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Three essays on labor market policies : job search assistance and digital tools

Sofía Dromundo Mokrani

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ÉCOLE D'ÉCONOMIE DE PARIS
UFR D'ÉCONOMIE - ED 465

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Sofia DROMUNDO MOKRANI.

Thèse :

Trois Essais sur les Politiques du Marché du Travail : L'aide à la Recherche d'Emploi et les Outils Numériques

Composition du Jury

Directeur de thèse :

Marc Gurgand (École d'Économie de Paris - Directeur de Recherche CNRS)

Rapporteurs : Patrick Arni (University of Bristol - Senior Lecturer/Associate Professor)
Ghazala Azmat (Sciences Po - Professeure des Universités)

Examineurs : François Fontaine (Université Paris 1 - Professeur des Universités)
Cyril Nouveau (Pôle emploi - Directeur DSEE)

UNIVERSITÉ PARIS 1 PANTHÉON SORBONNE
PARIS SCHOOL OF ECONOMICS
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Submitted for the Degree of Doctor of Philosophy in Economics

Defended on September 23, 2021, by:

Sofía DROMUNDO MOKRANI.

Ph.D. THESIS :

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Advisor:

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Ghazala Azmat (Sciences Po - Associate Professor)

Examiners : François Fontaine (Université Paris 1 - Associate Professor)
Cyril Nouveau (Pôle emploi (French PES) - Director DSEE)

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Sofía Dromundo Mokrani

Résumé:

Trois Essais sur les Politiques du Marché du Travail : L'aide à la Recherche d'Emploi et les Outils Numériques

Depuis les années 1970, face à l'augmentation du taux et de la durée du chômage, les pouvoirs publics ont orienté leurs efforts vers des politiques dites « actives » du marché du travail. Parmi celles-ci, les politiques d'accompagnement se sont révélées particulièrement efficaces. Par ailleurs, les changements technologiques ont modifié les pratiques d'embauche et de recherche d'emploi au cours des dernières décennies. L'utilisation d'Internet est devenue la norme, tant pour les employeurs que pour les demandeurs d'emploi.

Cette thèse a deux objectifs : comprendre les mécanismes qui se cachent derrière la réussite des politiques d'accompagnement et déterminer le rôle du numérique dans cet accompagnement. Elle se présente en trois chapitres.

Le premier chapitre montre qu'un programme d'accompagnement numérique conçu pour une population qui n'avait jusqu'ici fait l'objet d'aucune étude scientifique -les demandeurs d'emploi les plus autonomes- peut augmenter leur chance de retrouver un emploi.

Le deuxième chapitre explore le rôle des conseillers. Il mesure l'influence considérable de ces derniers sur la quantité et la qualité des sorties vers l'emploi et souligne que certains conseillers semblent favoriser l'une plutôt que l'autre, notamment à travers les services proposés aux demandeurs d'emploi.

Enfin, le dernier chapitre analyse simultanément la réaction des entreprises et des demandeurs d'emploi suite à une stimulation de la recherche d'emploi via une plateforme en ligne. Fournir des recommandations ciblées augmente le retour à l'emploi des femmes et les embauches en CDI des établissements.

Mots clés: Marché du travail, Chômage, Accompagnement, Conseillers, Politiques Actives, Internet, Numérique, Mécanismes d'appariement, Politiques Publiques.

Summary:

Three Essays on Labor Market Policies: Job Search Assistance and Digital Tools

Since the 1970s, the massive rise in the rates and duration of unemployment has led public authorities to orient their action towards so-called "active" labor market policies. Among these policies, job-search assistance proved to be particularly effective.

Additionally, technological changes have significantly altered job searches and hiring practices in recent decades. The use of the internet has become a new norm in the job searching process for both employers and job seekers.

The objectives of this thesis are twofold: understand the mechanisms behind the success of job search assistance and determine the role of digital technology in this type of program. It is presented in three chapters.

The first chapter shows that a digital support program targeting a rarely studied population in the scientific literature -relatively autonomous job seekers- can increase their chances to find a job.

The second chapter explores the role of job counselors. It measures the considerable influence they have on the quantity and the quality of exits to employment, showing that some counselors seem to favor one over the other, particularly through the services they recommend.

Finally, in an experiment building upon an online platform, the last chapter simultaneously analyses the reaction of both jobseekers and firms in response to a job search stimulation. It concludes that providing targeted match recommendations increases job finding rates among women and firm hires on indefinite duration contracts.

Keywords: Labor Market, Unemployment, Job Search Assistance, Job Counselors, Active Labor Market Policies, Internet, Digital, Matching, Public Policy.

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Introduction Générale

Les politiques actives du marché du travail et l'accompagnement des demandeurs d'emploi

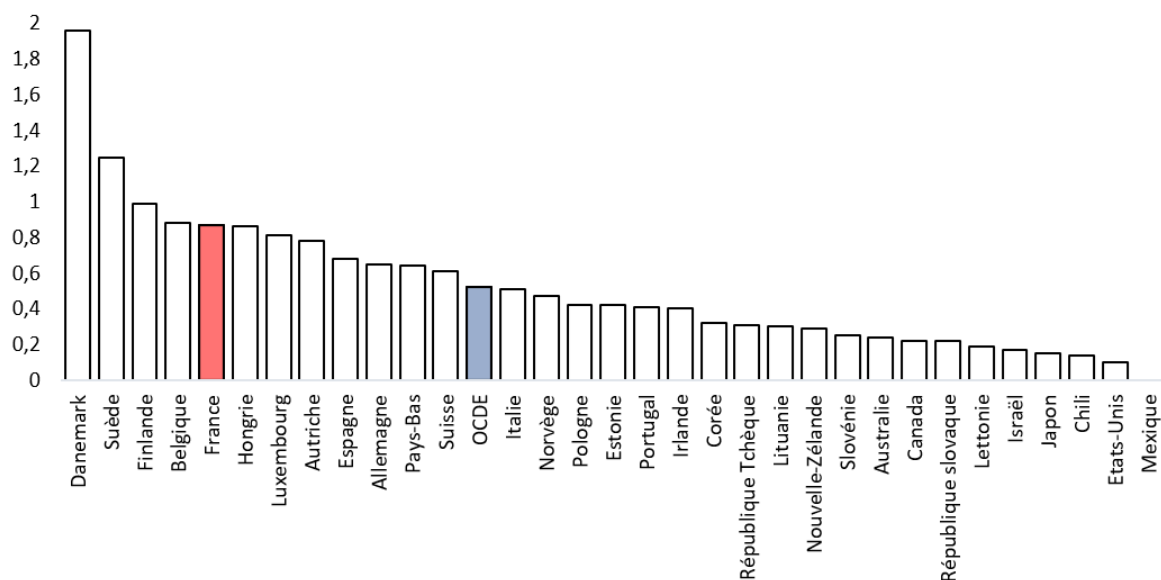
Depuis les années 1970, de nombreux pays européens ont connu une augmentation massive du nombre de demandeurs d'emploi ainsi que de la durée des épisodes de chômage. En hausse jusqu'aux années 1990, le chômage se maintient aujourd'hui à des niveaux élevés. Le coût croissant de l'assurance-chômage a accentué le besoin de réformes, et mis la question de la gestion du chômage au cœur du débat public. Dans cette perspective un consensus a émergé sur le fait que les pouvoirs publics, qui avaient jusqu'alors favorisé la mise en place de politiques dites « passives », devaient réorienter leur action vers des politiques dites « actives » du marché du travail.

Les politiques actives du marché du travail comprennent toutes les politiques destinées à accroître les possibilités d'accès à l'emploi des demandeurs d'emploi et à améliorer l'adéquation entre les offres d'emploi et les candidats. On les oppose dans le débat public aux politiques passives, telles que l'assurance chômage, qui ont pour objectif de subvenir aux besoins financiers des demandeurs d'emploi. On retrouve parmi les politiques actives les politiques de formation qui visent à aider les demandeurs d'emploi à acquérir les compétences requises pour avoir accès à de nouvelles possibilités d'emploi ; les politiques d'accompagnement et d'aide à la recherche d'emploi qui peuvent, par exemple, permettre aux demandeurs d'emploi d'identifier les emplois viables et construire un projet professionnel cohérent avec le marché du travail ; les emplois subventionnés ou encore les aides à la création d'entreprise.

Étudiées par les institutions internationales dans les années 1990 (OECD, 1994), les politiques actives de l'emploi ont depuis été systématiquement préconisées par les institutions publiques comme des solutions potentielles au problème du chômage de masse. Ces mesures, visant à améliorer l'employabilité, ont notamment été considérées par les spécialistes comme une issue à la crise de l'emploi découlant de la crise financière mondiale de 2008 (Martin, 2015), et sont actuellement mises en avant comme un moyen d'assurer une réintégration rapide des nombreux demandeurs d'emploi suite à la crise du COVID-19 (OECD, 2021).

En conséquence, les dépenses consacrées aux politiques actives sont importantes dans la plupart de pays à revenus élevés. En 2017, les pays de l'OCDE consacraient en moyenne 0,5% de leur PIB aux politiques d'activation (voir Figure 1). La France est le cinquième pays de l'OCDE dont la part du PIB consacrée aux politiques actives est la plus élevée, ce qui en fait un cas d'étude particulièrement intéressant.

Figure 1: Dépenses publiques consacrées aux politiques d'activation par les pays de l'OCDE en 2017 (en % du PIB)



Source: OECD.stat

Dans cette thèse, nous nous intéressons à une catégorie particulière des politiques actives : les politiques d'accompagnement/d'aide à la recherche d'emploi. Ces politiques englobent à la fois l'orientation dans la recherche d'emploi et le contrôle de l'effort de recherche. Elles se caractérisent généralement par une relation personnalisée entre le demandeur d'emploi et son conseiller référent (ou avec des opérateurs privés en cas d'externalisation).

Les politiques d'accompagnement sont fondées sur l'idée que les demandeurs d'emploi ont des connaissances imparfaites (des offres, des compétences requises, des méthodes de recherche etc.), participant ainsi aux déséquilibres présents sur le marché du travail. Leur but est donc de réduire ces frictions en offrant aux demandeurs d'emploi des conseils en matière de carrière et d'évaluation des compétences, en les aidant à rédiger des dossiers de candidature, à se préparer aux entretiens d'embauche ou à trouver des offres d'emploi appropriées etc.

Les politiques d'accompagnement et d'aide à la recherche d'emploi agissent à travers plusieurs canaux : elles ont pour objectif d'augmenter la motivation et l'effort fourni par les demandeurs d'emploi en les informant et en les conseillant, en améliorant l'efficacité de leur stratégie de recherche et en favorisant un appariement plus rapide et durable entre candidats et offres d'emploi.

Les évaluations disponibles de ces politiques d'accompagnement montrent qu'elles constituent l'un des éléments les plus efficaces de l'ensemble des politiques actives du marché du travail, en contribuant notamment à raccourcir la durée des épisodes de chômage (Card et al., 2010, 2018). La littérature a notamment montré les effets positifs de ces dispositifs lorsqu'ils sont destinés

aux demandeurs d'emploi les plus défavorisés (ANSA, 2017).

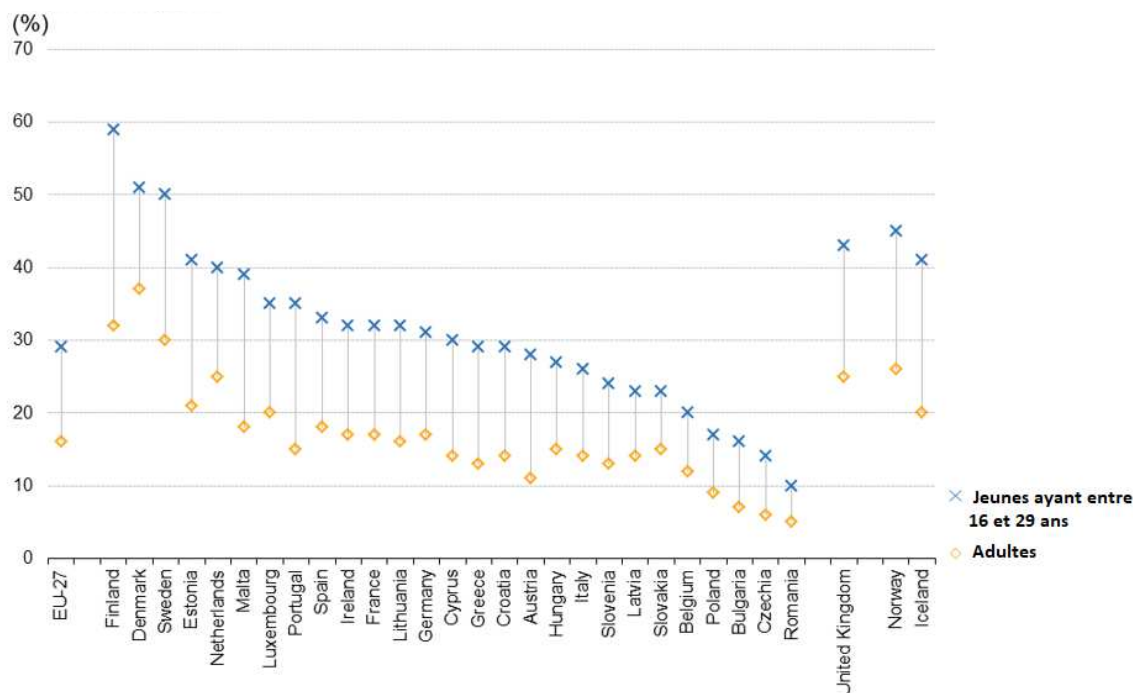
Cependant, les phénomènes à l'origine de ces performances et la manière dont ces programmes agissent sur différentes populations ont été peu étudiés. Cette thèse cherche donc avant tout à éclaircir les mécanismes en jeu derrière le succès ou l'échec de des politiques d'accompagnement.

