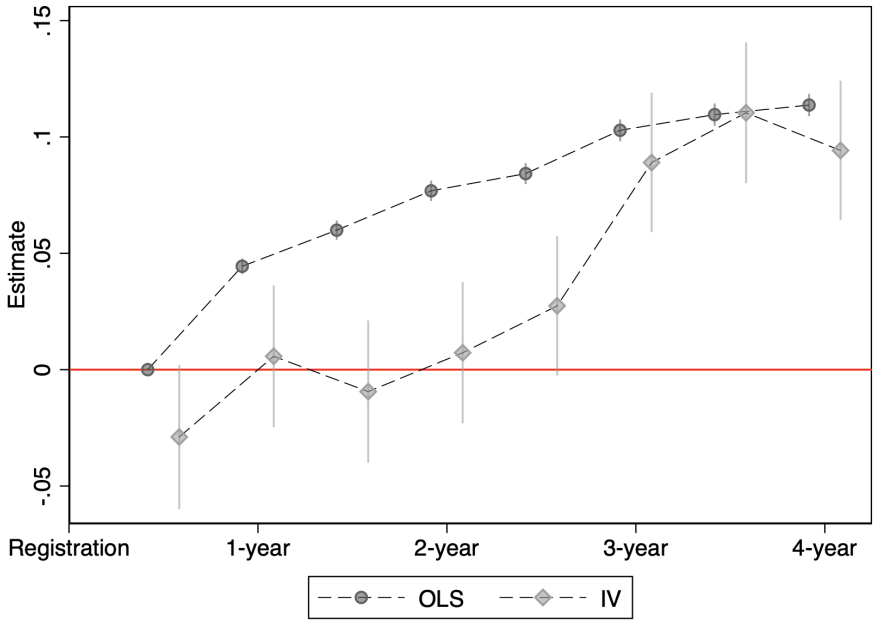
	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4
Panel A. Baseline (linear covariates)				
PACEA	1.4014	1.8074	8.9930*	11.6379**
	(3.8355)	(4.9592)	(5.1944)	(5.3978)
First-stage F	818.8	818.8	818.8	818.8
Panel B. No individual covariates (saturated in strata)				
PACEA	3.0156	3.9578	11.5742**	14.6313**
	(4.3292)	(5.5324)	(5.8338)	(5.9753)
First-stage F	779.9	779.9	779.9	779.9
Panel C. DML–PLIV, linear learners				
PACEA	1.3232	1.7891	9.1067*	11.7182**
	(3.8144)	(4.9322)	(5.1646)	(5.3669)
First-stage F	828.9	828.9	828.9	828.9
Panel D. DML–PLIV, gradient boosting				
PACEA	-0.1486	1.2192	8.8002*	10.5995**
	(3.7864)	(4.8889)	(5.1275)	(5.3656)
First-stage F	810.0	810.0	810.0	810.0
Observations	298,142	298,142	298,142	298,142

Note: IV estimates of the impact of PACEA on the total number of days in employment during each year. Panels are defined as in Table A.6.7. Panel A adds the individual covariates linearly as the baseline specification in Table 2; Panel B includes only center × entry-quarter fixed effects; Panels C and D partial the covariates out by DML partially-linear IV with linear learners and gradient boosting, respectively, cross-fitting by caseworker. Standard errors clustered at the caseworker level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.7 Additional results on the impact of PACEA

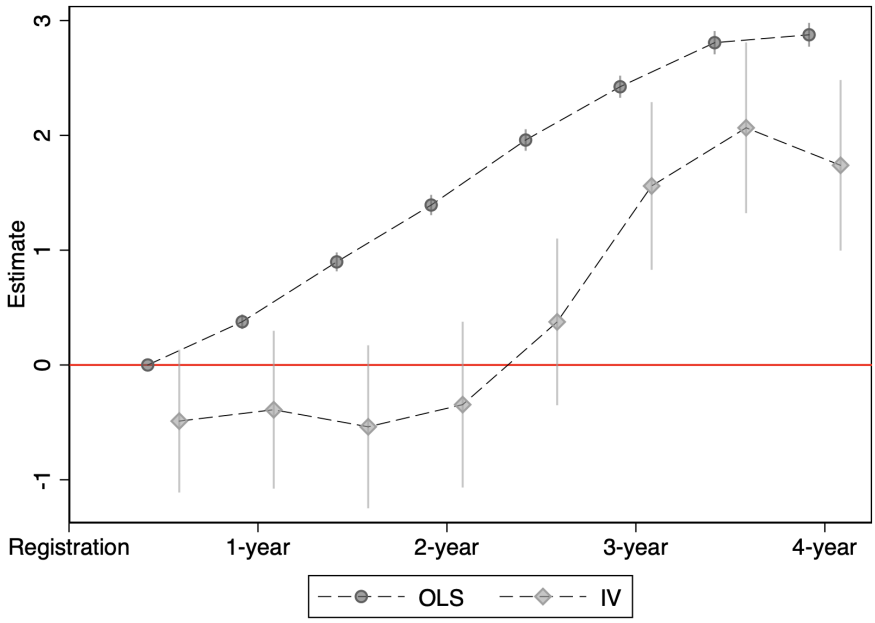
This appendix collects the additional results and the econometric tables referenced in Section 5. Figures A.7.1 and A.7.2 display the dynamic, monthly means at semester-level profiles of the effect of PACEA on the probability of employment and on employment intensity, complementing the year-by-year estimates reported in the text. The tables then report the underlying IV estimates for employment probability, the number of labor contracts and distinct employers, the probability of signing a permanent contract, cumulative earnings and wages, and the main occupations. Across all outcomes, the estimates confirm the two central findings of the paper: sizeable medium-run gains along the extensive and intensive margins, and the absence of effects on wages, contract stability, and occupational upgrading.

Figure A.7.1: Impact of PACEA on employment experience over time



Note: Employment experience is defined as having at least 1 day of employment within the semester.

Figure A.7.2: Impact of PACEA on monthly mean employment days over time



Note: Employment is defined as the mean number of monthly employment days within the semester.

