Are Active Labor Market Policies Directed at Firms Effective? Evidence from a Randomized Evaluation with Local Employment Agencies

Yann Algan — Sciences Po
Bruno Crépon — CREST
Dylan Glover* — INSEAD

January 19, 2020

Abstract

We evaluate the effect of an innovative active labor market policy (ALMP) implemented by the French Public Employment Service (PES) that targeted the vacancy costs of thousands of small and medium sized firms. We find that this policy increased labor demand among treatment firms on average: a 24% increase in vacancy postings with the PES and a 10% increase in permanent contract hires of registered jobseekers, a large proportion of which were still in employment after 12 months. The increase in firm labor demand is consistent with a drop in vacancy costs due to a shift in the pre-screening and filtering burden of the recruitment process away from the firm to the PES counselor. These results suggest that ALMPs directed at firm recruiting costs may be a valuable addition to the labor policy toolkit, yet theory and simulations illustrate that care must be taken when targeting future interventions of this type due to equilibrium effects.

Keywords: Labor demand, Active labor market policies, Unemployment, Vacancies

JEL Codes: J23, J63, J64, J68.

*We would like to thank Ghazala Azmat, Pierre Cahuc, François Fontaine, Xavier d'Haultefoeuille, Esther Duflo, Barbara Petrongolo, Alexandra Roulet and Philippe Zamora for their very helpful comments and suggestions and are grateful for feedback received during seminars and conferences at Barcelona GSE, CREST, CEPR/IZA in London, EALE Conference 2019, France Stratégie, INSEAD, Pôle Emploi, Sciences Po, Stockholm School of Economics, University of Bristol and the University of Gothenburg. We would also like to thank Agathe Pernoud for splendid research assistance, and Pôle Emploi, notably François Aventur, Yannick Galliot, Marie-Jose Rabner and Stéphane Ducatez at the DSEE for their input and guidance during the project and to Aude Busson and Catherine Touati at the DSE for their excellent collaboration during the implementation phase of the project. We gratefully acknowledge funding from the Chaire de Sécurisation des Parcours Professionnels. A previous version of this paper was circulated under the title, "The Value of a Vacancy: Evidence from a Randomized Evaluation with Local Employment Agencies." AEA RCT Registry number: AEARCTR-0005117

1 Introduction

Active labor market policies (ALMPs) have primarily focused on assisting jobseekers through job-search assistance or training programs. Numerous studies have shown that these programs can be effective at improving labor market outcomes for participants (Card et al., 2015). Yet their gains may be limited in equilibrium because they can induce substantial displacement effects between jobseekers, especially in weak labor markets (Crépon et al., 2013). In this paper, we explore the effectiveness and potential limitations of a symmetric intervention that assists firms in their recruitment operations in a slack labor market.

In the seminal Mortensen and Pissarides (1994) equilibrium job search and matching framework, the recruitment or "vacancy" cost is a key parameter that helps determine both labor demand and the unemployment rate. The model tells us that if these costs fall it will stimulate firm labor demand as the threshold for job creation is lowered. Yet we lack, perhaps surprisingly, evidence of the impact on labor demand of intermediation services that target vacancy costs.

These costs are related to collecting relevant information about the labor market in order to generate applicants, screen them, formalize the hire and integrate the new employee into the firm. Indeed, private labor market intermediation services, such as temp agencies, play a key role in the functioning of the labor market because they help provide this information (Autor, 2008). Public Employment Services also offer many different labor market intermediation services designed to address these costly search frictions. They exploit the information they have about jobseeker skills, firm needs and how they fit for different possible positions. Yet these services – and the studies that have evaluated their effectiveness – have focused on jobseekers.

We address this gap in the literature using a large-scale randomized experiment in which the French Public Employment Service (PES) prospected thousands of small and medium sized firms in order to offer them free recruitment services. In line with the theory, our results indicate that targeting recruitment costs can have strong positive effects on labor demand. We show that this shock led to a 24% increase in open-ended vacancy postings with the PES that translated into a 10% increase in permanent contract hires for registered jobseekers, on average over a six-month treatment period. In addition, these hiring effects are largely driven by the creation of stable employment (contracts for which jobseekers stay off the PES roster for at least 12 months).

We find that treatment firm vacancies are similar on a wide range of characteristics including the wage. The one exception is that treatment firm vacancies posted with the PES are more likely to require low skills, on average. This suggests that the marginal jobs created through the intervention are of lower productivity and profitability post-hiring, but this was offset by a reduction in the initial recruitment cost. Indeed, we find that vacancies posted by treatment firms were more likely to benefit from a robust set of pre-selection, or pre-screening, and filtering services that allowed PES counselors to reduce costs related to the initial screening phase of the hiring process. In line with the implementation of these types of services, we find that treatment firms receive significantly less applicants for final review, that they expend significantly less effort themselves in generating applicants and that their vacancies are significantly more likely to be filled by counselor-made matches over an 8-week period. We find little evidence that other parts of the recruitment process were affected by the intervention such as vacancy drafting and posting, interview support or post-hiring testing and adaptation services.

We provide a theoretical framework by extending insights from Michaillat (2012) and Crépon et al. (2013), through which we analyze our results in the presence of equilibrium effects. Our simulations highlight that labor market tightness and the scale of the intervention play key roles both in the interpretation of the experimental impacts and in any discussion of targeting and scale-up of similar interventions. First, we clarify the difference between the true causal effect of the intervention and what our experimental estimates capture: a comparison between treated and control firms' labor demand when aggregate labor market conditions have potentially changed *due to the intervention itself*. We highlight the fact that since the size of the experiment is just a small proportion of all recruiting firms in local labor markets, indirect effects on control firms should be negligible when measuring the impact on treated firms. We further argue that this measured effect is actually of great interest as it measures the shift in labor demand that occurs at constant labor market tightness.

Second, our framework allows us to examine the role of "firm-level displacement effects" that might mitigate aggregate employment effects and how they evolve as the intervention scales-up. We show that they are especially important to take into account when underlying market tightness is high but are less of an issue in slack labor markets, where firms have a plethora of candidates to choose from. Our simulations also show that displacement effects are not to be neglected even when the size of the intervention is small, but that they actually vary little with the size of the intervention. Hence, though our experiment was not designed to measure spillovers, our results are informative about the potential of firm-based ALMPs when economic conditions call for labor market intervention.

Finally, our simulations also show that the measured impact should be an increasing function of labor market tightness. The intuition behind this result is that firms have a harder and harder time finding applicants for their vacancies when the market is strong. Accordingly, we test whether hiring impacts are heterogeneous over local-level tightness and find that impacts are similar in both slack and tight markets. The lack of statistically significant heterogeneous impacts over tightness indicates that firms may incur significant costs trying to generate applicants in tight markets – where their is strong competition over candidates – as standard theory predicts, but also when there might be many candidates to evaluate, the case in slack markets. This suggests that the vacancy cost itself may be a function of labor market conditions.

While these empirical results are encouraging, they are accompanied by several additional caveats. First, we observe that the impact on employment creation in permanent contracts is stable, but find that the impact on vacancy creation with the PES is much more transitory. Second, we find small positive effects on fixed-term/temp vacancy postings with the PES, but see a reduction in the number of hires of registered jobseekers in these types of contracts. These results indicate that not all firms were fully confident in counselor applicant recommendations, deciding not to hire the recommended registered jobseeker and then further refrained from recruiting through the PES, post-intervention. Indeed, a prerequisite for the effectiveness of pre-screening services is that the PES has the relevant information about firm needs and how jobseekers can fullfill them. Consistent with this hypothesis, we find substantial heterogeneity in estimated treatment effects on hiring over the pre-existing relationship between the PES and firms.

This paper is primarily linked to the literature on the effectiveness of active labor market policies, but contributes to several strands in the labor literature. Indeed, job search assistance policies have been evaluated on many occasions and have most often been found to be effective and profitable given their low cost (Card et al., 2015). The field has also tried to better understand and improve the aim and intensity of the job search process using lessons from behavioral economics (see Babcock et al. (2012); Altmann et al. (2018); Abel et al. (2018)) or by providing advice to jobseekers about alternative occupations (Belot et al., 2018). However, these policies can be particularly ineffective in weak labor markets because displacement effects between jobseekers can substantialy weaken the aggregate employment effect (Crépon et al., 2013). We contribute by flipping an ALMP's focus to firms in a market characterized by a large surplus of jobseekers to study changes in actual job creation.

Our study is thus further related to the literature focusing on hiring processes, especially when there is heterogeneity in applicants and vacancies. On the one hand, firms may develop strategies to limit the risk of failing to attract relevant profiles in the applicant pool.[1] But on the other, they might develop strategies aimed at limiting the risk to attract irrelevant profiles.[2] These strategies relate to information about the quality of applicants and how

[1]This includes offering higher wages (Dal Bó et al., 2013; Ashraf et al., 2018; Deserranno, 2019), or subsidizing applicant job search in order to improve the quality of the applicant pool (Abebe et al., 2019).

[2]This includes self-selection mechanisms (Hardy et al., 2016) or screening (Autor and Houseman, 2010),

costly it is is to acquire this information.[3] Our study focuses on public sector intermediation as another channel through which labor market actors can aquire this information. As Autor (2008) notes, information provided to the market about vacancies and jobseekers can be thought of as a public good and intermediation exists because it can supply the relevant information at a lower cost than firms (or jobeekers) can obtain from their own effort. For firms, they reduce the fixed costs that are sunk when identifying and screening workers before the hire. This paper provides the first experimental evidence on how labor market intermediation impacts this fixed cost, and, in turn, job creation.

Because PES counselors can provide valuable information to firms about an applicant's productivity our paper is also linked to the literature studying the stigma associated with unemployment status. For example, Altmann et al. (2018) inform jobseekers about the difficulty of convincing a company of their skills as the duration of their unemployment spell lengthens and several audit studies have been conducted in recent years which vary the unemployment history of fictitous applicants (Oberholzer-Gee, 2008; Kroft et al., 2013; Eriksson and Rooth, 2014). Consistent with the growing evidence that firms are strongly receptive to productivity signals when hiring,[4] these studies have shown that unemployment history can convey a negative productivity signal to employers.[5] Our study shows that screening services offered by the PES are services that firms demand in part because they may alleviate the uncertainty about hiring the unemployed.

Finally, our study is also related to the growing literature on field experiments with firms (Bandiera et al., 2011; Quinn and Woodruff, 2019). These studies have mainly been conducted in developing countries and focus on topics such as access to funding or business training. More recently some studies have also focused on firm labor demand and the frictions that firms face in these developing economies.[6] To our knowledge, this study is the first

or also the referral process (Burks et al., 2015; Pallais and Sands, 2016) as well as job testing (Autor and Scarborough, 2008; Alonso, 2018; Bassi et al., 2017)

[3]See Abebe et al. (2018a) for an example in Ethiopia in which both jobseekers and firms hold inaccurate beliefs about what to expect from the labor market.

[4]Several recent papers have shown the positive impacts on labor market prospects of improved ability for jobseekers to signal their skills. This starts with the simple fact of having being hired in the past Pallais (2014), tests scores (Carranza et al., 2019), having a reference letter (Abel et al., Forthcoming) or simply help in writing resumes (Abebe et al., 2018b)). While this improvement can come from the intensity or direction of search efforts, the audit study in Carranza et al. (2019) or (Cahuc et al., 2019) highlights that it might also meet a demand for accurate productivity signals from employers.

[5]They show that being currently unemployed for more than 8-9 months reduces the chances to pass the first step of the hiring process. Specifically, Kroft et al. (2013) further show that this is consistent with firms using the long unemployment spell in their screening process and correlate it with a negative signal on productivity.

[6]This includes (de Mel et al., 2019) and Alfonsi et al. (2017) who study the long term effect of wage subsidies, as well as Hardy et al. (2016) and Crépon and Premand (2019) which look at the hiring process of apprentices. It also relates to studies focusing on the matching process (Bassi et al., 2017; Abebe et al., 2018a)

randomized control trial addressing labor market frictions faced by firms in a developed country.

The remaining structure of the paper is as follows. Section 2 describes the context, intervention and experimental protocol. Section 3 provides the theoretical framework through which we analyze the experimental results. Section 4 describes treatment effects on vacancies and employment creation. Section 5 presents evidence on potential mechanisms driving the main effects. Section 6 discusses concerns about equilibrium effects, targeting and scale-up. Section 7 concludes while the appendices provide supporting information and results.

2 Context and experimental design

Having devoted resources almost exclusively to assisting jobseekers since 2008, the PES, known as *Pôle Emploi*, revamped their firm services program in 2015.[7] The objective was to move towards a more balanced approach between aiding jobseekers and firms. The "new firm services offer" (*nouvelle offre de services aux entreprises*) was designed to provide free, comprehensive support to firms for their recruitment needs. The service was based on two complementary components: (1) prospection and (2) recruitment support (*accompagnement au recrutement*). In collaboration with the PES, we integrated the evaluation within a six and half month pilot phase of the prospection and firm services offer campaign. 7,438 unique firms participated in the experiment of which half were randomized into treatment. Counselors in the 129 participating local employment agencies intensively prospected treatment firms for 3 months starting on September 15th, 2014 while the control group was "sanctuarized" until March 31, 2015: No proactive action was to be taken towards these firms. Importantly, control firms were not refused services if they requested it. Counselors were required to have an "in-depth interview" with treatment firms during this intense period during which counselors were required to market the recruitment services and make a concerted effort to encourage firms to create vacancies and gauge the firm's future recruitment needs. Following the intensive prospection period, counselors were instructed to continue to nurture relations with treatment firms. After this six and half month treatment, or "sanctuary" period, agencies were free to contact and propose services to the control group.

and Bertrand and Crépon (2019) who examine the frictions associated to employers' imperfect knowledge of labor laws.

[7]Pôle Emploi has over a thousand local agencies throughout mainland France and its overseas territories. In 2008, it was created as the result of a merger between the ANPE (*Agence nationale pour l'emploi*), the government agency concerned with job counseling and recruitment services, and l'Assedic (*Association pour l'emploi dans l'industrie et le commerce*) the agency that dealt with the distribution and oversight of unemployment insurance benefits.

2.1 Sampling and Randomization

The research team collaborated with the Firm Services Department (FSD) at the PES in the targeting phase of the the intervention. First the FSD chose 129 local employment agencies to participate in the study. These local employment agencies were chosen so that all 22 mainland French regions were represented in the sample.[8] We then developed a targeting algorithm to sample firms that were administratively attached to these agencies so that the intervention could affect low-tightness and low job-finding rate professions.[9] Targeting professions in depressed labor markets was made simple by the fact that the study was conducted at a time when the French labor market was at its weakest point following the "Great Recession." Figure A.1 in the appendix provides the overall context of the evaluation. It shows aggregate, detrended quarterly tightness from 2007-2017 using data from the French Employment Ministry. The highlighted grey area represents the 6-month sanctuary period of our study. We see that the intensive treatment and sanctuary phases took place when aggregate tightness was roughly 0.42. This sampling strategy also ensured that the proportion of treatment firms among all recruiting firms in a local market remained small.[10] We created a priority ranking of professions per agency based on local-level tightness and job finding probabilities weighted by the stock of jobseekers registered in the agency. Using a profession-sector correspondence table, we then merged these "priority professions" to sectors. This gave us a ranked list of sectors at the agency level in which firms were most likely to recruit within the prioritized professions.

These sector identifiers were then linked to local firms that had between 5-250 employees and had responded to the PES' annual survey *Besoin en Main d'Oeuvre* (BMO) or "Labor Needs" survey.[11] We sampled in BMO 2014, a survey conducted in autumn 2013 on recruitment needs for 2014. Each agency was then given an oversampled list (roughly double) of "priority firms" to potentially prospect drawn out of the BMO survey (those that were at the top of the sector rankings). Counselors then selected roughly half of the firms in these lists using their own local expertise. The final agency-validated list was then sent back to the research team for randomization.

