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# The Value of a Vacancy: Evidence from a Randomized Evaluation with Local Employment Agencies in France

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**Abstract**

This paper analyzes the impact of a large scale randomized experiment that targets firm labor demand by supporting its recruitment practices. We evaluate the effect of a Public Employment Service's (PES) intensive firm prospection campaign in which free recruitment services were proposed to a large number of small and medium sized firms. We find large impacts of this new active labor market policy: a 30% increase in vacancy postings with the PES and a 9% increase in permanent contract hires, translating into 48 more workdays created by treated firms on average over a six-month period. We highlight that the intervention does not suffer from the same displacement effects as traditional job-placement interventions which are especially high when labor demand is low. Additionally, job creation impacts are centered in slack labor markets, suggesting that firm-level displacement effects are likely minimal. We confront a simple model of firm search for candidates against data on vacancy characteristics and services delivered to these vacancies by the PES. We find non-experimental evidence that candidate prescreening may be a key component of the intervention because it reduces matching frictions associated with slack labor markets. Finally, the intervention might be significantly more cost-effective in low-demand labor markets than traditional job-placement policies that focus on jobseekers. These results suggest that active labor market policies that focus on firm labor demand may be a valuable addition to the labor policy toolkit.



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

**JEL Codes**: J23, J63, J64, J68.



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# 1 Introduction

The seminal Mortensen and Pissarides (1994) equilibrium job search and matching framework explicitly models recruitment costs as a key parameter in determining labor demand and the unemployment rate. And while firm recruitment costs have been found to be non-negligible (see the survey by Manning (2011)),[1] we still lack experimental evidence on the impact of recruitment costs on labor market outcomes (Oyer et al., 2011). Indeed active labor market policies, and the studies that try to measure their effects, have focused almost exclusively on the impacts of assisting jobseekers through training programs or job-placement policies. Evaluations have shown that these programs are generally effective (Card et al., 2015), but their gains maybe transitory because they can induce large displacement effects (Crépon et al., 2013), especially in slack labor markets. In this paper we explore whether a symmetric intervention that assists firms in their recruitment operations might have net benefits in the labor market and whether it could be more cost-effective in increasing employment.

Using a large-scale randomized experiment, we evaluate the effect of the French Public Employment Service's (PES) intensive firm prospection campaign in which free recruitment services were proposed to a large number of small and medium sized firms. Having devoted resources almost exclusively to assisting jobseekers since 2008, the PES, known as *Pôle Emploi*, revamped their firm services program for 2015.[2] The new firm services were based upon a more intensive and dynamic treatment of vacancies, most notably with the introduction of preselection, or prescreening, services to help firms more efficiently find the right candidate for the job. Delivery of these new services was to be carried out through prospection campaigns: Agency counselors were to actively study firm needs and proactively contact and offer the new recruiting services in order to improve jobseeker placement.[3]

We study the effects of these new recruitment services by randomizing which firms were prospected. 8,232 firms participated in the study with half of them prospected intensively during an 8-10 week window during late 2014. Caseworkers were then instructed to incubate and keep relations with the prospected firms until April 2015, while no proactive action

[1]Estimates of the cost of a vacancy vary from 1.5% to 11% of the wage bill depending on context and job-type with the bulk of the cost coming from the screening of applications to obtain a good match.

[2]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.

[3]In qualitative interviews conducted in the feasibility phase of the study in early 2014, local job counselors who had previously worked for the ANPE highlighted anecdotally that the loss of relations with firms since the merger was a loss to job placement opportunities.

was taken towards the control group.[4] By imposing random variation in firms expected recruiting costs we thus attempt to "shock" the value of a vacancy to the firm and examine how variation in this value affects a firm's demand for labor.

Using our most conservative estimates, we find that this shock led to a 30% increase in vacancy postings with the PES and to a 9% increase in permanent (open-ended) contract hires, translating into 48 more workdays created by treatment firms, on average over the sixth month sanctuary period. We find impacts on the intensive and extensive margins, with prospected firms significantly more likely to hire a registered jobseeker. And while we find strong impacts on vacancy creation with the PES on all firms within the sample, the impact on hires is exclusively centered on firms that were previously in contact with the PES. We thus systematically explore our results and potential mechanisms across this dimension of heterogeneity.

In testing the robustness of our results, we are able to rule out that the strong positive effects on vacancy creation and hires are being driven by intertemporal substitution, i.e. an acceleration of the recruitment process. We can also rule out that the program simply led to a vacancy substitution effect between posting mediums or a substitution across contract types: We find large impacts on real hires and show sizable net positive effects on overall employment creation using an exhaustive measure of workday creation.

We also show that these impacts are not likely to be mitigated by different types of "firm-level displacement effects." We show that the large hiring effect is almost completely centered in local labor markets with very low levels of tightness, meaning the pool of available jobseekers is large and thus firms' competition over candidates is relatively low. Additionally, our sample firms are relatively small in size and make up only a very small proportion of the local pool of firms. Thus general equilibrium effects linked to program implementation are likely to be very small. Finally, it could be that posts may be left vacant, both within and outside sample firms, as individuals move into posts created by the intervention. This would complicate the interpretation of results because it could entail a potential for zero net creation in real employment if the previous post was destroyed or left vacant. To address this, we document the origin and destination of employment flows that enter and leave the sample and show that these flows are weak and proportionate across treatment and control groups.

To structure our understanding of the results, we present a multi-channel recruitment model in which a firm's valuation of a vacancy is based not only on the profitability margins through wage and vacancy flow costs, as is standard in the literature, but also on the costly selection process available in each channel. We believe it provides insight into how a firm's

[4]Control firms were not denied service if they requested it during this "sanctuary" period.

demand for labor can shift given a shock to non-profit margins. Though non-experimental, we find suggestive empirical evidence to support the predictions of the model.[5] We find no difference in the wage profitability margin between treatment and control vacancies, but find that significant, free cost-reducing services were delivered to treatment vacancies. These services were centered on the implementation of candidate prescreening by caseworkers, which led to a reduction in firm search effort and also a drop in the average number of candidates received by the firm for examination. These results are robust to controlling for selection effects correlated with vacancy characteristics.

Though only suggestive, we present evidence that the boost in vacancy creation and hires may result from a reduction in matching frictions in the recruitment process itself.[6] Though not explicit in the model, this provides a rationale for the stronger effect of the intervention on permanent contracts rather than on fixed-term or temp hires: Screening costs for finding a good match are simply much more important for hires within open-ended contracts.[7] In addition, these results show that matching frictions linked to the screening of applicants are much stronger in weak labor markets in which employers may receive a large number of applicants when opening a vacancy.

We then combine PES survey and administrative data to estimate the cost of the intervention and compare its cost-effectiveness to a standard job-search assistance program analyzed by Behaghel et al. (2014). Under various hypotheses regarding the potential displacement effects of both programs, we find that the firm prospection intervention dominates the standard job-search assistance program in the large majority of scenarios. Using the baseline scenario, we estimate that firm prospection creates over 3 times more workdays for registered jobseekers as job-search assistance for every euro spent by the PES (5.5 vs 1.8 workdays created per 100 euros). Although this comparison says nothing about the quality of the match made under the two different policies, it highlights the potential differences in effectiveness between active labor market programs that stimulate demand versus those that rearrange the ordering of jobseekers to existing vacancies.

In sum, our paper distinguishes itself on several dimensions from the traditional literature that examines active labor market policies. To our knowledge, this is the first study to

[5]We use the data set of vacancies posted by control and treatment firms to explore impact mechanisms. Vacancies are not randomly assigned to treatment, only firms. Thus selection bias must be taken into account.

[6]Recent work by Horton (2017) shows that algorithm-generated candidate proposals improve the short-term filling rate for newly opened vacancies by expanding the pool of applicants for existing vacancies for which there is a paucity of candidates that apply for the job. In contrast, prescreening in weak labor markets may incite real vacancy creation as it limits the costly screening of many applicants.

[7]In France, permanent, or open-ended contracts, are very difficult to end by the employer without incurring large, legally imposed fines.

demonstrate a strong link between recruitment costs and labor demand and to compare the cost-effectiveness of interventions devoted to firm labor demand instead of policies that focus on improving the quality of the labor supply. Thus a key contribution for this study is not only to examine firm labor demand when we shock the underlying parameters of vacancy valuation, but also to test the effectiveness of an innovative policy that may provide significant benefits is slack labor markets.

The structure of the paper is as follows. Section 2 describes the intervention. Section 3 presents the data, the sampling and randomization strategies, as well as our baseline specification to measure average treatment effects. Section 4 presents descriptive statistics, balance checks, treatment compliance and treatment intensity. Section 5 illustrates aggregate and heterogeneous treatment effects. Section 6 reports robustness checks for substitution and displacement effects. Section 7 presents the model to test potential mechanisms and section 8 reports the cost-effectiveness analysis of the program. Section 9 concludes.

# 2 Description of the intervention and heterogeneity dimension

## 2.1 The intervention

The public employment service's new firm services or *nouvelle offre de services aux entreprises* is based on providing more comprehensive support to firms for their recruitment needs. The overall objective of the PES is to place jobseekers and in 2015, it moved towards a more balanced approach between aiding both jobseekers and firms. To accompany this renewed focus on firm relations, the PES elaborated two new services: Reinforced vacancy monitoring and follow-up (*accompagnement à l'offre*) and prospection. This reinforced vacancy monitoring included several specific services:

- Support with vacancy creation, drafting and posting
- Candidate prescreening and selection
- Interview support
- Profile promotion (spontaneous candidatures sent by counselors to firms)
- Creation of a personal online recruitment web site and access to the PES résumé bank
- Adaptation and testing periods subsidized through continued UI benefits

Counselors in the 129 participating local employment agencies were instructed to prospect the treatment firms intensively for 2 months starting on September 15th, 2014 while the control group was to be "sanctuarized" for 6 months: no proactive action was to be taken towards these firms, but they were not refused service if they requested it.[8] Agencies were required to have an “in-depth interview" with treatment firms during the intense period either through a face-to-face visit with a counselor or over the phone. During the interview, counselors were required to market the new and existing services to firms and gauge the firm’s recruitment needs. Following the intensive prospection period, agencies were instructed to continue to nurture relations with treatment firms. After this six and half month sanctuary period, agencies were free to contact and propose services to the control group.

## 2.2 Heterogeneity

Firm prospection was put into place as a vehicle to promote and present the PES recruitment services to firms that may, or may not, already have an existing relationship with the PES. Thus the intervention entailed actively engaging with two types of firms to learn about their recruiting needs and discover placement opportunities for jobseekers. We define this dimension of heterogeneity as a PES caseworker having made at least one successful phone call to the firm between January and August 2014. It is primordial to understanding the extent of the impact on our sample firms because the marginal impact of any introduction of new services provided to firms may be very different across "in-contact firms" and "no-contact firms." As we detail below, our sampling strategy ensured that we had a substantial proportion of both types of firms (36% were in contact with the PES during 8 months preceding the intervention). Hence throughout the analysis, we will systematically display aggregate impacts along with heterogeneous effects across this dimension.

# 3 Data, sampling and randomization

## 3.1 Data

We have access to rich historical administrative data from the public employment service at the firm and jobseeker level. This includes vacancies posted with the PES, their characteristics and the recruitment services provided to them. Vacancy data includes the posting date and type of contract: permanent (open-ended), fixed-term or interim (temp). We also have the posted characteristics such as the minimum annual salary, the profession, the required

[8] We explain in detail the sampling and randomization procedures below.

qualification, the minimum required experience and duration (for fixed-term contracts) and the weekly working hours. 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 initiated by the jobseeker, the firm and by the PES caseworker. Finally, we have rich data on the specific services that were provided to vacancies posted with the PES as listed above in subsection 2.1.

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 outlet for vacancies among many other actors present on the market. This is counterbalanced by our data on hires. We have an exhaustive measure of hires through legally required hiring declarations called DPAE, *Déclaration préalable à l'embauche*. All firms are required to submit a hiring declaration before, or shortly after the contract start date.[9] 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 provide us with 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 each contract created in month $t$.[10] 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 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 overall employment creation within and across contract types.

[9] Exceptions to the requirement for this hiring declaration concern internships and volunteer contracts and for the recruitment of private child care professionals and some public sector jobs. In the sampling phase, described in section 3.2, we target firms that were unlikely to make hires that do not require a declaration.

[10] 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 workdays created in January 2015.

## 3.2 Sampling and Randomization

It was important for the study's external validity that the intervention targeted firms which were representative of local agencies' firm portfolios and, at the same time, target low-tightness and low-job finding rate professions. As highlighted above, the public employment service's main goal is to assist jobseekers. Thus making sure that the intervention included low-tightness and low job-finding rate professions was an important criteria used in the selection of participating agencies.[11] The research team collaborated with the Firm Services Department at the PES to develop a sampling algorithm to target pertinent firms attached to the 129 local employment agencies chosen to participate in the study. We started by calculating labor market tightness over the 12 months preceding the randomization using jobseeker rosters and vacancy postings for each profession within each local agency. We also calculated the job-finding rate within these "micro-markets" for the same period. We then created a priority ranking of professions per agency using these two parameters as well as the stock of jobseekers registered in the agency.[12] Using a profession-sector correspondence table, we then aggregated the weights per sector and ranked them. This gave us a ranked list of sectors in which firms were most likely to recruit within the prioritized professions.

Finally, these sector identifiers were linked to local firms that had responded to the PES' annual survey *Besoin en Main d'Oeuvre* (BMO) or "Labor Needs" survey. Roughly 400,000 firms are surveyed in France each year 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 hirings declared in the survey. We sampled in BMO 2014, a survey conducted in autumn 2013 on recruitment needs for 2014. This ensured that a significant portion of sample firms would have had contact with the PES in 2014, preceding the intervention. Each agency was then given a list of "priority firms" to potentially prospect drawn out of the BMO survey (those that were at the top of the sector rankings). They were then instructed to select roughly half of the firms in the list using their own local expertise. The final agency-trimmed list was then sent back to the research team for randomization.

We stratified the final sample by indicators for the agency, if the firm intended to recruit in 2014 and by the number of employees on the firm's payroll (in four categories). We were unable to stratify by the in-contact heterogeneity dimension because we did not have access to the administrative data for contacts at the time of the sampling and randomization. Within each stratum we randomly assigned treatment with probability one-half. For strata with

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

[12]The function used to assign the weights to the professions was convex in the stock of the job seekers and concave in tightness and the job finding rate.

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.[13]

### 3.3 Empirical Specification

We follow Imbens and Rubin (2015) and measure average treatment effects as the sum of the weighted difference in means within strata,

$$\widehat{ATE} = \sum_{s=1}^{S} q_s \left(\widehat{\mu}_{1,s} - \widehat{\mu}_{0,s}\right) \tag{1}$$

where $\widehat{\mu}_{1,s} = \overline{y}^{s,T=1}$ for outcome $y$ and $\widehat{\mu}_{0,s} = \overline{y}^{s,T=0}$ and $q_s$ is equal to the sample share of observations in stratum $s$. The benefit of equation 1 is that it exploits the stratified sampling design of our study to the fullest. Section A.1 in the appendix discusses several other possible estimates.

We estimate the variance of our estimate following the influence function methodology developed in Hirano et al. (2003). This allows us to cluster the standard errors at the local employment agency level (see appendix section A.2 for details) to account for correlation in outcomes among firms attached to the same agency.

## 4 Balance, sample description and compliance

### 4.1 Balance and sample description

Table 1 shows distribution statistics and balance checks for the final 8,232 firms retained in our sample. We also show these statistics and balance check estimates across our dimension of heterogeneity for the 7,859 firms for which we have within-stratum variation in baseline contact with the PES. Each row presents the weighted control group mean and the treatment group difference as defined in 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 and create an indicator for the sum being larger than zero.

Across the board we see treatment coefficients close to zero and insignificant at the 10% level for all but four specifications out of a total of 69 regressions, indicating that the stratified randomization was successful.

[13] For the analysis, these single-firm strata are reabsorbed into the closest stratum based on size, local agency and 2014 recruitment in order to have a minimum of 4 firms per stratum.

Examining the baseline characteristics of firms, we see that 72% of firms have less than 26 full time employees and that they are predominantly in the service (42%) and commerce sectors (27%) while manufacturing and construction make up 27% of the sample. 50% of firms hired someone in a short-term contract (1 day to 6 months in duration) and 43% hired at least one employee in a permanent contract during this time period. Yet, relatively few firms post vacancies with the PES compared to the proportion that hire. For example, only 9% of firms posted a permanent contract vacancy with the PES over the eight month pre-intervention period.

The final column of Table 1 formally tests the difference between in-contact and no-contact firms using a logistic regression that includes all the shown variables as explanatory variables. We see that in-contact firms are larger in size, but are similar to no-contact firms on the other dimensions of firm characteristics that we measure. In contrast, in-contact firms also display larger vacancy posting and hiring rates and, unsurprisingly, they receive visits and emails, and benefit from existing PES services at a significantly higher rate.