Table A.7.1: Heterogeneous IV effects of PACEA on four-year employment

	(1) Dropout=0	(2) Dropout=1	(3) Rural=0	(4) Rural=1	(5) PEA=0	(6) PEA=1	(7) Female	(8) Male	(9) Youth<18	(10) Adult
PACEA (IV, 4-yr employment)	0.0468**	0.0399	0.0477**	0.0229	0.0462	0.0384*	0.0331	0.0519**	-0.0156	0.0542***
	(0.0219)	(0.0298)	(0.0205)	(0.0385)	(0.0312)	(0.0218)	(0.0250)	(0.0257)	(0.0465)	(0.0198)
Control mean	0.523	0.367	0.437	0.528	0.414	0.487	0.451	0.458	0.462	0.453
Observations	168098	130044	242940	55095	108245	189896	142866	155276	62221	235913

Note: IV-2SLS estimates of the effect of PACEA participation on the probability of employment four years after registration, estimated separately within each subgroup (columns). "Dropout" denotes youths who left school without a diploma; "Rural" denotes small-municipality (village) residence; "PEA" denotes prior registration with the public employment service; "Adult" denotes age 18 or older. Each regression uses the caseworker-propensity instrument, center × entry-quarter fixed effects, and the individual characteristics of Table 1. Control mean is the outcome mean among non-participants in the subgroup. Standard errors clustered at the caseworker level in parentheses. Clustered standard errors at the caseworker level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table A.7.2: Impact of PACEA on the number of signed labor contracts (conditional to employment)

	All	1st Year	2nd Year	3rd Year	4th Year
	(1)	(2)	(3)	(4)	(5)
PACEA	0.5004	-0.1555	0.7091**	0.1499	-0.1680
	(0.7528)	(0.3472)	(0.3359)	(0.3248)	(0.3274)
Observations	229,513	150,573	168,967	177,737	188,707
R-squared	0.0162	0.0246	0.0061	0.0033	0.0028
Outcome mean	11.6409	4.9153	4.395	4.0724	4.0573
Individual characteristics	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. The outcome in Column (1) is the total number of contracts signed by young individuals for those who had at least 1 employment spell in the labor market over a 4-year period after registration. Column (2) restricts the time window to the first year only after registration, Column (3) to the second year only, Column (4) to the third year only, and Column (5) to the fourth year only. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7.3: Impact of PACEA on the number of firms where young individuals worked (conditional to employment)

	All	1st Year	2nd Year	3rd Year	4th Year
	(1)	(2)	(3)	(4)	(5)
PACEA	0.1048	0.0126	-0.0393	-0.0021	0.0811
	(0.1258)	(0.0609)	(0.0572)	(0.0549)	(0.0618)
Observations	229,513	150,573	168,967	177,737	188,707
R-squared	0.0201	0.0361	0.0064	0.0031	0.0055
Outcome mean	3.8705	1.9359	1.8455	1.8282	1.8664
Individual characteristics	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. The outcome in Column (1) is the total number of firms where young individuals worked for those who had at least 1 employment spell in the labor market over a 4-year period after registration. Column (2) restricts the time window to the first year only after registration, Column (3) to the second year only, Column (4) to the third year only, and Column (5) to the fourth year only. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7.4: Impact of PACEA on signing a permanent contract (conditional to employment)

	All	1st Year	2nd Year	3rd Year	4th Year
	(1)	(2)	(3)	(4)	(5)
PACEA	-0.0213	-0.0629***	-0.0259	-0.0044	-0.0286
	(0.0214)	(0.0220)	(0.0218)	(0.0221)	(0.0219)
Observations	225,282	148,660	166,902	175,520	185,973
R-squared	0.0173	0.0132	0.0156	0.0135	0.0124
Outcome mean	.5302	.2957	.3754	.4268	.4727
Individual characteristics	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. The outcome in Column (1) a dummy equal to 1 if the individual signed at least one permanent contract over a 4-year period. Column (2) restricts the time window to the first year only after registration, Column (3) to the second year only, Column (4) to the third year only, and Column (5) to the fourth year only. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7.5: Impact of PACEA on wages using all labor market contracts (conditional to employment)

	Mean Wage (€)				Max Wage (€)			
	1st Year	2nd Year	3rd Year	4th Year	1st Year	2nd Year	3rd Year	4th Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PACEA	63.0301	-19.8900	82.9997	-9.7153	146.3086**	-4.6360	160.9230	112.5558
	(41.3375)	(40.4894)	(56.6828)	(47.8649)	(61.3001)	(58.3667)	(98.5516)	(137.4658)
Observations	146,792	162,682	168,565	177,975	146,792	162,682	168,565	177,975
R-squared	0.0058	0.0127	0.0072	0.0088	0.0203	0.0139	0.0014	0.0010
Outcome mean	956.26	1065.89	1144.77	1240.53	1446.38	1503.31	1572.31	1686.64
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. The outcome in Column (1) is the mean wage associated to employment experienced during the first year. Column (2) is the mean wage associated to employment experienced during the second year, Column (3) during the third year, and Column (4) during the fourth year. Column (5) to (8) shows the highest wage associated to a labor market contract over the different years respectively. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7.6: Impact of PACEA on wages using only permanent labor market contracts (conditional to employment)

	Mean Wage (€)				Max Wage (€)			
	1st Year	2nd Year	3rd Year	4th Year	1st Year	2nd Year	3rd Year	4th Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PACEA	90.7375	35.8059	61.8607	23.7362	115.3896	36.7490	27.9652	12.5942
	(71.4647)	(85.9239)	(70.5712)	(60.1676)	(80.6017)	(89.5330)	(74.9292)	(64.6495)
Observations	39,721	57,043	68,103	81,287	39,721	57,043	68,103	81,287
R-squared	0.0143	0.0061	0.0063	0.0080	0.0109	0.0054	0.0064	0.0071
Outcome mean	1304.9055	1376.4476	1437.5459	1538.5187	1380.1282	1449.0069	1509.6575	1615.355
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. The outcome in Column (1) is the mean wage associated to employment experienced during the first year. Column (2) is the mean wage associated to employment experienced during the second year, Column (3) during the third year, and Column (4) during the fourth year. Column (5) to (8) shows the highest wage associated to a labor market contract over the different years respectively. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7.7: Impact of PACEA on main occupations (conditional to employment)

	#1	#2	#3	#4	#5
	(1)	(2)	(3)	(4)	(5)
			Year 1		
PACEA	0.0123	0.0030	0.0242**	0.0079	-0.0107
	(0.0168)	(0.0158)	(0.0118)	(0.0119)	(0.0102)
Observations	148,660	148,660	148,660	148,660	148,660
R-squared	0.0485	0.0069	0.0062	0.0092	0.0017
Outcome mean	.1315	.1158	.0621	.0586	.0387
Occupation	676A	551A	676E	676C	674C
			Year 2		
PACEA	-0.0006	0.0122	0.0201**	0.0160	-0.0106
	(0.0144)	(0.0143)	(0.0101)	(0.0100)	(0.0087)
Observations	166,902	166,902	166,902	166,902	166,902
R-squared	0.0362	0.0040	0.0040	0.0053	0.0007
Outcome mean	.1017	.0987	.0498	.0445	.0318
Occupation	676A	551A	676E	676C	674C
			Year 3		
PACEA	-0.0070	-0.0088	0.0003	0.0025	0.0007
	(0.0140)	(0.0134)	(0.0094)	(0.0091)	(0.0073)
Observations	175,520	175,520	175,520	175,520	175,520
R-squared	0.0362	0.0032	0.0066	0.0062	0.0150
Outcome mean	.0922	.0941	.045	.0391	.0237
Occupation	676A	551A	676E	676C	525D
			Year 4		
PACEA	0.0141	-0.0150	-0.0048	0.0100	-0.0032
	(0.0132)	(0.0128)	(0.0091)	(0.0087)	(0.0072)
Observations	185,973	185,973	185,973	185,973	185,973
R-squared	0.0379	0.0039	0.0080	0.0077	0.0146
Outcome mean	.0838	.0893	.045	.0376	.0247
Occupation	676A	551A	676E	676C	525D
Individual characteristics	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter FE	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. Occupations are coded following the PCS ESE classification established by the French national institute of statistics (INSEE). The outcome in Column (1) a dummy equal to 1 if the individual signed at least one contract in the most represented occupation during a particular year after registration. Columns (2) to (5) are from the second most to the fifth most represented occupations during a particular year after registration. PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.8 Job search planing and methods during PACEA