L'utilisation croissante d'internet dans la recherche d'emploi et le besoin d'évaluations

Une analyse des politiques d'activation du marché du travail ne peut être menée sans prendre en compte l'utilisation accrue d'Internet dans la recherche d'emploi dans les dernières décennies. Il s'agit aujourd'hui d'un outil majeur permettant de faciliter l'accès à l'information, ainsi que de fluidifier les interactions entre demandeurs d'emploi et employeurs.

L'enquête communautaire concernant les statistiques sur la société de l'information (CSIS) publiée par l'Eurostat (voir Figure 2) montre qu'en 2019, dans la plupart des pays enquêtés, plus de 15% de la population entre 16 et 74 ans déclare avoir utilisé internet pour chercher un emploi ou envoyer une candidature dans les trois derniers mois. Cette part s'élève à environ 30% lorsque l'on considère les individus âgés entre 16 et 29 ans. Ces chiffres s'avèrent particulièrement élevés, surtout si l'on considère qu'il s'agit ici de la population dans son ensemble et non pas du sous-échantillon de la population active ou des demandeurs d'emploi. Par ailleurs, en France, une enquête conduite uniquement auprès d'individus inscrits à Pôle emploi, montre que près de 90% des demandeurs d'emploi déclarent avoir eu recours à internet dans leur recherche d'emploi (Gaumont et al., 2019). Parallèlement, les employeurs eux-mêmes utilisent internet dans leur processus de recrutement. L'utilisation du numérique devient donc une nouvelle norme tant du côté des employeurs que des demandeurs d'emploi.

Figure 2: Individus ayant utilisé internet pour chercher un emploi ou envoyer une candidature (en %)



Note: Source: Eurostat - enquête communautaire concernant les statistiques sur la société de l'information (CSIS). La population d'intérêt est constituée de tous les individus âgés de 16 à 74 ans. Pour les pays couverts par Eurostat, les individus ont été interrogés sur les activités qu'ils avaient effectuées sur Internet au cours des trois derniers mois.

Face à ce constat, les services publics de l'emploi ont, eux aussi, adapté leur offre de services. En France, Pôle emploi propose dorénavant de s'inscrire en ligne, d'être accompagné de façon dématérialisée (courriels, chats, vidéoconférences, etc.), de suivre des formations en ligne, de retrouver des offres d'emploi et d'y soumettre sa candidature. Ce recours au numérique a pour objectif de rendre les services publics de l'emploi plus efficaces. Il permet, d'une part, d'élargir l'accessibilité aux services proposés (par exemple, les demandeurs d'emploi n'ont plus besoin de se déplacer en agence pour bénéficier de l'aide à la recherche d'emploi) et, d'autre part, de libérer du temps supplémentaire à destination des demandeurs d'emploi les plus en difficulté.

Cependant, malgré ces changements majeurs, peu d'études ont mis en évidence l'effet d'Internet sur les frictions sur le marché du travail et la création d'emploi. En effet, les premiers travaux concernant la recherche d'emploi en ligne datant du début des années 2000 - dont l'article fondateur de [Kuhn and Skuterud \(2004\)](#) - n'ont pas réussi à trouver un effet d'Internet sur le taux d'embauche. Cette question a ensuite été réétudiée au début de la décennie 2010 et a abouti à plusieurs recherches mettant en valeur des effets positifs de l'utilisation d'Internet sur la probabilité de trouver un emploi ([Kuhn, 2014](#)).

Le deuxième objectif de cette thèse sera donc d'apporter des éléments pour éclaircir le débat en cours sur l'efficacité des outils numériques dans la recherche d'emploi ([Kircher, 2020](#)). Plus

précisément, ce travail cherche à étudier dans quelle mesure les services publics de l'emploi peuvent utiliser ce canal afin d'améliorer l'efficacité de la recherche d'emploi et de réduire les frictions informationnelles. A cet égard, les premier et troisième chapitre évalueront deux services de Pôle emploi délivrés sous format numérique.

L'évaluation micro-économétrique des politiques du marché du travail et l'importance des données administratives

Comprendre quels sont les programmes ou les politiques publiques qui ont fonctionné, pour qui, où et pour quelles raisons, est essentiel pour concevoir de nouvelles politiques et de nouveaux programmes plus efficaces. Il est donc crucial de mener des évaluations rigoureuses afin d'élaborer des politiques fondées sur une base factuelle solide.

L'évaluation d'impact des politiques du marché de travail pose de nombreux défis méthodologiques. L'un des principaux est la gestion du biais de sélection. Pour évaluer une politique donnée, il ne suffit pas de comparer les résultats des bénéficiaires de celle-ci à ceux des non-bénéficiaires directement. En effet, les bénéficiaires sont souvent «sélectionnés», c'est-à-dire qu'ils ne possèdent pas les mêmes caractéristiques que les non bénéficiaires et ne sont donc pas comparables (par exemple les bénéficiaires pourraient être mieux informés ou plus motivés...). Afin d'isoler l'effet causal d'une politique, il faudrait idéalement pouvoir comparer le taux de retour à l'emploi des bénéficiaires de cette politique à celui que ces mêmes individus auraient obtenu s'ils n'avaient pas bénéficié de cette politique. Évidemment il n'est pas possible d'observer ce qu'il serait advenu des bénéficiaires en l'absence de la politique.

L'objectif des évaluations micro-économétriques est donc de comparer les résultats (retour à l'emploi, durabilité de l'emploi etc.) des personnes ayant bénéficié d'un programme (le groupe traité) avec ceux d'un ensemble d'individus le plus similaire possible (le groupe témoin). La seule différence entre le groupe traité et le groupe témoin est que ce dernier n'a pas participé au programme. Le groupe témoin fournit donc des informations sur "ce qui serait arrivé aux individus soumis à l'intervention s'ils n'y avaient pas été exposés" : le cas contrefactuel.

Deux approches peuvent être distinguées parmi les évaluations économétriques : les évaluations expérimentales, que l'on appelle aussi les expérimentations randomisées ou contrôlées, et les évaluations quasi-expérimentales. Dans le cadre d'une évaluation expérimentale, le groupe traité et le groupe témoin sont tirés au sort dans une population donnée. Si le processus de sélection est réellement aléatoire, les caractéristiques des individus des deux groupes ne diffèrent pas en moyenne : les groupes sont ainsi statistiquement équivalents. Comparer les résultats de ces deux groupes à l'issue du programme nous permet d'isoler l'effet causal de celui-ci.

Si les expérimentations contrôlées représentent un cadre idéal pour évaluer les politiques publiques, elles ne sont pas toujours réalisables. Les méthodes expérimentales requièrent des moyens fi-

nanciers et logistiques conséquents, et il est fréquent que des programmes soient conçus sans suivre un protocole expérimental. De plus, ces méthodes peuvent parfois s'avérer peu éthiques: il n'est pas souhaitable de limiter l'accès à une politique potentiellement bénéfique à une partie restreinte de la population dans le seul objectif de l'évaluer .

L'évaluation de ces programmes reste toutefois nécessaire et, dans cette optique, des méthodes quasi-expérimentales peuvent être envisagées. Les approches quasi-expérimentales tentent essentiellement d'imiter le processus de randomisation décrit ci-dessus en construisant un groupe témoin aussi proche que possible du groupe de traitement, de sorte qu'ils soient statistiquement équivalents ex-ante.

Cette thèse mobilise ainsi diverses méthodes micro-économétriques qui incluent des évaluations quasi-expérimentales et expérimentales.

Ce type d'évaluations ne peut être réalisé que si l'accès à des données adéquates est possible. Il est nécessaire de pouvoir identifier et disposer des informations sur les participants au programme à évaluer, d'avoir accès aux informations permettant de sélectionner le groupe témoin (qui ne fait pas partie du registre du programme) mais également de pouvoir suivre les individus au cours du temps et de pouvoir observer leurs résultats sur le marché du travail.

Les données les plus pertinentes, qui recueillent toutes ces informations, sont les données administratives. Celles-ci sont des informations collectées, utilisées et stockées principalement à des fins opérationnelles, mais peuvent également être utilisées à des fins de recherche sans surcoût. Elles ont également l'avantage de couvrir la quasi-totalité des individus pertinents pour une étude donnée et réduisent ainsi le potentiel biais de non-réponse notamment présent dans les enquêtes. Enfin, elles peuvent être plus précises que les données d'enquêtes pour mesurer des caractéristiques complexes ou difficiles à mémoriser pour les individus (date de fin du dernier épisode de chômage, montant des allocations etc.).

Cette thèse mobilise principalement les données administratives de Pôle emploi. Ces données permettent d'observer les épisodes de chômage de tous les individus inscrits en tant que demandeurs d'emploi en France. Les dates d'entrée et de sortie du chômage sont renseignées ainsi que le type de contrat (CDI, CDD etc.) si la sortie correspond à un retour à l'emploi. En outre, plusieurs caractéristiques sociodémographiques sont renseignées au moment de l'inscription (sexe, âge, niveau d'éducation, qualification, expérience dans la profession souhaitée, etc.), ainsi que des informations sur les épisodes de chômage antérieurs (récurrence et durée du chômage), des éléments relatifs aux allocations de chômage (éligibilité, durée, montant...) et aux autres programmes d'aide sociale (RSA). Ces données fournissent également des informations très détaillées sur les actions des demandeurs d'emploi pendant leurs épisodes de chômage. Les programmes auxquels ils ont participé (programmes d'accompagnement, formations, ateliers etc.), leur conseiller référent et la nature des différentes interactions avec celui-ci (entretiens , appels, mise en relation avec les employeurs etc.) sont nettement identifiables. Afin d'analyser plus en détail les mécanismes derrière l'intervention menée, ces données administratives sont complétées,

dans le troisième chapitre, par des données sur les entreprises, des données d'enquête auprès des demandeurs d'emploi ainsi que des données d'utilisation de la plateforme-web étudiée.

Plan de thèse et principales contributions

Cette thèse pose deux objectifs : le premier est de comprendre les mécanismes qui se cachent derrière la réussite de l'aide à la recherche d'emploi ; le second est de déterminer le rôle du numérique dans cet accompagnement. Elle se présente en trois chapitres. Le premier est dédié à l'évaluation de l'impact d'un programme d'accompagnement numérique conçu pour une population qui n'avait, jusqu'ici, fait l'objet d'aucune étude scientifique : les demandeurs d'emploi les plus autonomes. Le deuxième chapitre explore la question du rôle des conseillers référents dans les politiques d'accompagnement. Enfin, le dernier chapitre propose une analyse simultanée de la réaction des entreprises et des demandeurs d'emploi suite à une stimulation de la recherche d'emploi via une plateforme en ligne.

Chapitre 1 : L'aide à la recherche d'emploi numérique : résultats du programme français "Activ'Emploi"

Depuis les dix dernières années, de plus en plus de programmes d'aide à la recherche d'emploi passent par des plateformes en ligne (par exemple, le site poleemploi.fr, l'Emploi Store, des outils tels que Bob Emploi et la Bonne Boîte etc.). C'est le cas du programme Activ'Emploi. Celui-ci est né de la nécessité d'une différenciation des services de soutien offerts aux demandeurs d'emploi afin de répondre de manière personnalisée à leurs besoins. Il a été mis en place suite à la deuxième convention tripartite de 2012-2014 entre Pôle emploi et l'Unédic .

Afin de mettre en contexte le programme Activ'Emploi, il convient de comprendre comment les demandeurs d'emploi sont traditionnellement pris en charge par les services publics de l'emploi. L'accompagnement traditionnel repose généralement sur des entretiens fréquents et longs entre les demandeurs d'emploi et leur conseiller. Les conseillers aident les demandeurs d'emploi à affiner leur projet professionnel et à analyser le marché du travail ; ils les conseillent sur les processus de candidature ; les guident dans leur recherche d'emploi ; les mettent en contact avec les employeurs ; et les orientent vers d'autres mesures telles que des formations ou des emplois plus adaptés à leurs objectifs professionnels. En France, les évaluations de l'aide à la recherche d'emploi ont porté sur trois programmes principaux : Le Plan d'Aide au Retour à l'Emploi (PARE) mis en place en 2001 (Crépon et al., 2005; Fougère et al., 2010) et deux expériences randomisées contrôlées de grande ampleur, la première auprès de demandeurs d'emploi présentant des risques de chômage de longue durée en 2007 (Behaghel et al., 2014) et la seconde auprès de jeunes diplômés en 2007-2008 (Crépon et al., 2013). Les résultats montrent que ces programmes ont encouragé les demandeurs d'emploi à reprendre le travail et ont contribué à réduire significativement la durée du chômage. Ces dispositifs ont également amélioré la stabilité des emplois

trouvés, rejoignant ainsi les résultats de [Blasco and Rosholm \(2011\)](#) au Danemark qui montrent que l'aide à la recherche d'emploi plus intensive est bénéfique à court et à long terme. Enfin, conformément à la littérature scientifique internationale, ces effets positifs pour les bénéficiaires ne sont pas accrus en cas de sous-traitance à des opérateurs de placement privés ([Behaghel et al. \(2014\)](#) ; [Benmarker et al. \(2013\)](#) ; [Krug and Stephan \(2013\)](#)).