## 4.2 Compliance and treatment intensity

Figure 1 plots the monthly cumulative evolution for visits, phone calls, candidate promotion and emails sent to firms 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 offered the improved 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 key strategy of the PES was the promotion, by counselors, of spontaneous candidatures adapted to firm needs.[14] We consider this a form of compliance that demonstrates the implication of the counselors in the intervention. Again we see 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 trajectory of the control group. As highlighted above, firms were free to contact the PES and request recruitment services and accordingly we do not observe a sudden change in the evolution of the control group trends:

[14]Spontaneous candidate promotion is defined as a counselor presenting a résumé to an employer in absence of a declared need or vacancy.

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 agencies 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 given to control group firms after this date. This is not the case and thus permits us to explore whether effects persist over time and has implications for the cost-effectiveness analysis below.

# 5 Impacts

## 5.1 Vacancy and hiring flows

Table 2 displays results for the average treatment effect on flows for each type of contract during the sanctuary period: September 15th 2014 - March 31st 2015 for vacancies and September 15th - April 30th for hires.[15] We top-code all vacancy and hiring variables at the 99th percentile of their distribution.[16] Panel A displays impacts on the whole sample, while Panel B exhibits heterogeneous impacts using our in-contact indicator. All estimations come from equation 1 and standard errors are clustered at the local employment agency level. For all regressions we show the strata weighted control mean of the dependent variable, in order to gauge effect magnitudes, and the p-value for a test of the equality of the average treatment effect between in-contact and no-contact firms. In examining columns 1-3 of Panel A, we see that prospection and the promotion of free recruitment services leads to large increases in vacancy flows posted with the PES. On average, treatment firms posted 0.064 and 0.048 more job offers for open-ended and fixed-term contracts, respectively, a 30% increase off the baseline mean for both types of contracts. During this period, we see no significant impact on the posting of temp work vacancies. Across all contract types (column 4) the intervention led to an increase of 0.11 vacancies posted with the PES, an increase of around 19% when compared to the control mean.

Column 5 in panel A of Table 2 shows that this increase in vacancy posting is accompanied

[15]We include hires in April to capture hiring processes that were started in March.

[16]Table A.1 in the appendix presents results using non top-coded data. Effect sizes are even larger with non-top-coded dependent variables. We believe it is important to show these effects in the appendix because this is administrative data so the data points in the upper distribution are unlikely to be errors. In addition, the PES naturally tries to create and improve relations with "large-recruiters," what they call *les grands comptes*. Thus, prospection of these firms could have also led to increased vacancy and hiring flows in these firms. Nevertheless, we prefer to display our most conservative estimates in the main text.

by an impact on hiring flows in permanent contracts. Treatment firms create 0.12 additional open-ended contracts, on average, equivalent to an increase of 8.8%. In contrast, we see non-significant point estimates for fixed-term and temp hires.[17] When looking over hires in all contract types the point estimate is positive, but insignificant.

Panel B of Table 2 displays results for the heterogeneity analysis. Firms that had previous contact with employment agencies in the months leading up to the intervention drive a significant portion of the effect. Though both types of firms significantly increase their vacancy postings with the PES, we see impact estimates for in-contact firms in columns 1-3 that are about twice the size as those for no-contact firms. The relative percentage change is also larger for in-contact firms (25.2% ) compared to no-contact firms (17.6%) when looking over all vacancies, though we cannot formally reject the hypothesis that the average treatment effect is equal between the two types of firms at a reasonable significance threshold (p-value of 0.195). But these heterogeneous effects are most striking when we look at hiring flows. We see in column 5 of panel B that the estimated effect on the whole sample is centered entirely on in-contact firms. Firms who had previous relations with the PES see a 24% increase in permanent contract hires. Even with the loss in statistical precision by splitting the sample along this dimension, this estimate is significant at the 1% level. When summing over all contract types we find a positive effect (column 8): in-contact firms create almost 3 more contracts than their control counterparts. For firms with no previous contact, the estimated impact on permanent contract flows is much smaller and insignificant. Considering all contracts, we estimate a small and negative coefficient for no-contact firms with the hypothesis of the equality in the effect between the two types of firms being rejected at the 5% level.

The evidence in Table 2 strongly suggests that the program generated substantial vacancy postings with the PES and that a substantial portion of these postings were vacancies that would not have been created in absence of the program. We can make this inference because we also see large impacts on real hires. Thus, the impact of the program cannot be reduced to a simple vacancy substitution effect whereby firms could have either duplicated their postings that would have been posted elsewhere anyway, or simply moved away from their existing posting medium to the PES.

[17] The substantially higher level of flows for fixed-term and temp contracts per firm as seen in the control means are due primarily to very short and mostly one-day contracts in which the same person may be hired multiple times by a firm in a short period of time. We address this in our analysis of workday creation.

## 5.2 Employment creation

Even though we see strong positive impacts on permanent hire contracts, it is insufficient to solely examine contract flows to determine the magnitude of job creation. It could be that the treatment leads to a substitution between contract types. For example, if the treatment leads to more permanent contracts and less fixed-term or temp work contracts being emitted by the firm, contract flows are inappropriate to measure real employment effects. So in order to estimate the impact on real employment creation we sum the workdays created within and across contract types using the start and end dates of the contract available in the hiring declarations.[18] This allows us to compare total employment creation between treatment and control firms.

Table 3 provides evidence on employment creation using workdays. We group the estimates from the model by type of jobseeker. Registered jobseekers are defined as individuals who were registered with the PES within the last 30 days of the contract start date. We consider hires who were not registered with the PES within the last 30 days or for whom we have no personal identifiers in the hiring declaration data as non-registered jobseekers. For these jobseeker types, we display results for employment creation within permanent contracts and aggregated across all contracts (omitting the specific results for fixed-term and temp contracts). Descriptively, we see that control group firms created, on average, 838 workdays in contracts that started during the sanctuary period of which 526 days were in permanent contracts. Interestingly, the majority of this employment was created for non-registered individuals (523 days versus 315 days for registered jobseekers). This illustrates the fact that much of the employment creation, destruction and turnover in the job market happens outside of the PES' purview: those either entering the labor market or engaging in on-the-job search.

Panel A of Table 3 provides strong evidence that the treatment increased net job creation. On average prospected firms create 48 more workdays in permanent contracts than control firms, an estimate that is significant at the 5% level. As can be seen in column 2 in Panel A, this effect leads treated firms to create 33 more days of employment on average when aggregating over all contract types, though due to the large additional variance induced by including short-term contracts this estimate is not statistically significant. The heterogeneity analysis in Panel B shows the striking differential effect between in-contact and non-contact firms: all of the strong positive impact on employment creation is centered on firms who were already in contact with the PES prior to the beginning of the intervention. On average,

[18] As described in section 3, we impute the end-date of contracts that terminate after the study period or that are open-ended (i.e. permanent and some temp contracts) as January 31st, 2016, the end of the observation period.

treated in-contact firms created 155 more work days than their control equivalents while we see no effect on no-contact firms. We measure a negative average effect for the hires of registered jobseekers as can be seen in columns 3 and 4 of panel B, with the effect on workdays in all contract types being significant at the 10% level.[19] All-in-all, across panel B we strongly reject the null hypothesis that impacts on employment creation are equal between in-contact and no-contact firms for all models except for employment creation for non-registered jobseekers.

In comparing columns 3 and 5 of panel B in Table 3, we also see that the strong, positive impact on workday creation for in-contact firms is roughly evenly split between registered and non-registered jobseekers. The ratio of these point estimates is $64.7/76.9 \approx .84$ and when we compare this to the ratio of control means for these firms $220.6/383.4 \approx .58$ it provides strong evidence that the intensive prospection campaign led not only to more hires, but a shift to hiring more registered jobseekers. Examining the effect on the distribution of hiring flows provides further evidence of this move towards hiring more registered jobseekers.

### 5.3 Impacts on distribution of hires

Overall, treatment firms are 1.7 percentage points more likely to make at least one hire in a permanent contract (significant at the 10% level) compared to the control group. And as discussed above, this impact on the extensive margin is also driven by the in-contact firms in our sample. Looking at the left-hand side graph in the second row of column a of Figure 2, we see that this increase on the extensive margin is driven by a massive increase in the probability to recruit registered jobseekers. Each bar represents the treatment impact on the probability to make at least the given number of hires as denoted by the horizontal axis. We overlay the bars with 95% confidence intervals and also highlight the quantiles of the underlying distribution of permanent contract hires with vertical red lines. We also systematically report the p-value for rank-sum tests of the equality of distributions of hires between groups (Mann-Whitney tests). We see large treatment impacts on the probability to hire at least one, two, three, four or five registered jobseekers for in-contact firms. Specifically, the treatment is associated with a 5.8 percentage point increase to hire at least one registered jobseeker for these firms. In addition to showing the stark differences in impact across our dimension of heterogeneity, these figures provide a key robustness check: They show that the impact on mean hires is not being driven exclusively by firms on the far right-hand side of the hiring distribution. Though only 25% of firms make at least one permanent contract hire, a large part of the impact is centered on the extensive margin of hiring registered jobseekers.

[19]This negative result may be indicative of the way counselors approached firms that were previously unknown to them and we will return to this result in the mechanisms section.

This is an important result: the intervention permits the unemployed to get back to work in a stable contract.

Finally, Figure 2 also provides evidence that the intervention could have also incentivized an increases in permanent contract hires for relatively large recruiting firms. Interestingly, this effect on the intensive margin is driven by the hiring of non-registered job seekers among in-contact firms as can be seen in the graph in the third row of column a. This suggests that, in addition to triggering effects on the extensive margin, that prospection may also have incentivized relatively large scale recruiters to shift their recruitment distribution a bit to the right.[20]

# 6 Robustness

## 6.1 Simply intertemporal substitution?

In order to rule out the possibility that impacts are simply an artifact of intertemporal substitution in which the treatment causes a simple acceleration of an existing or future recruitment process which would have happened anyway, we also explore cumulative vacancy and hiring flows over the entire 16.5 month observation period, allowing us to examine impacts for an additional 10 months after the end of the sanctuary period. Figure 3 illustrates these cumulative impacts on vacancies (first row) and employment creation (second row) in which bars represent the average treatment effect. Impacts are displayed for the whole sample and across our in-contact heterogeneity dimension for each month with bars overlaid with 95% confidence intervals. In examining vacancy creation with the PES we see that the difference in vacancy creation, for permanent contracts and for all types of contracts, remains positive 16.5 months after the start of the intervention. Again, the graphs underline how the persistent impact is driven by in-contact firms, with impacts relatively stable after the end of the sanctuary period, March 31st, 2015. For no-contact firms the effect on vacancy posting with the PES is much more transitory: we see the positive effects of vacancy creation during the September-March sanctuary period which then returns to a non-significant effect close to zero. This suggests that treatment, no-contact firms posted less vacancies with the PES than their control counterparts in the 10 months following the sanctuary period. This result informs the evolution of the cumulative effect on employment creation.

As with vacancies there is strong persistence in the number of workdays created by

[20] One possible reason for this could be the increased public exposure received when a vacancy is posted with the PES. Treatment firms are thus more exposed to applicants engaging in on-the-job search. Unfortunately, we cannot test this hypothesis directly.

treatment firms in permanent contracts.[21] The effect we detect is stable and statistically significant 10 months after the end of the sanctuary period (31 January 2016). This effect is clearly driven by the impact on in-contact firms. In comparing the cumulative effect on permanent contracts with the effect across all contract types we also see that workday creation in all contract types remains large and statistically significant (+168 workdays, p-val=0.02) for in-contact firms and is driven almost exclusively by creation in open-ended contracts.

These graphs show that the strong employment creation effects are not simply the result of intertemporal substitution whereby PES counselors simply incited the firm to recruit in the present rather than at a later date. For in-contact firms we measure a strong positive bump in real job creation during the treatment period that appears to remain stable in time (perhaps even increasing in time). On the other hand, treatment firms that did not have previous relations with the PES appear to actually reduce the number of vacancies and hire less after having been treated by the PES compared to their control group. This reflects the negative effect on the hires of registered jobseekers for these types of firms seen in Panel B of Table 3. In the mechanisms section below, we highlight this result and suggest that this may be evidence of the difficulty the PES had in effectively treating these types of firms.

## 6.2 Displacement?

Indeed, a motivation of this paper is to study an active labor market policy that may circumvent the potential congestion effects of programs aimed at jobseekers in which "queue jumping" externalities make it so that these policies only change the order in which vacant posts are filled, not actual employment levels. We have thus far provided evidence that an active labor market policy aimed at stimulating labor demand can actually create employment opportunities and thus potentially overcome these potential displacement effects related to supply-side interventions.

Yet a shift in the firm's demand curve may also be associated with a second form of displacement effects at the firm-level, whereby firms compete to hire the best candidate and the intervention changes not only labor demand, but also the ordering of access to candidates. Nevertheless, we believe that in this case these types of congestion effects do not hinder the interpretation of the large, positive hiring effect in equilibrium. Contrary to a shift in the labor supply, displacement effects between firms associated with a shift in labor demand

[21] As a reminder, the number of workdays created in month $t$ is the total number of workdays over the entire period ranging from the contract start date to end date or to January 31st, 2016 (the end of the observation period) for permanent and fixed-term contracts that end after January 31st, 2016. See section 3 for further details.

should be small in *slack* labor markets, a point made by Crépon et al. (2013).[22] It thus follows that if the hiring effects that we have presented are centered in local labor markets where tightness is indeed low, we can be confident that the average treatment effect holds even when general equilibrium effects are taken into account.

Figure 4 provides a formal test of the relationship between the hiring effect and tightness by plotting the marginal effect at varying levels of the local, sector-level tightness faced by the firm. Tightness is calculated using the stock of registered jobseekers on the day before the start of the intervention as well as vacancies posted with the PES in the three preceding months. It provides variation in tightness at the sector-commuting zone level and concerns 74 sectors and 85 commuting zones (*bassin d'emploi*). Results come from an OLS regression where the treatment is interacted with tightness and its square. We see that the average treatment effect is driven by firms that were hiring in very *slack* labor markets.[23] The effect is large at very low levels of tightness but quickly decreases, becoming insignificant and close to zero at levels of tightness above 0.4.[24]

The fact that employment creation is centered in low-tightness segments of local labor markets provides strong evidence that firm-level displacement effects are minimal or non-existent in this context. In addition, the share of firms involved in our experiment at the local market level is quite small. Sample firms make up only 1% of agencies' portfolio of local firms, on average. It follows that the treatment firms would have very little influence on aggregate labor market outcomes. Indeed in returning to Figure 3, we see that even when aggregating permanent contract flows over the entire 16.5 month observation period (15 Sept. 2014 - 31 Jan. 2016), a positive impact persists. Thus if treatment firms were displacing control firms during the sanctuary period, we might expect this difference to return to zero (or at least drop in magnitude) when aggregating over the longer period.

There is also another potential type of externality linked to job destruction that may be present and cause us to over-estimate the effect on aggregate job creation. It could be the

[22]Their simple theoretical framework clearly shows that displacement effects associated with a shift in the labor supply are quite large in weak labor markets, i.e. markets in which the equilibrium tightness is low. The intervention we consider in this paper instead consists in shifting the labor *demand* curve to the right. A simple intuition for this effect is also given in Michaillat (2012) who highlights that when tightness is low, firms have a plethora of choices among candidates to fill their posts and thus suffer less from recruitment competition by other firms.

[23]Overall French labor market tightness at the end of 2014 was at it lowest level since 2000 with an average value of 0.4 compared to the 0.6 long run average and the 0.8 high reached in 2000. See http://dares.travail-emploi.gouv.fr/IMG/pdf/2016-012.pdf for more details (in French).

[24]For ease of visual inspection we present the effect at tightness levels above the 95 percentile in appendix Figure A.1. We also see a potentially positive effect at around the 99th percentile, but we believe that the local markets with this level of tightness my be poorly measured. Notably they include the hotel and restaurant sector which emits a very large number of short term contracts, thus inflating the number of vacancies.

case that the impacts that we see are, in part, due to the movement of personnel from control firms to treatment firms (or vice versa) or within groups, thus affecting the internal validity of our results. If this were the case we would over estimate the benefits of the intervention in equilibrium because we do not observe employment destruction. To address this, we provide evidence on the flows of hires between groups by tagging the starting and ending situation of the recruited person in Table A.2 in the appendix. Row titles correspond to the origin of the hired individual, column titles to where the hired person was placed and in which type of contract. We thus categorize all flows between sample firms, but also those individuals coming in- and going out of our sample firms during the sanctuary period. The proportions are displayed above the number of total flows for each type of jobseeker.