This appendix documents how PACEA structures job-search trajectories, complementing the mechanisms discussed in Section 6.1. Table A.8.1 describes the sequential phases and objectives that participants move through during the program, from defining a project to securing employment. Table A.8.2 then shows that assignment to a pro-PACEA caseworker raises the likelihood that youths enter the key early phases and set objectives related to job orientation and mobility, indicating that caseworker heterogeneity translates into more structured and forward-looking job-search plans. Finally, Table A.8.3 reports the effect of PACEA on participation in intensive workshops and on the associated financial allowances.

Table A.8.1: Phases and objectives during PACEA

	%
	(1)
Phase 1 – Define project	**29.75**
Aim 1: Career orientation & information	42.46
Aim 2: Training & mobility	27.73
Aim 3: Skills recognition & job search	17.53
Phase 2 – Apply project	**33.62**
Aim 1: Skills recognition & job search	45.57
Aim 2: Training & mobility	32.05
Aim 3: Career orientation & information	10.59
Phase 3 – Secure project	**43.73**
Aim 1: Skills recognition & job search	39.53
Aim 2: Career orientation & information	26.35
Aim 3: Training & mobility	20.88
Phase 4 – Welfare-to-work program	**39.13**
Phase 5 – Other	**5.99**
Aim 1: Education pathway & citizenship	69.82
Aim 2: Training & mobility	9.34
Aim 3: Career orientation & information	8.69

Note: This table reports the main phases and three main objectives per phase defined during the PACEA program. Column (1) reports the share of young individuals who effectively reported such phases and objectives.

Table A.8.2: Effects of Caseworker Pro-PACEA on definition of job search plan and objectives within PACEA

	Phases			Objectives		
	Define Project	Apply Project	Secure Project	Job Orientation	Job Search	Mobility
	(1)	(2)	(3)	(4)	(5)	(6)
Cswk Pro-PACEA	0.0200***	0.0215***	0.0033	0.0119*	0.0083	0.0258***
	(0.0068)	(0.0072)	(0.0085)	(0.0067)	(0.0065)	(0.0069)
Observations	144,453	144,453	144,453	144,453	144,453	144,453
R-squared	0.0882	0.1192	0.1100	0.2183	0.1591	0.1859
Outcome Mean	.2975	.3362	.4373	.5237	.4248	.3923
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Center × Entry Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates obtained with OLS regressions. Cswk Pro-PACEA is defined in equation (20). Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8.3: Effects of PACEA on participation to intensive workshops and allowances

	GJ	Local	Allowances (€)
	(1)	(2)	(3)
PACEA	0.2530***	0.0986***	1,145.7838***
	(0.0186)	(0.0298)	(84.0404)
Observations	298,142	298,142	298,142
R-squared	0.2013	0.0277	0.1830
Outcome Mean	.00	.10	.09
Individual characteristics	Yes	Yes	Yes
Center × Entry Quarter Fixed Effects	Yes	Yes	Yes

Note: Estimates obtained with IV-2SLS regressions. GJ stands for *Garantie jeunes* in Column (1), Local for any local program that incorporates job search activities in column (2). PACEA is equal to one if youths enrolled in PACEA after registration, zero otherwise. Individual characteristics include all variables listed in Table 1. Robust standard errors clustered at the caseworker level below coefficients in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8.4: Mediation of the four-year employment effect: training vs. job search assistance

	Decomposition of PACEA → 4-yr employment
	(1)
Training channel	0.0052***
	(0.0002)
Job search assistance channel	0.0392***
	(0.0017)
N	298,142

Note: Product-of-coefficients decomposition of the effect of PACEA on four-year employment into the share operating through vocational training and an explained share that we refer to a direct component of the program. The two components sum to the total association. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.9 Treated shares in hires and employment

This appendix derives the treated shares among hires and among employed workers. The policy parameter π is the fraction of workers assigned to treatment upon entry into unemployment. The composition of hires and employment is endogenous.

A.9.1 Share of treated among hires

Under proportional random matching, each unit of effective search has the same probability of being matched with a vacancy. Hence, the probability that a vacancy meets a treated worker is proportional to treated workers' contribution to aggregate effective search, so that the treated share among hires is:

$$\phi \equiv \frac{e_1 u_1}{e_0 u_0 + e_1 u_1}. \tag{A.22}$$

Using (1)–(2), steady-state unemployment stocks satisfy:

$$e_0 u_0 = \frac{(1-\pi)\delta n}{p(\theta)}, \qquad e_1 u_1 = \frac{\pi \delta n}{p(\theta)}.$$

Substituting into (A.22) yields:

$$\phi = \frac{\pi \delta n / p(\theta)}{[(1-\pi)+\pi]\delta n / p(\theta)} = \pi. \tag{A.23}$$

A.9.2 Share of treated in employment

Let n_0 and n_1 denote non-treated and treated employment, with $n = n_0 + n_1$. Employment stocks evolve according to:

$$\dot{n}_0 = (1-\phi)\, m(eu, v) - \delta n_0, \qquad \dot{n}_1 = \phi\, m(eu, v) - \delta n_1.$$

In steady state, $\dot{n}_0 = \dot{n}_1 = 0$, hence:

$$n_0 = \frac{(1-\phi)\, m(eu, v)}{\delta}, \qquad n_1 = \frac{\phi\, m(eu, v)}{\delta}.$$

Therefore,

$$\frac{n_1}{n} = \phi = \pi, \tag{A.24}$$

so that:

$$n_0 = (1-\pi)n, \qquad n_1 = \pi n.$$

A.10 Equilibrium system: Jacobian and local uniqueness

This appendix reports the sign structure of the equilibrium system (F_1, F_2) defined by Equations (12)–(13) and used in the comparative statics of Section 2.2 and in the proofs below. Let $\mathcal{J}$ denote the Jacobian of (F_1, F_2) with respect to (n, θ). The relevant partial derivatives are

$$F_{1n} = 1, \qquad F_{1\theta} = -\frac{p'(\theta)\, A(\pi, e_1)}{\big(p(\theta) + A(\pi, e_1)\big)^2} < 0,$$

and

$$F_{2n} = \alpha(\alpha - 1)(1 + \pi\Delta)^{\alpha} n^{\alpha - 2} < 0, \qquad F_{2\theta} = \frac{c(r + \delta)q'(\theta)}{q(\theta)^2} < 0,$$

using $p'(\theta) > 0$, $A(\pi, e_1) > 0$, $\alpha \in (0, 1)$, and $q'(\theta) < 0$. The determinant of the Jacobian is

$$\det(\mathcal{J}) = F_{1n}F_{2\theta} - F_{1\theta}F_{2n} = F_{2\theta} - F_{1\theta}F_{2n} < 0.$$

Hence the equilibrium is locally unique, and by the implicit function theorem $(n^\star, \theta^\star)$ is locally differentiable with respect to any parameter entering F_1 or F_2.