The sample was stratified by indicators for the agency to which the firm was administratively attached, if the firm had intended to recruit in 2014 and by the number of employees

[8]French overseas regions and territories were not included in the experiment.

[9]For example, it was important for the PES that any publicity for the firm-based services made the distinction that they were provided to help jobseekers get back to work and not simply to help firms recruit.

[10]Figure A.2 shows the proportion of treated firms among all recruiting firms within a micro-market (commute-zone$\times$sector).

[11]Roughly 400,000 firms receive the BMO survey in France each year in order to gauge their recruitment needs for the following year. The results are entered into an online platform used by the agencies to follow-up on potential hiring declared in the survey.

on the firm's payroll (in four categories). Within each stratum we randomly assigned treatment with probability 0.5. For strata with odd numbers of firms we re-randomized the last firm within the stratum with probability 0.5 and did the same for single-firm strata.[12] A total of 8,232 establishments were randomized, yet we found *ex post* that some of these establishments were simply branches of the same firm. We thus exclude firms that had multiple branches included in the experiment due to the possibility of within-company spillover effects that would dilute treatment effects. Our final sample comprises 7,438 unique firms. Our empirical specification follows the experimental design.

2.2 Empirical specification

Our baseline specification corresponds to a least squares regression that includes a set of dummy variables to account for the stratification:

$$y_i = a + \beta T_i + \sum_{s=1}^{S} \gamma_s 1_{s,i} + u_i \tag{1}$$

We cluster standard errors at the local employment agency level.

We will also analyse the robustness of the average treatment effect along several dimensions. Following Young (2018), we implement randomization inference for the usual student test, using 10,000 permutations tests. This allows to obtain a consistent estimate of the exact p-value of our test. We also implement ranksum tests (Mann and Whitney, 1947) which have the advantage of being robust to outliers and, here also, compute their p-values using randomized inference. We will also estimate a specification that includes baseline covariates. To select covariates, we implement the *double post lasso* developed in Belloni et al. (2014) in order to avoid the risk of specification search. Finally, we compute standard errors without clustering at the employment agency level.[13]

2.3 Data

We have access to rich historical administrative data from the PES from January 2014 through January 2016. This includes vacancies posted with the PES, the applications made

[12]For the analysis, small sized strata are reabsorbed into the closest stratum based on size, local agency and 2014 recruitment in order to have a minimum of two control and two treatment firms per stratum.

[13]As described in section 2.1, the sample was selected so that the 129 agencies represented all mainland regions in France and firms sampled in BMO are administratively attached to a specific agency. Following (Abadie et al., 2017), these two features of the sampling procedure advocate for using clustered standard error. However, if we consider the study sample as given and consider design based variability of our estimates, there is room also to consider non clustered standard errors, as randomization was performed individually.

to them and data on hires.

Vacancy data from the PES include their characteristics and the specific recruitment services applied to them as well as the posting date and type of contract: permanent (open-ended) or fixed-term which includes interim or temp contracts. The characteristics include the minimum annual wage, the profession, the required qualification, the minimum required experience, duration (for fixed-term contracts) and the weekly working hours. It is important to note a limitation of the vacancy data. We do not have an exhaustive measure of vacancy creation because the PES is only one, though by far the largest, outlet for vacancies on the French job market.

Importantly for the mechanisms analysis, we also have the applications, or, potential matches made through the PES to these vacancies through three different channels: applications made by jobseekers and potential matches initiated by the firm and by the PES caseworker. These data are novel in that they provide a measure of search effort put forth by each of the three actors. Jobseekers are able to apply directly to vacancies posted with the PES and we can link this application to the vacancy characteristics. Symmetrically, firms can go into the PES CV bank and solicit an application from a jobseeker to apply to their vacancy. Counselors search for vacancies for their jobseekers, they then verify the interest of the jobseeker and either apply on their behalf or generate an official request whereby the jobseeker is compelled to apply directly to the job.

We have an exhaustive measure of hiring flows through legally required hiring declarations called DPAE, *Déclaration préalable à l'embauche*. This is the data we use to build our main outcome variables to measure changes in firm labor demand: a simple count of hires and the number of "theoretical" workdays created by and across contract types. All firms are required to submit a hiring declaration before, or shortly after the contract start date.[14] Interim, or, temp-work contracts also require a declaration, but this is done by the temp agency. Thus, we exploit a separate data set created by the PES that documents the final employer ("using employer") of the temp contract and append this to our data set of permanent and fixed-term contracts.

The hiring declarations data provide precise information about the contract type, its start and end dates (for fixed-term and temp-contracts) and whether the person hired was a registered jobseeker with the PES in the 30 days preceding the hiring date. Using the start- and end-dates for fixed-term and temp-work contracts that ended during the observation period (September 2014-January 2016) we calculate the number of workdays created within

[14]Exceptions to the requirement for the hiring declaration concern internships and volunteer contracts and for the recruitment of private child care professionals and some public sector jobs. Firms that that were sampled and eventually randomized were unlikely to make hires that do not require a declaration.

each contract signed in month t. For example, a contract with start date January 15th, 2015 and end date June 30th, 2015 would be counted as five and half months of theoretical workdays created in January 2015. For permanent contracts and fixed-term/temp-work contracts that ended after the observation period, we censor the end-date at January 31st, 2016. We do this because these declarations are contract flows and for a large proportion of them, we have no personal identifiers due to the individual privacy constraints faced by the PES. Personal identifiers are only available for individuals who were registered with the PES in the 3 years preceding the date of hire. This "theoretical number of workdays" allows us to have a standardized measure of employment creation. For example, a week of one-day (Monday to Saturday) hires for the same individual would be counted as 6 fixed-term contract flows, but as only one contract if it were a fixed-term contract that ran for the week. Thus, calculating workdays allows us to compare the overall theoretical employment creation within and across contract types.

We also use the DPAE to measure the quality of the match. Indeed, even a permanent contract can be terminated rapidly if there is a poor match.[15] To account for this, we calculate the number of hires in permanent contracts and over all contract types that results in a new hire staying off the PES register for at least 12 months. We can do this because we observe when a registered jobseeker is hired and when they eventually fall back into unemployment (if at all).

Finally, we top-code all administrative data at the 99th percentile of their distribution in order to make sure our linear estimates are not driven by a few, very large recruiting firms.

2.4 Sample description and compliance

Table 1 shows the distribution statistics and balance checks on important firm characteristics for which we have data. Each row presents the strata-weighted control group mean and the treatment group difference estimated using equation (1). All dependent variables are indicators. Firm characteristics are collected from the BMO survey. For hires, vacancy postings, contacts and use of PES services, we sum the variables from January 2014 to August 2014 (our available pre-treatment period) and create an indicator for the sum being larger than zero.

Examining the baseline characteristics of firms, we see that 73% of firms have less than 26 full time employees and that they are predominantly in the service (42%) and commerce sectors (25%) while manufacturing and construction make up 28% of the sample. 50% of firms hired someone in a short-term contract (1 day to 6 months in duration) and 44%

[15] Actually this can be done quite easily in France during the probation period, see Cahuc et al. (2016).

hired at least one employee in a permanent contract during this pre-treatment period. Yet, relatively few firms posted vacancies with the PES compared to the proportion that hire. For example, roughly 9% of firms posted a permanent contract vacancy with the PES over the eight month pre-intervention period for which we have data, while roughly 20% posted a fixed-term and/or a temp job with the PES. In contrast, we see that a significant proportion of firms (36%) completed at least one phone call with a PES counselor before the intervention and that a portion had received potential match proposals for their vacancies through the PES directly from jobseekers (15.4%) and/or counselors (18.3%).

Across the board we see treatment coefficients close to zero and statistically insignificant coefficients for all but two of the 25 regressions, indicating that the stratified randomization was successful. The two significant coefficients from the balance check indicate that treatment group firms are about 1.2 percentage points less likely to post a permanent contract vacancy with the PES during the pre-period and 1.2 percentage points more likely to have received a visit from the PES, on average. We'll see that this is a relatively small difference compared to impacts, but we'll also show that results are robust to controlling for pertinent baseline covariates.

We now turn to empirically examining the intensity of treatment. Figure 1 plots the monthly cumulative evolution for counselor visits to firms, meetings with the firm at the agency, and phone calls, from January 2014 through January 2016 using unconditional binned firm averages. The shaded region denotes the intense treatment period in which all treatment firms were expected to undergo an in-depth interview with a PES counselor and marketed the intensive recruitment services. We see an upward linear evolution in all forms of contact and a sharp discontinuity for the treatment group at the beginning of the intensive phase. The figures show a jump of about half a visit per firm on average and an increase of about one and a half more telephone calls made to the treatment group, representing 488% and 152% increases off of the control mean at the end of the intensive period.

A subservice that the PES also elaborated was the promotion, by counselors, of spontaneous candidatures. These spontaneous candidate promotions are defined as a counselor presenting a résumé to an employer in absence of a declared hiring need or vacancy. We consider this a form of compliance that demonstrates the implication of the counselors by showing that they studied firm needs and provide profiles that might interest the employer. We see in the last graphic in Figure 1 that treatment firms received close to one additional spontaneous candidature, on average, emanating from caseworkers, compared to the control group which received almost none during the initial months of the treatment.

It is important to note the uninterrupted linear trajectories of the control group. Control firms were free to contact the PES and request recruitment services and accordingly we do not observe a sudden change in the evolution of control group trends: They do not suddenly go flat starting in September 2014. Thus the counterfactual outcome represents simply what would have happened in absence of the prospection campaign, not what happens when firms are severed from PES services. Importantly, we also note that contacts do not substantially change on average after the sanctuary period end date, March 31, 2015. One could imagine that when counselors were permitted to proactively encourage the control group firms to take advantage of PES services, we might see a jump in the contact and service levels of control group firms after this date. This suggests that the intervention can be thought of as a temporary intensification of contacts between firms and counselors during which counselors learned about firms' recruitment needs while exposing them to the new services that they could use in their recruitment operations.

2.5 The services that were marketed

We now turn to what the PES really offered the firms. During the intensive and sanctuary phases, a strong emphasis was put on collecting "useful vacancies" (*offres utiles*) that would benefit registered jobseekers by getting them back into stable employment. This entailed a special focus on encouraging the creation and posting of vacancies corresponding to jobs in permanent, or open-ended contracts (*contrats de durée indeterminée, CDI*) with the PES. A permanent contract, for which over 80% of registered jobseekers are searching, offers a variety of advantages to the employee in France. Apart from providing stable employment, having a permanent contract is key to be able to access the French housing and credit markets. Yet permanent contracts are stable because it can be very costly for firms to unilaterally break the contract. It follows that the PES needed to elaborate a robust set of free services to encourage firms to hire registered jobseekers in permanent contracts.[16]

The basic service, *appui au recrutement*, or "hiring support," included,

1. Vacancy drafting support - French law requires that vacancies do not discriminate on the basis of demographic characteristics such as gender or race and that they contain adequate job descriptions: a baseline salary offer, skill requirements and working times and hours.

2. Vacancy posting - Free public vacancy posting online and on agency job boards

[16]The recent trend in the French labor market (see DARES (2017, 2018)) is that firms are using more and more fixed-term contracts because hiring in a permanent contract requires costly screening. In a study conducted by the French Ministry of Labor, 65% of firms declare that they use fixed-term contracts to provide an initial test of the employee's performance.

3. Access to the PES CV bank which allows employers to directly search for candidates and solicit them to apply to their vacancy

4. Information on local labor market characteristics and on how the vacancy is performing

These services existed in various forms before the intervention, but were renamed and highlighted for the counselors. In addition, counselors now had the ability to officially tag which service they applied to the vacancy in the computer system.[17] The real innovation that was marketed to firms during the prospection campaign came in the form of the "reinforced recruitment services" (*accompagnement au recrutement*). When a vacancy was selected into this new category it could then benefit from the following intensive services:

5. Pre-selection, or pre-screening services - Counselors became responsible for generating a restricted list of candidates that would be then sent to the firm for review.

6. Jobseeker emphasis - This included "Valorization" in which counselors put special effort into highlighting specific jobseeker attributes that might be unobservable to, or overlooked by the employer. This could also include an "evaluation" service whereby the candidate is evaluated through a simulation or immersion in the firm before the decision to hire is made.

7. Interview support - If the firm needed support in running interviews after candidate selection the counselors could provide this.

8. Post-hiring and adaptation services - This could take the form of subsidized pre- or on-the-job training. In addition, the counselors could market special wage subsidized contracts available for the recruitment of certain types of jobseekers.[18]

Being tagged for the reinforced recruitment service also ensured that a specific counselor was assigned to the vacancy and recruitment process. The firm thus benefited from a privileged contact throughout the recruitment process who was responsible for a successful candidate search.

Among these reinforced services, pre-selection was considered a key and novel component. It comprised two additional sub-components:

1. **Pre-selection on prerequisites** (*critère*) through which the firm and the counselor jointly define 1-5 prerequisites on which to pre-screen candidates for the firm.

[17] Indeed, some of the counselors who were designated to participate had little previous experience providing services directly to firms due to the strong focus on jobseekers.

[18] Known as *contrat aidé* these types of contracts are aimed at stimulating hiring of jobseekers at risk of long-term unemployment.

2. **Verification** (*vérification du profil*) which entails that a restricted number of applicants get through to the employer (a maximum of 5 to 10 per post) and that they exactly match the vacancy requirements. This service also encouraged the counselor to restrict access to the vacancy through additional *filtering* mechanisms.

With "verification" the PES counselor could recommend that firms choose to have applicants apply only through the counselor by making the vacancy private (not publicly available online). Or, if publicly posted, jobseekers could only see the brand or chain name and thus cannot contact the recruiter directly, thus shifting the pre-screening responsibility entirely to the counselor. In addition, verification required counselors to negotiate a time frame with the firm for the delivery of the applicant list and ways in which to adapt the vacancy if it was generating an insufficient number of applicants.

Hence the package of services marketed during the intervention was primarily directed at reducing the vacancy costs faced by firms in their recruitment operations. We now elaborate a conceptual framework to show how labor demand shifts in response to a reduction to these recruiting costs and how this shift affects equilibrium employment in the market.