Reassuringly, we find that there are relatively inconsequential flows between our sample firms. They represent roughly 3% of all flows for permanent contracts, and are almost all rehires or change of contracts within the same firm. We also measure almost no flows to and from other firms, and this regardless of the treatment status of the firm. In terms of flows coming into the sample, a large proportion come from unemployment i.e. registered jobseekers, thus there is no issue of employment destruction elsewhere. We also note a significant proportion of contract flows for whom we do not know the origin or who were possibly employed elsewhere. Unfortunately we are unable to obtain a clear picture of what these flows represent. Thus it is possible that some of these individuals are leaving vacant posts behind them as they move into employment in our sample.[25] Yet even if some posts become vacant outside of our sample due to these "on-the-job search" related transfers into our sample firms, it would only imply an overestimation of aggregate employment creation if firms destroyed the newly vacant job. On the contrary, the estimated average treatment effect is valid if the firm where the person was previously keeps the post open and hires someone new in their place. And as we have seen with the effect being centered in slack labor markets, these firms should have a plethora of candidates to choose from.

In sum, the evidence presented in this section supports the premise that our estimated parameters truly measure the impact on treated firms, and, perhaps more importantly, that firm level congestion effects are minimal, at most, due to the fact that impacts are almost exclusively centered in low-tightness labor markets. This doesn't imply, however, that potential displacement effects would not become an issue in the case of a large scale-up of the policy in which firms in tight labor markets were heavily prospected.

[25] As described in section 3 , the hiring declaration data only contains personal identifiers for the individual if they were registered with the PES at least once in the preceding three years before the hiring date.

# 7 Potential Mechanisms

We have thus far shown that the intervention led to unambiguously large impacts on firm vacancy posting with the PES and this was accompanied by a substantial increase in permanent contract hires, leading to an increase in the number of workdays created by treatment firms as compared to control firms. This increase in employment creation was, however, only observed for firms that had a previous relationship with the PES. We do not detect any impact on employment creation for firms which were not previously in contact with the PES. In this section we exploit non-experimental evidence using rich administrative data on the 2,052 permanent contract vacancies posted with the PES during the 6 month sanctuary period and confront it with a simple theoretical model in an effort to elucidate some important potential mechanisms driving the experimental results. We underscore that this part of the empirical analysis is non-experimental and therefore provides only suggestive evidence. 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 on the efficiency of vacancy filling. It is also important to highlight another limitation of the data. As described above, the PES is only one way in which firms may post vacancies and generate candidates. Hence we do not observe the behavior of the firm, the vacancies posted or contacts received outside of the PES. Nevertheless, we believe that focusing on vacancies posted with the PES provides considerable insight on some of the the underlying mechanisms driving the experimental results.

## 7.1 Service provision to vacancies

We begin with an exploration of the PES services that were delivered to sample vacancies. These vacancy services target different parts of the recruitment process and Table 4 shows results from simple OLS regressions of indicators for the delivery of the ensemble of services that the PES can provide to vacancies, as noted in the column titles, on a dummy variable indicating whether the vacancy comes from a treatment firm.

Looking at column 1 of Table 4, we find that treatment status is highly correlated with vacancies being tagged for intensive follow-up support. On average, treatment vacancies are 40% more likely to receive intensive follow-up support (known as *offre en accompagnement*) within the agencies, an increase of around 11.3 percentage points off the baseline mean. The counselor initiated act of categorizing the vacancy for follow-up support effectively opens the door to apply the whole gamut of new services. Looking at columns 2-4 we see that the tagging for follow-up support entailed an almost systematic implementation of preselection services and that this prescreening involved two additional key services: special preselection and verification. Special preselection involves working with the employer to establish specific criteria, or, a maximum of 5 prerequisites, on which to prescreen candidates that are sent to the employer for an interview. Verification entails that a maximum of 5 to 10 candidates per post are sent to the employer and that the way in which the candidates apply is appropriate. For example, the PES might recommend that the firms choose to have applicants apply only through the counselor. In addition, verification requires counselors to negotiate a time frame with the firm for the delivery of the applicants and ways in which to adapt the vacancy if there is an insufficient number of applicants. Interestingly we find that the service of valorization, in which counselors put special effort into highlighting specific jobseekers assets and abilities is not widely used. We see a small control group mean and no difference between the groups of vacancies.

In examining the remaining columns in Table 4 we find very low levels of provision of other service and no significant differences between treatment and control vacancies. We see no difference in the provision of services that might reduce costs associated with creating and drafting a vacancy appropriately (columns 7 and 8). Nor do we find that counselors applied their services after the preselection phase: we see nothing in terms of interview support (column 9) nor the implementation of helping jobseekers adapt to the job (column 10). It appears thus that treatment is associated almost exclusively with services that involve search and screening assistance and, as a result, the model's contribution will be to incorporate these parameters in the firm's valuation of a vacancy.

## 7.2 A model of firm recruitment choice

In reality, a vacancy posted with the PES can receive applicants through three main channels depending on the firm's preferences: (1) jobseekers may apply directly to the firm or only through the PES counselor. This caseworker can then filter candidates and decide whether to send them onto the firm. If the vacancy is not publicly posted,[26] then (2) the caseworker is solely responsible for both generating and prescreening applicants. Lastly, (3) the firm itself can search for candidates in the PES résumé bank and contact candidates directly, or through the PES caseworker.

We consider that each vacancy requires a specific skill set and workers have heterogeneous skills so that they are an imperfect quality match to the job. We decompose the hiring process into several steps including the search for candidates, their screening, interviews and hiring. Following the contextual description, we introduce three channels through which applications

[26] The majority of vacancies are posted online (see the control mean in column 1 of Panel A in Table A.6 in the appendix)

are made and these channels may or may not involve prescreening. The firm maximizes its valuation of a vacancy over these channels with respect to its recruitment effort and hiring threshold.

As we will illustrate, the model predicts unambiguously that, under preselection treatment, more vacancies will be posted and that these vacancies will receive less applicants due to reduced firm effort and the fact that jobseekers are more stringently filtered by the PES. It also predicts an ambiguous effect on the number of applicants coming from PES counselors because of the trade-off between increased counselor effort to generate candidates and the prescreening that they implement. Finally, the effect on hires will also be ambiguous because firms reduce their own search effort and become more picky even as they post more vacancies.

#### 7.2.1 Setup

The firm has an opportunity to produce output $y$ during a period of time that ends at an instantaneous rate $s$. For this production it offers a reference wage $w$. The value to the firm of this activity is $v = (y - w)/(r + s)$ where $r$ is the discount rate. The firm must recruit somebody to realize the production and there are three channels through which candidates arrive: (1) jobseekers apply on their own directly to the firm or through the PES, (2) a PES counselor generates candidates and sends them to the firm, and (3) the firm expends effort to search on its own. The arrival rate is $\delta$ for jobseekers, $\mu$ for the caseworker channel and $e$ for the firm which incurs a cost for its search effort $c(e)$. The candidate and caseworker channels are free. The firm may decide however to only use its own channel ($d = 0$) or to also consider applicants arriving through the caseworker and jobseeker channels ($d = 1$).

The firm looks for different skills and has imperfect knowledge of the labor market, meaning candidates in all channels are more or less suitable to the needs of the firm. Hired jobseekers can provide the firm an instantaneous profit $t \times v$, with $t$ being an applicant specific random draw from a uniform distribution over $[1 - 1/\gamma, 1]$.[27] This captures the idea that the job requires certain skills and that jobseeker skills are an imperfect match. This also implies that it might be difficult to find employees willing to do the job for the targeted wage $w$.

Under preselection, jobseeker applications are no longer direct and are filtered by the caseworker. With the implementation of preselection, candidates arriving through the PES (either through $\delta$ or $\mu$) are drawn from the same distribution but only the $\theta$ top quantile, $t > 1 - 1/\gamma + \theta/\gamma$, get through. We assume that the firm always prescreens candidates. Once received in an interview the characteristic $t$ is revealed to the firm. This is a final screening

[27] $\gamma > 1$, the larger $\gamma$ is the larger the range of skills of potential candidates.

phase with cost $\kappa$ in all channels. After interviewing a candidate, the firm decides whether to hire. This decision is based on the characteristic $t$ being above a threshold $\bar{t}$.

- Absent the program, jobseekers arrive at rate $\mu_0$ and $\delta$ under the caseworker and jobseeker channels respectively, and are only prescreened under the firm channel. The firm decides the optimal search effort $e_0^\star$ and selects applicants with skills above $t_0^\star$.
- With the intervention, jobseekers arrive at a rate $\mu_1$ and $\delta$ under the caseworker and jobseeker channels and are prescreened under *all three channels*. The firm makes optimal search effort $e_1^\star$ and selects applicants with skills above $t_1^\star$.

The value of a vacancy thus depends on the four dimension parameter $\nu = (v, \delta, \kappa, \gamma)$ and the decision to post a vacancy will depend on this parameter. It involves the profitability of the vacancy, but also the three other parameters. Specifically, these parameters are linked to labor market characteristics: the parameter $\delta$ measures the quality and size of the market - some job offers might be atypical and attract more or less applicants; $\kappa$ measures screening or interview costs - the firm might have needs outside the scope of its recruiting expertise hence the possibility of large screening costs; and lastly $\gamma$ measures the quality of a randomly selected applicant.

We derive the value of vacancies characterized by $\nu$ under the two regimes, with the program $\Pi(\nu, \mu_1, 1)$ and without the program $\Pi(\nu, \mu_0, 0)$. Without the intervention the firm evaluates the value of a vacancy $\Pi(\nu, \mu_0, 0)$ by optimizing with respect to its effort and hiring threshold:

$$\begin{aligned} r\Pi(\nu, \mu_0, 0) = \max_{e,\bar{t},d\in\{0,1\}} \quad & \Big\{ -c(e) + e\theta \left( \gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu, \mu_0, 0)) \tfrac{dt}{\theta} - \kappa \right) \\ & + d\,(\delta + \mu_0) \left( \gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu, \mu_0, 0))\, dt - \kappa \right) \Big\}, \end{aligned} \tag{2}$$

and with the program,

$$\begin{aligned} r\Pi(\nu, \mu_1, 1) = \max_{e,\bar{t},d\in\{0,1\}} \quad & \Big\{ -c(e) + e\theta \left( \gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu, \mu_1, 1)) \tfrac{dt}{\theta} - \kappa \right) \\ & + d\,(\delta + \mu_1)\,\theta \left( \gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu, \mu_1, 1)) \tfrac{dt}{\theta} - \kappa \right) \Big\}. \end{aligned} \tag{3}$$

The following propositions highlights the key results:

**Propositions**:

1. *The value of a vacancy always increases when the intervention is implemented as long as $\mu_1 \geq \mu_0$: $\Pi(\nu, \mu_1, 1) > \Pi(\nu, \mu_0, 0)$.*

2. *Firms always use caseworker and jobseeker channels when the intervention is implemented:* $d_1^\star = 1$.

3. *Firms reduce their search effort and are more picky in their choice of candidates:* $e_1^\star \leq e_0^\star$ *and* $t_1^\star \geq t_0^\star$.

4. *The value function under preselection is increasing in* $v$, $\delta$, $\gamma$ *and decreasing in* $\kappa$.

*Proofs*: See appendix section A.3.

## 7.3 Predictions

We further assume that opportunities arrive at a rate $\lambda$ per unit of time and that they are drawn from a distribution with density $\phi(\nu)$ and that there is a fixed cost $F$ in posting a vacancy.[28]

This allows us to derive expressions for the number of vacancies posted, number of applicants to these vacancies and finally, the number of hires.

### 7.3.1 Number and types of vacancies posted

Given that opening a new vacancy has a fixed cost $F$, a new opportunity $\nu$ will lead to the opening of a vacancy absent the program when $S_0(\nu) = \mathbf{1}(\Pi(\nu, \mu_0, 0) > F) = 1$ and when $S_1(\nu) = \mathbf{1}(\Pi(\nu, \mu_1, 1) > F) = 1$ when the program is implemented. Because $\Pi(\nu, \mu_1, 1) > \Pi(\nu, \mu_0, 0)$, all opportunities such that $\Pi(\nu, \mu_1, 1) > F > \Pi(\nu, \mu_0, 0)$ will be opened as a result of program implementation. Therefore the flow of new vacancies opened by the firm, $N_v(\mu_1, F, 1)$ under the program and $N_v(\mu_0, F, 0)$ absent the program can be expressed as,

$$\begin{aligned} N_v(\mu_1, F, 1) &= \lambda P_\nu \left(S_1(\nu)\right) \\ N_v(\mu_0, F, 0) &= \lambda P_\nu \left(S_0(\nu)\right) \end{aligned} \tag{4}$$

and we have

$$N_v(\mu_1, F, 1) - N_v(\mu_0, F, 0) = \lambda P_\nu \Big( \Pi(\nu, \mu_1, 1) > F > \Pi(\nu, \mu_0, 0) \Big) > 0.$$

[28] There is a condition for the value of the vacancy to be positive which writes as $v(1 - 1/(2\gamma)) > \kappa\theta$. Because we'll assume a fixed cost for a vacancy to be posted, we assume the condition is always satisfied.

One important dimension is that there are several margins on which the number of vacancies is increasing and new vacancies are not necessarily vacancies of smaller profitability. Consider an opportunity $\nu_0 = v_0, \delta_0, \kappa_0, \gamma_0$ at the margin absent the program, i.e. $\Pi(\nu_0, \mu_0, 0) = F$ in conjunction with Proposition 4. We can describe how these margins are affected

- Profitability margin: less profitable vacancies are posted. $v_1$ such that $\Pi(v_1, \delta_0, \kappa_0, \gamma_0, \mu_1, 1) = F$, satisfies $v_1 < v_0$.
- Jobseeker arrival rate margin: vacancies that will naturally receive less candidates are posted. $\delta_1$ such that $\Pi(v_0, \delta_1, \kappa_0, \gamma_0, \mu_1, 1) = F$, satisfies $\delta_1 < \delta_0$.
- Skills assessment cost margin: vacancies with larger screening costs are posted. $\kappa_1$ such that $\Pi(v_0, \delta_0, \kappa_1, \gamma_0, \mu_1, 1) = F$, satisfies $\kappa_1 > \kappa_0$.
- Skills signaling margin: vacancies that attract more heterogeneous applicants are posted. $\gamma_1$ such that $\Pi(v_0, \delta_0, \kappa_0, \gamma_1, \mu_1, 1) = F$, satisfies $\gamma_1 < \gamma_0$.

We now explore the selection of vacancies posted by firms in the treatment and control groups. It is impossible to get complete measures of all of the underlying parameters thus we are not able to fully document the selection issue. Rather, we rely on key vacancy characteristics recorded in the PES administrative data: the minimum wage offered, hours, the skill and experience requirements as well as the occupation. Our results show that the sets of vacancies posted by firms in the treatment and control groups are not the same, but we do not find evidence that these differences are related to differences in profitability. This suggests that differences in the sets of vacancies in the two groups are more related to the labor market parameters $\delta$, $\kappa$ and $\gamma$. To document this point, we simply run OLS regressions of vacancy characteristics on a treatment indicator.

Table 5 presents these results. In columns 1-3 we look at a key job search parameter, the posted wage.[29] 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. The main motivation behind using this prediction is that it proxies

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

for the output linked to the job and thus the underlying components of its profitability.[30] Again we see no reasonable selection effects on the predicted wage. Column 3 shows results of regressing the difference between the real posted wage and the predicted value on our treatment indicator. Again, we see small and insignificant coefficients indicating that wage determinants between treatment and control vacancies are similar. In addition, we see no significant differences when splitting the population by our in-contact indicator. In sum, we cannot provide credible evidence that vacancies in the treatment group were selected for posting (or not) based on a wage profitability margin.

In contrast we see in columns 4-9 of Table 5 that treatment vacancies posted by in-contact firms require less experience, less qualification and more hours worked per week. On aggregate, we only see significant correlation between treatment status and the type of required qualification posted. Treatment vacancies are 11 percentage points more likely to be jobs requiring lower skills.[31] It is difficult to link these differences to the three other parameters characterizing vacancies in our model. However, we could consider that low skill jobs without experience are jobs for which the relevant skills might be difficult to read and might correspond to a lower $\gamma$. Similarly we observe that, for in-contact firms, treatment vacancies more often require hours above the 35 hour legal limit. One interpretation in line with the model would be that this corresponds to jobs with atypical hours that are therefore less easy to fill (smaller $\delta$).

In the rest of this section, when comparing vacancies in treatment and control group, we will account for these differences in characteristics using inverse probability weighted regressions (IPW). We do not believe this fully solves the selection bias, but we believe this makes the comparisons more meaningful. We apply this method to first test the robustness of the service delivery results. Appendix Table A.3 replicates results from Table 4 using IPW regressions.[32] We find that the IPW results are very similar to our simple regression results.