A.11 Proof of Proposition 1

Proposition 1 (Search assistance and congestion). *Holding all other parameters fixed, an increase in search assistance e_1 raises equilibrium employment but lowers labor market tightness:*

$$\frac{\partial n^\star}{\partial e_1} > 0, \qquad \frac{\partial \theta^\star}{\partial e_1} < 0.$$

Proof. Search assistance affects equilibrium only through the Beveridge relation, so that $F_{2e_1} = 0$. From (12),

$$A(\pi, e_1) = \delta \left[\frac{1-\pi}{e_0} + \frac{\pi}{e_1} \right], \qquad \frac{\partial A(\pi, e_1)}{\partial e_1} = -\delta \frac{\pi}{e_1^2} < 0,$$

and therefore

$$F_{1e_1} = \frac{p(\theta)}{\big(p(\theta) + A(\pi, e_1)\big)^2} \frac{\partial A(\pi, e_1)}{\partial e_1} = -\frac{p(\theta)}{\big(p(\theta) + A(\pi, e_1)\big)^2} \, \delta \frac{\pi}{e_1^2} < 0.$$

Applying Cramer's rule to the equilibrium system (F_1, F_2) gives:

$$\frac{\partial n^\star}{\partial e_1} = -\frac{F_{1e_1} F_{2\theta} - F_{2e_1} F_{1\theta}}{\det(\mathcal{J})} = -\frac{F_{1e_1} F_{2\theta}}{\det(\mathcal{J})}, \qquad \frac{\partial \theta^\star}{\partial e_1} = -\frac{F_{1n} F_{2e_1} - F_{2n} F_{1e_1}}{\det(\mathcal{J})} = \frac{F_{2n} F_{1e_1}}{\det(\mathcal{J})}.$$

Since $F_{1e_1} < 0$, $F_{2\theta} < 0$, $F_{2n} < 0$ and $\det(\mathcal{J}) < 0$, it follows that $\partial n^\star / \partial e_1 > 0$ and $\partial \theta^\star / \partial e_1 < 0$. □

A.12 Proof of Proposition 2

Proposition 2 (Training and job creation). *Holding all other parameters fixed, an increase in training intensity Δ raises both equilibrium employment and labor market tightness:*

$$\frac{\partial n^\star}{\partial \Delta} > 0, \qquad \frac{\partial \theta^\star}{\partial \Delta} > 0.$$

Proof. Training affects equilibrium only through labor demand. In particular, it does not enter the Beveridge relation, so that $F_{1\Delta} = 0$, while it raises expected post-hire productivity and therefore shifts the job-creation condition outward.

Formally, differentiating the job-creation condition yields:

$$F_{2\Delta} = \alpha^2 \pi (1 + \pi\Delta)^{\alpha-1} n^{\alpha-1} > 0.$$

Applying Cramer's rule to the equilibrium system (F_1, F_2) gives:

$$\frac{\partial n^\star}{\partial \Delta} = -\frac{F_{1\Delta}F_{2\theta} - F_{2\Delta}F_{1\theta}}{\det(\mathcal{J})} = \frac{F_{2\Delta}F_{1\theta}}{\det(\mathcal{J})}, \qquad \frac{\partial \theta^\star}{\partial \Delta} = -\frac{F_{1n}F_{2\Delta} - F_{2n}F_{1\Delta}}{\det(\mathcal{J})} = -\frac{F_{2\Delta}}{\det(\mathcal{J})}.$$

Since $F_{1\theta} < 0$ and $\det(\mathcal{J}) < 0$ at the steady-state equilibrium, both derivatives are strictly positive. □

A.13 Proof of Proposition 3

Proposition 3 (Coverage, employment, and displacement). *Holding all other parameters fixed, an increase in program coverage π raises equilibrium employment, $dn^\star/d\pi > 0$, while its effect on labor market tightness is ambiguous, $d\theta^\star/d\pi \gtrless 0$.*

Proof. Program coverage shifts both equilibrium conditions. From (12) and (13),

$$F_{1\pi} = \frac{p(\theta)}{\big(p(\theta) + A(\pi, e_1)\big)^2}\,\delta\left(\frac{1}{e_1} - \frac{1}{e_0}\right) < 0 \quad \text{since } e_1 > e_0 > 0, \qquad F_{2\pi} = \alpha^2\Delta(1+\pi\Delta)^{\alpha-1}n^{\alpha-1} > 0.$$

Applying Cramer's rule to the equilibrium system (F_1, F_2) gives (14). For employment,

$$F_{1\pi} < 0, \qquad F_{2\theta} < 0, \qquad F_{2\pi} > 0, \qquad F_{1\theta} < 0 \quad \Longrightarrow \quad F_{1\pi}F_{2\theta} - F_{2\pi}F_{1\theta} > 0,$$

so that, since $\det(\mathcal{J}) < 0$,

$$\frac{dn^\star}{d\pi} = -\frac{F_{1\pi}F_{2\theta} - F_{2\pi}F_{1\theta}}{\det(\mathcal{J})} > 0.$$

For tightness, the numerator of $d\theta^\star/d\pi$ in (14) contains two terms of opposite sign: the job-creation effect $F_{2\pi} > 0$ and the congestion effect $-F_{2n}F_{1\pi} < 0$. Their balance is not signed by the model's restrictions alone, so $d\theta^\star/d\pi \gtrless 0$. □

Two remarks bear on the empirical analysis. First, since $F_{2\pi} = 0$ at $\Delta = 0$ and is increasing in Δ, the congestion effect necessarily dominates for training intensity low enough: $d\theta^\star/d\pi < 0$ whenever Δ is sufficiently small. Second, both terms in the numerator vary with Δ – F_{2n} through $(1+\pi\Delta)^\alpha$ and both through the equilibrium $(n^\star, \theta^\star)$ – so the intensity at which the balance tips is not a closed-form object and must be located numerically for given parameters.