Dans cette configuration, les demandeurs d'emplois les plus autonomes sont ceux qui trouvent le moins de soutien de la part de Pôle Emploi. C'est pour ce type de public que le programme Activ'Emploi a été mis en place. Ce programme ne nécessite pas la présence physique des demandeurs d'emploi et est caractérisé par sa flexibilité et par la liberté accordée à ses bénéficiaires. En raison de ces caractéristiques, le programme cible les demandeurs d'emploi les plus autonomes. Ce programme sous-traite l'accompagnement de ses bénéficiaires à des opérateurs privés. Leur objectif est d'optimiser et de rendre plus "active" la recherche de ses bénéficiaires.

Dans le cadre de ce programme, les demandeurs d'emploi peuvent accéder, via une plateforme web, aux informations et aux outils mis à disposition par le prestataire à tout moment. Les prestataires privés suivent les demandeurs d'emploi à travers différents canaux : lors de réunions physiques, mais aussi par le biais d'appels en vidéoconférence, de chats, de conférences en ligne, de cours en ligne, de courriels et d'appels téléphoniques. Sur la plateforme, ils ont également la possibilité d'accéder à des outils de formation, des ateliers en ligne, des carnets d'adresses, des offres d'emploi (auxquelles ils peuvent directement postuler), des forums, des ressources documentaires, etc.

Activ'Emploi exige un certain niveau d'autonomie de ses bénéficiaires : ils doivent être capables de naviguer sur la plateforme, d'identifier les outils pertinents pour leur recherche et de les utiliser aussi souvent qu'ils jugent nécessaire. Le profil de la population cible du programme est donc très particulier par rapport aux populations de demandeurs d'emploi communément étudiées dans la littérature scientifique. Quelques évaluations indiquent des effets plus importants sur le retour à l'emploi des demandeurs d'emploi ayant moins de difficultés ([Kruppe, 2006](#)). Cependant, à notre connaissance, cette étude est la première à évaluer l'impact de l'aide à la recherche d'emploi lorsqu'elle est destinée aux demandeurs d'emploi les plus autonomes. Au premier abord, il peut sembler contre-intuitif de faire des efforts particuliers pour les demandeurs d'emploi qui se caractérisent par leur proximité à l'emploi. Néanmoins, cette population peut aussi mettre du temps à retrouver un emploi, ce qui pose un problème particulier pour l'assurance chômage, puisque leurs allocations sont généralement plus élevées. Comme le souligne ?, trouver un emploi dépend non seulement des caractéristiques intrinsèques des demandeurs d'emploi, mais aussi de leurs préférences et de leurs croyances. Si les demandeurs d'emploi les plus autonomes entreprennent des efforts de recherche plus importants et espèrent de meilleurs résultats en contrepartie, ils sont susceptibles d'augmenter leur salaire de réserve ([McGee \(2015\)](#)) et de rester au chômage plus longtemps ([Lancaster and Chesher \(1983\)](#) ; [Jones \(1988\)](#)). Un accompagnement personnalisé pourrait donc être bénéfique pour cette population en aidant les bénéficiaires à avoir

une vision plus réaliste de leurs chances de retour à l'emploi, en réduisant leur salaire de réserve, en augmentant leur efficacité de recherche et en améliorant leurs compétences non cognitives (Arni (2015)).

Notre objectif, dans ce chapitre, est ainsi de tester le lien entre utilisation du programme et retour à l'emploi, tout en nous intéressant à la qualité de l'emploi obtenu.

La participation au programme Activ'Emploi n'est pas obligatoire : il y a donc un fort degré de sélection dans le programme. Notre analyse montre ainsi que les participants aux programmes sont ceux qui ont le plus de chances de trouver un emploi. Par conséquent, les bénéficiaires et les non-bénéficiaires d'Activ'Emploi ne sont pas directement comparables. Nous utilisons une stratégie de doubles différences pour identifier l'effet du programme sur les taux de retour à l'emploi à différents horizons. En substance, nous comparons l'évolution des taux de retour à l'emploi entre les cohortes, entre les agences qui ont rapidement augmenté leur taux d'inscription à Activ'Emploi et celles où la proportion d'inscrits y est restée faible. Dans le cas où Activ'Emploi augmente la probabilité de sortie du chômage vers l'emploi, il est attendu que cette sortie soit plus rapide dans les premières agences que dans les secondes.

Les résultats issus des données administratives de Pôle emploi, mettent en valeur une augmentation de la probabilité de trouver un emploi directement en lien avec l'utilisation d'Activ'Emploi. Nous observons ainsi une augmentation d'environ 20% de la probabilité de trouver un emploi avant 6 mois. Par ailleurs, les demandeurs d'emploi qui bénéficient du programme voient également leurs chances de retrouver un emploi stable augmenter. Ces effets semblent être homogènes selon les caractéristiques des demandeurs d'emploi (sexe, âge, etc.).

Chapitre 2 : Quelle est l'importance des Conseillers? Mesurer des « Effets Conseiller » dans la recherche d'emploi

Si l'effet positif des politiques publiques d'accompagnement sur le retour à l'emploi est généralement admis dans la littérature scientifique (Card et al., 2010, 2018), il existe une zone d'ombre concernant les mécanismes qui produisent ces effets positifs. L'un des aspects intéressants et peu étudié des politiques actives du marché du travail est le rôle des conseillers, pourtant au cœur de leur mise en œuvre. En effet, dans de nombreux pays de l'OCDE (France, Royaume-Uni, Suisse, Allemagne etc.), les services publics de l'emploi mobilisent les conseillers pour trois missions principales : i) aider les demandeurs d'emploi dans leur recherche d'emploi ; ii) les contrôler ; iii) les orienter vers d'autres programmes actifs du marché du travail. Ils exercent ainsi une fonction centrale et disposent d'une marge de manœuvre considérable pour façonner les trajectoires des demandeurs d'emploi. Si la littérature s'est beaucoup intéressée à l'analyse de l'impact des politiques du marché du travail, il convient de s'intéresser également au rôle des acteurs de terrain.

L'objectif de ce chapitre est de combler, dans la mesure de nos moyens, ces lacunes, tout en

répondant aux questions suivantes : dans quelle mesure les conseillers aident-ils les demandeurs d'emploi à trouver un (bon) emploi ? Quels sont les différents outils qu'ils mobilisent ? Sont-ils plus importants pour certains types de demandeurs d'emploi ? Répondre à ces questions pourrait être décisif, tant pour définir de meilleures stratégies de ressources humaines pour les conseillers que pour élaborer des politiques actives du marché du travail plus efficaces.

Nous utilisons les bases administratives de Pôle Emploi, grâce auxquelles nous suivons environ 5000 conseillers en charge de plus d'un million de demandeurs d'emploi en région parisienne pendant 5 ans. Nous pouvons observer les demandeurs d'emploi depuis leur inscription à Pôle emploi jusqu'à leur éventuel réemploi. Nous disposons également d'informations très détaillées sur les interactions entre conseillers et demandeurs d'emploi ainsi que sur les différents programmes/services qu'ils proposent, capitales à notre analyse.

Pour mesurer la valeur ajoutée des conseillers de façon causale, nous tirons parti du caractère quasi-aléatoire du processus d'appariement entre les demandeurs d'emploi et les conseillers au sein des agences. Nous adaptons la procédure d'estimation de la valeur ajoutée développée pour les enseignants dans la littérature d'économie de l'éducation ((Koedel et al., 2015; Kane and Staiger, 2008; Chetty et al., 2014)) au contexte des politiques actives du marché du travail. Nous effectuons plusieurs vérifications pour nous assurer que nos estimations de la valeur ajoutée ne sont pas influencées par la sélection des caractéristiques observables des demandeurs d'emploi. Les résultats de notre analyse témoignent d'une divergence des trajectoires des demandeurs d'emploi selon le conseiller qui leur est assigné : le fait d'être suivi par un conseiller classé 1 écart-type plus haut dans la distribution de la valeur ajoutée, se traduit par une augmentation de 8.4% de la probabilité de trouver un emploi dans les 6 premiers mois et de 13.2% de trouver un emploi stable. Il est intéressant de noter que les conseillers à forte valeur ajoutée ne sont pas nécessairement les mêmes lorsqu'on considère le retour à l'emploi dans les 6 premiers mois que lorsqu'on considère le retour à un emploi stable dans les 6 premiers mois. Par ailleurs, les conseillers à forte valeur ajoutée ne se trouvent pas forcément dans les zones dans lesquelles les demandeurs d'emplois sont le plus ou le moins en difficulté. Dans un second temps, nous documentons les pratiques associées à une forte valeur ajoutée des conseillers. Nous construisons des mesures des différents outils déployés par les conseillers (entretiens, propositions d'offres d'emploi, de programmes de formation, etc.). Nous observons une grande variabilité dans leur utilisation. Celle-ci corrobore l'idée selon laquelle les conseillers ont une grande marge de manœuvre.

Nous calculons ensuite la corrélation entre ces mesures et les mesures de la valeur ajoutée des conseillers estimée précédemment. Ce calcul de corrélations nous montre que les conseillers à forte valeur ajoutée ont tendance à proposer plus souvent des entretiens et des offres d'emploi aux demandeurs d'emploi. Ils sont cependant moins susceptibles de leur proposer des formations ou des réorientations professionnelles qui peuvent avoir un impact négatif à court terme (effet de verrouillage) mais positif à long terme. Par ailleurs, nous ne trouvons pas de lien entre le recours aux sanctions ou les caractéristiques des conseillers et la valeur ajoutée de ceux-ci. En comparant

les conseillers qui ont une forte valeur ajoutée selon différents objectifs (placer les demandeurs d'emploi rapidement ou les placer dans des emplois stables), nous observons que leurs pratiques les plus fréquentes diffèrent. Les conseillers qui favorisent des emplois de qualité plutôt que des sorties rapides du chômage mettent de côté certains outils : ils proposent moins souvent des offres d'emploi, des contacts dématérialisés (courriels, appels téléphoniques etc.), ainsi que des services externalisés (qui ont été évalués, dans la littérature, comme inefficaces dans l'objectif de maintenir les individus hors du chômage pendant de longues périodes (Behaghel et al., 2014)). En somme, nos résultats soulèvent un arbitrage potentiel entre des pratiques qui favorisent une sortie rapide du chômage et d'autres qui favorisent l'acquisition de nouvelles compétences et des appariements de meilleure qualité, deux éléments cruciaux dans le long terme.

Enfin, dans l'optique de tester l'hétérogénéité des résultats entre les différents types de demandeurs d'emploi) nous avons calculé les effets des conseillers sur différents sous-groupes de demandeurs d'emploi (homme/femme, niveau d'éducation faible/élevé et probabilité prédite de trouver un emploi faible/élevé). Nous constatons que les conseillers semblent avoir plus d'importance pour les individus moins susceptibles de trouver un emploi d'après leurs caractéristiques observables.

Cet article s'inscrit dans le cadre d'une littérature encore limitée mais croissante sur les conseillers. En explorant séparément certains des canaux à travers lesquels les conseillers agissent, ces contributions mettent en lumière le fort impact que ces agents peuvent avoir sur les trajectoires des demandeurs d'emploi.

Les conseillers jouent un rôle dans l'activation de l'effort de recherche et l'acquisition d'informations des demandeurs d'emploi. Cette dimension apparaît d'autant plus fondamentale que l'importance des rencontres en face à face avec les conseillers (Schiprowski, 2020) et les effets positifs de l'augmentation du nombre d'offres d'emploi proposées par les conseillers aux demandeurs d'emploi (Glover, 2019) ont été démontrés. Ces interventions d'activation/information englobent souvent un rôle moral/psychologique non cognitif des conseillers qui est difficile à isoler (Arni, 2015).

En Suisse, Behncke et al. (2010b) constatent que les conseillers moins coopératifs ont de meilleurs taux de placement et Huber et al. (2017) approfondit cette question en montrant que cet effet est susceptible d'être une conséquence des dimensions du conseil telles que les menaces de sanctions et la pression pour accepter des emplois.

En outre, les conseillers peuvent agir en assignant les demandeurs d'emploi à différents programmes actifs du marché du travail, bien que Bolhaar et al. (2020) ne parviennent pas à détecter des effets de traitement hétérogènes entre les conseillers plus ou moins susceptibles d'utiliser certains programmes.

Au-delà de l'utilisation effective des programmes actifs du marché du travail, Arni et al. (2020) montre que les conseillers peuvent influencer la recherche d'emploi par la perception qu'ont les demandeurs d'emploi de l'utilisation qu'ils comptent faire de ces outils (régime politique).

Enfin, les conseillers agissent non seulement par leurs pratiques mais aussi par leur ressemblance

avec les demandeurs d'emploi qu'ils conseillent. Behncke et al. (2010a) montrent que les chances de trouver un emploi s'améliorent lorsque les demandeurs d'emploi appartiennent au même " groupe " (sexe, éducation et ethnicité) que leurs conseillers référents. Dans le contexte spécifique des attentats de janvier 2015 dans les bureaux du journal " Charlie Hebdo " en France, Glover (2019) montre que les actions des conseillers appartenant à une minorité ethnique ont compensé le choc négatif sur le marché du travail subi par les demandeurs appartenant à ce même groupe ethnique.