3 Conceptual framework

This section will help us to conceptualize the potential congestion externalities present between firms when a proportion receive recruitment services and will guide our interpretation of the ensuing empirical results. It builds on Michaillat (2012), using the set of parameter values used in his simulations (Table A.1), and extends the framework used in Crépon et al. (2013) to analyze displacement effects between firms. The key features of the model include both decreasing returns to scale in the production function and sticky wages in the canonical job search and matching model (Pissarides, 2000).[19] We start with a "Beveridge" curve describing how the employment level n is linked to flows in and out of unemployment in steady state. This curve depends on the tightness of the labor market $\theta = v/u$, where v is the stock of available vacancies and u the level of unemployment:

$$n_B(\theta) = \frac{1}{(1-s) + s/f(\theta)} \tag{2}$$

In this equation $n_B(\theta)$ is the level of employment when tightness is θ. s is an exogenous separation rate and $f(\theta)$ is the job finding probability of unemployed individuals. f is derived from a homogeneous matching function $f(\theta) = M(u,v)/u = \theta M(\theta, 1) = \theta q(\theta)$. The

[19]We refer the reader to Michaillat (2012), Sections IA-IIIB, for the derivation and extended discussion of the basic assumptions behind the equations.

other piece of the model is a "labor demand" curve that links the marginal productivity of workers, $a\alpha n^{\alpha-1}$ to the wage, $a^{\gamma}\omega$, and the cost of search $a\frac{c}{q(\theta)}(1-(1-s)\delta)$. a is the productivity level used to describe the strength of the economy and $\alpha \in (0,1)$ is the elasticity of output to employment in a Cobb-Douglas production function. The wage partially adjusts to productivity shocks through $\gamma \in [0,1)$. c is the instantaneous vacancy cost which has to be paid over the search period $1/q(\theta)$ and δ is the discount rate. In a static environment the firm's optimality condition is,

$$\alpha D(0,\theta)^{\alpha-1} = a^{\gamma-1}\omega + \frac{c_0}{q(\theta)}(1-(1-s)\delta) \tag{3}$$

In this equation $D(0,\theta)$ and c_0 are the demand for labor, as a function of θ, and the instantaneous recruitment cost *absent the intervention*, respectively. Thus firms optimally equate the marginal product of labor to the wage *and* the cost of search: $\frac{c_0}{q(\theta)}(1-(1-s)\delta)$, which is simply the recruiting cost minus the present discounted value of this cost in the future. This equality is the job creation condition for firms. Absent the intervention, equations (2) and (3) jointly determine the level of employment $n(0)$ and labor market tightness $\theta(0)$. As we've seen above, the intervention consists of proposing cost-reducing recruitment services to a share σ of firms, such that $c_1 < c_0$ where 1 denotes firms receiving the service.[20] Similarly, employment in these firms satisfies the labor demand equation,

$$\alpha D(1,\theta)^{\alpha-1} = a^{\gamma-1}\omega + \frac{c_1}{q(\theta)}(1-(1-s)\delta) \tag{4}$$

In this equation the employment level in firms receiving services is denoted $D(1,\theta)$. The new labor demand curve is the combination of the labor demand for both types of firms. Firms receiving services represent a share σ of all other exiting firms.[21] Aggregate demand $D(\sigma,\theta)$ is simply defined as:

$$D(\sigma,\theta) = \sigma D(1,\theta) + (1-\sigma)D(0,\theta) \tag{5}$$

[20] We could alternatively consider that the intervention also improves the matching process. The instantaneous probability of a match for vacancies posted in the program when the tightness is θ would write $q_1(\theta) = \eta q(\theta)$ with $\eta > 1$. There would be two direct effects in this case. First, there would be an increase in firm labor demand (as with a reduction in c) because of the reduction in the total hiring cost $c(1-(1-s)\delta)/q(\theta)$. The second would be to shorten the duration needed to fill a vacancy, shifting the flow of future hires to the present. The intervention in this case would also involve an increase in the instantaneous probability $f(\theta)$ of a match for jobseekers and thus a shift of the Beveridge curve. This shift would actually mitigate the increase in the tightness associated with the intervention and the related displacement effect. To be conservative on displacement effects, we stick to the model with a change in c but keep in mind that the intervention could also involve an acceleration of the matching process for treated firms.

[21] Assuming that firms in the experiment have been randomly drawn from the existing pool of firms in the market, and represent a share λ of theses firms, and that they have been further randomly assigned to treatment and control with proportion π, the share of treatment firms in the pool of existing firms is $\sigma = \pi\lambda$.

When vacancy cost reducing services are implemented through the intervention, the labor demand curve shifts, from $D(0,\theta)$ to $D(\sigma,\theta)$ as can be seen from Figure 2a. This figure shows the upward sloping Beveridge curve and the downward sloping labor demand curves in the (θ, n) space for both a weak and regular labor market. For exposition, the figure shows the aggregated demand curve for a large value of σ, but as will be highlighted below, the intervention proposed services to only a very small share of firms operating in local labor markets. Labor demand for control firms and firms outside the experiment does not shift and is determined by equation (3).

Market equilibrium is given by the equality of the aggregate demand and Beveridge curves:

$$D(\sigma,\theta) = n_B(\theta) \tag{6}$$

The set of equations (3), (4) and (6) jointly determine the new employment level and average labor market tightness. Labor demand is a decreasing function of labor market tightness, $\theta(\sigma)$. The Beveridge curve and new labor demand for both types of firms determine the new equilibrium employment levels. Figure 2b zooms in to detail the firm-level congestion effects that get us to the new equilibrium. There is first an outward shift in labor demand (AB) for benefiting firms from $D(0,\theta)$ to $D(1,\theta)$. This leads to a new aggregate labor demand curve $D(\sigma,\theta)$ which also shifts outward, with the size of the shift depending on the σ share of benefiting firms in the market. The new aggregate labor demand curve leads to a new equilibrium (D) in which tightness increases causing a downward adjustment in the level of employment in both treated and non-treated firms (respectively C and F) compared to the employment level with unchanged labor market conditions (B and A).

This framework allows us to conceptualize what the experimental results actually capture comparing employment demand in treated and control firms within our sample. Our measured impact c_m is FC:

$$c_m(\sigma) = D(1,\theta(\sigma)) - D(0,\theta(\sigma)) \tag{7}$$

which is different from the true causal impact on the treated EC:

$$c_t(\sigma) = D(1,\theta(\sigma)) - D(0,\theta(0)). \tag{8}$$

The difference is simply the indirect negative indirect impact on "control" firms FE:

$$c_i(\sigma) = D(0,\theta(\sigma)) - D(0,\theta(0)). \tag{9}$$

The three measures are linked through the simple relation:

$$c_m(\sigma) = c_t - c_i. \tag{10}$$

This leads to the following key points:

- Even if the comparison between treatment and control firms provides only a proxy for the causal impact, this comparison is informative. It represents the shift in the demand function only due to the change in the vacancy posting cost c. It is the true parameter of impact defined from the perspective of the model, compared to the measured treatment effect which combines changes due to the change in c and the tightness adjustment.

- The error made using the measured impact c_m (FC) instead of the true impact on the treated c_t (EC) is c_i (FE) and it is a function of the size of the experiment. Clearly if the size σ of the experiment is small, the adjustment to aggregate labor market tightness will be small and the measured and true impacts will be comparable. To illustrate this point the solid curve (2) in Figure 2c shows the adjustment in labor market tightness and the ratio of the true (causal) impact to the measured impact (EC/FC) as a function of the size of the experiment.[22] As can be seen, and quite intuitively, the adjustment in labor market tightness is very small when the size of the experiment is small relative to the total population of firms. As a result, the difference between the measured and true impact on the treated is very small. But we can clearly see that the tightness adjustment and ratio change considerably as the proportion of treated firms grows. Hence to be able to interpret our estimated impacts, it is very important to examine the share of firms that received treatment among all firms in the same local market. Figure A.2 shows the distribution of the proportion of treated firms among all recruiting firms within a "micro-market" (commute-zone$\times$sector) represented in the experiment.[23] We see that the vast majority of the density is concentrated at very small values of σ. The median proportion is 2% while 95% of sample firms are in micro markets with a proportion of 33% or less, hence $\sigma = \varepsilon$, with ε very small. We are thus confident that $c_m(\varepsilon) \approx c_t(\varepsilon)$ meaning that our estimates in the next section capture an extremely "close-to-causal" impact on firm labor demand.

[22] In the same spirit as what Cahuc and Le Barbanchon (2010) do for jobseeker counseling programs, we study how equilibrium and true and measured impacts vary with the share of treated firms.

[23] There are a total of 322 commute zones (*zone emploi*) in mainland France and Corsica of which 85 are represented in our sample. We aggregate sectors to 74 categories using the first tier of the French business activities nomenclature (*Nomenclature d'activités française, NAF*). For clarity, these micro markets are distinct from the strata in which the firms were grouped and randomized.

We now turn to impacts on vacancy creation and hires to examine whether the intervention did indeed affect treatment firms' labor demand. We will return to questions concerning equilibrium effects and scale-up in Section 6.

4 Impacts

4.1 Vacancies

Using our baseline specification, Table 2 displays results for the average treatment effect on vacancies for permanent contracts and all contract types posted with the Public Employment Service during the sanctuary period (September 15th, 2014 - March 31st, 2015). Column 1 shows that the program had a strong and significant impact on vacancy posting with the PES for jobs in permanent contracts. On average, treatment firms posted 0.047 more vacancies than control firms, a 24% increase relative to the control group mean (0.199 vacancies per firm). This is consistent with the model's prediction that vacancy cost reducing services offered to firms led them to open new vacancies. Columns 2-6 investigate whether the intervention also had an impact on fixed term and temp job postings with the PES. We see smaller positive effects which are insignificant at conventional levels of statistical significance, but the ranksum test on all fixed-term and temp contracts rejects the equality of the distributions. When looking at total vacancy creation with the PES (column 6) we estimate a 14% increase, significant at the 5% level. Overall, the intervention led to relatively large increases in vacancy posting with the PES, and the most robust increase happened for the most sought after jobs, those in permanent contracts. Hence the program was successful at one of its core objectives: prospecting firms in order to collect job offers that could translate into stable employment for jobseekers. Yet we must be quite cautious about interpreting this as increased vacancy creation overall as we only measure vacancies posted at the PES, and not all opened vacancies. Indeed a portion or all of this impact could simply be duplication of vacancies posted elsewhere or a substitution effect where the intervention led firms to post with the PES rather than through another outlet. Impacts on hires will allow us to infer whether these results are evidence of increased labor demand in treatment firms, but first we explore differences in characteristics of vacancies posted by treatment and control firms.

4.2 Characteristics of vacancies

We focus on the characteristics of permanent contract vacancies posted with the PES and whether we find differences in the types of vacancies that were selected for posting with the PES by treated firms. Table 3 presents this non-experimental evidence by displaying results from OLS regressions of key job characteristics on a treatment status indicator using the 1,825 permanent contract vacancies posted with the PES during the sanctuary period. The vacancy data include the minimum wage posted (usable for 1705 of the vacancies), hours, the skill and experience requirements as well as the occupation. In columns 1-2 we explore selection on perhaps the most important search parameter, the posted wage.[24] In the first column we see small and insignificant coefficients on the treatment indicator for the log of the annual posted wage. In the second column, we predict the wage on a sample of 1,921,148 permanent contract vacancies posted with the PES by firms outside of our sample during the sanctuary period. We construct this prediction by regressing the log of the posted annual wage on indicator variables for the number of hours in 8 categories and indicators for the required experience (in years) in 6 categories. Finally we include 95 indicators for the profession and the required qualification in 9 levels along with their interactions. Again, we see a small and statistically insignificant difference using a measure of the wage typically offered given these observable characteristics. In sum, we do not find evidence that vacancies created by treatment firms differed on the wage margin.

We see in columns 4-7 of Table 3 that jobs posted by treatment firms are also not statistically different in terms of the required experience or working hours. In contrast, we find a large difference in the qualification required for the job (column 3). Treatment vacancies are roughly 12 percentage points more likely to correspond to low skilled jobs.[25] This is an important result because it connects directly with our theory. This evidence conforms to the idea that the drop in vacancy costs allowed treatment firms to create less productive jobs. We can clearly see this through the firms optimality condition (equation 3) in which marginal productivity is equated to the wage and the vacancy cost. To create a job, a drop in productivity implies a drop in either the wage or a drop in the recruiting cost and we find no effect on the actual or predicted wage. [26] This suggests that the intervention led to the creation of jobs with initially lower expected net profitability "on-the-job" but that this loss was compensated by the drop in the recruitment cost.

Overall, the examination of vacancy characteristics highlights that treatment firm vacancies differed on average from control firm vacancies and that this difference was centered on the skills required for the job. We find only small and insignificant differences on other dimensions, suggesting that there was a trade-off between lower productivity and lower vacancy

[24]We use the log of the annual minimum posted wage in the vacancy. The max wage is missing for a large percentage of vacancies.

[25]In the PES system qualification is categorized in six categories. Low qualification is defined as laborers, production workers and unqualified employees. High qualification jobs are defined as supervisors, technicians and management.

[26]Though purely speculative, the lack of impacts on the offered wage could be linked to the existence of France's relatively high minimum wage which would likely be binding for entry level low-skilled work.

costs that determine the expected net profitability of marginal jobs created by treatment firms. Given that the structure of the marginal vacancy between groups has changed due to the intervention, it will be important to account for this when we turn to exploring mechanisms in section 5.

4.3 Impact on hiring and workdays

We now turn to impacts on our exhaustive measure of hiring outcomes to definitively test whether the intervention caused a change in firm labor demand. Table 4 provides evidence of impacts on hiring flows (panel a) and employment creation using our workday measure (panel b) during the sanctuary period.[27] We group the estimates obtained from our baseline model by type of hire: registered jobseekers are defined as individuals who were registered with the PES during the 30 days that preceded the contract start-date (columns 1 and 2).[28] Non-registered jobseekers thus correspond to people who were not registered with the PES in the last 30 days before the start of the contract (columns 3 and 4). Lastly, we present results across all types of jobseekers in columns 5 and 6. For each category of jobseeker, we first display results for employment creation within permanent contracts and then aggregated across all contract types, omitting the specific results for fixed-term and temp contracts.

Consistent with the results on vacancies, treatment firms increased their hires in permanent contracts and created significantly more workdays in these contracts (panel (a), column 5). Not surprisingly, this effect is driven by hires of registered jobseekers: The average treatment effect on the number of hires in permanent contracts increases by 0.046, an increase of 10.2% off the control group mean (panel (a), column 1). This leads to a statistically significant increase of 17.5 theoretical workdays created for registered jobseekers, on average per firm (panel b, column 1).

For hires of registered jobseekers in all types of contracts (column 2) we find positive point estimates that are attenuated (a point we will come back to when we discuss possible substitution effects), but with much larger standard errors. As a reminder, the hiring declarations data are simply contract flows of which a significant proportion are very short-term contracts. For example, roughly 53% of all flows concern fixed or temp contracts of a week or less. Not surprisingly, the standard deviation of hires over all contracts is 9.9 times as large as that of permanent contracts. This obviously inhibits our ability to detect a significant effect on overall hiring as the impact on permanent contract hires is diluted.

[27] We use cumulative hires and theoretical workdays until April 31st, 2015 in order to capture any hiring processes that started in March.

[28] The definition of who is a registered jobseeker is not arbitrary. It is defined by the PES and coded directly into the hiring declaration data.

In addition the intervention explicitly encouraged the creation of-, and use of services for permanent contracts to place registered jobseekers in stable employment. We saw that the effect on vacancy creation was centered on permanent contracts and we find no statistically significant effects on fixed-term hires of any length (results not reported).

Columns 3 and 4 also show positive differences between treatment and control group firms on the number of hires and workdays created for non-registered jobseekers. However, only the effect on workdays in permanent contracts is significant at the 10% level. When looking at all types of jobseekers together, we can reject the null hypothesis of no effect on employment creation in permanent contracts (column 5), but are unable to infer an effect from treatment when aggregating over all types of contracts and jobseekers (column (6)).

4.4 Quality of hires

Next, we address questions related to the quality of the hire by looking at the stability of the contract. Our previous analysis has shown that the number of hires in permanent contracts increases, but even hires on a permanent contract can be terminated rapidly if there is a poor match. To account for this, we calculate the number of hires, in permanent contracts and over all contract types that result in the new hire staying off the PES register for at least 12 months. We can do this because we observe when a registered jobseeker is hired and when she or he eventually falls back into unemployment (if at all). We examine impacts on the number of such hires that occurred during the sanctuary period. Panel (c) in Table 4 presents these results for registered jobseekers, non registered jobseekers and all jobseekers.[29] Again, we identify a significant impact centered on registered jobseekers hired in permanent contracts, significant at the 1% level. This represents a 14% increase in these "quality" hires off the control group mean of 0.33. We also see that the point estimate is essentially the same across panels (a) and (c) in column 1 suggesting that all of the intervention's impact on permanent contract creation lead to stable employment for registered jobseekers. In examining columns 3 and 4 of Panel (c), we see that there are no significant effects on non registered jobseekers (for whom we have identifiers) and that the point estimates are close to zero (in contrast to Panel (a)).[30] This provides evidence that the program helped firms hire workers that made it past the formal trial period.[31] Yet caution is warranted when interpreting these results as it captures a mix of a change in the quality of hires that would have happened in treatment

[29]We have this data for only a subset of non registered jobseekers: those that had been registered at the PES within the last 3 years. Return to the description of the DPAE data in section 2.3

[30]Below, we examine the impact on the entire distribution of hires below and present evidence that only effects on registered jobseekers are robust which is consistent with this result.