[30]Formally, the predicted wage takes the form

$$\widehat{w} = \hat{a} + \sum_{h=2}^{8} \hat{H}_h + \sum_{e=2}^{6} \hat{E}_e + \sum_{p=2}^{140} \hat{P}_p + \sum_{q=2}^{9} \hat{Q}_q + \sum_{p=2}^{95} \sum_{q=2}^{9} \hat{I}_{pq} \qquad (5)$$

Where $\widehat{w}$ is the predicted log of the wage posted and $\widehat{a}$ is a the predicted constant. $\hat{H}_h, \hat{E}_e, \hat{P}_p$ and $\hat{Q}_q$ are predicted coefficients on dummy variables for the weekly hours, experience, profession and qualification categories, respectively, and $\hat{I}_{pq}$ are the coefficients on the interaction of the (profession $\times$ qualification) indicators from an OLS regression. We then apply these estimated parameters to predict the wage within our sample of vacancies.

[31]We define low qualification as laborers, production workers and unqualified employees. High qualification jobs are defined as supervisors, technicians and management.

[32]See table A.3 notes for details on the specification.

### 7.3.2 Number of applications

The model allows us to obtain the instantaneous probability that an application arrives through a channel on opportunity $\nu$. The average number of applications for vacancies posted by firms in the treatment and control groups directly follows and we summarize these average predicted flows in Table 6. Given these expressions we clearly observe that the model predicts a reduction in the average number of applications to treatment firm vacancies through two of the channels: a smaller application rate generated by the firm itself as $e_1^\star < e_0^\star$ while jobseeker applications are subject to caseworker screening ($\delta\theta$). The only potential positive impact passes through an increase in counselor effort $\mu_1$, but this increase is counterbalanced by the preselection of the best applicants. Moreover these predictions also hold in the presence of selection bias. For example, vacancies posted at the margin have lower jobseeker application rates, all else being equal.

Table 7 reports the difference in application rates between firms in the treatment and control groups using IPW regressions.[33] The table shows the number of applications per vacancy along the three channels (employer, caseworker and jobseeker) during the two weeks following the posting date of the vacancy.[34] We see that treatment vacancies receive significantly fewer applicants through the firm and jobseeker channels and that these represent very large percent changes off the control mean. There are about 2.4 less jobseeker initiated matches in the treatment group of of a baseline of 6.5 for the control group. Employers make roughly 0.78 less potential matches off an average of 1.14 in the control group. Interestingly, we see no treatment effect on mean applications coming from the counselors themselves, but it appears that, though insignificantly different (with a p-value of 0.21), treatment vacancies for in-contact firms had a lower mean application rate through $\mu$ while the no-contact firm rate was higher.

This empirical evidence provides support for the model's predictions. Using expressions in Table 6 for firm and caseworker effort ratios and prescreening intensity, we now apply the IPW estimates from Table 7. Results are reported in Table 8. For the application rate ratio through the employer channel in treatment and control groups, we obtain $E(e_1^\star)/E(e_0^\star) \approx 0.31$. Using the application rate ratio through the jobseeker channel in treatment and control groups, we obtain an estimate for $\theta E(\delta)/E(\delta) \approx 0.63$. Lastly, if we consider the ratio of caseworker effort we get a ratio of $\mu_1/\mu_0 \approx 1.5$. Thus we observe evidence that the intervention led to recruitment conditions in which $e_1^\star \leq e_0^\star$, $\mu_1 > \mu_0$ and $\theta < 1$. Put into

[33]See Table 7 notes for details. Appendix Table A.4 shows that unweighted regression results are very similar to the IPW estimates.

[34]Appendix Table A.5 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.

words, this implies that firms reduced their own search effort, caseworkers increased their effort and that there was a significant increase in the level of prescreening in the jobseeker and counselor channels. In line with these results, column 1 in Appendix Table A.6 shows that treatment vacancies are about 8 percentage points less likely to be posted publicly PES website. As noted above, when the vacancy is not posted publicly, the caseworker alone is responsible for generating and screening applicants. This interpretation is also supported by column 1 in Table A.7 which shows that counselors dramatically intensified their vacancy monitoring.

It must be said, that our ability to use the model to derive the ratios of effort and prescreening rates relies on strong assumptions. Indeed, we would not be able to identify the caseworker channel ratio if we allowed the screening rate to be channel dependent. However, we believe these results are highly illustrative. They provide strong evidence that prescreening took place and (given that the prescreening rate does not differ substantially by channel) that there was significant substitution between firm and caseworker search effort.

#### 7.3.3 Number of hires

To close discussion of the model we now turn to its prediction on hiring rates. The instantaneous probability of a hire on a specific opportunity $\nu$ both with and without the program can be formalized as,

$$\begin{aligned} P(Hire|\nu,\mu_1,1) &= (e_1^\star + \mu_1 + \delta)\,\gamma(1-t_1^\star)S_1(\nu) \\ P(Hire|\nu,\mu_0,0) &= (e_0^\star + d_0^\star(\mu_0 + \delta))\,\gamma(1-t_0^\star)S_0(\nu) \end{aligned} \tag{6}$$

and we can also derive the corresponding number of hires at the firm level:

$$\begin{aligned} N_h(\theta,\mu_1,1) &= \lambda E_\nu\left[(e_1^\star + \mu_1 + \delta)\,\gamma(1-t_1^\star)S_1(\nu)\right] \\ N_h(\theta,\mu_0,0) &= \lambda E_\nu\left[(e_0^\star + d_0^\star(\mu_0 + \delta))\,\gamma(1-t_0^\star)S_0(\nu)\right]. \end{aligned} \tag{7}$$

Clearly the impact on hires is ambiguous. The initial positive effect of a broader set of vacancies posted ($S_1 \geq S_0$) and the increase in caseworkers' effort ($\mu_1 > \mu_0$) is counterbalanced by the reduction in firm search effort $e_1^\star < e_0^\star$ and also by its more selective behavior $t_1^\star > t_0^\star$.

In light of these predictions, we now discuss the divergent impacts on hires across our contact heterogeneity dimension. Recall that we have shown large and significant increase in workday creation for registered jobseekers in in-contact firms, but that the point estimate is small and negative for no-contact firms. One possibility could be that the program is simply implemented differently for the two sets of firms. Table 8 presents the estimates of effort ratios and the screening parameter for the two different types of firms. As can be seen in the table, the underlying parameters we estimate are broadly identical between in- and no-contact firms. We see small differences overall, albeit with slightly less prescreening, and higher caseworker and firm effort ratios for no-contact firms. Hence, it does not appear that differences in implementation alone are the driving factors for the difference in hiring impacts between the two groups, though these small differences would be coherent with the differential hiring effect.

Another possible explanation could be that the distribution of vacancies in the two sets of firms is different. Looking again at Table 5 we see that indeed the distributions are not the same. Vacancies opened by in-contact firms require less experience, lower qualification and have atypical working hours. It is unclear how these specific characteristics relate to the hiring impact. To answer this question we would need to know for which vacancies the intervention leads firms to reduce their effort the most or for which they are more picky about applicants. Unfortunately, we are unable to detail the actual candidate hiring threshold $t^{\star}$. However, we see that, at the very least, the firm effort ratio $E(e_1^{\star})/E(e_0^{\star})$ is similar between the two types of firms, suggesting that the difference in vacancy characteristics may not be the main factor driving the differential impact.

The differential hiring effect between in-contact and no-contact firms could simply be that the program was not implemented effectively for the no-contact firms, despite the PES' best efforts and we are unable to fully capture this with the estimated screening rates and effort ratios. Indeed it may be difficult for PES counselors to effectively prospect and prescreen for firms they are just getting to know or who have not previously needed PES services. This could explain the negative effect on registered jobseeker hires within no-contact firms seen in Panel B of Table 3 and on the evolution of the overall cumulative vacancy and employment creation effect examined in Figure 3. During the 6 and half month treatment period we see moderate increases in vacancies posted with the PES by no-contact firms, but these impacts tend to zero or become negative in the following 10 months. This means that no-contact treatment firms refrained from posting their vacancies with the PES after having been treated. This interpretation is reinforced if we examine employment creation for registered versus non-registered jobseekers in each type of firm. Figure 5 breaks down the evolution of employment creation by type of jobseeker. We immediately see the growing negative effect on employment creation for registered jobseekers within no-contact firms. This effect grows sharply in magnitude during the treatment period and becomes statistically significant when we aggregate workdays created for jobseekers in all contract types. In contrast we see positive point estimates of employment creation for non-registered jobseekers in no-contact firms that remains stable and positive throughout the observation period.

These results are striking and highlight two things. First, no-contact firms may have benefited from the increased public vacancy exposure provided through the PES. Second, and perhaps more importantly, PES counselors may have been unable to propose adequate profiles to no-contact firms. Recall that we have shown that all treated firms substituted counselor effort for their own effort in the recruitment process. If counselors propose candidates that are a "bad fit" for the job (despite intense effort) this could lead to less registered jobseekers being hired because the firm's normal effort and returns to search for registered jobseekers have been, in part, replaced by counselor effort. And this could have had lasting consequences for these no-contact firms in terms of the recruitment of the unemployed coming from the PES.[35] It may be that the distribution of skills the no-contact firms have access to through the PES might have little overlap with the set of skills it needs. Indeed, this could have also reinforced a stigma associated with the quality of PES candidates promoted by counselors. This might be the reason the firm was not in contact with the PES in the first place and speaks to the challenge that the PES faces in creating a productive relationship with certain types of firms, given the PES' primary responsibility of helping find employment for those most in need.

# 8 Cost-effectiveness estimates

Since this paper provides the first experimental evidence on the impacts of a demand-side active labor market policy, we find it useful to provide a simple cost-effectiveness comparison between this intervention and a standard job-search assistance program directed at jobseekers.

The PES evaluation service conducted an exhaustive qualitative survey of the agency managers and counselors that participated in the prospection campaign. In this survey, managers were asked to report the man-hours that were devoted to the intervention from mid September, 2014 to mid November 2015, i.e. the intensive treatment period.[36] Importantly, these reports included the counselors who actively engaged with the firms and the counselors working in support, dedicated to the search and prescreening of jobseekers. Thus we have a rough estimate of the marginal effort put into the program by counselors. We do not include (nor could we) time spent by agency managers or the cost of the PES infrastucture because we are interested in the marginal cost-benefit change in the time allocation of counselor activity. We have this data for 124 agencies (out of 129) and we impute the 5 missing

[35]We do not interpret this as a crowding out effect i.e. in-contact firms had access to the best registered jobseekers, because control, non-contact firms would have had faced the same problem.

[36]The agency managers are responsible for setting and recording the schedules of their counselors in half-day units called "plages" which are equal to 3.5 hours.

agencies' data using the agency sample average. Using these declarations combined with the latest estimate of a PES counselor's daily cost we are able to generate an estimate of the amount spent on average per treated firm.

To account for the possibility of firm-level displacement effects we provide three scenarios for the cost-benefit analysis using the workday creation estimate at 12 months.[37] A low scenario in which we consider that only the workdays created for registered jobseekers are valid, an intermediate scenario where we include the effect for registered jobseekers and half of the days created for non-registered jobseekers. And finally a high scenario in which we use the total average treatment effect. In addition we provide three scenarios in terms of effort the counselors put into treatment firms after the intensive treatment period: 0%, 10% or 25% of the effort expended during the intensive phase. We do this because we only observe the hours put into the firms during the intensive phase while the sanctuary period lasted until the end of March 2015. As can be seen in Figure 1, it appears that interaction with treatment firms is highly isolated to the intensive period. Increases in visits, calls and emails to treatment firms appears to have ceased by the end of November. Candidature promotion appears to have slightly evolved into the new year, but it is clear that vast the majority of the "action" was punctual in nature. Thus we believe that assuming 10% continuing effort is already quite conservative.

As a comparison, we use the estimate of the cost effectiveness of a normal jobseeker counseling for 12 months presented in Behaghel et al. (2014). This program studied newly registered jobseekers identified by the counselor as "at risk of long term unemployment." Similar to the firm services and prospection intervention, this program lasted for 6 months with jobseekers and counselors meeting on a weekly basis. It was a demanding program in terms of counselor time: to complete the objective of the weekly meeting, counselor portfolios were limited to 30 jobseekers, whereas a standard counselor may have up to 300. This program was offered by both the public and the private sector and we use the estimate of the public sector service because it was the most effective.

The authors estimated that this job search program induced an extra 20 days off the PES registers per 660 euros spent per jobseeker, on average. This number however includes any type of exit and their study highlights that exits to employment represent only 60% of total exits. For instance, the other types of exit from the PES roster may be due to automatic registration cancellations, transitions to training programs, entrepreneurship, illness, etc.[38] In addition, displacement effects could be a major concern. As noted above, Crépon et al.

[37] As discussed in section 6.2, we find workday creation effects for both registered and non-registered jobseekers. Though we find very little flows between firms in our sample (see Table A.2), some of these non-registered jobseekers may come from employment in other firms, possibly destroying existing matches.

[38] Jobseekers are requred to re-register with the PES each month to stay on the rosters.

(2013) show that although job search interventions may have positive impacts on the job finding rate of registered jobseekers within an experimental sample, once displacement effects are accounted for, the impacts may be null. Hence, we also provide three scenarios for the job search program: A high scenario in which we ignore displacement effects and count all days not registered as days of work, an intermediate scenario where we ignore potential displacement effects, but only count 60% as real workday creation, i.e. exit to employment. And finally, a low scenario where, due to displacement effects, the program does not create any additional employment on aggregate.

Table 9 presents results of the comparison. We see that when comparing within work creation hypotheses the firm prospection intervention dominates the standard job search assistance program in the vast majority of the the scenarios presented. We start with a focus on job creation for jobseekers registered with the PES. In column "Low" under the "Firm Prospection" program we see that 8 additional days of employment were created for registered jobseekers in the treatment group at 12 months. In our baseline estimation this corresponds to 5.5 days of work created for every 100 euros spent by the PES. This estimate strictly dominates the low, intermediate and high scenarios under "Job Search Assistance." In terms of job creation for registered jobseekers, the improvement in cost-benefit of firm prospection over job search assistance varies from "infinitely better" in the case of full displacement effects between jobseekers to 205% (5.5 vs 1.8) in the intermediate case with no displacement effects and 60% real job placement, to 83% (5.5 vs 3) when we assume all time off the roster is job placement and there are no displacement effects.

In the case where we also take into account that firm prospection and recrutement services also increased hires of non-registered jobseekers, the difference is even more striking. For example, in the intermediate hypothesis for both programs we estimate that firm prospection creates over 7 times more workdays as job search assistance for every 100 euros spent by the PSE (12.8 vs 1.8 workdays created). Finally, when we assume that significant effort (25% of the intensive period effort) was put forth by counselors for the remainder of the sanctuary period and only look at days created for registered jobseekers and compare this to the job search assistance in which all days off the PES roster is true employment creation - and abstract away from displacement effects - the job search assistance program could be more cost-effective (2.6 vs 3 days/100 euros). All things considered, Table 9 provides evidence that a firm-level intervention such as the one studied in this paper may be significantly more cost-effective in creating employment.

Given that these cost estimates are based on declarations and are most likely imprecise in both interventions, we do not want to put too much emphasis on the difference in magnitudes. On the contrary we do emphasize the point that, when strong displacement effects exist under typical job search programs, services directed at firms are almost certainly more cost-effective when it comes to real employment creation on aggregate, especially in weak labor markets. To be clear, this says nothing about the quality of the match made, but only about the difference in real employment creation between active labor market programs that stimulate demand versus those that rearrange the ordering of jobseekers to existing vacancies.

# 9 Conclusion

We present evidence on the impacts of an intervention that targets firm labor demand by supporting its recruitment practices. We study the effect of a Public Employment Service's (PES) intensive firm prospection campaign in which free recruitment services were proposed to thousands of firms. We find large impacts on vacancy postings, permanent contract hiring flows and the number of workdays created by firms, suggesting that active labor market policies that focus on firm labor demand may have large positive effects. And because of the existence of displacement externalities, they may be significantly more cost-effective than traditional active labor market policies that are based on job-search assistance.

To understand potential mechanisms driving the effect, we examine the characteristics of the job vacancies that were created by sample firms and find that treatment vacancies were subject to much higher levels of candidate prescreening by the PES, but do not differ from vacancies created by control firms on typical wage profitability margins. We develop a multi-channel firm search model to better explore this finding and confront it with empirical evidence on the way vacancies were handled by the PES and find suggestive evidence that the delivery of these additional preselection services may indeed be the key component of the intervention: It transfers search and screening costs away from the firm, but also pushes the firm to put forth less search effort and also become more picky about who it hires, leading to a potentially ambiguous effect on hires. This reflects the strong heterogeneous effects in the experimental results. Indeed, we find impacts on vacancy creation across all types of firms, but only firms that had a previous relationship with the PES increase their hiring rates. These result suggest that the public employment service's relationship with firms, in light of their primary responsibility to place marginalized jobseekers, is an important area for future research.