Notre article ouvre de nouvelles perspectives sur le rôle des conseillers. Nous ne nous concentrons plus uniquement sur un aspect spécifique de l'accompagnement mais nous donnons une mesure de l'impact agrégé des actions des conseillers sur les résultats des demandeurs d'emploi. Ce faisant, nous sommes les premiers à donner un classement global à une population entière de conseillers en ce qui concerne leur valeur ajoutée. Ces résultats s'inscrivent également dans la littérature plus large et plus établie sur l'importance des individus dans la mise en œuvre des politiques publiques (Chetty et al., 2014; Best et al., 2018). Nous contribuons également à cette littérature en étudiant simultanément les multiples rôles joués par les conseillers. Cela nous permet de dresser un tableau complet des nombreux déterminants de l'efficacité des conseillers et de les comparer les uns aux autres. Nous nous rattachons ainsi à la littérature sur la façon dont les différences de productivité dans la prestation de services publics peuvent être expliquées par des différences dans les pratiques (Bloom et al., 2015b,a). Dans la mesure où nous considérons différents résultats de la recherche d'emploi, nous sommes également en capacité d'évaluer comment les déterminants de l'efficacité des conseillers changent en fonction de l'objectif poursuivi (c'est-à-dire la quantité par rapport à la qualité des placements).

Chapitre 3 : Ouvrir la Bonne Boîte : Réduire les frictions sur le marché du travail grâce à une plateforme en ligne

La théorie du *matching* ou « mécanismes d'appariement » stipule que les frictions sur le marché du travail (information imparfaite, rigidités, etc.) sont à l'origine du chômage. Outre leurs conséquences sur la recherche d'emploi, elles sont supposées être un facteur clé des coûts d'embauche qui contribuent à la détermination de la création d'emplois par les entreprises (Pissarides, 2000). Cependant, bien qu'il existe une riche littérature micro-économétrique sur la recherche d'emploi, peu d'études se sont intéressées à quantifier la réponse des entreprises à la variation des frictions d'embauche (Oyer and Schaefer, 2011). Malgré des changements majeurs dans les technologies d'appariement et d'embauche avec l'arrivée d'Internet, il reste à éclaircir dans quelle mesure les coûts d'embauche des entreprises ont diminué, et dans quelle mesure cela a stimulé les créations d'emplois (Kuhn, 2014).

Cette lacune empirique, concernant un élément clé d'une théorie standard du chômage, peut s'expliquer par le manque de sources crédibles de variation des coûts d'embauche qui sont nécessaires pour identifier les effets sur les décisions de recrutement des entreprises.

Ce troisième chapitre analyse les réactions des demandeurs d'emploi et des entreprises à une tentative de réduire les frictions d'appariement en fournissant des recommandations ciblées. En nous appuyant sur une plateforme en ligne existante gérée par Pôle emploi, nous menons une expérience randomisée bilatérale impliquant environ 1,2 million de demandeurs d'emploi et 100 000 établissements. L'échantillon de demandeurs d'emploi comprend tous les demandeurs d'emploi inscrits à Pôle emploi dans 94 bassins d'emploi (environ un quart du marché du travail français). Les établissements sont sélectionnés par la plateforme "La Bonne Boîte" (ci-après LBB), sur la base d'un algorithme qui prédit les recrutements par entreprise et par métier. L'objectif de Pôle emploi avec ce service est de fournir aux demandeurs d'emploi un accès au "marché caché" des entreprises qui recrutent sans publier d'offres d'emploi. Dans son fonctionnement habituel, le site Internet LBB dirige chaque demandeur d'emploi vers une liste d'entreprises les plus susceptibles de l'embaucher en fonction des critères de localisation géographique et de métier qu'il a renseignés. Pendant l'expérience, alors que la plateforme reste accessible à tous, nous introduisons deux traitements expérimentaux. Premièrement, nous sélectionnons aléatoirement un sous-ensemble d'entreprises. Pendant quatre semaines, ces entreprises "traitées" sont affichées en priorité en réponse aux demandes des demandeurs d'emploi sur le site web, tandis que les entreprises "témoin" restantes ne sont pas affichées (ou affichées en bas de la liste s'il y a trop peu d'entreprises traitées répondant aux critères de recherche).

Deuxièmement, nous tirons au sort deux tiers des 1,2 million de demandeurs pour qu'ils reçoivent des courriels rappelant l'importance des candidatures spontanées et recommandant une ou deux « bonnes boîtes » sélectionnées parmi les établissements traités. Cette randomisation bilatérale fournit une variation aléatoire permettant d'étudier simultanément les réponses de l'offre et de la demande aux recommandations ciblées.

Plus précisément, la comparaison entre les groupes « traité » et « témoin » de demandeurs d'emploi nous permet d'étudier la réponse de l'offre de travail aux recommandations personnalisées. En outre, si les demandeurs d'emploi répondent aux courriels ou à la mise en avant des entreprises sur le site, en envoyant davantage de candidatures aux entreprises traitées, notre expérimentation fournit une variation unique pour étudier la réponse de la demande de main-d'œuvre aux changements du nombre et du type de candidatures spontanées reçues par les entreprises.

Du côté des demandeurs d'emploi, nous constatons que recevoir des courriels contenant des recommandations ciblées augmente légèrement les taux de retour à l'emploi. Cet impact est toutefois faible et concentré sur les femmes : la probabilité qu'elles commencent un nouvel emploi dans les 4 mois augmente de 0,2 point de pourcentage (une augmentation de 2 % par rapport à un niveau de base de 12,9 %). Malgré la taille importante de l'échantillon, nous ne sommes pas en mesure de détecter un effet statistiquement significatif sur les hommes. Du côté des entreprises, nous constatons une augmentation significative des taux d'embauche. Il est

important de noter que si l'augmentation du retour à l'emploi est concentrée sur les femmes et pour les contrats à durée déterminée, les embauches supplémentaires des entreprises ne sont pas particulièrement concentrées sur les femmes et concernent les contrats à durée indéterminée. Ceci suggère que l'effet sur les entreprises est dû à un afflux supplémentaire de candidats causé par l'affichage systématique des entreprises traitées sur le site Internet de LBB, plutôt que par l'envoi de courriels. Par ailleurs, les prédictions de l'algorithme LBB sont globalement correctes : les entreprises dont on prédit qu'elles embaucheront plus embauchent effectivement plus. Cependant, elles n'embauchent que marginalement plus lorsqu'elles sont mises en avant par LBB. La première contribution de cet article est donc de montrer que la publicité des entreprises susceptibles d'embaucher mais qui ne publient pas nécessairement des offres d'emploi a des effets positifs mais limités sur les recrutements.

La deuxième contribution de notre stratégie empirique est d'explorer l'effet de l'élargissement de la recherche d'emploi à d'autres occupations. En effet, notre modèle empirique comprend des bras de sous-traitements supplémentaires : dans un premier bras, il est recommandé aux demandeurs d'emploi qui effectuent des recherches pour un métier donné de postuler auprès des entreprises susceptibles d'embaucher dans le même métier ou dans un métier très proche ; dans le second bras de traitement, il est recommandé aux demandeurs d'emploi de postuler auprès des entreprises susceptibles d'embaucher dans des métiers voisins. Symétriquement, dans un premier bras, les entreprises sont sélectionnées pour recevoir des demandeurs d'emploi cherchant dans le métier pour lequel elles sont susceptibles d'embaucher ; dans un second bras, les entreprises sont signalées aux candidats plus éloignés dans l'espace occupationnel. Cela nous permet d'étudier comment l'élargissement de la recherche d'emploi à des occupations voisines permet de réduire le mauvais appariement occupationnel, une question qui a suscité un intérêt considérable dans la littérature récente (Marinescu and Rathelot, 2018; Belot et al., 2018). Ici encore, notre modèle de randomisation bilatérale nous permet d'évaluer les conséquences de l'extension de la distance occupationnelle dans les recommandations proposées, tant du point de vue des entreprises que des demandeurs d'emploi. En théorie, deux forces opposées sont en jeu : l'extension de la distance occupationnelle permet à l'entreprise (resp. au demandeur d'emploi) d'accéder à un ensemble de choix plus large, mais elle peut aussi augmenter les coûts de sélection et réduire la productivité attendue des appariements proposés. Empiriquement, ces deux aspects tendent à se compenser : en moyenne, nous ne constatons pas que les entreprises (resp. demandeurs d'emploi) orientées vers des demandeurs d'emploi (resp. entreprises) dans des métiers plus proches sont plus susceptibles de recruter (resp. retrouver un emploi).

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Chapter 1

Digital job search assistance:

Evidence from the French program

“Activ’Emploi”

Digital job search assistance: Evidence from the French program “Activ’Emploi” *

Sofia Dromundo[†]

Marc Gurgand[‡]

Abstract

Job search assistance can be an effective tool to help job-seekers exit unemployment faster. So far, however, in the literature, face-to-face intensive counseling is at the center of the attention. Using exhaustive administrative data from the French Public Employment Service, we evaluate a program that focuses on the most autonomous job-seekers and that is mainly offered in digital form. Although the program was launched nationally at the same time, unemployment agencies deployed it at different speed. We perform tests that suggest that these differences across agencies and time are plausibly exogenous and use them to identify the causal effect of the policy. We find that the program increases the probability of finding a job before the 6th month in unemployment by around 20%. The program further increases job quality, as manifested by a higher likelihood of finding a permanent job and not re-registering in unemployment.

JEL Classification: J64 J68

Keywords: Job Search Assistance, Unemployment Duration, Digital, Policy Evaluation

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[†]Paris School of Economics, sofia.dromundo@psemail.eu.

[‡]Paris School of Economics, CNRS, marc.gurgand@psemail.eu

I Introduction

Since the 1970s, most developed countries are facing a rise in the scope and duration of unemployment. To tackle this, public employment services (PES) have oriented much of their efforts towards "active" labor market policies (i.e, training, special programs for young people, etc.). Among these policies, job search assistance has proven to be particularly effective (Card et al. 2010, 2018), especially for the most disadvantaged unemployed (ANSA (2017)). Another major change over the last few decades has been the growing importance of internet and digital tools for job search. In France, for instance, close to 90% of job-seekers declare having relied on the internet in their job search.¹ Despite these major changes in matching technologies, it remains unclear how effective digital tools are (Kuhn and Mansour, 2014).

In order to respond to this growing dematerialisation of job search, PES have to increase their provision of counselling and career advice using digital tools. However on-line job search assistance might not be adequate for all job-seekers. For instance, job-seekers with strong difficulties and lacking digital skills or internet access might not benefit from this technological upgrade and should continue to receive traditional intensive support. Shifting from traditional face-to-face servicing to tailored online experiences could therefore be effective in helping autonomous job seekers with sufficient digital skills to find a job. Moreover these tools could free up more time for PES counselors to spend with job-seekers with higher needs.

In this paper, we measure the efficiency of a job-search assistance program that is provided mostly in a digital form. Naturally, the target of this program is job-seekers who are relatively autonomous in their search, i.e. not facing major barriers to find a job (i.e. handicap), having a coherent professional project and job search strategy. Therefore, we will measure the efficiency of online job search assistance on a relatively qualified population.

Traditional job search assistance relies usually on frequent and long interviews between job-seekers and their counselor. Counselors help job-seekers refine their professional projects and analyze the labor market; advise them on application processes; guide them in their job searches; put them in contact with employers; and direct them towards other measures such as training or jobs more suitable to their professional goals. In France evaluations of job search assistance have focused on three main programs: The Return to Work Assistance Plan (*Plan d'Aide au Retour à l'Emploi (PARE)*) introduced in 2001 (Crépon et al. (2005); Fougère et al. (2010)) and two large-scale randomized controlled experiments, the first among job-seekers with long-term unemployment risks in 2007 (Behaghel et al. (2014)) and the second among young graduates in 2007-2008 (Crépon et al. (2013)). The results show that those programs encouraged the unemployed to return to work and contributed to significantly reduce the duration in unemployment. These devices also improved the stability of jobs found, meeting the results of Blasco and

¹Sandra GAUMONT, Sophie HAMMAN and Franck MANOGIL: "Les demandeurs d'emploi et leur usage du numérique", Pôle emploi, Éclairages et synthèses, n49, 2019.

Rosholm (2011) in Denmark that show that more intensive counseling and job search assistance are beneficial both in the short and in the long run. Finally, consistent with the international literature, these general positive effects for beneficiaries are not increased when outsourced to private placement operators (Behaghel et al. (2014); Bennmarker et al. (2013); Krug and Stephan (2013)). The bulk of this literature focuses (1) on job-seekers who face particular difficulties in accessing employment and (2) on traditional job search support that relies on the physical presence of job-seekers.

In contrast, our study focuses on Activ'Emploi, an online job search counseling service operating in France since July 2015. It does not require the physical presence of job-seekers and it is characterized by the flexibility and freedom given to its beneficiaries. Because of this features the program targets the more autonomous job-seekers, a population that receives almost no support otherwise. This program outsources support of its beneficiaries to private placement operators. Their goal is to optimize and make more "active" the search of its beneficiaries through a flexible, demand-lead, and personalized service. Job-seekers can access the information and tools made available by the provider at any time, through a web-platform. Private providers follow-up job-seekers through different channels: during physical meetings and also through webcam calls, chat, web conferencing, e-learning, serious games, emails and phone calls. In the platform they can also access training tools, online workshops, address books, job offers (to which they can directly apply), forums, documentary resources, etc. Therefore this paper gives insight on the importance of the use of internet oriented tools in job search assistance.