[31]A trial period or *période d'essai*, typically lasts 1-3 months for permanent contracts in France in which either side can unilaterally end the contract without consequence.

firms anyway and the quality of new, marginal hires that occurred in treatment firms due to the intervention.

4.5 Substitution effects

We now discuss the potential substitution effects induced by the intervention. While we see an increase in the number of hires of registered jobseekers in permanent contracts, this increase might come at the expense of a reduction in hires of (1) other jobseekers, (2) in fixed term contracts and (3) hires that that would have occurred in the future.

We address first whether firms simply substituted between hiring non-registered jobseekers and registered jobseekers proposed by the PES. Table 4 clearly shows that this is unlikely to be the case. When we consider hires, workdays or employment spells of at least 12 months in permanent contracts for registered jobseekers, we detect no negative impact. Estimated impacts are either positive (and significant at the 10% level) or very close to zero. Similarly, when we consider impacts on all jobseekers, we see a statistically significant increase in each three variables due to the program (column 5).

Substitution may also occur between contract types as firms move from fixed-term/temp contracts to permanent contracts. When comparing columns 1 and 2 in Table 4, we see that the point estimates of impacts on job flows and workdays are attenuated when we aggregate over all contract types. The difference between impacts in column 2 and 1 for example is -9.75 and corresponds to the impact on workday creation in fixed term/temp contracts. It is not significantly different from zero but a ranksum test would reject the assumption that the distributions in both groups are the same (not reported). Yet, the fact that part of the increase in the hiring of registered jobseekers in permanent contracts was accompanied by a reduction of fixed-term/temp contracts does not mean that the initial intent of firms was a planned substitution. Returning to Table 2, we do not see a similar pattern with vacancy creation. The intervention is associated with an overall positive increase in vacancy creation with the PES when aggregating over all contract types (column 6) that is statistically significant and *larger* than the impact on permanent contract vacancies alone. We also see in column 5 that the the point estimate is positive and the ranksum test rejects the hypothesis of equality in distributions. Rather than a planned substitution, the results appear to indicate that the intervention was a success to post and fill vacancies in permanent contracts (as the 1-1 correspondence of impacts on vacancies and hires of registered jobseekers further suggests), but perhaps only successful in posting and not necessarily filling vacancies for fixed term/temp jobs. Furthermore, when we turn to examining heterogeneous impacts in section 6.1.2 we'll find that this result is more consistent with the PES' inability to place registered jobseekers in certain types of firms rather than a subsitution effect between contract types.

Another possibility to consider is that the intervention simply shifted hires that would have occurred in the future, absent any intervention, to the present. To address this question, we return to Figure 1. We see that the intervention, and thus the direct reduction in hiring costs, only took place during a limited window period from 15 September 2014 to 31 December 2014 and was not followed by a recovery period in which counselors turn their attention to control firms. Thus we could expect a net, positive impact on cumulative hires and vacancy posting that lasts over time. We explore whether this was the case by calculating cumulative vacancy and hiring impacts over the entire 16.5 months for which we collected data (15 September 2014 - 31 January 2016). Figure 4 presents these monthly cumulative effects on the number of vacancies posted with the PES and hires of registered jobseekers in permanent contracts.[32] Figure 4b shows that impacts on firm-level employment creation for registered jobseekers appear progressively until April 2015 and then remain stable until the end of the observation period. The increase in hires in treatment firms clearly occurred during the sanctuary period and was not followed by a regression towards zero, as a pure shift of future hires to the present would imply. These results are not consistent with the assumption that the increase in hires we observe would be the result of the acceleration of hires that were planned for the future. However, in examining the cumulative effect on vacancy postings with the PES in Figure 4a, we see positive and significant effects during the sanctuary period (15 Sep. - 31 Dec. 2014), but we see also they progressively decline starting May 2015. Although standard errors are large, these results indicate that treatment firms posted relatively *less* vacancies than control firms after the sanctuary period. This could be due to the fact that certain treatment group firms started to refrain from using the PES to post their vacancies and that subsequent hiring flows were partly operated outside the PES channel. This would be the case if these firms were dissatisfied with the service provided through the intervention. We will again return to this point when exploring heterogeneous treatment effects in section 6.1.2.

4.6 Robustness checks

As a first robustness check, we explore which part of the hiring distribution is affected by the intervention. Figure 3 presents results on the estimated cumulative distributions of the number of hires and workdays in permanent contracts for both registered and non-

[32] A reminder on the interpretation: For month "x" the reported estimate corresponds to the impact on the total number of vacancies or hires during the period starting on 15 September 2014 and ending on the last day of the month "x" considered. The interpretation of the bars for vacancies and hires are the same as the main results previously reported in Tables 2 and 4 which are represented by the bars and CIs for March 2015 (vacancies) and April 2015 (hires).

registered jobseekers at the end of the sanctuary period. The figures also display p-values for ranksum permutation tests of the null hypothesis of the same distribution in hires for both treatment and control firms, using 10,000 permutations. We see that in all four cases the dependent variable is zero for more than 60% of firms: relatively few firms make permanent contract hires during the sanctuary period, consistent with the fact that 90% of our sample consists of firms with less than 50 employees (Table 1).[33] The figures confirm the previous inference on the average treatment effect. For non-registered jobseekers the distributions in the treatment and control groups are very close throughout the distribution and the p-values for the ranksum permutation tests are both above 30%. In contrast, we see a clear difference at the bottom of the distribution for the number of hires and workdays for registered jobseekers. It clearly shows that the main effect driving the average treatment effect is due to the hiring of at least one registered jobseeker in a permanent contract. The robustness of the effects we detect is also confirmed by the ranksum permutation tests which both have a p-value below 1%.

The robustness of our analysis is further confirmed in Appendix Section A and Table A.2. Our point estimates are robust to the introduction of additional covariates following Belloni et al. (2014), the computation of standard errors is not strongly affected by clustering level and, lastly, inference is not affected by how we estimate the distribution of the test statistics under the null hypothesis, the detection of a significant impact on hires being confirmed when using randomization inference (Young, 2018).

5 Potential mechanisms

We now present evidence on some of the potential mechanisms underpinning the vacancy and hiring effects. We begin by focusing on the services applied to permanent contract vacancies posted by sample firms. We find that treatment is associated with a significant increase in the implementation of preselection or, pre-screening and filtering, services. Next, we look at the potential matches made to vacancies initiated through three channels: counselors, firms and jobseekers. We show that the intervention was associated with a significant reduction in the number of candidates finally received by the firm and that firms significantly reduced their own search effort. This finding is supported by evidence that treatment vacancies are substantially more likely to be successfully filled by a counselor initiated match.

[33] This is also consistent with only 57% of firms having hired a permanent employee in the pre-treatment period (January 1st 2014 - September 15th, 2014)

5.1 Service provision to vacancies

As highlighted in section 2.5, the vacancy services provided by the PES targeted different parts of the recruitment process. We group the services into indices that cover four categories: (1) services directed at the preparation of the vacancy for publication; (2) those that involve, preselection or, pre-screening and filtering candidates; (3) those that highlight and market jobseeker attributes to firms, and (4) services aimed at the final screening and post-hiring phase.[34] In order to stay within the experimental sample we sum the services by firm. Hence firms that did not create a vacancy receive a zero for these outcomes.

Table 5 shows the results. Column 1 replicates the main vacancy creation impact from column 1 in Table 2 for ease of comparison. Columns 2 and 3 break down this effect into impacts on the number of vacancies that did not receive services and those that did. We see that the majority of the vacancy effect is driven by vacancies created with at least one of the services, roughly 70% of the effect. Columns 4-7 then show impacts on vacancies created with the different types of service indices. We see small and insignificant effects on vacancies created with preparation services in column 4. In contrast, we see robust impacts on vacancies that received prescreening and filtering services: 5.8% of control firms received this index of services and we detect an increase of 3.4 percentage points (+58%) in the treatment group.[35] In comparing effects and control group means in columns 3 and 5 we see that essentially all treatment vacancies that were created with services were given the presreening and filtering services. We also see a large percentage increase in the application of marketing jobseeker attributes to firms (Emphasize jobseeker) - whereby the counselor attempts to highlight characteristics that may be overlooked or initially unobservable to the firm - but the baseline rate is extremely small. Finally, results on the implementation of final screening and post-hiring services are also small and insignificant. These results indicate that that the vacancy creation results are driven by vacancies that received PES services and that these services were primarily based around pre-screening and filtering candidates.

Table A.3 breaks down the indices to better understand what types of frictions these precreening and filtering services could have targeted, as described in Section 2.5. In column 3, we see that treatment firms create 0.032 more vacancies with what the PES labels as the standard preselection service, an increase of 97% off the control group mean. With this service the counselor takes responsibility for consolidating a list of candidates that are pre-selected for the firm to examine. We also see that the implementation of the basic

[34]The same vacancy can receive multiple services.

[35]Remember that only 20% of firms posted a permanent contract vacancy in the control group hence this corresponds to a 17 percentage point increase in the probability that a treatment firm vacancy receives this pre-screening and filtering service.

pre-selection was accompanied by strong effects on two additional services: special criteria (column 4) and verification (column 5). Special criteria involves working with the employer to establish a maximum of 1-5 prerequisites, on which to select candidates that are not necessarily observable in the vacancy posting. This requires the counselor to get to know the employer to understand the firms needs. The coefficient and control mean indicates that treated firms are much more likely to receive this service for their vacancies, +122%. Treatment vacancies were also much more likely to receive a service called "verification". This service entails that the profile of the candidates matches exactly with the observable job requirements in terms of experience, skill and education. But it also requires that counselors restrain the number of candidates so that the employer eventually receives a restricted number of preselected candidates, usually 5-10 maximum. Importantly, implementation of verification implies that counselors take control of how firms receive applications. For instance, they can make the vacancy private so that it is not publicly available on the internet, only in agencies. This implies that organic applications by jobseekers are mechanically reduced, diminishing in turn the burden on the firm to filter candidates themselves. We can test specifically if firms took-up this filtering service by looking at how many vacancies were made private, i.e. not publicly available (column 6). We see that treatment increases the proportion of vacancies that were made private by 57%, suggesting that firms wanted to reduce the number of applicants that would apply directly to the firm. These results provide evidence that the counselors were charged with finding good candidates to fill the vacancy and that the number of candidates to review needed to be restrained. This comes from both the counselor's direct effort in prescreening candidates and also reducing the number of organic candidates the firm has to filter and pre-screen.

We find that the intervention was associated with cost-reducing services related to two fundamental types of frictions faced by firms in their recruitment process. One is the more typical friction present when the labor market is strong. In this context, firms have trouble generating candidates because there is higher competition among firms over relatively few candidates. The other is perhaps more novel, in that firms may have demand for services that reduce costs associated with having too many candidates to prescreen. To test whether treatment led to an augmentation or reduction in the number of applicants received by the firm, we now turn to examining effects on the matching process by looking at *potential* matches made to vacancies though the counselor, employer and jobseeker channels.

5.2 Matches made to vacancies

The PES collects data on the number of applications made to vacancies and also distinguishes the channel of the application. We described these search effort data in Section 2. They allow us to measure the search effort of (1) counselors who initiate applications on behalf of their jobseekers; (2) firms who search for candidates for their vacancies in the PES CV bank and invite applications to their vacancies; and (3) jobseekers who apply directly to vacancies. It is also important to reiterate the fact that the PES is only one way in which firms post vacancies and generate candidates. Hence we do not observe the behavior of the firm, the vacancies posted or applications received outside of the PES. Nevertheless, the PES is the largest recruitment platform in France and focusing on vacancies posted with the PES provides considerable insight on the underlying mechanisms driving the experimental results.

Since we now look at matching channels to vacancies we use the dataset of permanent contract vacancies posted with the PES during the 6.5 month sanctuary period. We underscore that this part of the empirical analysis is non-experimental. Indeed, the treatment increased the number of vacancies posted with the PES and it would therefore be imprudent to consider the two sets of vacancies as identical: comparisons between the two sets confound a selection effect linked to new types of vacancies posted and a treatment effect related to the efficiency of vacancy filling. We thus use inverse probability weighted regressions (IPW) (Hirano et al., 2003) to try to account for the selection effects. We do not claim this fully solves the issue of potential differences in the types of vacancies posted by treatment and control firms, but we believe this makes the comparisons between treatment and control vacancies more meaningful.[36]

Table 6 reports the difference in potential match volume to vacancies posted by treatment and control firms and thus provides evidence on prescreening and search effort costs. The dependent variable is the total number of applications during the two weeks following the posting date of the vacancy.[37] We see that treatment vacancies receive 3.7 fewer potential matches overall, on average (column 4) and this effect is driven by the firm and jobseeker

[36] We predict vacancy selection S into treatment using the observable vacancy characteristics presented in Table 3, Pr(S = 1 | wage, pred. wage, hours, experience, qualification) =

$$F\big(\beta_0 + \beta_1 w + \beta_2 \widehat{w} + \beta_3 w * \widehat{w} + \beta_4 Low\ Qual. + \sum_{h=2}^{8} \gamma_h 1(Hours_h = 1) + \sum_{e=2}^{6} \alpha_e 1(Exper_e = 1)\big)$$

with F being the logistic function. We then run OLS regressions on an indicator for the specific type of service that can be given to a vacancy on a treatment indicator with observations weighted by $\frac{T}{\widehat{Pr}(\mathrm{S}=1)} + \frac{1-T}{1-\widehat{Pr}(\mathrm{S}=1)}$.

[37] Appendix Table A.4 shows results at 8 weeks. They are very similar suggesting that the majority of the "action" on vacancies happens within the first couple of weeks after posting.

channels. Treatment firm vacancies receive 2.7 less jobseeker applications, on average. This represents a 39% drop in the number of jobseekers that "organically apply" to a vacancy. This drop is not due to observable differences in the characteristics of vacancies because we are using IPW estimates. Rather, this drop is more likely associated with the way counselors handled the vacancies. The other portion of the overall reduction comes from the firms themselves. Employers generate roughly 0.84 less potential matches off an average of 1.1 firm-made potential matches in the control group, This shows that treatment firms expended significantly less effort in presceening applicants for their vacancies. Interestingly, we see a small, negative and insignificant treatment effect on the number of potential matches generated by counselors (column 1). Given that we found large differences in service provision that entailed that counselors were responsible for generating candidates for the vacancy, we might expect large positive effects in this channel. This is not the case and further reflects the nature of the pre-screening and filtering services: The large increase in the use of prescreening and filtering services implies that counselors generate suitable candidates for the firm, but also to limit the number of these candidates. Thus this matching channel captures a potential change in the quantity and quality of the counselor-generated candidates.

This provides evidence that the implemented services had big impacts are the matching process. Treated firms significantly reduce the amount of effort they expend in searching for candidates to fill their vacancies as the PES engages in preselecting candidates for them. In addition, the counselors are able to significantly reduce the number of jobseekers who apply directly to the firm through filtering mechanisms. Overall, these results indicate that treated firms were able to reduce their recruitment costs as caseworkers increased their effort. This substitution between firm and caseworker search effort is an important result and is consistent with further evidence on successful counselor-made matches.

5.3 Counselor made matches

The PES tracks another important metric related to the counselor initiated applications: its success rate. Indeed this is one of the main metrics on which counselor performance is evaluated in general.[38] We saw previously that there was a negative, but statistically insignificant difference in the number of applications that came through the counselor matching channel between treatment and control vacancies. Yet this estimation hides potentially important information on the efficiency of the counselor's effort, i.e. the success rate and the speed at which a successful match is achieved through the counselor channel.

[38]The successful match rate in the firm and jobseeker channels is subject to much more measurement error according to discussions with the PES. This is simply because counselors have much less incentive in tagging the potential match as successful in these other channels.