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# Tables

Table 1: Balance check and descriptive statistics

| | Total | | Contact= 1 | | Contact= 0 | | Logit |
|---|---|---|---|---|---|---|---|
| | (1) Control Mean | (2) Treatment | (3) Control Mean | (4) Treatment | (5) Control Mean | (6) Treatment | (7) Contact |
| Heterogeneity | | | | | | | |
| Contact with PES | 0.357 | -0.003 | | | | | |
| | | (0.009) | | | | | |
| Firm Characteristics | | | | | | | |
| $\leq$ 10 employees | 0.402 | -0.001 | 0.301 | 0.004 | 0.463 | -0.003 | -0.623*** |
| | | (0.001) | | (0.003) | | (0.002) | (0.108) |
| $>$ 10 & $\leq$ 25 employees | 0.324 | 0.004 | 0.330 | 0.002 | 0.320 | 0.004 | -0.381*** |
| | | (0.005) | | (0.010) | | (0.006) | (0.099) |
| $>$ 25 & $\leq$ 50 employees | 0.169 | -0.003 | 0.210 | -0.003 | 0.145 | -0.003 | -0.195** |
| | | (0.005) | | (0.010) | | (0.006) | (0.099) |
| $>$ 50 employees | 0.106 | 0.000 | 0.158 | -0.002 | 0.072 | 0.002 | |
| | | (0.001) | | (0.004) | | (0.002) | |
| Manufacturing | 0.113 | -0.004 | 0.108 | 0.004 | 0.114 | -0.007 | -0.032 |
| | | (0.006) | | (0.011) | | (0.008) | (0.165) |
| Construction | 0.157 | 0.002 | 0.180 | -0.020 | 0.150 | 0.007 | 0.145 |
| | | (0.007) | | (0.015) | | (0.008) | (0.160) |
| Commerce | 0.269 | -0.010 | 0.250 | -0.004 | 0.278 | -0.002 | -0.043 |
| | | (0.008) | | (0.016) | | (0.011) | (0.135) |
| Service | 0.418 | 0.008 | 0.420 | 0.015 | 0.416 | 0.001 | -0.035 |
| | | (0.010) | | (0.021) | | (0.012) | (0.131) |
| Other sectors | 0.043 | 0.004 | 0.043 | 0.005 | 0.042 | 0.001 | |
| | | (0.005) | | (0.009) | | (0.006) | |
| Hires by contract type | | | | | | | |
| Fixed-term $<$ 6 months | 0.500 | -0.005 | 0.593 | -0.010 | 0.443 | -0.001 | 0.330*** |
| | | (0.011) | | (0.021) | | (0.014) | (0.051) |
| Fixed-term $\geq$ 6 months | 0.157 | 0.004 | 0.209 | -0.002 | 0.128 | 0.006 | 0.184*** |
| | | (0.007) | | (0.015) | | (0.009) | (0.067) |
| Permanent | 0.434 | 0.010 | 0.517 | 0.015 | 0.385 | 0.003 | 0.247*** |
| | | (0.011) | | (0.019) | | (0.014) | (0.061) |
| Temporary | 0.220 | -0.006 | 0.275 | -0.007 | 0.194 | -0.006 | 0.111 |
| | | (0.008) | | (0.015) | | (0.010) | (0.080) |
| Vacancies posted at PE | | | | | | | |
| Fixed-term | 0.073 | -0.003 | 0.132 | -0.021* | 0.039 | 0.002 | 0.302** |
| | | (0.006) | | (0.013) | | (0.007) | (0.127) |
| Permanent | 0.087 | -0.008 | 0.137 | -0.009 | 0.058 | -0.008 | 0.187 |
| | | (0.005) | | (0.012) | | (0.005) | (0.135) |
| Temporary | 0.107 | 0.002 | 0.138 | 0.002 | 0.088 | 0.003 | -0.153 |
| | | (0.006) | | (0.011) | | (0.007) | (0.125) |
| Contact with PE | | | | | | | |
| Emails | 0.218 | 0.008 | 0.351 | 0.015 | 0.137 | 0.004 | 0.862*** |
| | | (0.008) | | (0.016) | | (0.009) | (0.103) |
| Visits | 0.060 | 0.009* | 0.103 | 0.006 | 0.037 | 0.012** | 0.628*** |
| | | (0.005) | | (0.013) | | (0.005) | (0.127) |
| PE services | | | | | | | |
| Jobseeker initiated match | 0.147 | -0.008 | 0.200 | -0.004 | 0.112 | -0.007 | -0.251** |
| | | (0.007) | | (0.014) | | (0.008) | (0.126) |
| Counselor initiated match | 0.177 | -0.009 | 0.266 | -0.019 | 0.120 | -0.005 | 0.271** |
| | | (0.007) | | (0.015) | | (0.008) | (0.127) |
| Employer initiated match | 0.022 | 0.002 | 0.031 | -0.002 | 0.015 | 0.007* | -0.238 |
| | | (0.003) | | (0.006) | | (0.003) | (0.174) |
| Successful match | 0.057 | -0.001 | 0.098 | -0.002 | 0.031 | -0.002 | 0.279** |
| | | (0.005) | | (0.010) | | (0.005) | (0.140) |
| Spontaneous candidature | 0.013 | -0.001 | 0.022 | 0.003 | 0.008 | -0.003 | 0.489** |
| | | (0.003) | | (0.006) | | (0.003) | (0.236) |
| N | | 8232 | | 2686 | | 5173 | 8232 |

Note: Rows display results from separate estimates of equation 1 for the given dependent variable. Columns 1 and 2 display results for all firms while columns 3-6 show results across our dimension of heterogeneity. All dependent variables are $\{0, 1\}$ indicators for which we display the weighted control mean along with the difference in the treatment group. Column 7 presents results from a logistic regression in which an indicator for being an in-contact firm is regressed on all variables in the table. Missing coefficients denote the reference categories. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

Table 2: Impact on vacancy and hiring flows

| | Vacancies | | | | Hires | | | |
|---|---|---|---|---|---|---|---|---|
| | Permanent (1) | Fixed-term (2) | Temp (3) | All (4) | Permanent (5) | Fixed-term (6) | Temp (7) | All (8) |
| Panel A: Overall | | | | | | | | |
| Treatment | 0.064*** | 0.048*** | -0.001 | 0.110*** | 0.118** | 0.296 | -0.084 | 0.331 |
| | (0.017) | (0.013) | (0.019) | (0.032) | (0.051) | (0.380) | (0.584) | (0.643) |
| Control Mean | 0.202 | 0.142 | 0.249 | 0.592 | 1.335 | 5.257 | 7.685 | 14.277 |
| N | 8232 | 8232 | 8232 | 8232 | 8232 | 8232 | 8232 | 8232 |
| Panel B: Heterogeneity | | | | | | | | |
| Contact= 1 | | | | | | | | |
| Treatment | 0.106*** | 0.077*** | 0.002 | 0.185*** | 0.366*** | 1.047 | 1.368 | 2.782** |
| | (0.036) | (0.029) | (0.038) | (0.068) | (0.124) | (0.711) | (1.182) | (1.323) |
| Contact= 0 | | | | | | | | |
| Treatment | 0.051** | 0.035** | -0.001 | 0.085** | 0.028 | -0.067 | -0.542 | -0.581 |
| | (0.021) | (0.015) | (0.025) | (0.039) | (0.057) | (0.330) | (0.713) | (0.742) |
| Control Mean(Contact= 1) | 0.291 | 0.171 | 0.271 | 0.733 | 1.530 | 6.847 | 8.561 | 16.938 |
| Control Mean(Contact= 0) | 0.159 | 0.107 | 0.216 | 0.482 | 1.213 | 4.336 | 7.227 | 12.777 |
| p-value Equality of Coefs. | 0.184 | 0.204 | 0.937 | 0.195 | 0.020 | 0.130 | 0.160 | 0.025 |
| N | 7859 | 7859 | 7859 | 7859 | 7859 | 7859 | 7859 | 7859 |

Note: This table presents impacts on vacancy postings with the PES and hiring flows for the three contract types (columns 1-3 and 5-7) as well as total flows across all contracts (columns 4 and 8) for the sanctuary period. Panel A presents average treatment effects on the whole sample while Panel B displays impacts across our heterogeneity dimension (having previous contact with the PES) along with the p-value for a test of equality of treatment effects between in-contact and no-contact firms. Only firms that have within-stratum variation in contact status are used in the heterogeneity analysis. Average treatment effects are estimated per equation 1. Strata weighted control group means are also shown. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

Table 3: Impact on workday creation

| | All Jobseekers | | Registered Jobseekers | | non-Registered Jobseekers | |
|---|---|---|---|---|---|---|
| | (1) Permanent | (2) All | (3) Permanent | (4) All | (5) Permanent | (6) All |
| Panel A: Overall | | | | | | |
| Treatment | 48.1** | 33.8 | 17.8* | 10.0 | 30.2** | 23.8 |
| | (20.4) | (23.4) | (9.4) | (10.9) | (14.6) | (17.2) |
| Control Mean | 525.6 | 837.5 | 190.4 | 314.7 | 335.1 | 522.7 |
| N | 8232 | 8232 | 8232 | 8232 | 8232 | 8232 |
| Panel B: Heterogeneity | | | | | | |
| Contact= 1 | | | | | | |
| Treatment | 141.6*** | 154.7*** | 64.7*** | 65.9** | 76.9** | 88.8** |
| | (49.5) | (55.7) | (21.6) | (26.5) | (33.9) | (37.8) |
| Contact= 0 | | | | | | |
| Treatment | 14.7 | -6.2 | -8.2 | -20.0* | 23.0 | 13.9 |
| | (22.7) | (25.1) | (9.9) | (10.6) | (17.4) | (20.4) |
| Control Mean(Contact= 1) | 604.0 | 965.4 | 220.6 | 370.4 | 383.4 | 595.0 |
| Control Mean(Contact= 0) | 477.0 | 749.3 | 171.4 | 274.2 | 305.6 | 475.1 |
| p-value Equality of Coefs. | 0.028 | 0.009 | 0.002 | 0.002 | 0.185 | 0.091 |
| N | 7859 | 7859 | 7859 | 7859 | 7859 | 7859 |

Note: This table presents impacts on workday creation within permanent contracts and over all contract types for the sanctuary period. Columns 1 and 2 present results for all hires while columns 3-4 and 5-6 display results for registered and non-registered jobseeker hires, respectively. Workdays are calculated using the start and end dates of the contract, with end dates for permanent and fixed-term contracts censored at 31 January 2016 (this concerns fixed term contracts that end after this date). Panel A presents average treatment effects on the whole sample while Panel B displays impacts across our heterogeneity dimension (having previous contact with the PES) along with the p-value for a test of equality of treatment effects between in-contact and no-contact firms. Only firms that have within-stratum variation in contact status are used in the heterogeneity analysis. Average treatment effects are estimated per equation 1. Strata weighted control group means are also shown. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

## Table 4: Selection on vacancy services provision

| | (1) Intensive Follow-up | (2) Preselection | (3) Special Preselection | (4) Verification | (5) Valorization | (6) Evaluation | (7) Analysis of post | (8) Drafting support | (9) Interview support | (10) Adaptation support |
|---|---|---|---|---|---|---|---|---|---|---|
| Panel A: Overall | | | | | | | | | | |
| Treatment | 0.113*** | 0.109*** | 0.115*** | 0.106*** | 0.008 | 0.001 | 0.007 | 0.003 | 0.003 | -0.003 |
| | (0.033) | (0.033) | (0.032) | (0.032) | (0.008) | (0.004) | (0.011) | (0.020) | (0.004) | (0.006) |
| Control Mean | 0.283 | 0.270 | 0.235 | 0.261 | 0.025 | 0.007 | 0.022 | 0.113 | 0.005 | 0.014 |
| N | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 |
| Panel B: Heterogeneity | | | | | | | | | | |
| Contact= 1 | | | | | | | | | | |
| Treatment | 0.098** | 0.092** | 0.089** | 0.093** | 0.003 | 0.001 | 0.015 | -0.005 | -0.004 | 0.000 |
| | (0.046) | (0.046) | (0.043) | (0.045) | (0.010) | (0.007) | (0.015) | (0.029) | (.) | (0.008) |
| Contact= 0 | | | | | | | | | | |
| Treatment | 0.133*** | 0.129*** | 0.148*** | 0.123*** | 0.014 | 0.002 | -0.004 | 0.012 | 0.011 | -0.006 |
| | (0.039) | (0.037) | (0.037) | (0.037) | (0.011) | (0.005) | (0.012) | (0.028) | (.) | (0.010) |
| Control Mean(Contact= 1) | 0.285 | 0.273 | 0.244 | 0.263 | 0.027 | 0.008 | 0.015 | 0.123 | 0.008 | 0.015 |
| Control Mean(Contact= 0) | 0.279 | 0.265 | 0.224 | 0.260 | 0.022 | 0.005 | 0.033 | 0.101 | 0.000 | 0.014 |
| p-value Equality of Coefs. | 0.562 | 0.534 | 0.303 | 0.599 | 0.452 | 0.856 | 0.313 | 0.680 | . | 0.621 |
| N | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 |

Note: This table presents selection effects of PES services applied to permanent contract vacancies during the sanctuary period using linear probability models. Indicator variables for service type (displayed in the column headers) are regressed on a treatment indicator. Panel A presents these effects on the whole vacancy sample while Panel B displays effects across our heterogeneity dimension (having previous contact with the PES) along with the p-value for a test of equality of selection effects between in-contact and no-contact firms. Control group means are also shown. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

Table 5: Selection on vacancy characteristics

| | Vacancy Characteristics | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) $w$ | (2) $\widehat{w}$ | (3) $w-\widehat{w}$ | (4) Experience | (5) Low Qualif. | (6) Hours | (7) Hours< 35 | (8) Hours= 35 | (9) Hours> 35 |
| Panel A: Overall | | | | | | | | | |
| Treatment | -0.032 | -0.015 | -0.008 | -0.200 | 0.106$^{***}$ | 0.291 | -0.017 | -0.018 | 0.034 |
| | (0.035) | (0.032) | (0.014) | (0.127) | (0.030) | (0.483) | (0.024) | (0.030) | (0.026) |
| Control Mean | 9.916 | 9.892 | 0.022 | 2.179 | 0.632 | 33.874 | 0.136 | 0.664 | 0.200 |
| p-value rank-sum (Mann-Whitney) | 0.405 | 0.124 | 0.887 | 0.241 | 0.000 | 0.040 | 0.267 | 0.411 | 0.067 |
| N | 1921 | 2052 | 1921 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 |
| Panel B: Heterogeneity | | | | | | | | | |
| Contact= 1 | | | | | | | | | |
| Treatment | -0.050 | -0.019 | -0.021 | -0.298$^{**}$ | 0.141$^{***}$ | 0.315 | -0.002 | -0.087$^{**}$ | 0.089$^{**}$ |
| | (0.056) | (0.049) | (0.018) | (0.147) | (0.043) | (0.718) | (0.031) | (0.042) | (0.036) |
| Contact= 0 | | | | | | | | | |
| Treatment | -0.007 | -0.011 | 0.009 | -0.073 | 0.059 | 0.258 | -0.035 | 0.072$^{*}$ | -0.037 |
| | (0.038) | (0.037) | (0.018) | (0.206) | (0.040) | (0.659) | (0.039) | (0.043) | (0.037) |
| Control Mean(Contact= 1) | 9.923 | 9.889 | 0.031 | 2.244 | 0.615 | 33.810 | 0.125 | 0.702 | 0.173 |
| Control Mean(Contact= 0) | 9.907 | 9.896 | 0.010 | 2.094 | 0.656 | 33.958 | 0.150 | 0.615 | 0.235 |
| p-value Equality of Coefs. | 0.520 | 0.895 | 0.245 | 0.373 | 0.161 | 0.953 | 0.508 | 0.008 | 0.014 |
| N | 1921 | 2052 | 1921 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 |

Note: We display characteristics for permanent contract vacancies during the sanctuary 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 (see section 7.3.1 for details on the prediction). Only 1,921 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. Panel A presents these correlations on the whole vacancy sample along with the p-value for rank-sum tests for the equality of distributions between treatment and control vacancies. Panel B displays correlations across our heterogeneity dimension (having previous contact with the PES) along with the p-value for a test of equality of coefficients between in-contact and no-contact firms. Control group means are also shown. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

Table 6: Theoretical application rates with or without the intervention

| | Firm | Counselor | Jobseeker |
|---|---|---|---|
| Application rate for a given application | | | |
| With the program | $e_1^\star\theta$ | $\mu_1\theta$ | $\delta\theta$ |
| Absent the program | $e_0^\star\theta$ | $\mu_0$ | $\delta$ |
| Average application rate for vacancies posted under the two programs | | | |
| With the program on all opened vacancies | $E_\nu\left(e_1^\star\theta \mid S_1=1\right)$ | $E_\nu\left(\mu_1\theta \mid S_1=1\right)$ | $E_\nu\left(\delta\theta \mid S_1=1\right)$ |
| With the program on vacancies opened absent the program | $E_\nu\left(e_1^\star\theta \mid S_0=1\right)$ | $E_\nu\left(\mu_1\theta \mid S_0=1\right)$ | $E_\nu\left(\delta\theta \mid S_0=1\right)$ |
| Without the program on vacancies opened absent the program | $E_\nu\left(e_0^\star\theta \mid S_0=1\right)$ | $E_\nu\left(\mu_0 \mid S_0=1\right)$ | $E_\nu\left(\delta \mid S_0=1\right)$ |

Note: The theoretical application rate derived from the model and its average are shown for each channel. $S$ indicates selection into the PES vacancy services program. $E_\nu\left(e_1^\star\theta \mid S_0=1\right)$, $E_\nu\left(\mu_1\theta \mid S_0=1\right)$ and $E_\nu\left(\delta\theta \mid S_0=1\right)$ are unobservable counterfactuals.