Our paper thus relates to the nascent literature on the impact of internet-oriented tools in the labor market (Kircher, 2020). Leveraging innovative algorithms, Belot et al. (2019) show that providing online advice can expand job-seekers's occupational scope of search and Horton (2017) finds that recommending workers to employers in an online labor market can boost hires. Similarly, in the French context, two online platforms were launched recently: "Bob Emploi" and "La Bonne Boîte". "Bob Emploi" gathers, processes and analyzes big data on the job market, in order to propose a list of steps for job-seekers to overcome the obstacles they face in their job search. The preliminary evaluation of this device shows that it had no impact on employment outcomes (Ben Dhia, 2020). "La Bonne Boîte" is based on an algorithm predicting hirings at the firm \times occupation level. The goal is to provide job-seekers with access to the so-called "hidden market" of firms that recruit without posting job ads. The preliminary evaluation of this tool (Behaghel et al. (forthcoming)) shows positive results: an increase in job finding rates among women, while establishments advertised on the website increase their hirings.

Activ'Emploi requires a certain level of self-sufficiency from its beneficiaries: They need to be able to navigate through the platform, identify the relevant tools for their search and use them as frequently as they consider necessary. Therefore the profile of the target population is very unique as compared to the literature. A few evaluations point to higher impacts on exit rates for unemployed with fewer difficulties. In Germany, Kruppe (2006) shows that vouchers giving access

to private job search assistance are mostly used by unemployed closer to access employment and seem to be efficient in helping those users find a job. However, to our knowledge, this study is the first to assess the impact of job search counseling when designed for the most autonomous job-seekers. At first, it may seem counter-intuitive to make special efforts for job-seekers that are characterized by their proximity to employment. Nevertheless, this population can also take more time to return to employment, which poses a particular problem for unemployment insurance, since they have generally higher unemployment benefits. As [Villeval \(2016\)](#) points out, finding an employment depends not only on the intrinsic characteristics of job-seekers, but also on their preferences and beliefs. If more autonomous job-seekers undertake larger search efforts and hope for better results in return, they are likely to increase their reservation wage ([McGee \(2015\)](#)) and remain unemployed for a longer period ([Lancaster and Chesher \(1983\)](#); [Jones \(1988\)](#)). A personalized assistance could therefore be beneficial for this population. Such support could, for instance, increase the exit to employment by helping beneficiaries to have a more realistic outlook on their chances, reduce their reservation wage and increase their search efficiency by improving their non-cognitive skills ([Arni \(2015\)](#)).

Counselors recommend Activ'Emploi and job-seekers ultimately decide if they want to enroll. As we will see, there is a high degree of selection into the program, the beneficiaries being the ones with higher chances to find a job. As a result, the beneficiaries and non-beneficiaries of Activ'Emploi are not directly comparable. We use a difference in differences strategy to identify the effect of the program on the exit rates to employment, at different horizons. In essence, we compare the evolution of exit rates of job-seekers across cohorts, in agencies that have rapidly increased their enrolment rates with those where the proportion of beneficiaries remained lower. If Activ'Emploi increases the outflow to employment, it is expected that it will accelerate more in the first agencies than in the second. As we reason with variations, we neutralize the effects of systematic differences between the agencies (composition of jobseekers, local labor market, characteristics of the agencies themselves, etc.) and periods.

Using exhaustive administrative data from the French Public Employment Service (PES), we find that the program can increase the probability of finding a job during a given month by one or two percentage points depending on the exit horizon considered. This corresponds to an increase of around 20% in the probability of finding a job before 6 months. In addition, job-seekers who benefit from the program also see their chances to find a stable employment increase. These effects seem to be fairly homogeneous across categories of unemployed (gender, age, etc.).

The paper is organized as follows. Section [II](#) presents the institutional context and the characteristics of Activ'Emploi. Section [III](#) describes the data and presents descriptive statistics. Section [IV](#) presents the identification strategy and assesses threats to its validity. Section [V](#) discusses the results and additional robustness checks. We conclude in Section [VI](#).

II Institutional Setting

II.1 The French Public Employment Service and the need for personalized services

The second tripartite convention of 2012 between the French PES, the State and Unédic (the unemployment insurance provider) highlighted among its three priorities, the need for a differentiation in the support services offered to job-seekers in order to respond in a personalized way to their specific needs and expectations. The PES launched a new structure for its services to job-seekers during the first half of 2013. It assigned of job-seekers into three categories depending on their level of autonomy in the job search. Counselors, mirroring job-seekers, are specialized and counsel job-seekers from only one these categories:

- "Follow-up" : aimed at job-seekers whose autonomy in the search for employment is the greatest. The counselors in charge of this modality can follow between 200 and 350 job-seekers.
- "Guided": for job-seekers who need to be supported by their referral counselor in the search for employment. The counselors in charge of this modality can follow between 100 and 150 job-seekers.
- "Reinforced": aimed at job-seekers who need strong support from their referral counselor, particularly through contacts whose pace and content meet the needs of the applicants. The counselors in charge of this modality can follow up to 70 job-seekers.

Hence the more autonomous the unemployed are, the less job search assistance they receive. For instance in the Follow-up category, the workload of counselors is large and thus counseling is very limited. To illustrate this feature Table [A1](#) in the appendix provides the descriptive statistics on the average number of services counselors provide to job-seekers, during their first 6 months in unemployment, according to their category at registration. We observe that individuals registered as Follow-up receive a lower amount of any type of service than their "Guided" and "Reinforced" counterparts. Putting aside mandatory meetings, job-seekers in the Follow-up category meet three times less frequently with their counselors than job-seekers in the Reinforced category. Furthermore, job-seekers in the Follow-Up category benefit from less than half support services than job-seekers in the Reinforced category. As we focus on the Follow-up category (cf. next section), we evaluate the existence of job search assistance for a population that was almost not exposed to it before. This makes our setting unique since the programs evaluated in the literature aimed at intensifying an already existing job search assistance.

Individuals are allocated to those different categories during their registration interview. The advisor that directs the interview is given guidelines that help her choice.² However, it is important to understand that "autonomy" does not depend only on observable characteristics. The

²The tools for judging an applicant's autonomy are: the existence of job search instruments (updated Curriculum Vitae, cover letter), whether or not the job-seeker has been looking actively for a job, whether or not the

level of support a person needs is not necessarily related to her level of qualification or education. In fact, the advisor perceives many factors that are not apparent in the administrative data that help her differentiate job-seeker's level of autonomy. This case-by-case study may lead the advisor to place, for example, an executive who does not know how to use internet-oriented tools in the "reinforced" or "guided" category for job-seekers who need more support. Similarly, an unskilled worker, in a region where the sector in which she wishes to work is developing and lacking manpower (i.e. a tight market), may be assigned to the "follow-up" category.

In June 2014, 16% of job-seekers enrolled in the PES were classified as needing "reinforced" support, 48% as "guided" and 36% as "follow-up" ³.

II.2 Activ'Emploi

II.2.1 The framework

In the context of a need for more personalized services, the PES has put in place an outsourced service named "Activ'Emploi". It started in mid-2015 and targets the more autonomous job-seekers (the "Follow-up"). This program has two main objectives. Firstly, the complementarity between the services offered by the PES and the private placement operators should enable the PES to increase its support capacities by diminishing counselors workload. The second objective, and the one of interest in this paper, is to give more flexibility to the providers in order to allow them to deliver adapted job search services for autonomous job-seekers. In fact, previous outsourcing experiences were characterized by strict rules from the PES. The activities and tasks that the service provider had to perform were highly detailed preventing the provision of services adapted to specific job-seekers' needs.

Activ'Emploi targets the more autonomous job-seekers, the ones registered in the "follow-up" category, that satisfy four criteria: 1. They have a profile and professional project coherent with the job market; 2. They have a well-defined job search strategy; 3. They do not face a major barrier to find employment (reduced mobility, health conditions, etc.); and 4. They need methodological support to organize and implement their job search processes. If eligible, job-seekers can enroll into the program in two main ways: through self-registration (only before the first meeting) or with a recommendation of the counselor in charge of their follow-up. Counselors can advise the job-seeker to enroll but the final decision depends solely on job-seekers' will.

The maximal duration of Activ'Emploi is 4 months and cannot be renewed. Private operators are required to provide 2 interviews (start and end). Between those interviews contact with the

job-seeker faces major obstacles to employment (handicap, childcare, etc.), the adequacy between the job wanted and the job market and the quality and precision of the professional project.

³Yannick GALLIOT, Eric RENARD – Direction des statistiques, des études et de l'évaluation, Premier bilan après 18 mois de mise en œuvre, les effets des nouvelles modalités de suivi et d'accompagnement des demandeurs d'emploi, octobre 2014.

provider depends on the demand and needs of the job-seeker. Mandatory tasks are limited since Activ'Emploi aims to be flexible and personalized. However, providers are expected to intervene in three ways:

- Providing information and tools related to the functioning of the labor market and the various job search techniques.
- Strengthening the effectiveness of job search procedures (recruitment interviews, job proposals, etc.)
- Providing methodological support for the organization and monitoring of job search procedures.

Activ'Emploi relies on new technologies, with an interactive web platform at the heart of the program. This platform provides, in line services (i.e. Massive learning open online courses) and remote contacts with job counselors (through web-cam meetings, chat and e-mails). Job-seekers can upload information about their competences and experience and apply to exclusive job offers. They also have access to diverse online tools such as workshops, forums, remote meetings with employers, serious games, address books, etc. However, as we will see in next section, the content of the platform varies importantly from one provider to the other.

Furthermore, private placement operators that deliver Activ'Emploi are remunerated in two parts: a lump sum amount and one conditional on job-seekers' recruitment. The lump sum remunerates the provision of reception centers for job-seekers. The fee-for-service payment includes a fixed portion equal to 35% of the unit price if the service is completed and a variable portion up to the remaining 65% which is conditional on the placement of the job-seeker (contract of more than 78 hours per month). This variable part is reduced to 32.5% for a short contract (between 3 and 6 months).

II.2.2 The content of Activ'Emploi's web-platform: different from one provider to another

To hire the private operators in charge of Activ'Emploi's implementation, the PES launched a call for tenders at the county level (French *départements*). Each of the 101 counties was designated to a specific provider, however the same provider could be in charge of many counties. In total 13 private operators were designated across the French territory.

As mentioned before these private operators were given a lot of freedom in the design of the service to provide. Consequently the content and the quality of the web-platform varies from one provider to the other. To illustrate this heterogeneity, Table [A2](#), extracted from the qualitative evaluation made by the consulting firm Geste for the PES, displays the differences in the content of the platform for 4 of the providers. Although there are clear similarities as they all provide some basic services and information, the quality of the platform in terms of its usability (design,

user-friendliness, etc.) and its content diverges. We observe mainly two types of platforms. On the one hand the ones limited to be a resource tool providing job offers, methodological support (help with the CV and Cover Letter) information or training materials, e-learning space etc. (i.e. Anvéol). On the other hand, the more exhaustive ones, that aim to provide a complete online job search assistance (i.e. Onlineformapro). On top of the services offered by the former, the latter also include, for instance, job applications, remote interviews, action plans, etc.

The completeness of the platforms also gives insight of its importance and centrality in the overall service. Providers with less complete platforms are more likely to rely more on a more traditional type of job search assistance where, for instance, face to face meetings are more frequent. Unfortunately, we do not have data on its use by the beneficiaries. Hence, we won't be able to precisely relate the effects we find to the different elements present in the websites nor to their use.

III Data and Descriptive Statistics

III.1 Data

Activ'Emploi started on July 2015. We can follow the first two years since its establishment. Due to the empirical strategy (see section [IV](#)) we will also use the information on the individuals registered at the PES during the two years prior to the implementation. Moreover, since they are the target of the program, the sample is restricted to individuals registered in the "follow-up" category of the PES.

We will measure the impact of the program on the probability for an individual to find a job during months 1 to 6 in unemployment. The data collected for this study ends on the 31 of October 2017, the sample is thus restricted accordingly to individuals enrolled before April 2017.

The population of interest includes therefore all individuals registered as job-seekers (for the first time or after more than 6 months outside of unemployment) between July 1st 2013 and April 1st 2017, and who belong to the "Follow-up" category. We obtain a final sample of 3,358,738 job-seekers. Marginally, some job-seekers oriented into the "Guided" or even "Reinforced" categories, integrated the device, but they are not taken into account in this evaluation.

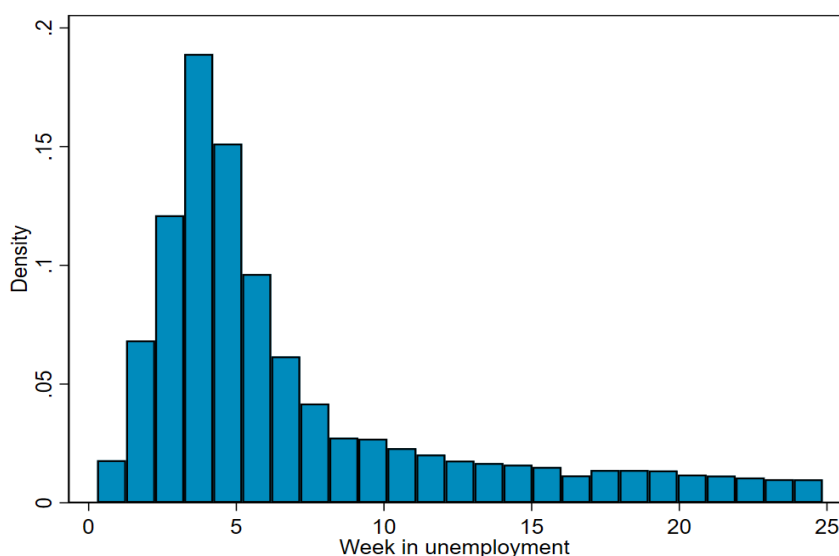
The study uses administrative data from the French Public Employment Service which contains:

- The historical record of job-seekers in unemployment (*Fichier Historique (FH)*). It provides socio-demographic characteristics at registration (gender, age, level of training, qualification, experience in the desired occupation, etc.) as well as information on previous unemployment spells (recurrence and duration of unemployment).