Figure 5 illustrates the estimated nonparametric vacancy "survival function" (Kaplan-Meier) censored at 8 weeks after vacancy creation, adjusted using our inverse propensity weighting. Failure is defined as having received a successful counselor match to the permanent contract vacancy. The event date is the day the counselor proposed the candidate that would eventually be hired, not the actual hiring date. We see that treatment and control survival rates diverge in the very first day of vacancy creation with the difference continuing to diverge over the observation period. We can formally reject the null of equality of the survival functions using a Cox regression based test with a p-value of 0.02.[39] This result indicates that counselors were able to propose attractive candidates quickly. To be clear this result does not tell us that treatment vacancies are filled more quickly or more often, in general.[40] But, assuming that once a successful counselor match happens the vacancy cannot be filled again, this does provide evidence that treatment vacancies were more likely to be filled through counselor effort compared to control group vacancies.

Overall, we see that treatment significantly lowered the number of total potential matches the firm generates or receives organically (jobseeker). And, on average, treatment vacancies did not receive more or less counselor initiated matches. But we do find evidence that the counselor matches they do receive are more likely to result in a successful match in the treatment group. It is important to note that reduction in the firm and jobseeker channels may be due to two distinct but non exclusive mechanisms. (1) The time to fill treatment vacancies is reduced as counselors successfully fill vacancies faster. Thus the total number of potential matches declines mechanically in the other two channels because the vacancy is simply closed earlier. (2) For a given interval of time, the firm confers the recruitment process to the counselor, thus reducing its own search for candidates while less "organic" jobseeker applications get through as the counselor puts in place the different filtering mechanisms highlighted above. We do not test exactly which effect dominates because the end result is the same: The $\frac{c}{q(\theta)}$ ratio decreases either through c decreasing as we move from c_0 to c_1, as free PES services substitute for firm effort and monetary costs, or through an increase in the vacancy filling rate, $q_0(\theta)$ to $q_1(\theta)$ (return to footnote 20 for further discussion), as matching efficiency increases. This directly affects the job creation condition, (equation 4), effectively lowering the marginal productivity threshold at which firms will create a job.

[39] Log-log and Shoenfeld residual tests (p-value = 0.88) support the proportional hazard assumption.

[40] Unfortunately, vacancy closing dates, regardless of a successful match, are subject to very high measurement error. If a successful match is not recorded by a counselor, the IT system automatically closes the vacancy only after three months with no other useful information. Nor do we have data on how and when vacancies are filled though channels outside the PES.

6 Implications

We now turn to discussing the experimental evidence in light of the congestion externalities induced by the experiment and what theory and our data can tell us about the potential for scaling-up a firm level ALMP of this type. We showed that our experimental results capture a “close-to-true" measure of a change in treatment firms' demand for labor because treated firms represent a very small proportion of all recruiting firms in their respective micro-markets. We now highlight that we must be more nuanced when (1) discussing employment effects on aggregate and (2) in what conditions these types of program are more or less effective. We'll show that labor market conditions and the PES' familiarity with firm needs are important factors to consider when thinking about a wider implementation of this type of intervention.

6.1 Equilibrium effects and scaling

6.1.1 Equilibrium effects

We made the claim in section 3 that the measure provided by our experiment (FC), is very close to the direct effect (EC) and that the indirect effect (EF) is negligible (Figure 2b). This is because our experiment is "marginal": the actual σ, calculated at the commute-zone$\times$sector (micro-market) is very small as Figure A.2 clearly shows.

However, this does not mean that indirect effects, even if they are very small, do not matter at the aggregate level. This is because the potential congestion externalities accumulate over a large number of firms. We can illustrate this using the ratio of the impact on the aggregate number of hired workers per treated firm ED/σ to the measured impact AB for a marginal experiment:

$$\lambda(\theta) = \lim_{\sigma \to 0} \frac{ED(\sigma)/\sigma}{AB} \tag{11}$$

Figure 2d, curve (2), shows this ratio is usually below 1. For example, we saw that at the moment of the intervention the French labor market was at one of its weakest points in modern history (Figure A.1), with tightness hovering around 0.42. With this level of tightness the simulations in Figure 2d show that the aggregate total effect per treated firm to the direct measured impact ratio (curve 2) is roughly 0.68. This means that for each hire in a treated firm the real increase in the total workforce in the market will be only 0.68. This suggests that even with a marginal intervention equilibrium effects can matter. They can be almost negligible at the individual level but these displacement effects end-up counting for a substantial share of the measured effect.

Moreover, and as the figure shows, this ratio decreases in the initial tightness of the labor market. This ratio is actually the ratio of the slope of the demand curve to the sum of the slopes of the Beveridge and demand curves. In weak labor markets, or low values of tightness, the Beveridge curve is flat, thus the tightness adjustment that compensates the disequilibrium in employment flows caused by a shift in the demand curve and the placement of some jobseekers is small. Conversely, when the market is tight the shift in the labor demand curve causes a large disequilibrium in employment flows.

As a result, we do not claim that the experimental impacts reflect the number of new hires in the labor market per treated firm. Rather, we claim that our experimental results correctly reflect the shift in firm labor demand associated with benefiting from vacancy cost reducing services when treatment and control firms share the same market conditions, $\theta(\sigma)$.

6.1.2 Scaling-up

Indeed, our study was designed to answer the question of whether a firm-level ALMP is able to boost labor demand. It was not designed to analyze questions related to scale-up. But again our theoretical framework can help shed light on this issue. We find that the size of the intervention is actually less of an issue than firm level displacement effects.

In returning to Figure 2c the dashed curve shows the ratio of the total aggregated effect per participating firm ED/σ, which accounts for displacement effects, to the marginal impact we measure with our marginal experiment (AB): $ED(\sigma)/\sigma AB$.[41] The figure shows that, although not constant, the ratio is quite stable. For σ close to zero the ratio is 0.68 and for $\sigma = 1$ it is 0.73. This suggests that, though through the lens of the model, what we learn from our marginal experiment in term of employment created per treated firm is informative about what to expect from an intervention implemented at a larger scale.

In sum, our marginal experiment shows that offering hiring services to firms has an impact on their hires. However the net increase in employment is just a fraction of this impact because of displacement effects. This fraction is unknown, in the case of our model we obtain a value of 0.7, but we know that the larger the tightness the smaller this fraction will be. Moreover, the framework shows that there is a simple close to linear relationship between our estimated parameters and the aggregated impact when the scale of the intervention varies.

6.2 Evidence on the role of tightness

The model also implies that impacts on treated firms should increase with in tightness. Our simulations reported in the curve (1) in figure 2d clearly shows this increasing pattern. The

[41] The curve is obtained assuming a tightness value of 0.42 which corresponds to the average market conditions in the French economy at the moment the experiment was launched.

intuition for this result being that in "bad times" firms don't need help recruiting because there is a plethora of candidates to choose from because the market is slack. Formally, when θ decreases, $\frac{c}{q(\theta)}$ tends to zero because vacancies are filled more and more quickly. Thus the returns to reducing vacancy costs should have considerably *smaller* effects as tightness diminishes.

Yet, at a glance, our experimental results run contrary to this. We find that a firm-level ALMP increased labor demand in a context where times were seriously bad for jobseekers and firms should have had no trouble in generating applicants.

Of course, one potential caveat is that the markets selected to participate in the experience may not have been representative of the French labor market at the time. To address this we calculate tightness again at the micro-market level (commute-zone$\times$sector) to obtain a more granular indicator of the labor market conditions faced by sample firms at the beginning of the intervention. A plot of the density appears in appendix figure A.3. It shows that micro-market tightness is representative of overall tightness in the French economy at that time, with a median value of 0.35.

Furthermore, this heterogeneity allows us to formally test whether the ATE on our main outcomes depends on the local market tightness that firms actually face. We split the sample into two sub-samples corresponding to below and above the median tightness, using,

$$y_i = a + \beta_0 T_i \times (1 - I_i) + \beta_1 T_i \times I_i + d \times I_i + \sum_{s=1}^{S} \gamma_s 1_{s,i} + u_i \tag{12}$$

where β_0 and β_1 represent the average treatment effects on the two specific sub-populations defined as $I = 0$ and $I = 1$. We then test the null hypothesis $H_0 : \beta_0 = \beta_1$.

Results are presented in Panel (a) of Table 7. In column 1 we see that the point estimate on vacancy creation with the PES is larger in relatively tighter micro markets, but the p-value from the test of equal effects by sub-sample is large as well as when comparing the average treatment effect by quintile of tightness. In addition, ranksum tests strongly suggest treatment impacts in both types of markets, with a p-value actually smaller in below median tightness markets.

Regarding vacancies created with prescreening and filtering services, we see very similar average effects by market conditions, again suggesting that the intervention had vacancy creation impacts in both slack and relatively tight labor markets. We see a similar story when turning to our main hiring outcomes in columns 3 and 4, hires of registered jobseekers, and quality hires of registered jobseekers, respectively. We see a marginally larger point estimate in low tightness micro markets which reverses when isolating to quality hires. But, again, we clearly fail to reject the assumption of the same ATE in the two sub-samples or by quintile for each hiring outcome. And the related ranksum tests give p-values both under 10% for the equality of distributions between control and treatment firms in both types of markets.

The take away from this analysis is that the effect appears to be present in relatively tight markets, as standard theory might suggest, but also in depressed markets. This (non) result is pertinent as it is in depressed markets that the PES might want to intervene and it is in those markets that the gap between marginal and scaled interventions is expected to be the smallest.

A critical question is why firms might benefit from the PES services under varying market conditions. In a tight market, where firms have trouble attracting candidates, pre-selection services address frictions simply by generating applicants for the firm to review. In a slack market, another type of friction may exist in which firms pay high prescreening costs if they open a vacancy as they have to screen many applicants, qualified or not that apply. These two types of frictions come from two distinct labor market conditions, but still affect the $\frac{c}{q(\theta)}$ ratio in the same direction: an increase in prescreening *or* candidate generating frictions leads to a larger instantaneous vacancy filling cost because it implies a longer time to find the (right) candidate and make the hire. It appears that the intervention may have been beneficial to firms under these different market conditions suggesting that recruitment costs may be a function of tightness, $c(\theta)$: Firms have trouble generating candidates for their vacancies "when times are good" as benchmark theory would predict, but also when times are bad due to the number of applications they receive when opening a position.

6.3 Quality of the service offered to firms

Results so far have shown that the services offered by the PES to firms in their recruitment activities are effective on average. The analysis has shown that the key aspect of these services has to do with recommendations of jobseekers to firms. Hence, we might wonder about the ability of the PES to deliver recommendations in an effective way to different types of firms and this should also be considered when thinking about the implementation or scale-up of similar interventions.

Delivering effective recruitment services requires that counselors understand the needs and particularities of the firm. In addition, the pool of jobseekers that counselors would like to place may be more or less adapted to the needs of firms. This creates another natural dimension of heterogeneity over which to examine the effectiveness of the intervention. We test whether treatment effectiveness varies with the PES' familiarity with the firm (or the firm's familiarity with the PES). We build an index based on the various types of contact that counselors had with the firm before the intervention to proxy for the counselor's familiarity with the firm. The index is based on visits, telephone calls and face-to-face meetings at the agency that counselors had with the firm in the 8 months preceding the prospection campaign (January and August 2014). We create an indicator variable equal to one if the firm had at least one of these contacts. This was the case for 39% of sample firms. We stress that this evidence is suggestive because this dimension of heterogeneity was not pre-specified and does not directly derive from the model.[42]

We again use equation (12) to estimate these heterogeneous treatment effects. Panel (b) of Table 7 reports results on the main outcomes. We see no discernible heterogeneous effects on vacancy creation with the PES. Impacts are very similar for both types of firms. In contrast we find that employment creation is completely centered on firms that were previously in contact with the PES. We find average treatment effects very close to zero for firms which were not formerly in contact with PES counselors. But for firms which were, we detect large significant effects: +0.124 hires of registered jobseekers in permanent contracts and we reject the null of equal impacts (p-value of 2.1%). Similarly the strong heterogeneous effect is apparent when isolating quality hires of registered jobseekers. The coefficient on the number of these hires is 0.116, which is the order of magnitude of the impact on overall hires. These results suggest that the new services helped the PES collect vacancies from both types of firms, but that counselors may have only been effective at placing registered jobseekers with firms they were already familiar with. Another interpretation is that the vacancy effect on "no contact" firms is not really employment creation, but simply a substitution or duplication of an existing vacancy. Table A.5 breaks down our main hiring impacts table by this baseline dimension. Column 2 shows negative point estimates that are relatively large, though not statistically significant, on registered jobseeker hires in firms that were not in contact with the PES prior to the intervention. This negative effect on no-contact firms is actually the main driver of the attenuation in overall impact on registered jobseekers when summing over all contact types (column 2 in Table 4). Thus, rather than substitution between contract types, this evidence may be more consistent with the idea that some firms reduced their hires of registered jobseekers due to the intervention because the candidates proposed by counselors were ill adapted to these firms needs. In addition, conferring a large part of the recruitment process to PES counselors would have displaced their normal recruitment operations without any benefit. Though only speculative, these striking heterogeneous impacts by the PES' familiarity with the firm implies prudence in the targeting phase of similar interventions. It

[42] Unfortunately, we were unable to stratify ex ante on this dimension because of data availability in the sampling phase. Nevertheless, sampling in the BMO database ensured that we had a significant proportion of firms that had interacted with the PES in the months prior to the intervention. See Section 2.1 for more details.

may also speak to the fact that public employment services in general are faced with the dilemma of finding the right balance between satisfying firms while at the same time fulfilling their primary responsibility of placing marginalized jobseekers in stable employment.

7 Conclusion

We study the effect of a Public Employment Service's (PES) intensive firm services program in which thousands of small and medium sized firms were randomly selected to be prospected and exposed to a robust set of free, vancancy cost reducing services. The experimental evidence shows that this intervention positively affected firm labor demand. We find large positive impacts on permanent contract vacancy postings with the PES which translates into significant increases in hiring flows and workdays created for registered jobseekers in these types of contracts. This suggests that active labor market policies (ALMPs) that focus on firm labor demand may have significant added value in the labor market.

We develop a theoretical framework that allows us to conceptualize the equilibrium effects and scale-up implications of this type of ALMP that targets the matching frictions that firms face. The framework highlights that displacement effects are an important concern even when the intervention is implemented on relatively few firms. However, these displacement effects are much more moderate in weak labor markets. Finally our model shows that aggregate employment impacts will be a fraction of the estimated impacts, but are only weakly dependent on the scale of the intervention.

In exploring mechanisms, we find that treatment-group vacancies received significantly more candidate prescreening and filtering services and that this was a key component of the intervention. These services led to a substitution of effort from the firm to the counselor to generate and filter applications. On average, treatment firms received significantly less applicants for final screening than control firms. This is consistent with the fact that we do not detect heterogeneous effects over the underlying level of market tightness. This implies that pre-screening services may be effective in reducing "candidate generating costs" when candidates are hard to come by, but also reduce the screening costs associated with large flows of applications when there are many jobseekers and relatively few jobs.

Overall, our findings show that prescreening costs matter, reflecting recent developments in the labor literature that have found that a more sophisticated understanding of vacancy costs is important for understanding firm recruitment behavior under different labor market conditions.