A.14 Derivation of the micro-macro wedge

To isolate the search-assistance channel, hold π and Δ fixed so that a change in e_1 affects equilibrium only through the Beveridge relation. The steady-state equilibrium $(n^\star, \theta^\star)$ solves

$$F_1(n, \theta, e_1) = 0, \qquad F_2(n, \theta) = 0,$$

where the job-creation condition F_2 is independent of e_1. Applying Cramer's rule to this system yields

$$\left|\frac{\partial n^\star}{\partial e_1}\right| = \frac{|F_{1e_1}|\,|F_{2\theta}|}{|\det(\mathcal{J})|} = \frac{|F_{1e_1}|\,|F_{2\theta}|}{|F_{2\theta}| + |F_{1\theta}|\,|F_{2n}|}, \tag{A.25}$$

using $\det(\mathcal{J}) = F_{2\theta} - F_{1\theta}F_{2n}$ with $F_{2\theta} < 0$ and $F_{1\theta}F_{2n} > 0$. Expression (A.25) shows that the aggregate employment response to a given outward shift of the Beveridge relation is decreasing in $|F_{2n}|$, the steepness of the job-creation condition with respect to employment. In the model this slope is

$$|F_{2n}| = \alpha(1-\alpha)(1+\pi\Delta)^{\alpha} n^{\alpha-2},$$

which captures the curvature of labor demand implied by the production technology: a larger $|F_{2n}|$ corresponds to a faster decline in marginal productivity as employment expands. Hence, for a given outward shift of the Beveridge relation induced by search assistance, a steeper job-creation condition implies that a larger fraction of the equilibrium adjustment occurs through labor-market tightness rather than through aggregate employment.

A.15 Calibration and simulation of the model

This appendix details the calibrated exercise of Section 6.3. Table A.15.1 shows the values assigned to the parameters, either exogenously or from the model.

A.15.1 Calibration

Normalization. The job-creation condition (11) is homogeneous of degree one in $(\bar{y}, w, c)$. We therefore express the wage and the vacancy cost in units of baseline expected output $\bar{y}_b$, and tightness relative to its baseline value, $\Theta \equiv \theta/\theta_b$. This reduces the number of parameters to calibrate. With a matching function $m(eu, v) = \chi\,(eu)^{\eta} v^{1-\eta}$, we have $p(\theta) = \chi\theta^{1-\eta}$ and $q(\theta) = \chi\theta^{-\eta}$, and the equilibrium conditions (10)–(11) become

$$n = \frac{p_b\,\Theta^{1-\eta}}{p_b\,\Theta^{1-\eta} + \delta\left[\frac{1-\pi}{e_0} + \frac{\pi}{e_1}\right]}, \tag{A.26}$$

$$\left(\frac{1+\pi\Delta}{1+\pi_0\Delta_0}\right)^{\alpha}\left(\frac{n}{n_b}\right)^{\alpha-1} = \omega + (1-\omega)\,\Theta^{\eta}, \tag{A.27}$$

where $p_b \equiv p(\theta_b) = \chi\theta_b^{1-\eta}$ denotes the baseline job-finding rate per unit of effective search, so that $p(\theta) = p_b\,\Theta^{1-\eta}$ in (10).

To obtain (A.27), let $\bar{y}_b \equiv \alpha(1+\pi_0\Delta_0)^{\alpha} n_b^{\alpha-1}$ denote baseline expected output (eq. 6) and $K_b \equiv c(r+\delta)/q(\theta_b)$ the baseline vacancy-cost term. Dividing (11) by $\bar{y}_b$ gives the left-hand side of (A.27). Since $q(\theta) = q(\theta_b)\,\Theta^{-\eta}$, the right-hand side becomes $\omega + (K_b/\bar{y}_b)\,\Theta^{\eta}$, with $\omega \equiv w/\bar{y}_b$. Evaluating (11) at the baseline ($\Theta = 1$) gives $\bar{y}_b = w + K_b$, hence $K_b/\bar{y}_b = 1-\omega$. The model is thus fully described by the parameters $(e_0, e_1, \delta, \eta, \alpha)$, the observed policy (π_0, Δ_0), and two baseline equilibrium values, p_b and ω, which we calibrate in place of χ, c and w. For any policy (π, Δ), equilibrium employment n and relative tightness Θ solve (A.26)–(A.27), with $\Theta = 1$ and $n = n_b$ at the observed policy.

Search efficiency. Equations (1)–(2) and (A.24) imply that a worker of type i is employed with probability

$$\rho_i = \frac{e_i\,p(\theta)}{e_i\,p(\theta) + \delta}, \qquad \text{so that} \qquad \frac{\rho_i}{1-\rho_i} = \frac{e_i\,p(\theta)}{\delta}. \tag{A.28}$$

Since participants and non-participants search in the same market, they face the same $p(\theta)$, and the relative search efficiency is given by the odds ratio of employment,

$$\frac{e_1}{e_0} = \frac{\rho_1/(1-\rho_1)}{\rho_0/(1-\rho_0)}, \tag{A.29}$$

irrespective of δ and $p(\theta)$. For the same reason, the micro-econometric comparison remains informative about e_1/e_0 in the presence of equilibrium effects, as both groups face the same tightness within a center × entry-quarter cell. Four years after registration, the employment rate of non-participants is $\rho_0 = 0.455$ and the IV estimate of the effect of PACEA is 4.4 percentage points, which gives $\rho_1 = 0.499$ and $e_1/e_0 = 1.193$. We interpret this effect, measured after the end of the program, as the steady-state difference between the two types. With annual job-finding and separation rates above 0.5, employment

rates converge within a couple of years. With $e_0 = 1$, (A.28) gives $p_b = \delta\rho_0/(1-\rho_0)$. Setting $\delta = 0.67$, in line with the employment dynamics of the control group, yields $p_b = 0.559$.

Wage share and hiring costs. A vacancy costs c per unit of time and is filled at rate q, so that the expected cost of a hire is c/q. Free entry equates this cost to the value of a filled job, $(\bar{y}-w)/(r+\delta)$ (Section 2.1.4), which at the baseline gives

$$\frac{c}{q_b} = \frac{\bar{y}_b - w}{r+\delta}.$$

If a hire costs τ years of wages, $c/q_b = \tau\, w$, and substituting yields $\bar{y}_b = w\,[1 + (r+\delta)\,\tau]$, or

$$\omega = \frac{w}{\bar{y}_b} = \frac{1}{1+(r+\delta)\,\tau}. \tag{A.30}$$

Hiring costs are small in France, particularly for fixed-term contracts (Abowd and Kramarz, 2003), and we set τ to one month of wages, which gives $\omega = 0.943$. This parameter determines how the adjustment to a shift in labor supply is split between tightness and employment, job creation being more elastic when the firms' surplus $1-\omega$ is small. Results for alternative values of τ are reported in Section A.15.3.