- The historical database of the elements related to their unemployment benefits (eligibility, duration, amount...) and other welfare programs (*Fichier National des Allocataires (FNA)*).
- The exit to employment indicator used by the PES (*Indice de Retour a l'Emploi (IRE)*). It completes the information available in the *FH*, which provides the exits from unemployment as declared by job-seekers, with employment declarations (*Déclaration Préalable à L'Embauche (DPAE)*) filled by the employer. It considers as an exit to employment a DPAE that indicates a contract of more than one month (permanent or temporary), a shift into part-time activity (more than 78 hours per month) while remaining enrolled in the PES, a shift into subsidized employment or firm creation, and the return to employment as declared by individuals.
- The information system SISF that allows to identify job-seekers registered as “Follow-up” as well as the beneficiaries of Activ'Emploi (and other services).

Entry into Activ'Emploi can happen in principle at any time during the unemployment spell. However, as we can see in Figure [A1](#), most of Activ'Emploi enrollments start during the first 6 months (around 70%), and especially during the first 2 months (more than 50%). In Figure [1](#) we zoom-in to look at the exact week enrollments take place during the first 6 months. We observe that enrollment during the first week represent barely more than 1% of enrollments during the first 6 months. Therefore even if they happen early in the unemployment spell, enrollments are not immediate. It takes some time to get information about the program and to get formally registered. Most of the enrollments take place during the 4th week in unemployment.

Figure 1: Histogram of the week of registration in Activ'Emploi among registrations happening during the first 6 months in unemployment.



III.2 Descriptive Statistics

III.2.1 The beneficiaries of Activ'Emploi: The most qualified among the autonomous

The purpose of this subsection is to provide an overview of the characteristics of Activ'Emploi beneficiaries and non-beneficiaries. Only cohorts enrolled as "Follow-up" after the beginning of Activ'Emploi in July 2015 are included.

Table [A3](#) (in the appendix) compares, in the "Follow-up" category, individuals registered in Activ'Emploi before the end of month T in unemployment with individuals who do not benefit from the program before that date.

Columns 1, 4, and 7 show the average of the selected characteristics for individuals treated before the end of the 1st, 3rd and 6th months, respectively. Columns 2, 5 and 8 indicate this average for untreated persons before these same months. Columns 3, 6 and 9 show the differences in characteristics between each pair of treated and untreated groups and indicate whether the difference is statistically significant.

The most striking difference between the two groups concerns levels of qualification and education. Among the beneficiaries of Activ'Emploi there is a higher proportion of technicians (+5 pp) and executives (+7 to 8 pp), and a lower proportion of laborers (-3 to -4pp) and unqualified employees (-5 pp). The level of education is also higher on average for the recipients of the program. University degrees are overrepresented, especially Master's degree and higher diplomas (+10-12pp), while the lack of diploma (-2pp) and school diplomas (-4pp) are underrepresented. Among the treated, a larger share of individuals seeks a permanent job (+6 pp), full-time (+4 to 5pp) and are immediately available (+9 to 12pp). Occupations such as banking and insurance (+1pp), sales and supermarkets (+1.5 pp), and especially business support (+10 to 11pp) are more prevalent in treatment groups, unlike construction (-2pp) and catering services (-3 to -4pp). A smaller proportion of the people who participated in Activ'Emploi benefit from the French welfare program, "*Revenu de solidarité active*"(RSA) (-2pp), which targets households facing greater economic difficulties. Previous enrollment to other services provided by the PES is on average less frequent (+1pp). Regarding unemployment benefits, their average duration and amount (this is a proxy of previous wage) are higher (between 4 and 45 days more and between 4 and 8 euros/day more). Finally the reservation wage (expected wage declared at registration in the PES) is much higher among users of Activ'Emploi (400 euros or about +20%).

Overall, Activ'Emploi is attracting individuals with fewer difficulties among the "Follow up" job-seekers, the ones already close to re-enter employment. The program is thus well-targeted. This finding is also supported by the qualitative evaluation [4](#) of Actv'Emploi that shows that, according to the counselors, the main reason that attracts job-seekers to the program is it is not constraining. Treated individuals could thus be more self-confident in their capacities to find

⁴This qualitative evaluation was made by the consulting firm Geste for Pole Emploi in April 2017

a job. One of the main challenges of Activ'Emploi will thus be to help ut of unemployment individuals that feel they do not need strong support or any support at all.

In addition, the higher level of education and qualification and the type of professions that are overrepresented (service sector) in the treatment can also be explained by the fact that Activ'Emploi relies strongly on the use of new technologies. It thus requires a certain level of familiarity with those tools that is more likely to be found for individuals with those characteristic.

The implication of this analysis for our evaluation is the following: there is clearly a high degree of selection in the program, and it may originate from the referral counselor as much as form the job-seeker herself. To characterize this selection more synthetically, we used individuals enrolled before 2015⁵ and we estimated their probability of finding a job within 6 months based on all the characteristics of Table A3. This gives us a synthetic index of the contribution of all these characteristics to the exit of unemployment. We then used this index to predict the probability of finding a job based on observable characteristics for the sample of cohorts registered after July 2015. Finally we regressed a dummy variable of participation in Activ'Emploi on this index.

The result of this regression is given in Table 1. The coefficient is equal to 0.2, which means that an individual 10 percentage points more likely to find a job according to her observable characteristic (i.e. in the absence of the program) has 2 percentage points more chances to participate in the program. The average probability of participating in Activ'Emploi during the first 6 months is 10%, thus the magnitude of the coefficient is considerable.

We can break down this selection based on the characteristics observed in the data, but it is also likely to depend on a large number of unobservable features or events. This justifies not trying to evaluate the device by simply comparing its beneficiaries and non-beneficiaries, because they are not comparable. Section IV discusses the empirical strategy we propose to overcome this problem.

⁵Knowing that the implementation of Activ'Emploi took place in July 2015, individuals enrolled before 2015 cannot be treated during their first 6 months in unemployment.

Table 1: Correlation between enrolling in Activ'Emploi and the predicted probability of finding a job.

	(1)
	Enrolling into Activ'Emploi during the first 6 months in unemployment
Predicted probability of finding a job within 6 months in unemployment	0.200 (0.00434)
Fixed Effects	YES
N	1920191
F	2135.7
Mean	0.102
Adjusted R2	0.0505

Notes: Standard errors clustered at the agency level are displayed in parenthesis. This equation concerns individuals registered after 2015 and therefore likely to be treated. The estimated probability of finding a job was calculated in a separate regression for individuals registered before 2015 using an OLS model. It regresses the probability of finding a job within 6 months from registration on agency and region*cohort fixed effects and includes as controls the variables listed in Table [A3](#).

III.2.2 The implementation of Activ'Emploi

Diversity in the implementation of Activ'Emploi across cohorts

Figures [2](#) and [3](#) show the rise in the take up of Activ'Emploi, in absolute and relative terms respectively. We consider the number of entrants in the program during the first 6 months in unemployment by entry cohort. In absolute terms, there is a gradual increase in enrollment from January 2015 that reached its maximum in September 2015 with 13,128 individuals enrolled in Activ'Emploi during their first 6 months in unemployment. After that, the number of beneficiaries decreases and fluctuates between 4,000 and 10,000 beneficiaries per cohort. These figures are partly driven by fluctuations in the number of individuals registered as unemployed in the "Follow-up" category.

In relative terms, the proportion of Activ'Emploi beneficiaries increases almost continuously over time: 10.5% of unemployed registered in the "Follow-up" category in September 2015 benefit from this program. The share of users reaches its maximum in March 2017 with 14.6% of "Follow-up" job-seekers benefiting from Activ'Emploi.

Figure 2: Number of Activ'Emploi beneficiaries during the first 6 months in unemployment by entry cohort.

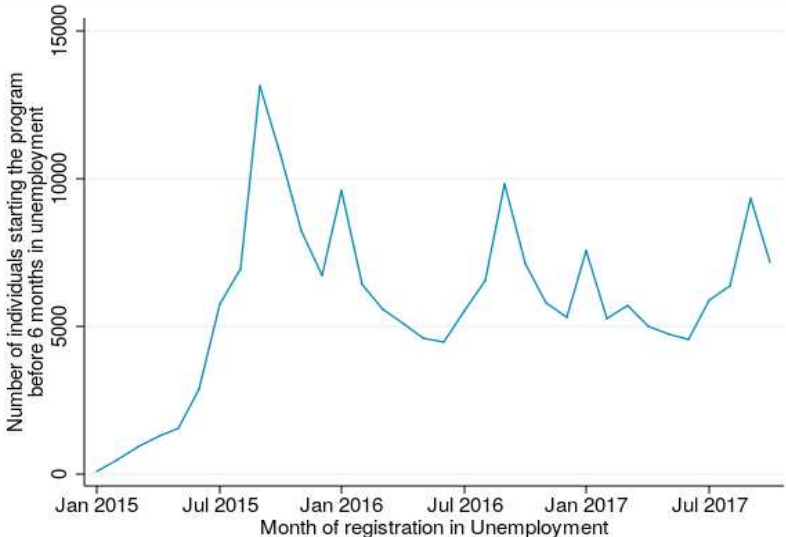
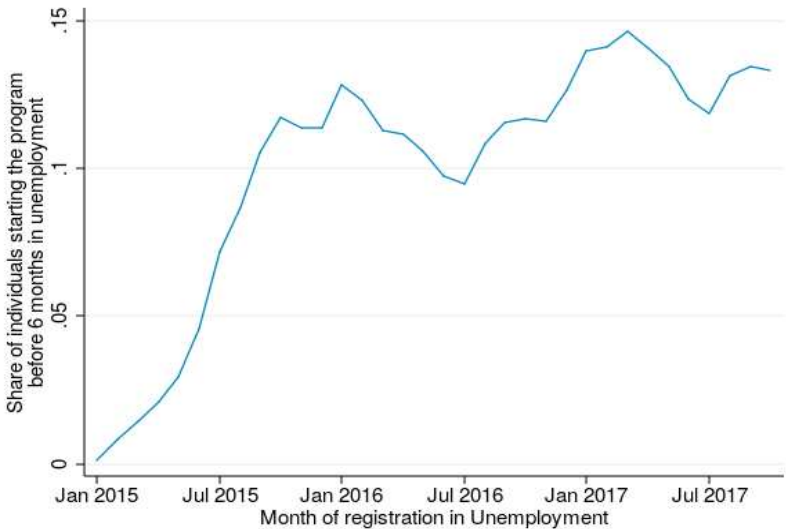


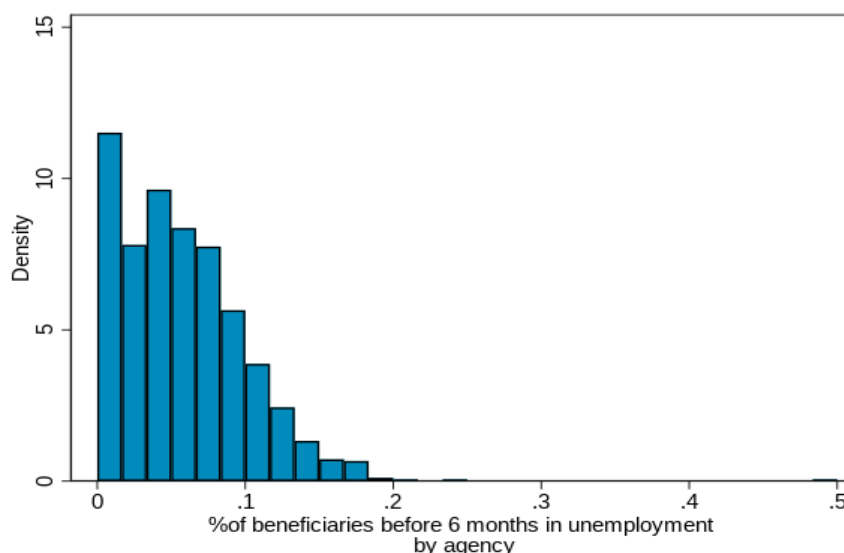
Figure 3: Share of Activ'Emploi beneficiaries during the first 6 months in unemployment by entry cohort.



Diversity in the implementation of Activ'Emploi across agencies

The implementation of the program not only varied over time but also across agencies. Some agencies did not implement the program at the beginning, while others, sent more than 10% of job-seekers to treatment during the first year. This is illustrated in Figure 4 that displays the distribution of agencies according to the share of individuals from cohorts between January and December 2015 that entered the program .

Figure 4: Distribution of the enrollment rate into Activ'Emploi during the first 6 months in unemployment by agency in the first year of the implementation.