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Tables

Table 1: Balance check and descriptive statistics

	Control Mean	Treatment	N
	(1)	(2)	(3)
[1em] Firm Characteristics			
$\leq$ 10 employees	0.409	-0.00166	7438
		(0.00158)	
$>$ 10 & $\leq$ 25 employees	0.322	0.00468	7438
		(0.00559)	
$>$ 25 & $\leq$ 50 employees	0.166	-0.00320	7438
		(0.00568)	
$>$ 50 employees	0.102	0.000189	7438
		(0.00201)	
Manufacturing	0.112	-0.000185	7438
		(0.00711)	
Construction	0.171	0.00279	7438
		(0.00833)	
Commerce	0.249	-0.00775	7438
		(0.00955)	
Service	0.422	0.00112	7438
		(0.0117)	
Other sectors	0.0458	0.00401	7438
		(0.00578)	
Vacancies posted with PES			
Fixed-term	0.0763	-0.00824	7438
		(0.00648)	
Permanent	0.0908	-0.0116**	7438
		(0.00573)	
Temporary	0.111	0.00147	7438
		(0.00684)	
Hires by contract type			
Fixed-term $<$ 6 months	0.498	-0.00380	7438
		(0.0120)	
Fixed-term $\geq$ 6 months	0.158	0.00216	7438
		(0.00822)	
Permanent	0.433	0.0110	7438
		(0.0125)	
Temporary	0.226	-0.00493	7438
		(0.00942)	
Contact with PES			
Calls	0.362	-0.00533	7438
		(0.00971)	
Visits	0.0554	0.0123**	7438
		(0.00572)	
Mail and faxes	0.147	-0.00687	7438
		(0.00814)	
Emails	0.225	0.00324	7438
		(0.00927)	
PES matching services			
Jobseeker initiated match	0.154	-0.0115	7438
		(0.00771)	
Counselor initiated match	0.183	-0.0129	7438
		(0.00786)	
Employer initiated match	0.0236	0.000606	7438
		(0.00322)	
Successful match	0.0578	-0.000497	7438
		(0.00546)	
Spontaneous candidature	0.0138	-0.00245	7438
		(0.00325)	

Note: Rows display results from separate linear probability estimates of equation 1 for the given dependent variable. All dependent variables are $\{0, 1\}$ indicators for which we display the weighted control group mean. Standard errors, in parentheses, are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 2: Impact on vacancies posted with the Public Employment Service

		Fixed-term and temp contracts				
	Permanent	0-1 month	1-6 months	More than 6 months	All fixed-term & temp	All
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.047***	0.007	0.014	0.004	0.024	0.071**
	(0.017)	(0.008)	(0.016)	(0.005)	(0.022)	(0.030)
Ranksum p-value	0.001	0.147	0.082	0.288	0.027	0.000
Control Mean	0.199	0.078	0.200	0.037	0.314	0.513
N	7438	7438	7438	7438	7438	7438

Note: This table presents impacts on vacancies posted with the PES during the treatment period (15 September 2014- 31 March 2015) for different contract types. Impacts are estimated using equation 1. Standard errors are clustered at the agency level (in parentheses). * $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: Selection on vacancy characteristics

	w	$\hat{w}$	Low Qualif.	Experience	Hours<35	Hours$=35$	Hours>35
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.017	-0.005	0.121***	-0.156	-0.020	-0.003	0.022
	(0.027)	(0.028)	(0.032)	(0.137)	(0.024)	(0.029)	(0.025)
Control Mean	9.918	9.893	0.633	2.169	0.130	0.661	0.209
N	1705	1825	1825	1825	1825	1825	1825

Note: We display characteristics for permanent contract vacancies during the treatment period and their correlation with treatment status. w and $\hat{w}$ are the log of the posted minimum yearly wage and the log of its outside sample prediction obtained using dummy variables for the weekly hours, experience, profession and qualification categories, respectively, and the interaction of the (profession $\times$ qualification) indicators from an OLS regression. Only 1,705 permanent contract vacancies have usable wage data. Experience is defined as the minimum required experience for the post in years. Low qualification, Hours$<$35, Hours = 35, Hours$>$35 are indicator variables. Strata weighted control group means are also shown. Standard errors are clustered at the agency level (in parentheses). * $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Impact on employment creation

Jobseekers	Registered		Non-registered		All	
	Permanent	All	Permanent	All	Permanent	All
	(1)	(2)	(3)	(4)	(5)	(6)
(a) Impact on hires						
Treatment	0.046**	0.033	0.063	0.438	0.109**	0.472
	(0.021)	(0.214)	(0.040)	(0.540)	(0.050)	(0.654)
Control Mean	0.450	3.963	0.828	9.642	1.278	13.605
N	7438	7438	7438	7438	7438	7438
(b) Impact on workdays						
Treatment	17.522**	7.764	26.243*	21.287	43.764**	29.050
	(8.523)	(10.547)	(15.647)	(18.162)	(19.879)	(23.300)
Control Mean	176.845	295.415	325.488	511.064	502.333	806.479
N	7438	7438	7438	7438	7438	7438
(c) Impact on quality hires						
Treatment	0.046***	0.034	-0.002	-0.019	0.045**	0.012
	(0.017)	(0.023)	(0.013)	(0.017)	(0.023)	(0.032)
Control Mean	0.330	0.562	0.263	0.375	0.596	0.941
N	7438	7438	7438	7438	7438	7438

Note: Panel (a) displays impacts on contract flows. Panel (b) reports impacts on the number of workdays created within these contracts. In Panel (c), the dependent variable is defined as contract flows that lead individuals to stay off the PES rosters for at least 12 months after the contract start date. Results are displayed for all hires (column (1) and (2)), hires of registered jobseekers (column (3) and (4)) and non-registered jobseekers (column (5) and (6)). Results are presented on permanent contracts and then aggregated over all contract types, denoted in column headers. For each outcome, the table presents the average treatment effect estimated using equation 1. Standard errors are clustered at the local employment agency level (in parentheses). * $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Impacts on vacancy services

	Vacancy creation			Vacancy creation with service type			
	All	No services	With services	Vacancy Prep	Prescreening and filtering	Emphasize jobseeker	Screening and post hiring
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.047***	0.015	0.032***	0.005	0.034***	0.006***	0.001
	(0.017)	(0.014)	(0.007)	(0.003)	(0.007)	(0.002)	(0.001)
Control Mean	0.199	0.137	0.062	0.015	0.058	0.003	0.002
N	7438	7438	7438	7438	7438	7438	7438

Note: Columns 1-3 provide impact estimates on all permanent contract vacancies (replicating column (1) of Table 2) then breaks down the impact by vacancies with and without services. Columns 4-7 show impacts on the number of vacancies created with the specific service index. Standard errors are clustered at the agency level (in parentheses). * $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: Impact on matching rates by channel

	Counselor	Firm	Jobseeker	All
	(1)	(2)	(3)	(4)
Treatment	-0.261	-0.841***	-2.670***	-3.707***
	(0.398)	(0.289)	(0.997)	(1.145)
Control Mean	3.588	1.102	6.862	11.715
N	1705	1705	1705	1705

Note: This table presents inversely propensity weighted (IPW) regression results for the intervention's impact on the number of applicants generated through each potential matching channel within the first two weeks after vacancy creation with the PES.

Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: Heterogeneous impacts on vacancies and hires

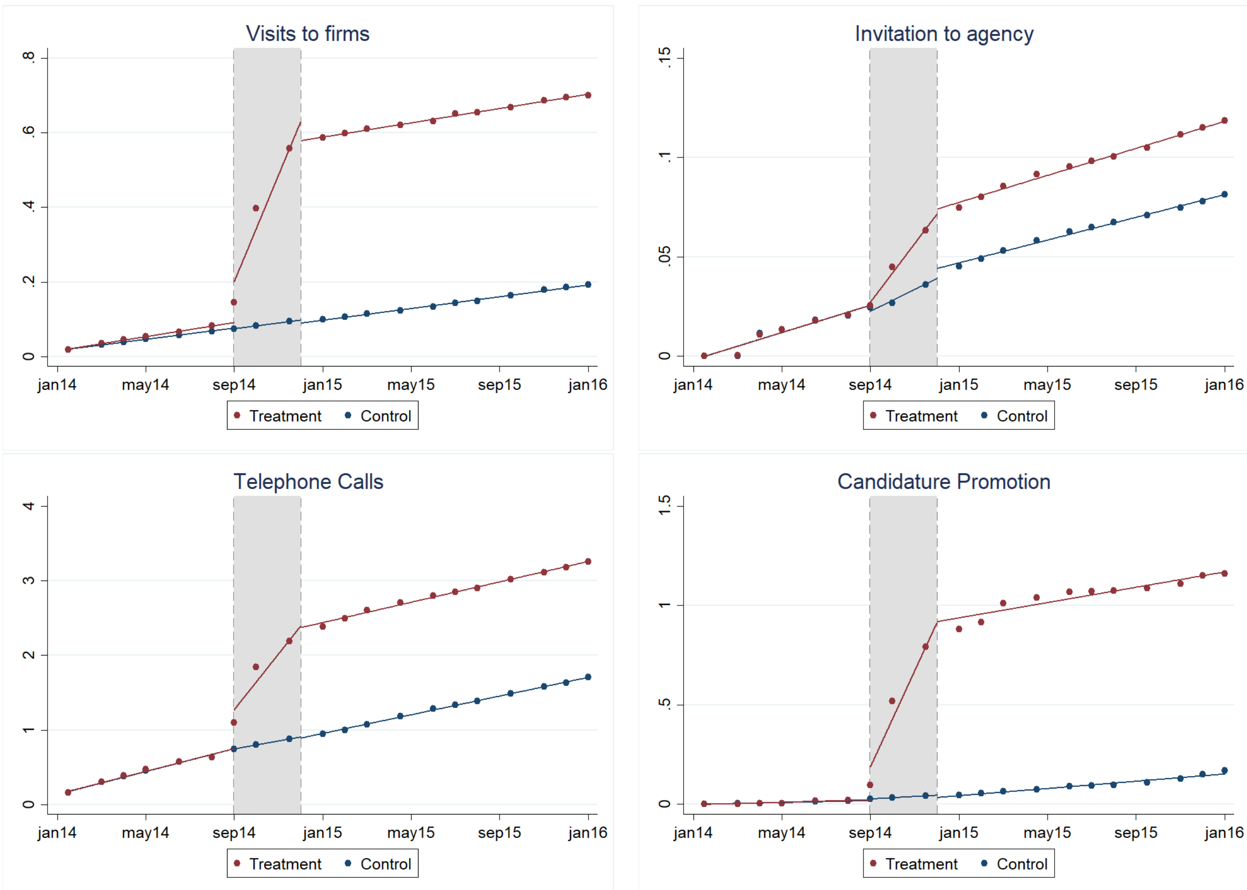
	Vacancies	Vacancy with services	Hires	Quality hires
	(1)	(2)	(3)	(4)
(a) Tightness				
Below median tightness	0.033	0.031***	0.056*	0.035
	(0.023)	(0.008)	(0.033)	(0.029)
Above median tightness	0.061**	0.038***	0.038	0.068*
	(0.025)	(0.009)	(0.031)	(0.037)
Same ATE	0.386	0.550	0.711	0.507
Same ATE by quintile	0.613	0.519	0.679	0.794
Ranksum p-value low tightness	0.009	0.000	0.068	0.081
Ranksum p-value high tightness	0.020	0.000	0.087	0.011
(b) Previous contact				
No contact	0.043**	0.031***	-0.005	0.008
	(0.021)	(0.007)	(0.024)	(0.024)
Contact	0.052*	0.038***	0.124***	0.116**
	(0.031)	(0.012)	(0.046)	(0.047)
Same ATE	0.819	0.549	0.021	0.045
Ranksum p-value no contact	0.001	0.000	0.435	0.569
Ranksum p-value contact	0.111	0.000	0.005	0.000

Note: The table presents heterogeneous impacts on the main outcome variables (permanent contract vacancies and hires of registered jobseekers in permanent contracts). Panel (a) presents results with respect to baseline tightness: whether the firm faces slack (below median) or tight (above median) market conditions. Tightness is constructed at the (commute zone×sector) level. Regression results are obtained using equation 12 with the p-values of the test of same average treatment effects in the two sub-populations and across quintiles. Panel (b) presents results depending on whether the firm had previous contact with the PES. The contact indicator is constructed using an index of baseline counselor visits to the firm, telephone exchanges and meetings at the agency. Results are obtained using equation 12 with the p-value of the test of same average treatment effects in the two sub-populations displayed below estimation results.

Standard errors in parentheses are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

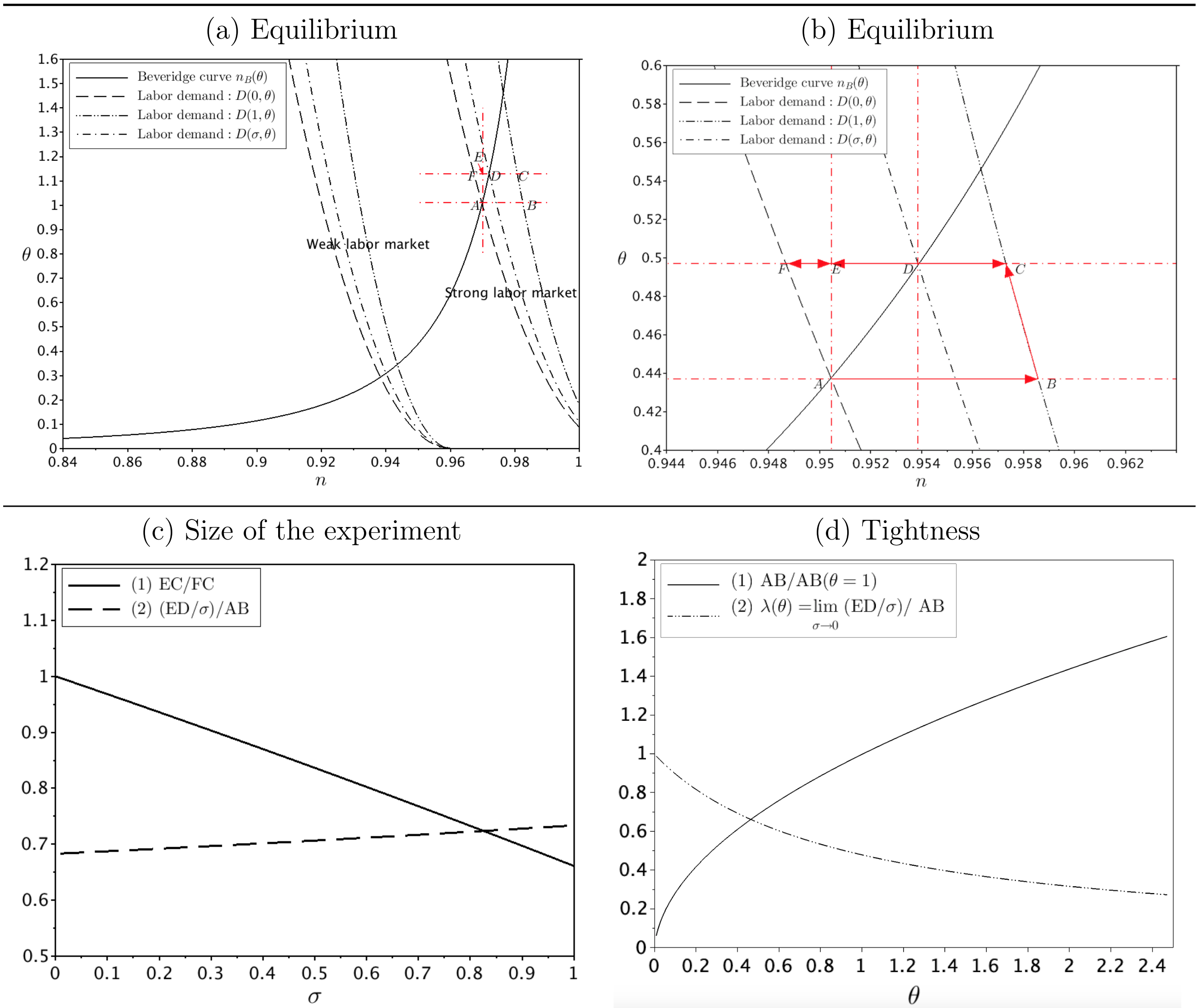
Figures

Figure 1: Compliance and treatment intensity



Note: Figures illustrate the average number of counselor initiated visits, meetings at the agency, phone calls and jobseeker CVs spontaneously sent to firms (candidate promotion). The numbers are averaged into bins corresponding to each month during the observation period for treatment and control firms. The shaded region indicates the intensive treatment period (September - December 2014) in which caseworkers were supposed to engage in in-depth interviews with firms to learn about their recruitment needs and market the services.