Other parameters. We set $\eta = 0.7$ (Petrongolo and Pissarides, 2001), $r = 0.05$ and $\alpha = 0.7$, which implies a labor share $wn/Y = \alpha\omega = 0.66$, in line with standard values. The productivity effect of training, Δ_0, is not identified. In the model, wages are fixed and firms do not observe a worker's type before hiring, so neither wages nor individual employment effects are informative about it. Given the short duration of training spells, we set $\Delta_0 = 0.01$ as a conservative benchmark and report results for $\Delta_0 \in \{0, 0.005, 0.02\}$.

Consistency checks. The calibration targets ρ_0 and $\rho_1 - \rho_0$ (Table A.15.1). The implied aggregate employment rate is 0.476, against 0.48 in the data. This comparison is not an independent test, however. By (1)–(2) and (A.24), aggregate employment is the weighted harmonic mean of the type-specific rates, $1/n = (1-\pi)/\rho_0 + \pi/\rho_1$, and therefore follows almost mechanically from the targeted moments and observed coverage. The effect of coverage on non-participants is not targeted either. The model predicts $d\rho_0/d\pi = -0.018$, in the same units as the OLS estimates of Table 6. These estimates, which range from -0.012 to -0.030, are compatible with the model's prediction but too imprecise to validate it.

A.15.2 Comparative statics

Table A.15.2 reports the effect of a small increase in each program parameter on employment, in percentage points, and on tightness, in percent, at the calibrated equilibrium. The signs are those predicted by Propositions 1–3.

Search assistance. An increase in e_1 raises employment and lowers tightness (Proposition 1). Search assistance increases effective search but leaves firms' incentives to post vacancies unchanged, so that each vacancy faces more job seekers.

Table A.15.1: Calibration of the job-search and matching model

Parameter	Value	Source
Panel A. Assigned parameters		
Search efficiency of non-participants, e_0	1	Normalization
Program coverage, π_0	0.50	Data (mean share)
Separation rate, δ	0.67	Data (control group)
Matching elasticity, η	0.70	Petrongolo and Pissarides (2001)
Discount rate, r	0.05	Standard
Curvature of production, α	0.70	Labor share
Hiring cost, τ (years of wages)	1/12	Assumption, cf. Abowd and Kramarz (2003)
Training-induced productivity, Δ_0	0.01	Assumption (conservative)
Panel B. Implied parameters		
Relative search efficiency, e_1/e_0	1.193	Eq. (A.29)
Baseline job-finding rate, p_b	0.559	$\delta\rho_0/(1-\rho_0)$
Wage share of expected output, ω	0.943	Eq. (A.30)
	Model	Data
Panel C. Targeted moments		
Employment rate of non-participants, ρ_0	0.455	0.455
Employment gap, $\rho_1 - \rho_0$	0.044	0.044
Panel D. Consistency checks (not targeted)		
Aggregate employment rate, n	0.476	0.480
Spillover on non-participants, $d\rho_0/d\pi$	−0.018	−0.012 to −0.030

Note: Calibration of the model described in Section 2.

Training. An increase in Δ raises both employment and tightness (Proposition 2), as higher expected productivity leads firms to post more vacancies. The response of tightness is large because the firms' surplus, $1-\omega$, is small, so that a given productivity gain translates into a proportionally larger increase in profits per hire.

Coverage. An increase in π always raises employment, as more job seekers search with the higher efficiency e_1 (Proposition 3). Its effect on tightness depends on training intensity (bottom panel of Table A.15.2). With little or no training, congestion dominates and tightness falls, whereas above Δ_c the induced job creation is large enough for tightness to rise. At the calibrated value $\Delta_0 = 0.01$, PACEA lies in the first region.

Labor demand (Appendix A.14). The employment response to search assistance depends on how fast the marginal product of labor declines with employment, which is governed by α. The lower α, the larger the share of the adjustment that takes place through tightness rather than employment. In the calibrated model, the effect of a 0.1 increase in e_1 on employment rises from 0.37 to 0.68 percentage points as α increases from 0.6 to 0.9, while its effect on tightness falls in absolute value from −7.8% to −3.6%.

A.15.3 Coverage and displacement

Non-participants. Figure A.15.1(a) plots the employment rate of non-participants against coverage. At the calibrated training intensity, it declines from 0.463 without the program to 0.455 at

Table A.15.2: Comparative statics at the calibrated equilibrium

	Employment n (pp)		Tightness θ (%)	
	Model	Simulations	Model	Simulations
Search assistance, e_1 +0.10 (Prop. 1)	+	+0.44	−	−6.92
Training intensity, Δ +0.01 (Prop. 2)	+	+0.30	+	+4.01
Coverage, π +10 pp (Prop. 3)	+	+0.26	±	−2.39
Effect of a 10 pp increase in coverage for different training intensities				
$\Delta = 0$	+	+0.20	−	−3.24
$\Delta = \Delta_0 = 0.01$	+	+0.26	±	−2.39
$\Delta = \Delta_c = 0.04$	+	+0.44	0	0.00
$\Delta = 0.08$	+	+0.66	±	+2.94

Note: Change in the employment rate, in percentage points, and in tightness, in percent, following the indicated increase in each parameter (first-order approximation at $\pi = \pi_0$, all other parameters at their calibrated values). Model: signs implied by Propositions 1-3. Simulations: values in the calibrated model. Δ_c is the training intensity at which coverage leaves tightness unchanged (eq. A.31).

current coverage and 0.446 at full coverage. As more assisted job seekers compete for the same vacancies, tightness falls, by about 21% between $\pi = 0$ and $\pi = 1$, and non-participants find jobs more slowly. A one percentage point increase in coverage lowers their employment rate by 0.018 percentage points.

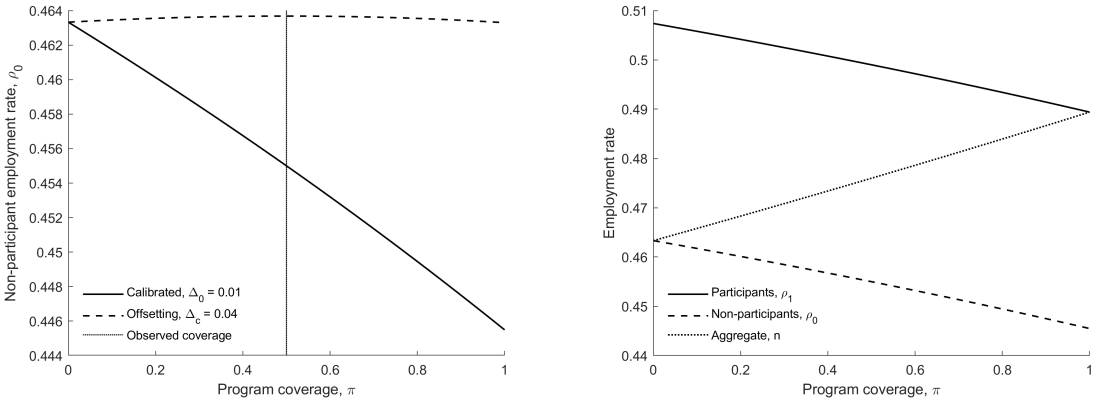
Participants. Congestion also lowers the employment rate of participants, from 0.507 when the program is very small to 0.489 at full coverage (Figure A.15.1(b)). Since both groups are affected, the gap $\rho_1 - \rho_0$ remains nearly constant, and the individual effect measured in the data does not capture displacement.