Table 7: Match selection at 2 weeks by channel

| | Counselor $E(\mu_1\theta) - E(\mu_0)$ (1) | Employer $E(e_1^\star\theta) - E(e_0^\star\theta)$ (2) | Jobseeker $E(\delta\theta) - E(\delta)$ (3) | Refusals by counselor (4) |
|---|---|---|---|---|
| Panel A: Overall | | | | |
| Treatment | -0.190 | -0.781*** | -2.407** | -0.058 |
| | (0.367) | (0.279) | (0.963) | (0.132) |
| Control Mean | 3.502 | 1.137 | 6.556 | 0.519 |
| p-value rank-sum (Mann-Whitney) | 0.141 | 0.036 | 0.000 | 0.540 |
| N | 1921 | 1921 | 1921 | 1921 |
| Panel B: Heterogeneity | | | | |
| Contact= 1 | | | | |
| Treatment | -0.566 | -0.927* | -2.361 | -0.242 |
| | (0.531) | (0.491) | (1.521) | (0.180) |
| Contact= 0 | | | | |
| Treatment | 0.307 | -0.584* | -2.454*** | 0.185 |
| | (0.451) | (0.304) | (0.906) | (0.151) |
| Control Mean(Contact= 1) | 3.776 | 1.320 | 6.883 | 0.642 |
| Control Mean(Contact= 0) | 3.136 | 0.894 | 6.120 | 0.355 |
| p-value Equality of Coefs. | 0.210 | 0.553 | 0.958 | 0.069 |
| N | 1921 | 1921 | 1921 | 1921 |

Note: This table presents inversely propensity weighted (IPW) regression results for the intervention's impact on the number of applicants coming through each channel. We predict vacancy selection into treatment $S$ using our observable vacancy characteristics. 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 an OLS regression of the number of applications in each channel on a treatment indicator with observations weighted by $\frac{T}{\widehat{Pr}(\mathrm{S}=1)} + \frac{1-T}{1-\widehat{Pr}(\mathrm{S}=1)}$. The p-values for Mann-Whitney tests of the equality in distributions are displayed for the overall sample of vacancies. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

Table 8: Estimates of screening rate and caseworker and firm effort ratios

| | $\widehat{\theta}$ | $\widehat{\mu_1/\mu_0}$ | $\widehat{e_1^\star/e_0^\star}$ |
|---|---|---|---|
| Overall | 0.63 | 1.52 | 0.31 |
| Contact=1 | 0.66 | 1.29 | 0.30 |
| Contact=0 | 0.60 | 1.83 | 0.35 |

Note: These estimates are calculated by applying the estimates from Table 7 to the expressions derived in Table 6.

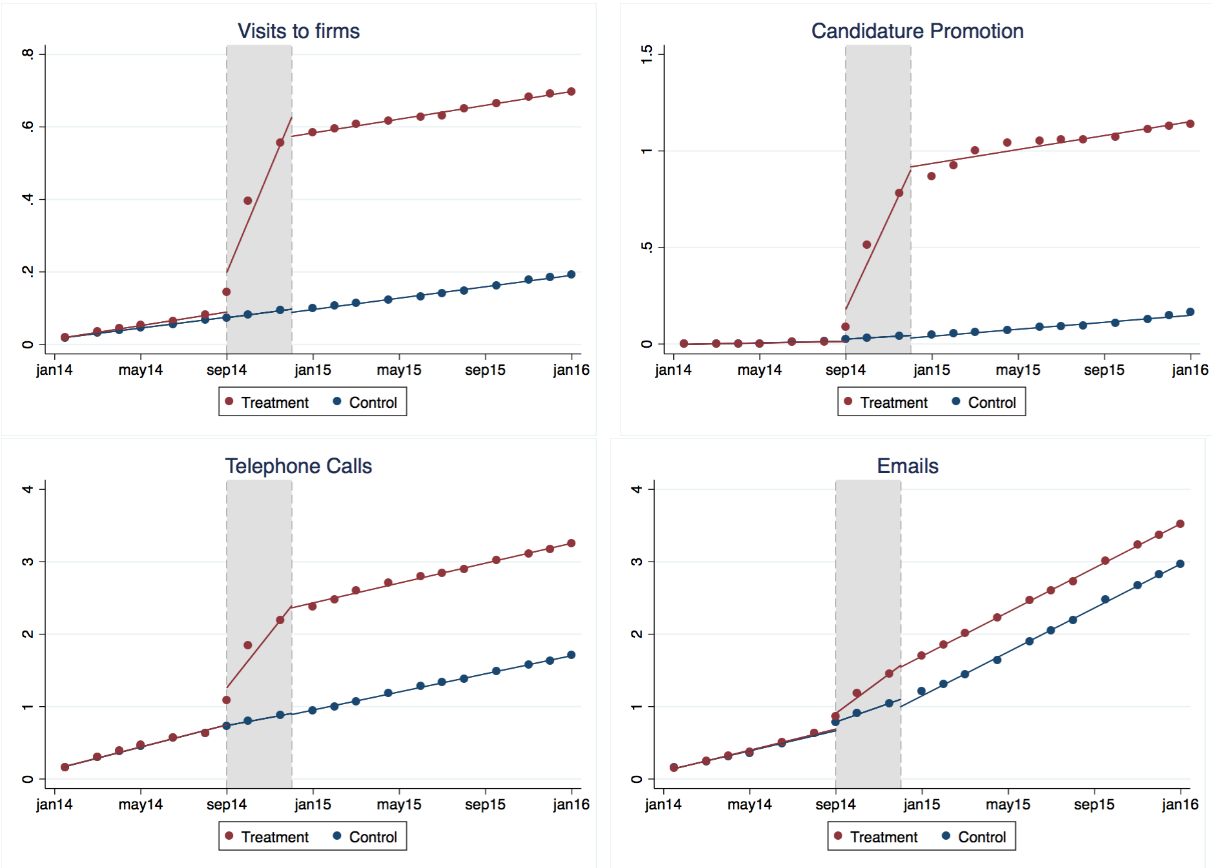
Table 9: Cost-effectiness comparison

| Program | **Job Search Assistance** | | | **Firm Prospection** | | |
|---|---|---|---|---|---|---|
| Work creation hypothesis | Low | Intermediate | High | Low | Intermediate | High |
| Days of work created at 12 mo. | 0 | 12 | 20 | 8 | 18.5 | 29 |
| Cost | | €660 per jobseeker | | | €145 per firm | |
| Days of work/100 € | 0 | 1.8 | 3 | 5.5 | 12.8 | 20 |
| With effort post intensive period: | | | | | | |
| Cost w/ sanctuary period effort=10% | | | | | €210 per firm | |
| Days of work/100 € | | | | 3.8 | 8.8 | 13.8 |
| Cost w/ sanctuary period effort=25% | | | | | €308 per firm | |
| Days of work/100 € | | | | 2.6 | 6 | 9.4 |

Note: This table presents a comparison of cost-effectivness between job search assistance program offered by the PSE and firm prospection. Refer to the text, Section 8, for a complete description of the data, assumptions and calculations.

# Figures

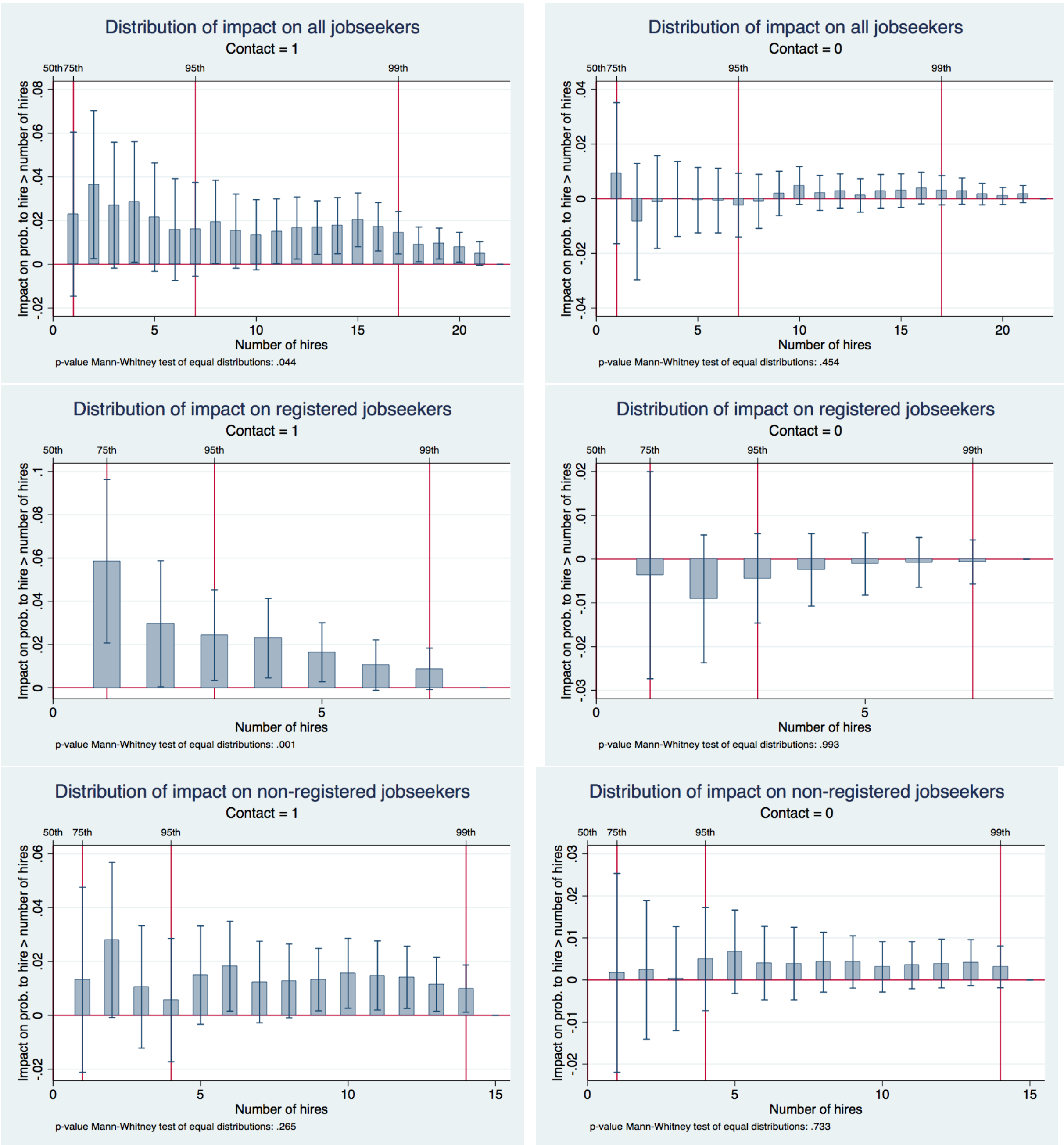
Figure 1: Compliance and treatment intensity




Note: Figures illustrate the average number of counselor initiated visits, phone calls, emails and jobseeker résumés spontaneously sent to firms. 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: Impact on the distribution of hiring flows

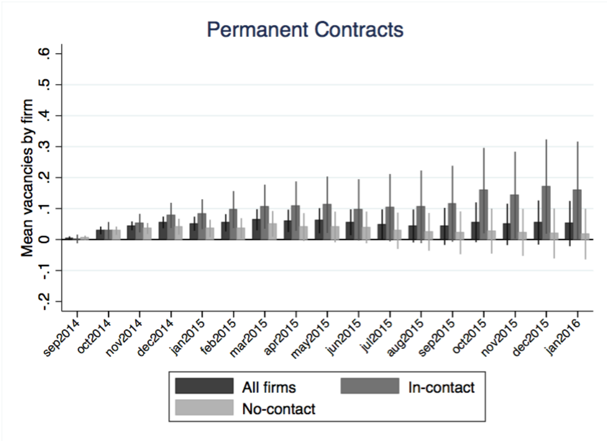
(a) In-contact firms (b) No-contact firms



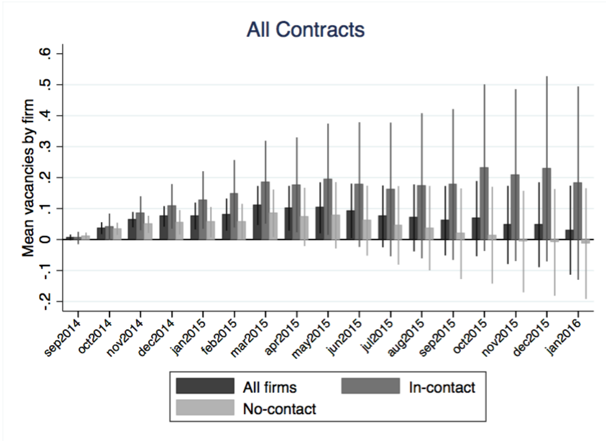

Note: Figures illustrate the impacts on the distribution of permanent contract hiring flows for different types of jobseekers for in-contact firms (figures in the left column) and no-contact firms (right column). Vertical bars show the percentage point impact on an indicator for making at least the number of hires as denoted by the horizontal axis. Bars are overlaid with 95% confidence intervals. Vertical lines mark the quantiles of the underlying distribution of hires. Impacts are estimated per equation 1 and standard errors are clustered at the agency level. The p-value for rank-sum tests for the equality of distributions between treatment and control flows are added below each graph.