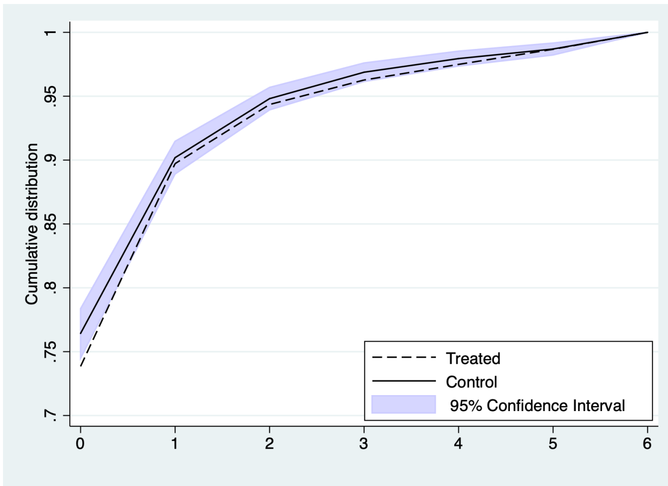
Figure 2: Equilibrium employment and the role of the size of the experiment and labor market tightness



(a) Equilibrium from Beveridge and labor demand curves in both weak and regular labor markets. Labor demand correspond to the aggregation of labor demand with and without the intervention ($\sigma = 35\%$ of firms offered recruitment services).
(b) Zoom on adjustment around equilibrium ($\sigma = 60\%$ of firms offered recruitment services).
Letters in figures (c) and (d) refers to letters in figure (b). AB represents the impact of a marginal experiment - FC and EC represents respectively the measured effect and the impact on the treated for an experiment of scale σ - ED represents the total aggregated effect, accounting for the size of the experiment and displacement effects

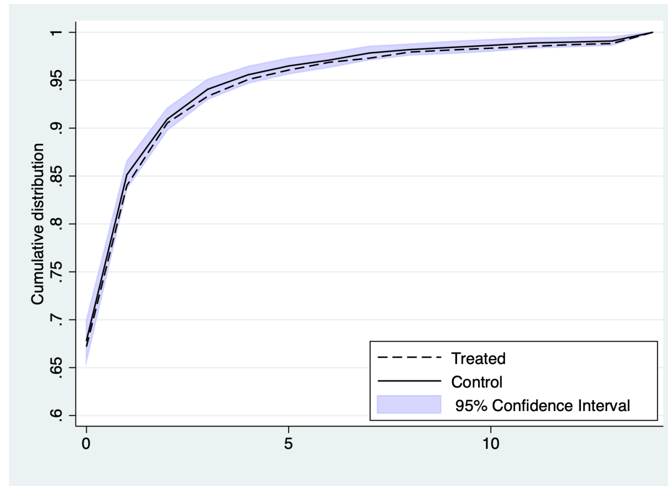
Figure 3: Impacts on cumulative distributions of hires and workdays

(a) Hiring Registered Jobseekers



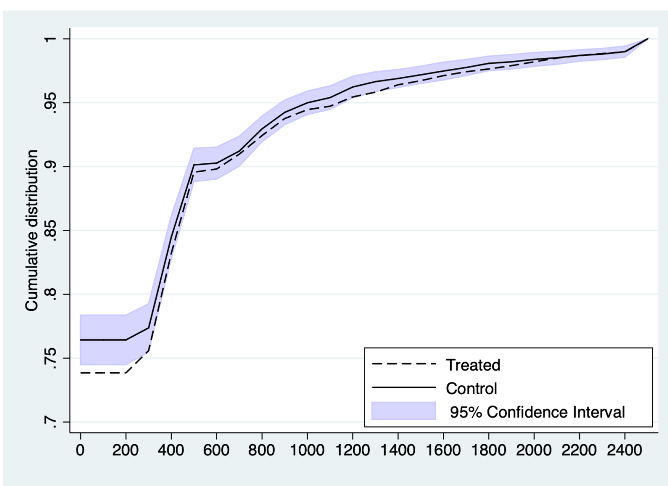
Ranksum 10,000 permutation test p-value: 0.008

(b) Hiring Non-registered Jobseekers



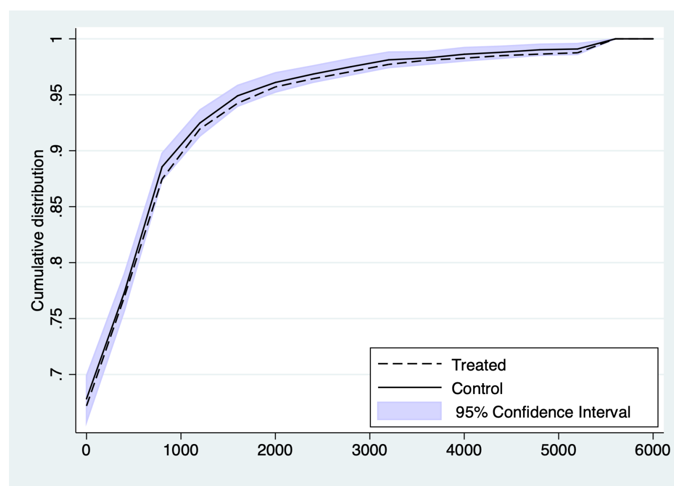
Ranksum permutation test p-value: 0.340

(c) Workdays Registered Jobseekers



Ranksum 10,000 permutation test p-value: 0.010

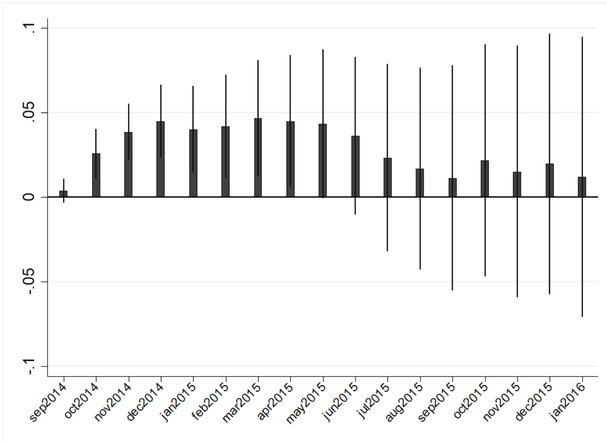
(d) Workdays Non-registered Jobseekers



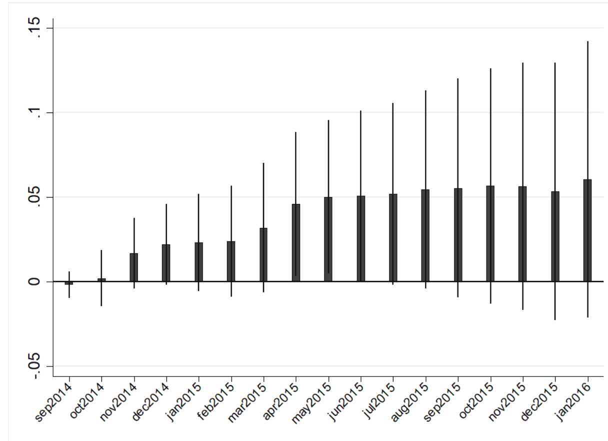
Ranksum 10,000 permutation test p-value: 0.381

Note: These figures present estimates of the difference in the cumulative distribution of hiring outcomes of registered jobseekers in the control and treatment groups. The solid line provides the average m_0 of the outcome variable y in the control group of the dummy variable $d = 1(y \leq x)$ with x the value given on the horizontal axis. The dashed line provides the average m_1 of d in the treatment group. The shaded area is delimited by adding to m_0 $\pm 1.96 se$ where se is the standard error of the difference between m_1 and m_0. Below the figure appears the p-value of ranksum tests computed using 10,000 permutations within assignment strata.

Figure 4: Cumulative impacts over entire study period



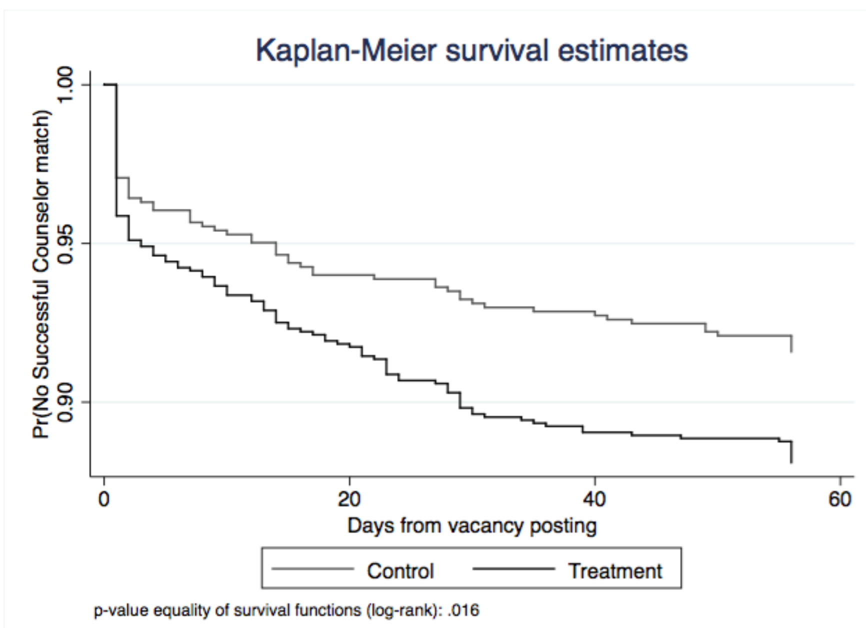
(a) Vacancies



(b) Hires

Note: Bars display average treatment effects on cumulative outcomes by month estimated using equation 1. Vertical lines around treatment effects represent 95% confidence intervals. (a) Vacancies posted for permanent contracts from 15 September 2014 to the month considered. (b) Hiring in a permanent contracts for registered jobseekers.

Figure 5: Successful counselor match “survival functions"



Note: Curves illustrate nonparametric survival functions for treatment and control group vacancies. Failure is defined as a successful counselor initiated match and vacancy filling times t are censored at 8 weeks. We observe t_1 if $t_1 < t_2$ where t_k is the date at which the event "match initiated by actor k" occurs, $k = 1$ correspond to a match initiated by the caseworker and t_2 a match initiated by the firm or by the jobseeker. Assuming t_1 is independent of t_2 and t_k has a constant hazard h_k, the survival of t_1 writes $\frac{h_2}{h_1+h_2} + \frac{h_1}{h_1+h_2} exp(-(h_1 + h_2)t)$.

A Robustness

We first test the robustness of our statistical inference using a different standard error computation. We then implement randomization inference for the usual student test, using 10,000 permutations tests. Finally, we estimate a specification that includes baseline covariates. The set of potential covariates is comprised of all sectors (23 sectors), dummy variables indicating prior contact with the PES (e-mail, profile promotion, telephone, visits); hires in fixed-term contracts, permanent contracts and temporary hires; potential matches initiated by the firm, a counselor or jobseeker; vacancies posted for fixed-term and permanent contracts; and finally local labor market tightness variables in 4 categories. To select the relevant covariates to be included we implement the *double post lasso procedure* developed in Belloni et al. (2014). It has the advantage of transparently selecting the relevant covariates in a way that avoids specification searching to then introduce them in the regression. The covariates selected for inclusion in the regression is the joint set of variables selected in two separate lasso procedures. The first lasso seeks to explain the dependent variable y_i while the second lasso seeks to explain the treatment variable T_i. Strata fixed effects are included in the analysis for both lasso procedures.[43] The specification is thus formulated as,

$$y_i = a + bT_i + selected(x)_i c + \sum_{s=1}^{S} \gamma_s 1_{s,i} + u_i \tag{13}$$

Results for all four robustness checks are presented in Table A.2. We consider both hires and workdays under permanent contracts for registered jobseekers (respecively column (1) and (2)) and for non registered jobseekers (column (3) and (4)). Panel (a) presents estimated standard errors either clustered at the local employment agency level (as they appear in table 4) or not clustered, simply obtained using the White-Huber robust formula. We see little difference between the two standard errors though non clustered standard errors are always larger due to potential negative correlation in firm hiring outcomes within agencies.

Panel (b) of Table A.2 presents the robustness analysis related to randomization inference. As shown in Young (2018), using the asymptotic approximation of the distribution of t-statistics as a standard normal can lead to severe problems with inference. This is especially the case when the distribution of the outcome variables have heavy tails and when there are outliers. The use of randomization inference allows us to compute an exact p-value of the implemented test. This is however at the cost of a switch of the null hypothesis to the stronger absence of any individual impact $H_0 : y(0) = y(1)$. This is in contrast to the former

[43] We use the *stata* iterated lasso command presented in (Ahrens et al., 2018), in which the penalization is computed iteratively from the data.

assumption of a zero ATE for the t-statistic. The first line in panel (b) provides the p-value associated with the use of asymptotic distribution (a standard normal variable) and the p-value associated with randomized inference obtained after 10,000 of permutation within our strata. As can be seen from the table, the results for randomized inference and the asymptotic distribution are very similar.[44] This does not come as a surprise for registered jobseekers for which Figure 3 show clear differences. However, for non-registered jobseekers we expected substantial differences in the two p-values. The similarity of p-values using the asymptotic distribution or randomized inference tend to show that although differences detected for non-registered jobseekers only appear at the top of the distribution, they are not related to outliers.

Last, we examine the robustness of the main results to the inclusion of covariates in panel (c). We present results of the estimation of equation 12 when we add covariates following the Belloni procedure as described above. Over the 43 additional variables we consider, very few of them are selected. Depending on the outcome variable, the lasso procedure selects the number of vacancies posted and the number of hires in permanent contracts in 2014 before the experiment; the number of hires in fixed-term contracts, as well as two sector dummy variables. There is almost no impact of adding the additional covariates either on the estimated coefficients or on their standard errors.

[44]We note that the p-value from randomized inference is smaller than the p-value obtained using the asymptotic distribution (it is actually outside the confidence interval calculated using 10,000 permutations). Thus using the asymptotic distribution is conservative in these cases.

Table and Figure Appendix

Table A.1: Value of parameters

a	Level of demand	1
δ	Discount factor	0.999
s	Separation rate	0.0095
μ	Efficacy of matching	0.233
η	Unemployment-elasticity of matching	0.5
γ	Real wage flexibility	0.7
c_0	Recruiting cost	0.215
α	Marginal returns to labor	0.666
ω	Steady-state real wage	0.671

Note: Parameter values are taken from **?**.

Table A.2: Standard error computation and adding covariates

Jobseekers	Registered		Non-registered	
	Hires	Workdays	Hires	Workdays
	(1)	(2)	(3)	(4)
(a) Clustered and non clustered standard errors				
Treatment	0.046**	17.522**	0.063	26.243*
	(0.021)	(8.523)	(0.040)	(15.647)
White-Huber robust SEs	0.023	9.224	0.043	16.658
(b) p-values from asymptotics and randomization inference for t-test				
p-val	0.034	0.042	0.116	0.096
p-val permutation	0.024	0.037	0.102	0.086
(c) Adding covariates				
Treatment	0.047**	17.812**	0.062	25.814
	(0.022)	(8.634)	(0.041)	(16.367)

Note: Panel (a) displays estimated coefficients from equation 1. Standard errors in parenthesis are clustered at the local employment agency level. White-Huber robust non clustered standard errors appear just below.

Panel (b) presents p-values of t=Coef./Std assuming a standard normal distribution and using permutation tests with 10,000 permutations.