Individual effect and displacement. The individual effect of PACEA (4.4 points) is estimated by IV and used in the calibration, whereas the effect of coverage on non-participants (−0.018) is a model prediction that can be compared with Table 7. The share of the individual gain lost through displacement is a model-based decomposition rather than a causal estimate. Absent displacement, a coverage of 50% with an individual effect of 4.4 points would raise employment by 2.2 points. In the model, the program raises employment by 1.27 points relative to no program, so that about 42% of the individual gain is taken from non-participants. The 50% displacement assumed in the cost-benefit analysis of Section 7 is in this sense conservative and within the range of displacement estimations as shown in Table A.15.3.

Training threshold. Displacement disappears when a marginal increase in coverage leaves tightness unchanged, that is, when the job creation induced by training offsets congestion. From eq. (14), this condition can be written as

$$\frac{\alpha\,\Delta_c}{1+\pi_0\Delta_c} = (1-\alpha)\,(1-n)\,\frac{1/e_0 - 1/e_1}{(1-\pi_0)/e_0 + \pi_0/e_1}. \tag{A.31}$$

Figure A.15.1: Employment as program coverage expands



(a) Non-participants (b) Participants, non-participants and aggregate

Note: Steady-state employment rates as coverage π varies from 0 to 1, all other parameters at the calibration of Table A.15.1. Panel (a): non-participant employment rate at the calibrated training intensity ($\Delta_0 = 0.01$, solid) and at the intensity that offsets congestion at π_0 ($\Delta_c = 0.04$, dashed). The vertical line marks observed coverage. Panel (b): calibrated training intensity.

The left-hand side is the productivity gain associated with a marginal increase in coverage, and the right-hand side the decline in the marginal product of labor caused by the additional employment at constant tightness. We refer to Δ_c as the critical training intensity. In the calibrated model, $\Delta_c = 0.04$, so training would have to raise participants' productivity by about 4%, four times our benchmark, for coverage to stop crowding out non-participants. Given the short training spells documented in Section 6.2, PACEA is very likely below this threshold.

Robustness. Table A.15.3 reports the results when one parameter is changed at a time and the calibration is redone. Displacement is present in all cases, with a displaced share between 26% and 56%. Its size depends mainly on hiring costs, α, η and Δ_0. From (A.31), Δ_c depends only on α, e_1/e_0 and n. It ranges from 0.023 ($\alpha = 0.8$) to 0.062 ($\alpha = 0.6$) and remains well above Δ_0 in all cases.

Table A.15.3: Sensitivity of the displacement results

	Marginal effect on non-participants, $d\rho_0/d\pi$	Share of micro gain displaced	Training threshold Δ_c
Baseline	-0.018	42%	0.040
Hiring cost: 2 weeks	-0.023	53%	0.040
Hiring cost: 3 months	-0.010	26%	0.040
$\eta = 0.5$	-0.024	56%	0.039
$\alpha = 0.6$	-0.022	53%	0.062
$\alpha = 0.8$	-0.011	28%	0.023
$\Delta_0 = 0$ (no productivity effect)	-0.024	55%	0.039
$\Delta_0 = 0.005$	-0.021	49%	0.040
$\Delta_0 = 0.02$	-0.012	30%	0.040
$\delta = 0.5$	-0.020	47%	0.040

Note: One parameter changed at a time; the calibration is redone in each case, so that ρ_0 and $\rho_1 - \rho_0$ are always matched. Share of micro gain displaced: $1 - [n(\pi_0) - n(0)]/[\pi_0(\rho_1 - \rho_0)]$.

A.16 Budget constraint and the coverage-intensity trade-off

This section formalizes the trade-off between program coverage and training intensity under a fixed budget. We take search assistance (e_1, e_0) as given and study how a planner allocates resources between program coverage, π, and training intensity, Δ, in order to maximize aggregate employment.

A.16.1 Budget constraint

Let $B > 0$ denote the program budget per new unemployment entrant. Treating a worker with training intensity Δ costs $\kappa(\Delta)$, where:

$$\kappa(0) > 0, \qquad \kappa'(\Delta) > 0, \qquad \kappa''(\Delta) \geq 0.$$

The condition $\kappa(0) > 0$ states that assisting a worker costs something even absent training – the light-touch component of the program – while $\kappa' > 0$ prices intensity. The normalized budget constraint is:

$$\pi\, \kappa(\Delta) \leq B. \tag{A.32}$$

Under an interior allocation, $\pi\kappa(\Delta) = B$, so that program coverage can be written as

$$\pi(\Delta) = \frac{B}{\kappa(\Delta)}, \qquad \frac{d\pi}{d\Delta} = -\pi(\Delta)\frac{\kappa'(\Delta)}{\kappa(\Delta)} < 0. \tag{A.33}$$

Thus, at a fixed normalized budget, increasing training intensity mechanically requires reducing program coverage.

A.16.2 Planner's problem

Let $n^\star(\pi, \Delta)$ denote steady-state equilibrium employment generated by (10)–(11). The planner chooses (π, Δ) to maximize employment subject to the budget and feasibility constraints:

$$\max_{\pi,\Delta}\ n^\star(\pi, \Delta) \qquad \text{s.t.} \qquad \pi\kappa(\Delta) \leq B, \qquad 0 \leq \pi \leq 1, \quad \Delta \geq 0. \tag{A.34}$$

The Lagrangian is:

$$\mathcal{L} = n^\star(\pi, \Delta) + \lambda\big(B - \pi\kappa(\Delta)\big).$$

Because $n^\star$ is strictly increasing in both arguments (Propositions 2 and 3), the budget constraint binds at any optimum. At an interior local optimum, with $0 < \pi < 1$, $\Delta > 0$ and a binding budget constraint, the first-order conditions are:

$$\frac{\partial n^\star}{\partial \pi} = \lambda\, \kappa(\Delta), \tag{A.35}$$

$$\frac{\partial n^\star}{\partial \Delta} = \lambda\, \pi\, \kappa'(\Delta). \tag{A.36}$$

Dividing (A.36) by (A.35) yields:

$$\frac{\partial n^\star/\partial \Delta}{\partial n^\star/\partial \pi} = \pi\frac{\kappa'(\Delta)}{\kappa(\Delta)}. \tag{A.37}$$

Equation (A.37) states that, at the optimum, the marginal increase in employment obtained by raising training intensity rather than expanding program coverage exactly equals the amount of coverage that must be given up to finance that increase under the budget constraint.