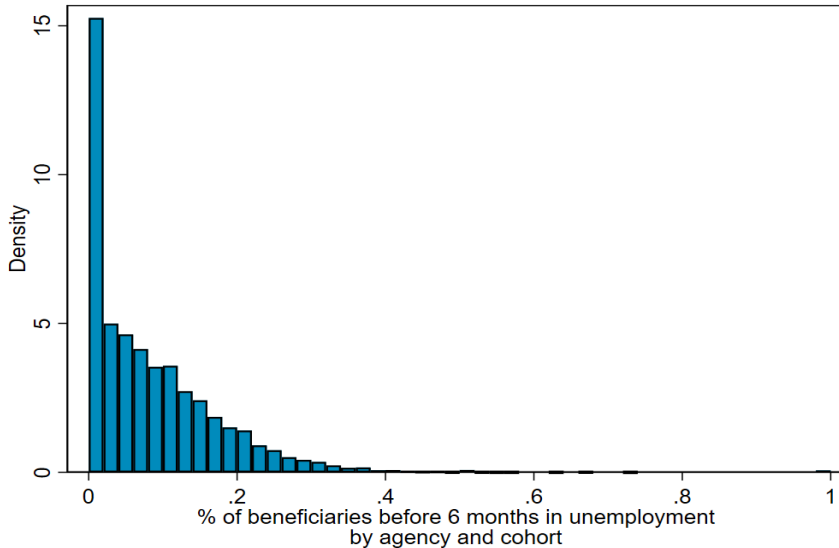


Notes: Only individuals registered as unemployed between January and December 2015 are included. During this period the average rate of participation into Activ'Emploi per agency is of 0.057 with a standard deviation of 0.044.

Diversity in the implementation of Activ'Emploi across agencies and cohorts

Finally, Figure 5 illustrates the diversity of program implementation across unemployment entry cohorts and agencies. The observation unit is each entry cohort after January 2015 in each agency. In more than 15% of these units, no one benefits from the program. Conversely, the number of cohorts where more than 40% of the individuals are treated in an agency is negligible. On average, agencies treat 8.4% of individuals of the same cohort with a standard deviation of 9.3%. This high standard deviation indicates significant heterogeneity in treatment rates across agencies and cohorts, and, as we will see later, this variation is valuable for our estimation strategy.

Figure 5: Distribution of the enrollment rate into Activ’Emploi during the first 6 months in unemployment by agency and cohort



Notes: Only individuals registered as unemployed after January 2015 are included. The average rate of participation into Activ’Emploi per agency and cohort is 0.084 with a standard deviation of 0.093.

IV Empirical Strategy

IV.1 Statistical Model and Empirical Strategy

Job seekers select into Activ’Emploi, as a result, the beneficiaries and non-beneficiaries of Activ’Emploi are not directly comparable. In addition, not only the fact of benefiting from the program but also the moment in unemployment the treatment takes place is endogenous. As pointed out by [van den Berg et al. \(2016\)](#), the identification of treatment effects in duration outcomes is not only hampered by static endogeneity but also by dynamic endogeneity. This second threat states that even if the assignment to treatment was randomized the distribution of unobserved characteristics among survivors some later point in time might differ across treatment arms. Therefore, in our setting, trying to determine the effect of the program on hazard rates would systematically lead to biased estimates. Consequently, we focus on the unconditional probability of finding a job during a given month in unemployment $P(T = t)$ and not on hazards $P(T = t|T \geq t)$. The estimated parameters will allow us to deduce the cumulative probability of finding a job $P(T \leq t)$.

We denote T a discrete random variable for the duration in unemployment. Individuals can enter treatment at any date a when they belong to a cohort exposed to treatment (i.e. post July 2015). As shown in Figure [1](#), most entries in the treatment during the first month happen

during the 4th week, it is thus unlikely that entries into treatment during the first month has a significant effect on exits during the first month

Therefore, we assume that entry into treatment during some period only affects exit during the following periods. We will however run an alternative specification where we allow treatment entry to affect exits occurring the same month, starting from the second month.⁶ Define:

$$\begin{aligned} H_0(t) &= P(T = t|a \geq t) \\ H_1(t) &= P(T = t|a = s) \quad \forall s < t \end{aligned}$$

where $H_0(t)$ is the unconditional probability to exit during t when untreated (treatment will happen during t or later, possibly never); and $H_1(t)$ the unconditional probability to exit during t for someone that has entered the treatment some earlier month s . We assume the latter function does not depend on the exact date of entry into treatment s . Note $\delta(t) = H_1(t) - H_0(t)$, the treatment effect at date t in this model.

We can write:

$$\begin{aligned} P(T = t) &= P(T = t|a \geq t)P(a \geq t) + \sum_{s=1}^{t-1} P(T = t|a = s)P(a = s) \\ &= H_0(t)[1 - \sum_{s=1}^{t-1} P(a = s)] + H_1(t) \sum_{s=1}^{t-1} P(a = s) \\ &= H_0(t) + [H_1(t) - H_0(t)] \sum_{s=1}^{t-1} P(a = s) \\ &= H_0(t) + \delta(t) \sum_{s=1}^{t-1} P(a = s) \end{aligned}$$

Consider those probabilities at the cohort \times agency level, where cohort is indexed by c and agencies by k .⁷ Our identification strategy will make use of the fact that before July 2015 no cohort is treated, and that different agencies included different cohorts into the treatment at different speed after 2015, generating variation in exposure to treatment. We will assume that the treatment rate during the first period $P(a = 1|c, k)$ for a given cohort \times agency group is exogenous to the exit rates conditional on cohort and agency fixed effects: this is a difference-in-difference

⁶As a matter of fact, Figure [1](#) also shows that entry into treatment tend to happen earlier in the month, past the first month.

⁷There is no identifying information from within cohort \times agency variation in our identification setup, so we only consider aggregates.

identification, which will be assessed below. Notice that the exogeneity assumption is not made over any further treatment probability $P(a = s|c, k)$: indeed, the unconditional probability to be treated is mechanically correlated with the probability to exit unemployment, because only those still unemployed can enter treatment, and the difference-in-difference hypothesis would certainly not hold. We make this explicit now.

Consider the additive model for $H_0(t)$:

$$H_0(t|c, k) = \alpha^t + \mu_c^t + \theta_k^t + \varepsilon_{ck}^t$$

where μ_c^t and θ_k^t are cohort and agency fixed effects, α^t a constant and $E(\varepsilon_{ck}^t) = 0$. Then, we have $P(T = 1|c, k) = H_0(1|c, k)$, and for all $t > 1$:

$$P(T = t|c, k) = \alpha^t + \mu_c^t + \theta_k^t + \delta(t) \sum_{s=1}^{t-1} P(a = s|c, k) + \varepsilon_{ck}^t \quad (1)$$

In this model, all ε_{ck}^t 's are likely to be correlated with each other, if only because all exit probabilities must sum to one. For instance, the groups (cohort \times agency) of a “good” type, that are more likely to exit early (high ε_{ck}^1 , say), are also less likely to exit late (low ε_{ck}^6 , say). Also, any $P(a = s|c, k)$ tends to be higher when more individuals from group (c, k) are still unemployed in period s (thus exposed to enter treatment), as can be seen from $P(a = s|c, k) = P(a = s|c, k, T \geq s)P(T \geq s|c, k)$. Therefore $P(a = s|c, k)$ is mechanically correlated with all ε_{ck}^t 's, as they altogether contribute to the probability to be still unemployed at any time. Thus, $P(a = s|c, k)$ is endogenous in equation (1).

An identification hypothesis could be:

$$\forall t, \quad E(\varepsilon_{ck}^t | P(a = 1|c, k)) = 0 \quad (2)$$

It says that, conditional on fixed effects, the groups where people are more likely to enter treatment during the first period would not be systematically more or less likely to exit unemployment early or late in the absence of treatment. This can be challenged for instance if agencies tend to encourage more into treatment those very cohorts that they anticipate to face idiosyncratic chocs (positive or negative) to unemployment exit. We will discuss this below.

Under this hypothesis, we can identify $\delta(t)$ in equation (1) using $P(a = 1|c, k)$ to instrument $\sum_{s=1}^{t-1} P(a = s|c, k)$. There is however an additional difficulty: hypothesis (2) is acceptable if T represents very short periods. But our data can only measure exits within a month. Assume many exits take place during the first week (which it does), and entry into treatment rarely happens so soon (As shown in Figure 1, more than 18% of the entries during the first 6 months happen

during the 4th week, whereas less than 2% happen during the first week), then groups that exit fast in the first month (ε_{ck}^1 is high) will have lower treatment rates $P(a = 1|c, k)$; and because the whole set of ε 's are correlated with each other, so is $P(a = 1|c, k)$ with all the other residuals, mechanically. Table [A4](#) shows the regression of $P(a = 1|c, k)$ on $\hat{\varepsilon}_{ck}^1$ for cohorts registered since July 2015. The correlation is negative and significant: when exit rates during the first month increase by 1 percentage point for a cohort of a given agency, the share of individuals enrolling in Activ'Emploi during the first month in that cohort×agency decreases by 0.04 percentage points (in relative terms this corresponds to a decrease of around 75%). Therefore, identification must exploit the fact that cohorts×agencies that have similar exit rates in month 1, may *still* have different treatment rates, which is an identifying source of variation we can exploit. Therefore, we restrict the identification hypothesis to: [8](#)

$$\forall t > 1, \quad E(\varepsilon_{ck}^t | P(a = 1|c, k), \varepsilon_{ck}^1) = E(\varepsilon_{ck}^t | \varepsilon_{ck}^1) = \rho^t \varepsilon_{ck}^1 + \varepsilon_{ck}^{\prime t} \quad (3)$$

In practice, it means that we can run:

$$P(T = 1|c, k) = \alpha^1 + \mu_c^1 + \theta_k^1 + \varepsilon_{ck}^1$$

form $\hat{\varepsilon}_{ck}^1$ and, for $t > 1$, run:[9](#)

$$P(T = t|c, k) = \alpha^t + \mu_c^t + \theta_k^t + \delta(t) \sum_{s=1}^{t-1} P(a = s|c, k) + \rho^t \hat{\varepsilon}_{ck}^1 + \varepsilon_{ck}^{\prime t} \quad (4)$$

where $\sum_{s=1}^{t-1} P(a = s|c, k)$ is instrumented by $P(a = 1|c, k)$.

IV.2 Validation of the identification strategy: Parallel trends test

An indirect test of hypothesis [\(3\)](#), in the spirit of parallel trend (or placebo) testing in pre-treatment periods, would check if those agencies that have increased treatment rates rapidly were already on an increasing or decreasing unemployment exit trend. Namely, in any agency k , we would take cohorts c' that entered unemployment before July 2015 (such that $P(a = 1|c', k) = 0$) and cohorts c that were exposed to the treatment (such that $P(a = 1|c, k) > 0$) and run the reduced form:

$$P(T = t|c', k) = \alpha^t + \mu_{c'}^t + \theta_k^t + \delta P(a = 1|c, k) + \rho^t \hat{\varepsilon}_{ck}^1 + \varepsilon_{c'k}^{\prime t} \quad (5)$$

⁸Note that we do not assume $E(\varepsilon_{ck}^t | \varepsilon_{ck}^1) = 0$. Indeed ε_{ck}^t and ε_{ck}^1 are serially correlated, and ε_{ck}^1 is an endogenous control. The assumption we make instead is less restrictive and is not hindered by the serial correlation of ε 's.

⁹We approximate a linear relation between the residuals, but we will test more flexible specifications.

and test that $\delta = 0$.

Put differently, we test that the agencies in which the enrollment rate in Activ'Emploi increases the fastest, are not particularly those whose effectiveness to place job-seekers was on a growing trend in the months before the beginning of the program. Otherwise, one might think that the agencies that increased the enrollment into the program are also those who, "on their way", would have also increased, anyway, their rates of return to employment.

Such a test is performed by ruining equation 5. In table 2 we show the results of this regression when we select c' as the cohorts registered between July and December 2014 (that could not enter the program during their first 6 months in unemployment); and we define $c = c' + 12$, so that c contains the first treated cohorts (from July to December 2015). Table 2 shows non significant coefficients and can't reject $\delta = 0$. This result is robust to including $\hat{\varepsilon}_{c'k}^1$ and/or controls in the specification.

Furthermore our identification strategy consists in a generalized difference in difference, meaning that our identifying variation does not come only from the before/after comparison but also exploits the comparison of treatment rates across adjacent cohorts between agencies after July 2015. Therefore we can run a more general version of this test, assuming again that the treatment took place 12 months after the actual treatment ($c = c' + 12$), but without making any further restriction on the sample. Results are shown in Table 3. If the common trend assumption holds, there should not be any significant effect on a given cohort of the treatment rate of cohorts 12 months away, even if the current cohort is also treated. We observe that this is the case: the coefficients obtained are again non significant and close to 0 in magnitude.

Table 2: Parallel trend test (among non treated c' cohorts)

	(1)	(2)	(3)	(4)
	$P(T=6 c',k)$	$P(T=6 c',k)$	$P(T=6 c',k)$	$P(T=6 c',k)$
$P(a=1 c,k)$	-0.00935 (0.00869)	-0.00576 (0.00877)	-0.00942 (0.00866)	-0.00581 (0.00873)
$\hat{\varepsilon}_{c'k}^1$			-0.0477 (0.0128)	-0.0475 (0.0124)
Controls	No	Yes	No	Yes
N	5823	5823	5823	5823
F	1.158	3.331	7.233	3.535
Adjusted R2	0.373	0.406	0.375	0.407

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used in columns (2) and (4) are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency and cohort are applied.