Panel (c) coefficients estimated with additional covariates from equation 13. Covariates to include follows the procedure explained in section 2.2. Standard errors clustered at the agency level. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$

Table A.3: Selection on vacancy services provision

	Vacancy Prep		Prescreening and filtering				Emphasize jobseeker		Screening and post hiring	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Analysis of post	Drafting support	Preselection	Prerequisites	Verification	Private	Valorization	Evaluation	Interview support	Adaptation support
(a) Vacancies created with [column title] service										
Treatment	0.001	0.005*	0.032***	0.033***	0.028***	0.030***	0.004***	0.002*	-0.000	0.001
	(0.001)	(0.003)	(0.005)	(0.005)	(0.006)	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)
Control Mean	0.002	0.013	0.033	0.027	0.034	0.053	0.003	0.000	0.001	0.002
N	7438	7438	7438	7438	7438	7438	7438	7438	7438	7438

Note: Panel (a) shows impacts on vacancy creation with the specific service (column header) available to the counselor used to create the services indices in columns 4-7 of Table 5. Standard errors, in parentheses, are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.4: Impact on matching rates by channel over 8 weeks

	(1) Counselor	(2) Firm	(3) Jobseeker	(4) All
Treatment	-0.279	-0.858***	-2.915***	-3.917***
	(0.645)	(0.292)	(1.028)	(1.326)
Control Mean	4.869	1.136	7.409	13.705
N	1705	1705	1705	1705

Note: This table presents inversely propensity weighted (IPW) regression results for the intervention's impact on the number of applicants generated through each potential matching channel within the first eight weeks after vacancy creation with the PES.

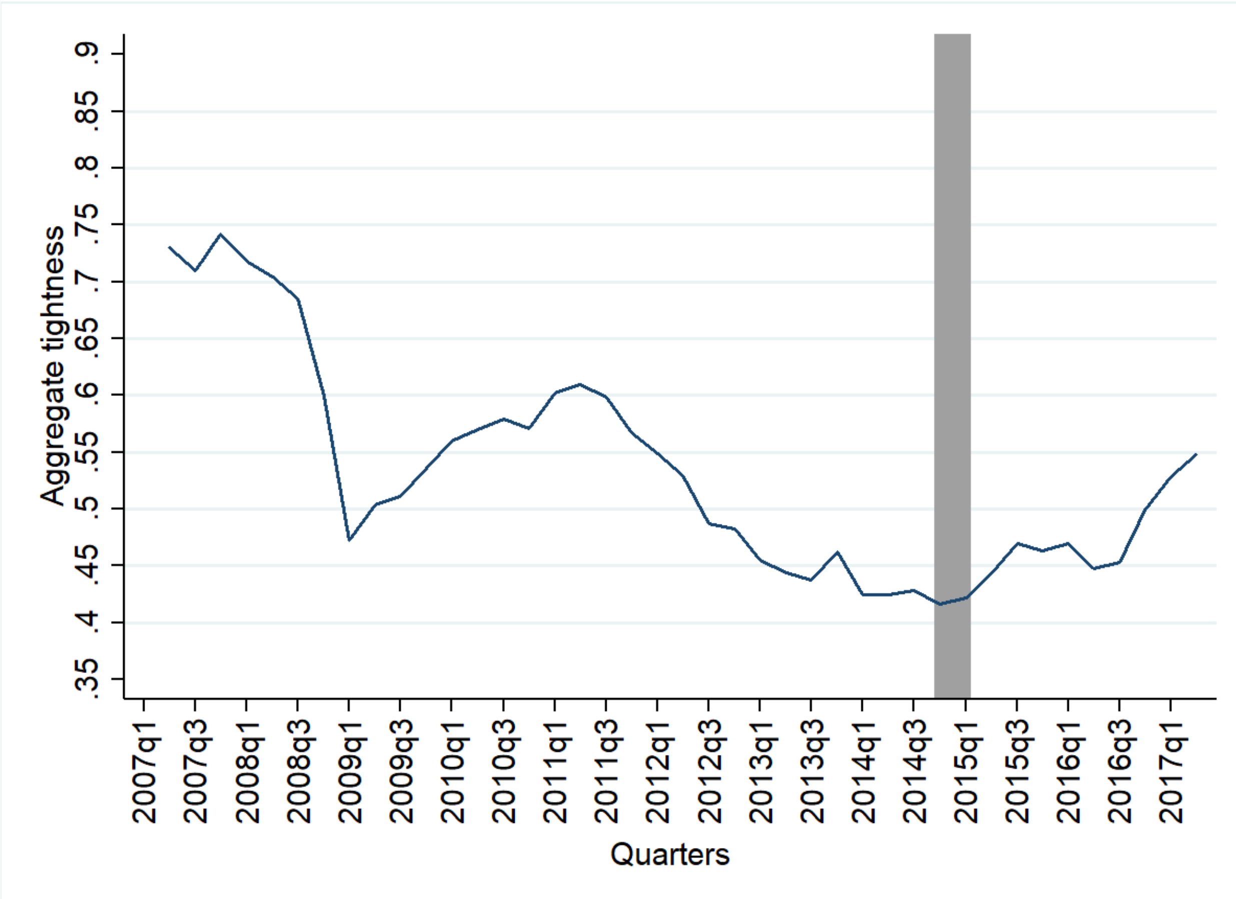
Standard errors, in parentheses, are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.5: Heterogenous impact on employment creation by previous contact with PES

Jobseekers	Registered		Non-registered		All	
	Permanent	All	Permanent	All	Permanent	All
	(1)	(2)	(3)	(4)	(5)	(6)
(a) Impact on hires						
No contact	-0.005	-0.270	0.044	-0.349	0.040	-0.619
	(0.024)	(0.186)	(0.051)	(0.641)	(0.063)	(0.718)
Contact	0.124***	0.494	0.090	1.643*	0.214*	2.138*
	(0.046)	(0.410)	(0.081)	(0.987)	(0.112)	(1.239)
Same ATE	0.021	0.067	0.662	0.098	0.219	0.057
(b) Impact on workdays						
No contact	-2.177	-13.618	20.608	10.849	18.431	-2.768
	(9.450)	(10.677)	(20.036)	(22.562)	(24.754)	(26.897)
Contact	47.817**	40.277*	34.276	36.418	82.093*	76.696
	(18.331)	(21.782)	(31.871)	(35.652)	(43.906)	(48.713)
Same ATE	0.023	0.029	0.738	0.568	0.252	0.178
(c) Impact on quality hires						
No contact	0.001	-0.017	-0.015	-0.032	-0.013	-0.051
	(0.018)	(0.023)	(0.016)	(0.020)	(0.026)	(0.034)
Contact	0.115***	0.112**	0.017	0.001	0.133**	0.109*
	(0.037)	(0.047)	(0.027)	(0.030)	(0.053)	(0.066)
Same ATE	0.007	0.012	0.346	0.378	0.026	0.033

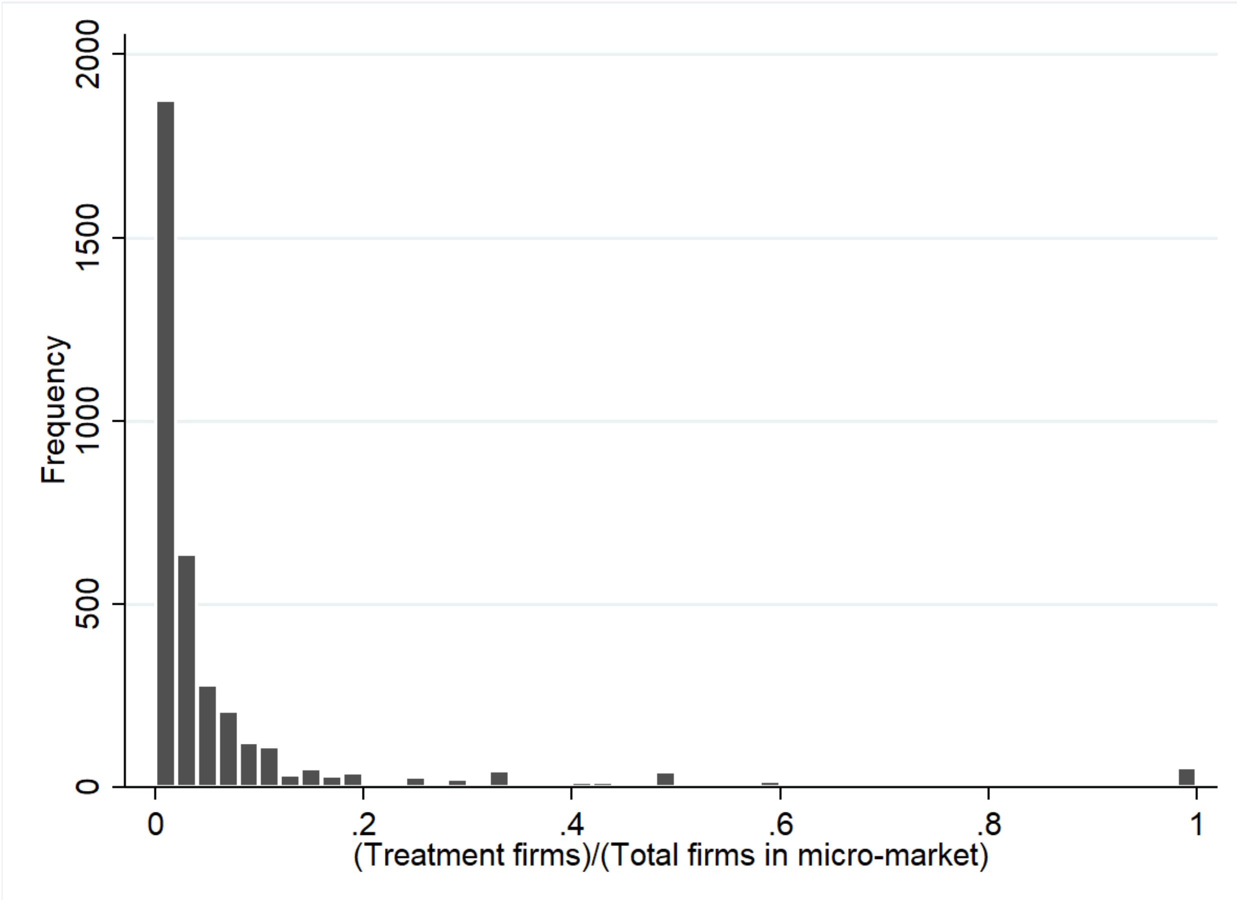
Note: Impacts depending on whether the firm had previous contact with the PES. The contact indicator is constructed using an index of baseline counselor visits to the firm, telephone exchanges and meetings at the agency. Results are obtained using equation 12 with the p-value of the test of same average treatment effects in the two sub-populations displayed below estimation results. Panel (a) displays impacts on contract flows. Panel (b) reports impacts on the number of workdays created within these contracts. Results are displayed for all hires (column (1) and (2)), hires of registered jobseekers (column (3) and (4)) and non-registered jobseekers (column (5) and (6)). Results are presented on permanent contracts and then aggregated over all contract types, denoted in column headers. Standard errors are clustered at the local employment agency level (in parentheses). * $p < .1$, ** $p < .05$, *** $p < .01$

Figure A.1: Aggregate tightness in France



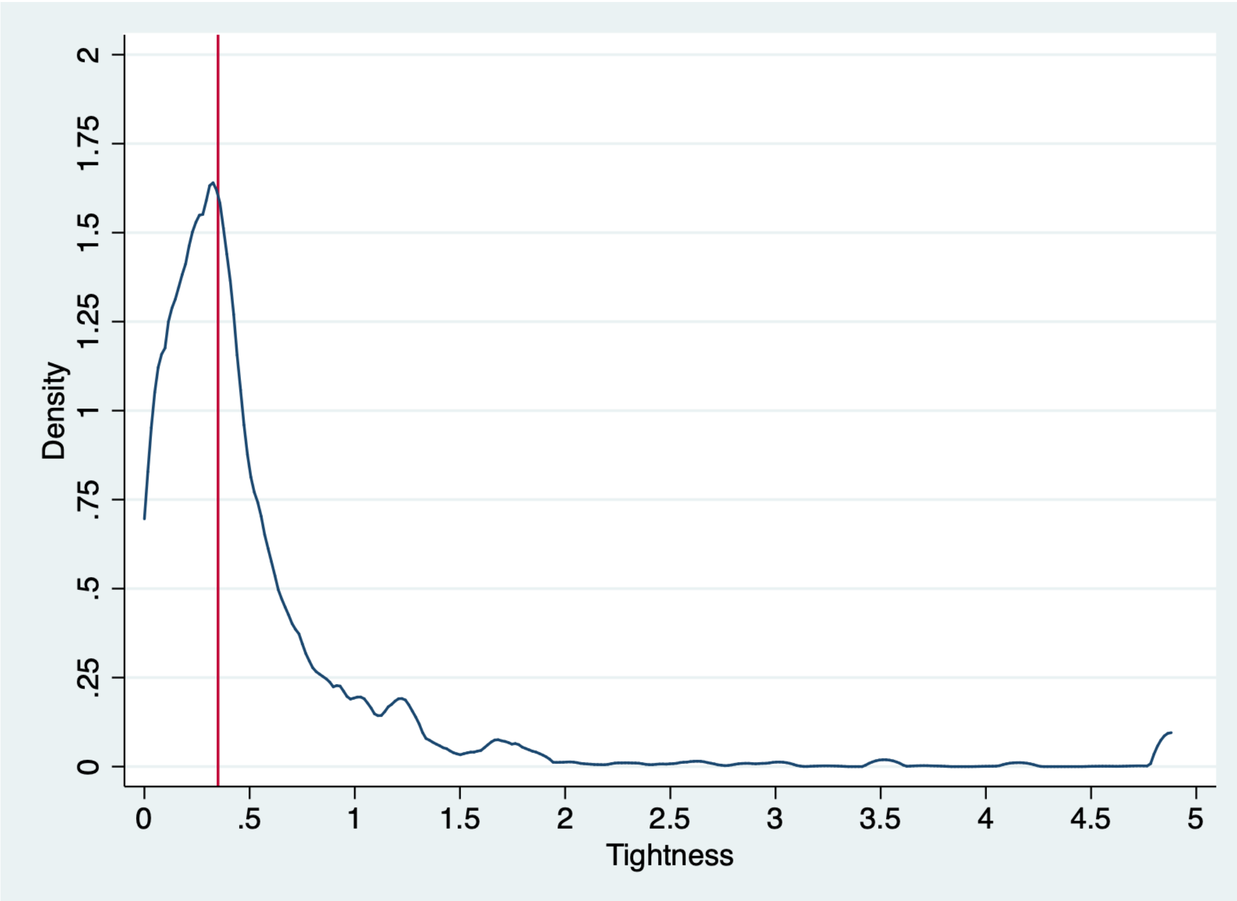
Note: The figure presents detrended aggregate tightness in the French labor market from the first quarter of 2007 to first quarter of 2017. The highlighted grey region indicates the sanctuary period of the study. Source: French Ministry or Labor (DARES). URL: https://dares.travail-emploi.gouv.fr/dares-etudes-et-statistiques/statistiques-de-a-a-z/article/les-tensions-sur-le-marche-du-travail-par-metier

Figure A.2: Proportion of experiment amoung all recruiting firms



Note: The figure illustrates variation in the proportion of treatment firms within all potentially recruiting firms in local labor markets (commute zone×sector) during the experimental period. The database of potentially recruiting firms comes from French governments open access data repository. URL: https://www.data.gouv.fr/fr/datasets/base-sirene-des-entreprises-et-de-leurs-etablissements-siren-siret/

Figure A.3: Density of tightness within experiment



Note: The figure presents the density of tightness at the commute-zone level. The vertical line corresponds to the median which is the value used to split the sample into low and high tightness sub-samples.