A.16.3 Why intensity can dominate coverage

Along the binding budget constraint, an increase in training intensity requires a reduction in program coverage according to (A.33). The resulting employment effect is:

$$\left.\frac{dn^\star}{d\Delta}\right|_B = \frac{\partial n^\star}{\partial \Delta} - \frac{\partial n^\star}{\partial \pi}\pi\frac{\kappa'(\Delta)}{\kappa(\Delta)}. \tag{A.38}$$

Consequently, increasing training intensity while reducing program coverage raises equilibrium employment if and only if:

$$\frac{\partial n^\star}{\partial \Delta} > \frac{\partial n^\star}{\partial \pi}\pi\frac{\kappa'(\Delta)}{\kappa(\Delta)}. \tag{A.39}$$

Condition (A.39) is more likely to hold when the marginal employment effect of program coverage is small relative to that of training intensity. In particular, displacement forces may limit the employment gains from expanding coverage, because coverage increases effective search and congestion (Proposition 3), whereas training intensity directly shifts the job-creation condition by raising expected post-hire productivity (Proposition 2). Depending on the relative marginal effects and on the curvature of the cost function, it may therefore be more effective to train fewer workers more intensively than to treat many workers lightly.

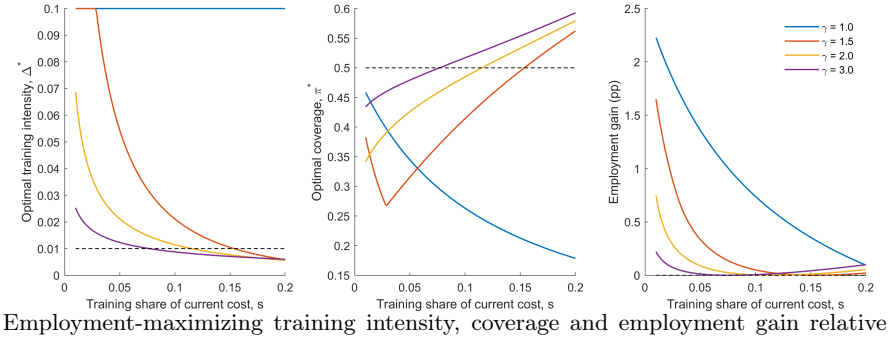
Note that (A.39) weighs the two marginal effects by the cost elasticity $\pi\kappa'(\Delta)/\kappa(\Delta)$, not by the marginal effects alone. A program whose intensity is very expensive relative to its light-touch component – a large $\kappa'(\Delta)/\kappa(\Delta)$ – can fail the condition even where displacement is severe and $\partial n^\star/\partial\pi$ is correspondingly weak. Whether the condition holds for PACEA is thus a quantitative question about the program's cost structure as much as about its displacement effects.

A.16.4 Quantitative implementation

Calibration and scope. We implement the planner's problem using the equilibrium conditions (10)-(11) and the calibration of Appendix A.15. The exercise is a calibrated sensitivity analysis: the empirical evidence disciplines the baseline, while the cost of changing productivity beyond that baseline is imposed parametrically. Search efficiencies and the remaining model parameters are held fixed across policy counterfactuals. In particular, changing training intensity does not change e_1 in this exercise.

Cost normalization and feasible policies. We specify $\kappa(\Delta; s, \gamma)$ as in equation (21). The fixed component, $1-s$, represents assistance costs that remain when the productivity-enhancing component is set to zero. Because $\kappa(\Delta_0; s, \gamma) = 1$, each specification reproduces the same normalized observed expenditure, $B = \pi_0\kappa(\Delta_0) = 0.50$. Multiplying both participant cost and the budget by a common monetary scale would leave the feasible policies unchanged. This normalization does not identify which observed expenditure categories produce productivity gains or establish their marginal causal cost.

Figure A.16.1: Optimal policy mix



Note: Employment-maximizing training intensity, coverage and employment gain relative to the observed policy, for $s \in [0.01, 0.20]$ and $\gamma \in \{1, 1.5, 2, 3\}$, with $\Delta \leq 0.10$. Dashed lines: observed policy.

We evaluate a continuum set of values of s on $[0.01, 0.20]$, separately for $\gamma \in \{1, 1.5, 2, 3\}$. The function is increasing and convex for $\Delta > 0$, with $\kappa(0) > 0$. For $\gamma > 1$, its right derivative at zero is zero; thus this specification weakens the strict condition $\kappa'(0) > 0$ at the endpoint while retaining positive marginal costs at every positive intensity. For $1 < \gamma < 2$, the second derivative is unbounded as intensity approaches zero; convexity is understood on the closed domain, and numerical optimization does not require evaluating that derivative at zero.

Since aggregate employment increases with coverage at fixed intensity, the largest feasible coverage is optimal conditional on Δ:

$$\pi(\Delta; s, \gamma) = \min\left\{\frac{B}{\kappa(\Delta; s, \gamma)}, 1\right\}. \tag{A.40}$$

For our parameter range, $\kappa(0) = 1 - s \geq 0.80 > B$. Consequently, coverage is strictly below one and the budget binds along the entire frontier. Universal assistance without training is infeasible. More generally, if $\kappa(\Delta) < B$, the coverage cap would bind and some budget would remain unspent at that intensity; a capped frontier need not exhaust the budget everywhere.

Equilibrium solution. For any feasible (π, Δ), define $H(\pi) = (1 - \pi)/e_0 + \pi/e_1$. We substitute the Beveridge relation into the job-creation condition and solve a scalar equation in $x = \log \theta$:

$$n(x, \pi) = \frac{A \exp[(1 - \eta)x]}{A \exp[(1 - \eta)x] + \delta H(\pi)}, \tag{A.41}$$

$$0 = F(x; \pi, \Delta) = \alpha(1 + \pi\Delta)^{\alpha} n(x, \pi)^{\alpha - 1} - w - \frac{c(r + \delta)}{A} \exp(\eta x). \tag{A.42}$$

With $0 < \alpha, \eta < 1$, employment increases with x, so the first term of F decreases with x while the vacancy-cost term increases. Moreover, F is positive as $x \to -\infty$ and negative as $x \to +\infty$. The equilibrium is therefore unique. Figure A.16.1 shows the evolution of optimal training intensity (Δ^*), coverage (π^*), and employment (n^*) with respect to the training current cost share (s) and cost technology (γ).



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ZEW – Leibniz-Zentrum für Europäische Wirtschaftsforschung GmbH Mannheim
ZEW – Leibniz Centre for European Economic Research

L 7,1 · 68161 Mannheim · Germany
Phone +49 621 1235-01
info@zew.de · zew.de

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