Figure 3: Cumulative impacts on vacancy and workday creation

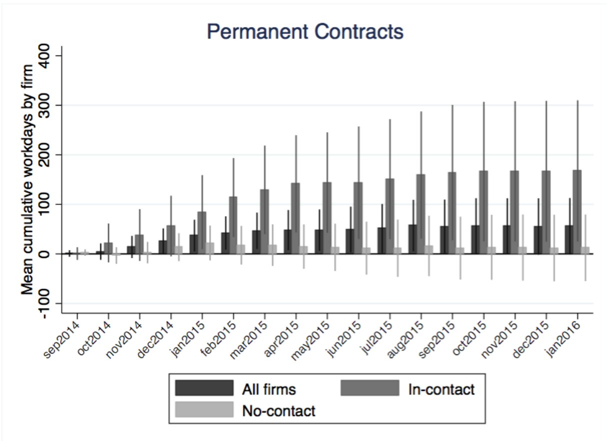
(a) Vacancies - permanent contracts



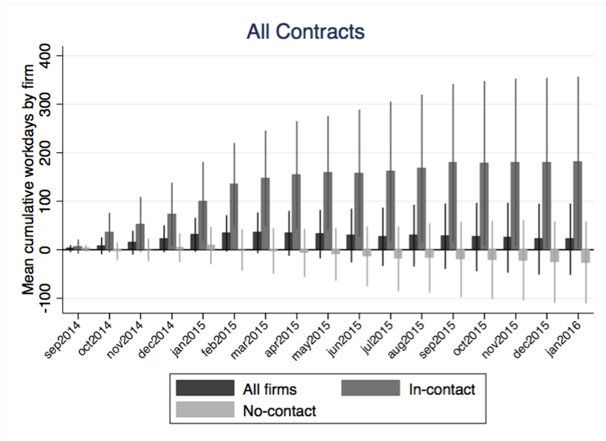

(b) Vacancies - all contract types




(c) Workdays - permanent contracts

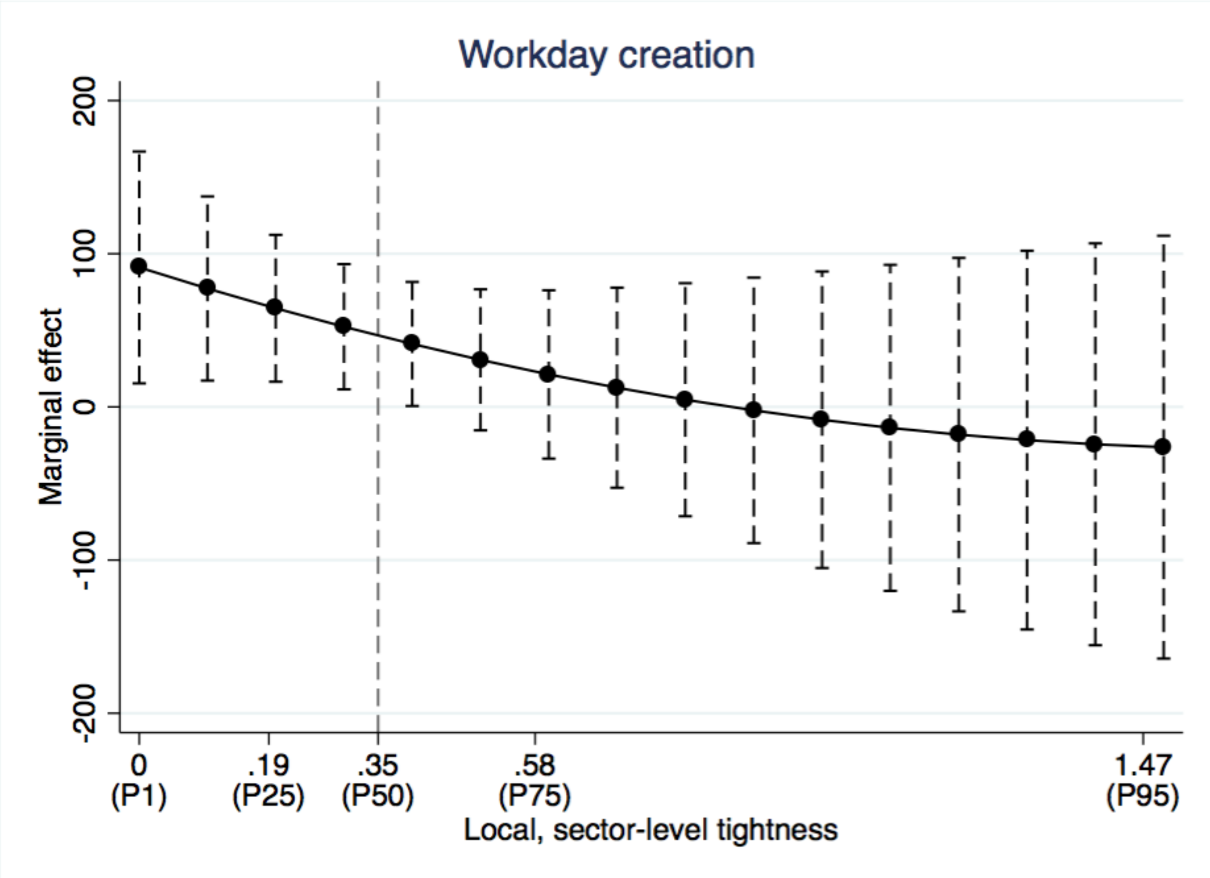



(d) Workdays - all contract types




Note: Figures illustrate the cumulative impact of the intervention on vacancy flows (figures in the top row) and workday creation (second row). Vertical bars show differences between treatment and control groups for each month during the entire 17 month observation period for all sample firms and across the in-contact heterogeneity. The impacts are overlaid with 95% confidence intervals. Impacts are estimated per equation 1 and standard errors are clustered at the agency level.

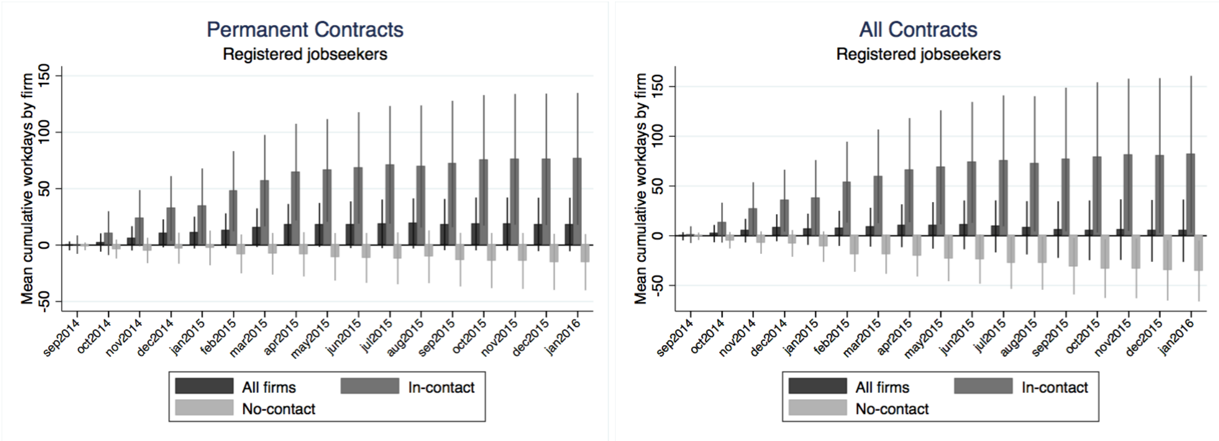
Figure 4: Marginal impact on workday creation over tightness



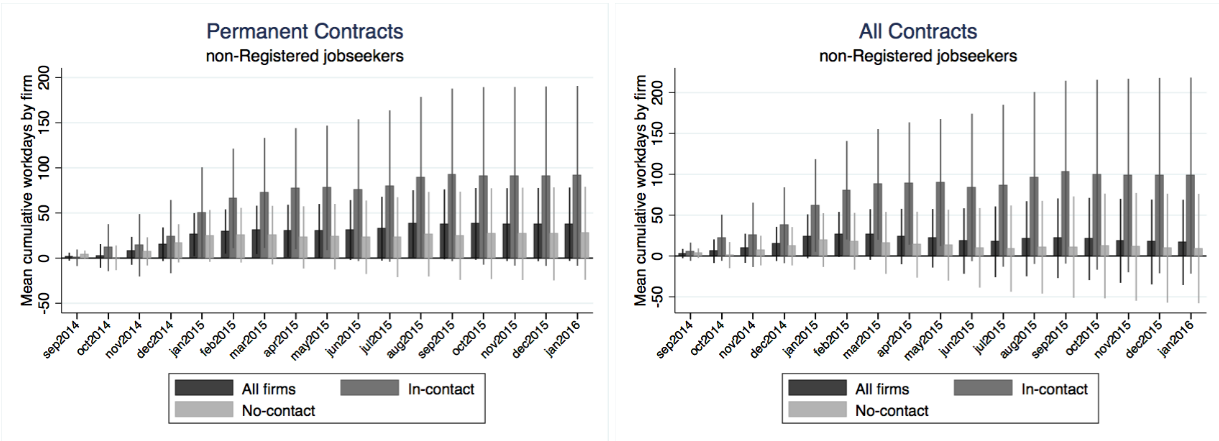

Note: This figure illustrates the marginal effect of the treatment on workday creation in permanent contracts at different levels of labor market tightness faced by the firm. Tightness is calculated at the commuting zone-sector level on the day preceding the intervention. Results come from an OLS regression where the treatment is interacted with the tightness measure and its square. Standard errors are clustered at the commute zone level with 95% confidence intervals denoted in dashed lines.

Figure 5: Cumulative impacts on employment creation by type of jobseeker

(a) Workdays created for **registered** jobseekers




(b) Workdays created for **non-registered** jobseekers




Note: Figures illustrate the cumulative impact of the intervention on workday creation for registered jobseekers (figures in the top row) and for non-registered jobseekers (second row). Vertical bars show differences between treatment and control groups for each month during the entire 17 month observation period for all sample firms and across the in-contact heterogeneity. The impacts are overlaid with 95% confidence intervals. Impacts are estimated per equation 1 and standard errors are clustered at the agency level.

# A Estimation and Variance Computation Appendix

## A.1 Other estimates

Our estimate is numerically analogous to estimating a model where treatment $T$ is interacted model with strata indicators that are centered at the mean rate of treatment assignment within strata,

$$y = a + bT + \sum_{s>1} \alpha_s 1_s + \sum_{s>1} \beta_s T(1_s - q_s) + u \tag{8}$$

Given the large number of strata in our study, equation 8 is computationally intensive, thus we directly calculate equation 1.

Other estimates could be (a) the simple difference in treatment group means across the whole sample or (b) a regression equation simply including dummy variables for the strata. Because the assignment rate is not exactly 0.5 due to uneven and singleton strata, the first estimate (a) cannot be exactly written as a weighted average of estimated impacts within strata. Estimate (b) can be written in such a away, but the weights also involve the assignment rate in each strata (see Imbens and Rubin (2015)). We prefer to consider the $ATE$ in (1) because it accurately reflects the experimental design. Estimates obtained using either (a) or (b) give very similar results.

## A.2 Standard error computation

Our estimates are obtained using formula in equation 1. This estimate is identical to the result of an inversely propensity weighted regression of the simple treatment model

$$y = a + bT + u \tag{9}$$

using as $T/e_s + (1-T)/(1-e_s)$ as weights in which $e_s$ is the empirical assignment rate to treatment.

To compute the variance, we use the framework developed in Hirano et al. (2003) in their paper on the efficient estimation of average treatment effects using propensity scores. They derive an influence function of the estimate which they use to compute the standard error of propensity weighted estimates. The case we considered is, however, far more simple than the general case considered in their paper in which the propensity function is a complicated function of the covariates entering the conditional independence assumption. In our case, the influence function is simply

$$\psi(y,s,T) = (y-\widehat{\mu}_{1,s})\frac{T}{e_s} - (y-\widehat{\mu}_{0,s})\frac{1-T}{1-e_s} + \widehat{\mu}_{1,s} - \widehat{\mu}_{0,s} \tag{10}$$

The sample variance of the function $\psi(y,s,T)$ can be rewritten as $\psi(y,s,T) = \widehat{ATE} + \varepsilon(y,s,T)$ and an estimate of the variance is

$$\widehat{V}(\widehat{ATE}) = \frac{1}{N^2}\sum_i \varepsilon(y_i,s_i,T_i)^2 \tag{11}$$

Throughout our paper we cluster the standard errors to correct for possible correlation in outcomes at the local agency level $a$, hence our estimate of the variance will simply be,

$$\widehat{V}(\widehat{ATE}) = \frac{1}{N^2}\sum_{a=1}^{A}\left(\sum_{i\in a}\varepsilon(y_i,s_i,T_i)\right)^2 \tag{12}$$

As noted in the main text, we will systematically display heterogeneous treatment effects along the baseline in-contact dimension $c \in \{0,1\}$. This estimation is straight forward as it simply involves separately estimating the $ATE$ and influence functions for the two subsamples.[39] Nevertheless, there will be some strata $s$ in which we have no variation in the heterogeneity dimension. Hence $\widehat{\mu}_{T,s,c=1}$ or $\widehat{\mu}_{T,s,c=0}$ may be undefined and observations for which this is the case are dropped from the heterogeneity analysis.

## A.3 Proof Appendix

Propositions:

1. $\Pi(\nu,\mu_1,1) > \Pi(\nu,\mu_0,0)$ as long as $\mu_1 \geq \mu_0$
2. $d_1^\star = 1$
3. $e_1^\star \leq e_0^\star$ and $t_1^\star \geq t_0^\star$
4. Value function under preselection is increasing in $v$, $\delta$, $\gamma$ and decreasing in $\kappa$

Proofs:

[39] We compute the simultaneous covariance matrix and report results of a Wald test $H_0 : ATE_{c=1} = ATE_{c=0}$

Consider the function $\Pi(\nu, \mu, x)$:

$$r\Pi(\nu,\mu,x) = \underset{e,\bar{t},d\in\{0,1\}}{max} \quad \Big\{-c(e) + e\theta \left(\gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu,\mu,x))\, \tfrac{dt}{\theta} - \kappa\right) \\ +d\,(\delta+\mu)\,(1-(1-\theta)x) \left(\gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu,\mu,x))\, \tfrac{dt}{1-(1-\theta)x} - \kappa\right)\Big\}, \tag{13}$$

with FOCs w.r.t. $\bar{t}$ and $e$:

$$\begin{aligned} e: &\quad -c'(e) + \left(\gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu,\mu,x))\, dt - \theta\kappa\right) = 0 \\ \bar{t}: &\quad \bar{t}v - \Pi(\nu,\mu,x) = 0 \\ &\quad \text{or } \bar{t} = 1 - \tfrac{1}{\gamma} \text{ if } (1-\tfrac{1}{\gamma})v \geq \Pi(\nu,\mu,x) \end{aligned} \tag{14}$$

Looking at the choice condition whether to include the counselor and jobseeker channels in the search process we have,

$$\gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu,\mu,x))\, dt - (1-(1-\theta)x)\kappa \geq 0 \tag{15}$$

and under preslection, $x = 1$, this condition rewrites as,

$$\gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu,\mu,x))\, dt - \theta\kappa \geq 0 \tag{16}$$

This proves proposition 2: under preselection, the jobseeker and caseworker channels are always active: $d_1^\star = 1$

Without preslection, $x = 0$, the condition rewrites

$$\gamma \int_{t>\bar{t}}^{1} (tv - \Pi(\nu,\mu,x))\, dt - \kappa \geq 0 \tag{17}$$

$\Rightarrow$ Without preselection, there might be cases in which the jobseeker or caseworker channels are not active: $d_0^\star = 0$

We now prove proposition 1:
The derivative w.r.t. to $x$ is,

$$r\Pi'_x = -(e + d(\delta+\mu))\gamma\Pi'_x(1-\bar{t}^\star) + d(\delta+\mu)\kappa(1-\theta).$$

Thus if $d = 0$, $\Pi'_x = 0$ and if $d = 1$, $\Pi'_x > 0$, this is enough to ensure $\Pi(\nu,\mu_0,1) > \Pi(\nu,\mu_0,0)$.

For $\mu$, we get,

$$r\Pi'_\mu = -(e + d(\delta+\mu))\gamma\Pi'_\mu(1-\bar{t}^\star) + d(\gamma\int_{t>\bar{t}}^1 (tv - \Pi(\nu,\mu,x))\,dt - (1-(1-\theta)x)\kappa).$$

Thus if $d=0$, $\Pi'_\mu = 0$ and if $d=1$, $\Pi'_\mu > 0$, this is enough to ensure $\Pi(\nu,\mu_1,1) > \Pi(\nu,\mu_0,1)$. Hence, $\Pi(\nu,\mu_1,1) > \Pi(\nu,\mu_0,0)$.

Proposition 3 directly follows:
The first order condition in $\bar{t}$ (see equation (14)) writes as: $\bar{t}v = \Pi(\nu,\mu,x)$ , as $\Pi(\nu,\mu_1,1) > \Pi(\nu,\mu_0,0)$ we directly get $t_1^\star > t_0^\star$.
The first order condition in $e$ (see equation (14)) rewrites as $c'(e) = \gamma(1-t^\star)^2/2 - \theta\kappa$, thus because $t_1^\star > t_0^\star$ this implies $e_1^\star < e_0^\star$.

Finally in turning to proposition 4, we can now derive the value function with respect to each of the components of $\nu$:

$$[r + (e_1^\star + \delta + \mu)\gamma(1-t_1^\star)] \begin{pmatrix} \pi'_\delta \\ \pi'_v \\ \pi'_\kappa \\ \pi'_\gamma \end{pmatrix} = \begin{pmatrix} \int_{t>\bar{t}}^1 (tv - \Pi(\nu,\mu,x))\,dt - \kappa\theta > 0 \\ (e_1^\star + \delta + \mu)\gamma(1-t_1^\star)^2/2 > 0 \\ -(e_1^\star + \delta + \mu)\kappa < 0 \\ \int_{t>\bar{t}}^1 (tv - \Pi(\nu,\mu,x))\,dt > 0 \end{pmatrix} \tag{18}$$

# Appendix Tables

Table A.1: Impact on flows using non top-coded data

| | Vacancies | | | | Hires | | | |
|---|---|---|---|---|---|---|---|---|
| | Permanent (1) | Fixed-term (2) | Temp (3) | All (4) | Permanent (5) | Fixed-term (6) | Temp (7) | All (8) |
| Panel A: Overall | | | | | | | | |
| Treatment | 0.101*** | 0.062** | 0.013 | 0.177*** | 0.214** | -1.338 | 0.779 | -0.344 |
| | (0.025) | (0.024) | (0.037) | (0.057) | (0.083) | (2.910) | (1.375) | (2.994) |
| Control Mean | 0.219 | 0.166 | 0.315 | 0.699 | 1.460 | 11.489 | 10.131 | 23.080 |
| N | 8232 | 8232 | 8232 | 8232 | 8232 | 8232 | 8232 | 8232 |
| Panel B: Heterogeneity | | | | | | | | |
| Contact= 1 | | | | | | | | |
| Treatment | 0.177*** | 0.161*** | 0.018 | 0.355*** | 0.637*** | -2.464 | 6.079* | 4.251 |
| | (0.059) | (0.048) | (0.078) | (0.125) | (0.199) | (5.094) | (3.659) | (6.184) |
| Contact= 0 | | | | | | | | |
| Treatment | 0.074*** | 0.029 | 0.014 | 0.117** | 0.096 | 2.116 | -1.131 | 1.081 |
| | (0.025) | (0.022) | (0.042) | (0.058) | (0.083) | (2.827) | (1.587) | (2.703) |
| Control Mean(Contact= 1) | 0.332 | 0.196 | 0.350 | 0.878 | 1.738 | 14.699 | 10.539 | 26.976 |
| Control Mean(Contact= 0) | 0.161 | 0.124 | 0.256 | 0.541 | 1.320 | 8.258 | 9.743 | 19.321 |
| p-value Equality of Coefs. | 0.120 | 0.011 | 0.969 | 0.086 | 0.013 | 0.357 | 0.070 | 0.609 |
| N | 7859 | 7859 | 7859 | 7859 | 7859 | 7859 | 7859 | 7859 |

Note: This table replicates results in Table 2, but uses non top-coded data. Average treatment effects are estimated per equation 1. Strata weighted control group means are also shown. Standard errors in parenthesis are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.2: Employment flows between firms

| | Fixed-term | | Permanent | | Temp | |
|---|---|---|---|---|---|---|
| | Control | Treatment | Control | Treatment | Control | Treatment |
| Individuals hired coming from: | | | | | | |
| Unemployment | 0.569 | 0.570 | 0.353 | 0.344 | 0.883 | 0.839 |
| | 61895 | 57023 | 5053 | 5400 | 74179 | 91449 |
| Employment in Sample Firm: | 0.106 | 0.093 | 0.031 | 0.029 | 0.003 | 0.005 |
| | 11494 | 9271 | 444 | 448 | 262 | 531 |
| From same firm | 0.105 | 0.091 | 0.030 | 0.028 | 0.003 | 0.005 |
| | 11393 | 9144 | 432 | 442 | 260 | 521 |
| From treated firm to - | 0.000 | 0.001 | 0.000 | 0.000 | 0.000 | 0.000 |
| | 38 | 84 | 7 | 5 | 1 | 5 |
| From control firm to - | 0.001 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | 63 | 43 | 5 | 1 | 1 | 5 |
| Inactivity or Employment Elsewhere | 0.051 | 0.050 | 0.218 | 0.209 | 0.003 | 0.049 |
| | 5553 | 5055 | 3123 | 3285 | 243 | 5335 |
| Unknown | 0.274 | 0.287 | 0.398 | 0.418 | 0.110 | 0.107 |
| | 29808 | 28763 | 5693 | 6550 | 9277 | 11662 |

Note: Data used are the hiring declarations made by all sample firms during the 6 month sanctuary period. Row titles correspond to the origin of the hired individual, columns titles where the hired person was placed and in which type of contract. The proportion of total flows by column and total volume of flows are displayed for each category. Unemployment is defined as jobseekers registered with the PES within the 30 days preceding the hiring date. "Employment in sample firms" is broken down into three categories: flows within the same firm, flows coming from treatment firms and flows coming from control firms. Inactivity and employment elsewhere is defined as hiring flows for people entering the labor market or who were employed in another firm outside the sample. Unknown is defined as flows for individuals for whom we have no identifiers and thus cannot trace their hiring or unemployment history.

## Table A.3: Selection on vacancy services provision (IPW estimates)

| | (1) Intensive Follow-up | (2) Preselection | (3) Special Preselection | (4) Verification | (5) Valorization | (6) Evaluation | (7) Analysis of post | (8) Drafting support | (9) Interview support | (10) Adaptation support | (11) EMTPR | (12) PMSMP |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Panel A: Overall | | | | | | | | | | | | |
| Treatment | $0.099^{***}$ | $0.094^{***}$ | $0.102^{***}$ | $0.093^{***}$ | 0.005 | 0.000 | 0.009 | 0.001 | 0.003 | 0.002 | 0.002 | -0.002 |
| | (0.033) | (0.033) | (0.031) | (0.033) | (0.008) | (0.004) | (0.012) | (0.021) | (0.004) | (0.005) | (0.003) | (0.002) |
| Control Mean | 0.272 | 0.260 | 0.223 | 0.249 | 0.026 | 0.007 | 0.017 | 0.109 | 0.004 | 0.010 | 0.005 | 0.003 |
| N | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 |
| Panel B: Heterogeneity | | | | | | | | | | | | |
| Contact= 1 | | | | | | | | | | | | |
| Treatment | $0.089^{*}$ | $0.084^{*}$ | $0.080^{*}$ | $0.087^{*}$ | 0.002 | -0.001 | 0.015 | -0.009 | -0.004 | 0.000 | 0.000 | -0.001 |
| | (0.046) | (0.046) | (0.043) | (0.046) | (0.011) | (0.006) | (0.015) | (0.029) | (.) | (0.008) | (0.005) | (0.003) |
| Contact= 0 | | | | | | | | | | | | |
| Treatment | $0.113^{***}$ | $0.109^{**}$ | $0.131^{***}$ | $0.101^{**}$ | 0.008 | 0.002 | 0.002 | 0.015 | 0.012 | 0.004 | 0.005 | -0.003 |
| | (0.044) | (0.043) | (0.040) | (0.042) | (0.009) | (0.006) | (0.013) | (0.030) | (.) | (0.006) | (0.005) | (0.003) |
| Control Mean(Contact= 1) | 0.284 | 0.272 | 0.241 | 0.254 | 0.030 | 0.008 | 0.014 | 0.121 | 0.008 | 0.015 | 0.006 | 0.003 |
| Control Mean(Contact= 0) | 0.256 | 0.245 | 0.198 | 0.242 | 0.019 | 0.006 | 0.021 | 0.093 | 0.000 | 0.004 | 0.003 | 0.003 |
| p-value Equality of Coefs. | 0.706 | 0.692 | 0.389 | 0.822 | 0.705 | 0.776 | 0.523 | 0.562 | . | 0.692 | 0.513 | 0.606 |
| N | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 | 1921 |

Note: This table presents inversely propensity weighted (IPW) regression results for the provision of services to vacancies. We predict vacancy selection into treatment $S$ using our observable vacancy characteristics. 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 $F$ denoting the logistic function. We then run an OLS regression of indicators for various services on a treatment indicator with observations weighted by $\frac{T}{\widehat{Pr}(S=1)} + \frac{1-T}{1-\widehat{Pr}(S=1)}$. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

Table A.4: Match selection at 2 weeks (non IPW estimates)

| | Counselor (1) | Employer (2) | Jobseeker (3) | Refusals by counselor (4) |
|---|---|---|---|---|
| **Panel A: Overall** | | | | |
| Treatment | -0.001 | -0.729*** | -2.014** | -0.024 |
| | (0.358) | (0.254) | (0.877) | (0.123) |
| Control Mean | 3.435 | 1.060 | 6.048 | 0.515 |
| p-value rank-sum (Mann-Whitney) | 0.141 | 0.036 | 0.000 | 0.540 |
| N | 2052 | 2052 | 2052 | 2052 |
| **Panel B: Heterogeneity** | | | | |
| Contact= 1 | | | | |
| Treatment | -0.394 | -0.905** | -1.893 | -0.176 |
| | (0.518) | (0.445) | (1.393) | (0.165) |
| Contact= 0 | | | | |
| Treatment | 0.508 | -0.496* | -2.157*** | 0.171 |
| | (0.432) | (0.265) | (0.811) | (0.140) |
| Control Mean(Contact= 1) | 3.679 | 1.260 | 6.406 | 0.592 |
| Control Mean(Contact= 0) | 3.115 | 0.798 | 5.579 | 0.415 |
| p-value Equality of Coefs. | 0.182 | 0.429 | 0.870 | 0.108 |
| N | 2052 | 2052 | 2052 | 2052 |

Note: This table is analogous to Table 7, but uses simple OLS regression estimates with no IPW. Standard errors in parenthesis are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

Table A.5: Match Selection at 8 weeks (IPW estimates)

| | Counselor | Employer | Jobseeker | Refusals by counselor |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Panel A: Overall | | | | |
| Treatment | -0.158 | -0.857*** | -2.613** | -0.073 |
| | (0.588) | (0.296) | (1.000) | (0.153) |
| Control Mean | 4.741 | 1.260 | 7.094 | 0.609 |
| p-value rank-sum (Mann-Whitney) | 0.009 | 0.064 | 0.000 | 0.594 |
| N | 1921 | 1921 | 1921 | 1921 |
| Panel B: Heterogeneity | | | | |
| Contact= 1 | | | | |
| Treatment | -0.726 | -0.975* | -2.591 | -0.286 |
| | (0.884) | (0.524) | (1.608) | (0.205) |
| Contact= 0 | | | | |
| Treatment | 0.593 | -0.697** | -2.627*** | 0.208 |
| | (0.617) | (0.323) | (0.949) | (0.183) |
| Control Mean(Contact= 1) | 5.147 | 1.437 | 7.465 | 0.733 |
| Control Mean(Contact= 0) | 4.200 | 1.024 | 6.598 | 0.444 |
| p-value Equality of Coefs. | 0.221 | 0.651 | 0.984 | 0.072 |
| N | 1921 | 1921 | 1921 | 1921 |

Note: This table is analogous to Table 7, but counts applications made up to 8 weeks after the vacancy posting date. Standard errors in parenthesis are clustered at the agency level. * $p < .1$, ** $p < .05$, *** $p < .01$

## Table A.6: Vacancy distribution

| | (1) Internet | (2) Anonymous | (3) Firm name and Contact info | (4) Firm name only |
|---|---|---|---|---|
| **Panel A: Overall** | | | | |
| Treatment | -0.078** | 0.053 | -0.044 | 0.004 |
| | (0.038) | (0.035) | (0.043) | (0.025) |
| Control Mean | 0.584 | 0.311 | 0.619 | 0.091 |
| N | 2052 | 2052 | 2052 | 2052 |
| **Panel B: Heterogeneity** | | | | |
| Contact= 1 | | | | |
| Treatment | -0.059 | 0.054 | -0.038 | 0.002 |
| | (0.056) | (0.047) | (0.063) | (0.041) |
| Contact= 0 | | | | |
| Treatment | -0.102** | 0.052 | -0.051 | 0.006 |
| | (0.049) | (0.043) | (0.045) | (0.018) |
| Control Mean(Contact= 1) | 0.588 | 0.294 | 0.617 | 0.115 |
| Control Mean(Contact= 0) | 0.579 | 0.333 | 0.623 | 0.060 |
| p-value Equality of Coefs. | 0.567 | 0.971 | 0.867 | 0.935 |
| N | 2052 | 2052 | 2052 | 2052 |

Note: This table presents results from simple OLS regressions of indicators for firm selection on the way vacancies are posted on a treatment indicator. Internet signifies whether the vacancy is posted publicly on www.pole-emploi.fr. Anonymous indicates that the vacancy does not shows any identifying information on the firm. "Firm name and contact info" indicates that the firm displays all contact information in the vacancy. "Firm name only" indicates that the vacancy only displays the name of the firm and no contact info. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

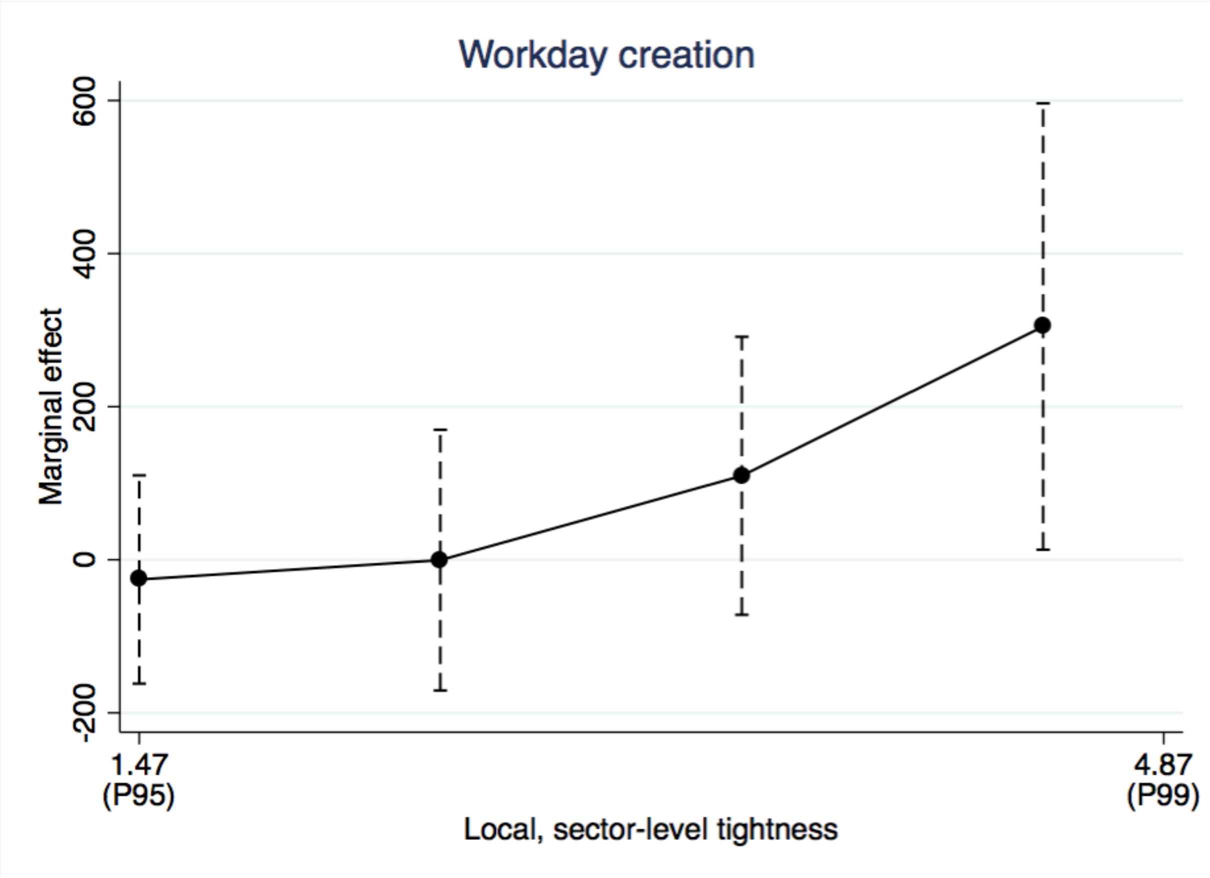
# Table A.7: Intensity and actor

| | Number of modifications made to vacancy | | | | Actor | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Counselor | Firm | Automatic | By internet | Vacancy created by employer | Vacancy posted by 3rd party | Posted with PES web space |
| Panel A: Overall | | | | | | | |
| Treatment | 0.859*** | -0.047 | -0.206 | -0.201* | 0.028 | -0.033 | 0.031 |
| | (0.312) | (0.051) | (0.160) | (0.116) | (0.017) | (0.039) | (0.025) |
| Control Mean | 3.324 | 0.368 | 2.708 | 1.741 | 0.096 | 0.264 | 0.232 |
| N | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 |
| Panel B: Heterogeneity | | | | | | | |
| Contact= 1 | | | | | | | |
| Treatment | 0.773* | -0.110 | -0.041 | -0.203 | 0.027 | -0.080 | 0.024 |
| | (0.437) | (0.069) | (0.223) | (0.155) | (0.021) | (0.060) | (0.035) |
| Contact= 0 | | | | | | | |
| Treatment | 0.971*** | 0.035 | -0.418** | -0.197 | 0.029 | 0.027 | 0.040 |
| | (0.370) | (0.071) | (0.188) | (0.191) | (0.026) | (0.048) | (0.034) |
| Control Mean(Contact= 1) | 3.375 | 0.390 | 2.698 | 1.788 | 0.081 | 0.271 | 0.223 |
| Control Mean(Contact= 0) | 3.257 | 0.339 | 2.721 | 1.680 | 0.115 | 0.254 | 0.243 |
| p-value Equality of Coefs. | 0.729 | 0.145 | 0.197 | 0.981 | 0.955 | 0.165 | 0.750 |
| N | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 | 2052 |

Note: Modifications during the life of the vacancy can either be made by the counselor or the firm. Automatic signifies that it is simply the PES computer system that automatically cancels the vacancy after a certain length of time with no activity. Modified by internet means that the firm itself made its modification by internet. The vacancy can also be created and posted by the firm or through a 3rd party actor on behalf of the firm via its personal web space. Standard errors in parenthesis are clustered at the agency level. $^{*}$ $p < .1$, $^{**}$ $p < .05$, $^{***}$ $p < .01$

# Appendix Figures

Figure A.1: Marginal impact on workday creation in upper distribution of tightness




Note: This figure illustrates the marginal effect of the treatment on workday creation in permanent contracts at upper distribution of labor market tightness faced by the firm. Tightness is calculated at the commuting zone-sector level on the day preceding the intervention. Results come from an OLS regression where the treatment is interacted with the tightness measure and its square. Standard errors are clustered at the commute zone level with 95% confidence intervals denoted in dashed lines.