Table 3: Generalized parallel tend test (all cohorts c')

	(1)	(2)	(3)	(4)
	P(T=6 c',k)	P(T=6 c',k)	P(T=6 c',k)	P(T=6 c',k)
P(a=1 c,k)	0.00160 (0.00362)	0.00157 (0.00366)	0.00173 (0.00361)	0.00170 (0.00365)
$\hat{\varepsilon}_{c'k}^1$			-0.0646 (0.00620)	-0.0595 (0.00478)
Controls	No	Yes	No	Yes
N	38128	38128	38128	38128
F	0.195	8.288	55.58	10.35
Adjusted R2	0.329	0.363	0.332	0.366

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used in columns (2)and (4) are the average per agency and cohort of the following variables: age , sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability),qualification level , profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency and cohort are applied.

V Results

V.1 Naive differences in differences

Before moving to the model, a naive difference in difference can give an initial insight into the data. We simplify the problem as if there were only 2 periods (before and after) and 2 treatment arms (treatment and control). We then look at the effect of Activ'Emploi at the individual level on the (unconditional) probability of finding a job before 6 months ($P(T \leq 6)$).

In this exercise we compare the cohorts registered from July to December 2015 (after, Time=1) to the cohorts registered from July to December 2014 (before, Time=0), seasonality is therefore controlled for. We will then divide the agencies by tercile according to the share of individuals treated during the first month in the period after the implementation. The treatment dummy is defined as follows: an individual is considered as treated if she is registered in an agency from

the top tercile (Treatment=1), she is considered as control if registered in an agency from the bottom tercile (Treatment=0).

The equation estimated is simply:

$$P(T \leq 6) = \mu * Treatment + \gamma * Time + \delta_{naive} Time * Treatment$$

The estimation results are presented in Table [A11](#) in the appendix. The coefficient δ_{naive} estimated is positive and statistically significant at 1%. The magnitude is considerable: an increase of 1 percentage point in the probability of finding a job before 6 months. Since the average treatment rate during the first month is 8 percentage points higher in agencies from the top tercile than in the ones from the bottom tercile, by doing a naïve extrapolation, this comes back to an effect of 12.5 percentage points of being treated during the first month at the individual level (100% instead of 8%) which corresponds to an increase of 35%. This result is pretty much coherent with our main findings.

V.2 Results on the probability of finding a job

This subsection presents the effects of the treatment on the probability of exiting unemployment in a given month based on our empirical strategy. The estimates of equation [4](#) are presented Table [4](#). The effect of enrolling into Activ'Emploi before a given month on the probability of finding a job that month is computed for months 2 to 6 in unemployment. For each of these equations we first exclude (odd columns) and then include (even columns) control variables. The contribution of $\hat{\varepsilon}_{ck}^1$ is also displayed. Moreover, since our main equation consists in a 2SLS, the endogenous OLS equation, the first stage and the reduced form equation are presented in the appendix in tables [A5](#), [A6](#) and [A7](#) respectively.

Since the data is aggregated by agency and cohort, the coefficients in Table [4](#) can be understood as follows: when the proportion of job-seekers enrolled in Activ'Emploi during the first 2 months in a cohort of a given agency increases by 1 percentage point, the exit rate during the 3rd month in this agency*cohort increases by 0.0174 percentage points. This is the correlation that the data describes. For such a thing to happen, if treatment effect was homogeneous, every individual who happens to be treated must have gained 1.74 points in her probability of exiting unemployment during the 3rd month.

We observe that irrespective of the month of exit considered the effect of Activ'Emploi ranges between an increase of 1 and 2 percentage point, except for the 2nd month where the effect losses significance with the inclusion of controls. This corresponds to a sizable and economically relevant effect as it represents, in relative terms, an increase that goes from +10% for exists during the 2nd month to +36% for exists during the 6th month.

As explained in section [IV](#), we do not estimate an effect of treatment during the first month on exit during the first month, and also make the assumption that treatment during some month only affects exit during the following months. This hypothesis can be relaxed, assuming that, from the second month-on, treatment during a given month could have an impact on exit during the same month. Tables [A8](#), [A9](#) and [A10](#) in the appendix, show respectively the 2SLS main equation, the OLS and the first stage¹⁰, when we run an alternative specification in which we let treatment affect exits the same month it occurs. Tables [A8](#) and [4](#) display almost identical results. Our specification is therefore robust to this hypothesis.

A complementary qualitative evaluation was conducted by the PES. Among the work carried out, a web survey conducted among job-seekers allows us to highlight potential mechanisms behind the effectiveness of Activ'Emploi. This survey has 14,000 respondents including 8,000 beneficiaries of Activ'Emploi. The control population consists of individuals in "Follow-up" that are comparable to the treated, based on observable variables. The results of the qualitative assessment are consistent with our findings. We note that the beneficiaries of Activ'Emploi make more frequent use of spontaneous applications, they widen their research more often to other geographical sectors and occupations, they mobilize more resources from the "Emploi Store" (official Pôle Emploi Website) and social networks, they feel better equipped and more autonomous than before, they are satisfied with the service and in particular the content of the service (the quality of the advice, the accessibility of the online platform, the flexibility of the monitoring procedures and its ease of use).

Finally, in order to make these results comparable to those of the literature, we can compute the overall effect on the cumulative probability of finding a job before the 6th month in unemployment ($P(T \leq 6)$). From the empirical model presented in section [IV](#), we can write:

$$P(T \leq t) = \sum_{x=1}^t H_0(x) + \sum_{s=1}^{t-1} \left[\sum_{i=s+1}^t \delta(i) \right] P(a = s)$$

Therefore, since we assume treatment effects do not depend on the exact date of entry into treatment, the effect of treatment during month s on the cumulative probability of finding a job before t is simply the sum of the treatment effects δ from s to t . For instance, the effect of entering treatment during the first month on finding a job before 6 months is given by the sum of the coefficients of table [4](#). This gives us an effect of 6.3 percentage points which corresponds to an increase of about 20% in the exit rate.¹¹ Furthermore, to have an idea of the effect of the program as it was implemented we can apply this equation to the observed treatment rates of a given cohort. For instance if we take the cohort that was the most exposed to the program

¹⁰The reduced form is the same for both specifications.

¹¹The baseline probability of finding a job before 6 months for this population is around 32%.

in our data, the one entering unemployment in March 2017, we observe that treatment rates ($P(a = s)$) from months 1 to 5 are 0.057, 0.061, 0.014, 0.008 and 0.003 respectively. Therefore we can infer that the effect of the treatment was, for this cohort, an increase of 0.755 percentage points (+2.2%) on exit rates before 6 months in unemployment¹².

These effects are large but remain lower than those observed in intensive support programs for job-seekers with severe difficulties. For example, Behaghel et al. (2014) obtain effects of an intensive job search assistance program that is about +50% of exits before 6 months in unemployment .

Table 4: Effect of Activ'Emploi on the probability of finding a job at different horizons (2SLS)

	% of Job-Seekers finding a job during month T in unemployment by agency and cohort									
	T=2 (1)	T=2 (2)	T=3 (3)	T=3 (4)	T=4 (5)	T=4 (6)	T=5 (7)	T=5 (8)	T=6 (9)	T=6 (10)
% of JS treated before T by agency*cohort	0.0198 (0.00642)	0.00983 (0.00636)	0.0231 (0.00499)	0.0174 (0.00500)	0.0179 (0.00467)	0.0131 (0.00461)	0.0105 (0.00417)	0.00880 (0.00417)	0.0152 (0.00458)	0.0139 (0.00474)
$\hat{\varepsilon}_{ck}^1$	-0.0668 (0.00809)	-0.0796 (0.00795)	-0.0732 (0.00643)	-0.0768 (0.00613)	-0.0708 (0.00636)	-0.0725 (0.00612)	-0.0608 (0.00504)	-0.0602 (0.00467)	-0.0620 (0.00570)	-0.0576 (0.00446)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	44251	44246	44251	44246	44251	44246	44251	44246	44251	44246
F	40.07	21.28	75.65	219.0	70.64	95.07	78.14	317.7	61.50	706.7
Mean	0.0958	0.0958	0.0667	0.0667	0.0525	0.0525	0.0437	0.0437	0.0391	0.0391

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency and cohort are applied.

V.3 Results on the quality of the job found

We found that Activ'Emploi has a positive effect on job finding rates. In other words, the beneficiaries of Activ'Emploi exit faster unemployment. However, this does not mean that the job found is of good quality and suitable for the jobseeker. If this is not the case, even with a positive effect on the return to employment, the program might not be desirable. The administrative

¹²Applying the equation, we have (in percentage points): $(0.057*6.3) + (0.061*5.3) + (0.014*3.6) + (0.008*2.3) + (0.003*1.4) = 0.755$.

data of Pôle Emploi allow us to approximate the quality of the job found by informing us about the sustainability of it thanks to two particular information: on the one hand we know if the job found is a permanent contract (*CDI*), on the other hand we can track exiting job-seekers and find out if they re-registered into unemployment and when.

Table 5 shows the results on the quality of the job found. The explained variables are the percentage of individuals who find a permanent job during months 2 to 6 in unemployment, in a given cohort and agency (columns 1 to 5), and the percentage of individuals who find a job in the same month and do not re-register in unemployment within 6 months after this exit (columns 6 to 10). For the second type of outcomes the sample is restricted to cohorts that we can observe at least one year (i.e. 6 months for exit to employment and 6 more months for re-registration).

We find positive and sizeable effects on both the return to employment on permanent contracts and the fact of not re-registering on the PES lists within 6 months, although they are not always statistically significant. The estimated effect, on the probability to find a permanent job is always positive and significant at the 5% level except for exits during the 6th month in unemployment. The effect on exits during the 3rd month, for instance, is of about 0.06 percentage points, which corresponds to an increase of the order of 40%.

The effect on finding a job and not re-registering in unemployment in the 6 following months is always positive but less often significant and of lower magnitude in relative terms. The estimates on exits during the second and fifth months in unemployment are insignificant. During the 3rd month in unemployment the effect is about 0.10 percentage points which corresponds to an increase to the order of 17%.

According to these results, Activ'Emploi would therefore help job-seekers not only to find a job faster but also a stable job.

Table 5: Effect of Activ'Emploi on the quality of the job found at different horizons (2SLS)

	% of Job-Seekers finding a permanent job during month T by agency and cohort					% of Job-Seekers finding a job during month T and not re-registering in the following 6 months by agency and cohort				
	T=2	T=3	T=4	T=5	T=6	T=2	T=3	T=4	T=5	T=6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
% of JS treated before T by agency*cohort	0.0103 (0.00345)	0.00673 (0.00281)	0.00846 (0.00248)	0.00669 (0.00239)	0.00243 (0.00205)	0.00480 (0.00631)	0.00982 (0.00532)	0.00895 (0.00468)	0.00548 (0.00433)	0.0122 (0.00477)
$\hat{\varepsilon}_{ck}^1$	-0.0250 (0.00712)	-0.0266 (0.00594)	-0.0116 (0.00513)	-0.0136 (0.00449)	-0.0113 (0.00412)	-0.0752 (0.00720)	-0.0658 (0.00574)	-0.0592 (0.00519)	-0.0530 (0.00480)	-0.0427 (0.00439)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	44246	44246	44246	44246	44246	38560	38560	38560	38560	38560
F	11.00	317.0	49.98	10948.0	178.8	16.90	56.27	705.8	446.6	48357.6
Mean	0.0256	0.0179	0.0141	0.0115	0.00990	0.0829	0.0567	0.0438	0.0363	0.0316

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency and cohort are applied.

V.4 Heterogeneity

To better understand the mechanisms behind the impact of Activ'Emploi it is interesting to study the heterogeneity of it. Indeed, studying whether the magnitude of this effect differs between subgroups of the population could inform us about how Activ'Emploi acts on individuals. For this purpose, the population will be divided into 2 groups of similar size for each of the following characteristics: sex, education, age, and reservation wage. Table 6 shows the effects of Activ'Emploi on exit to employment for each of these 8 sub-populations for the exit rate at 6 months.

The difference in difference at the agency level reveals no heterogeneity of the effects and therefore does not allow speculations on the mechanisms. The standard errors are too large to demonstrate statistically different effects between subgroups. There is therefore no reason to consider that the effects are heterogeneous depending on the dimensions examined, despite the high precision of our estimates.

Table 6: Heterogeneity of the effect of Activ'Emploi on the probability of finding a job at 6 months (2SLS)

% of Job-Seekers finding a job during the 6th month in unemployment by agency and cohort among:								
	Woman	Man	High Education	Low Education	Old	Young	High Reservation Wage	Low Reservation Wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
% of JS treated before the 5th month by agency*cohort	0.0204 (0.00571)	0.0124 (0.00538)	0.0149 (0.00440)	0.0164 (0.00638)	0.0133 (0.00569)	0.0129 (0.00486)	0.0116 (0.00518)	0.0198 (0.00605)
$\hat{\epsilon}_{ck}^1$	-0.0470 (0.00404)	-0.0543 (0.00433)	-0.0516 (0.00443)	-0.0490 (0.00447)	-0.0507 (0.00446)	-0.0540 (0.00405)	-0.0601 (0.00486)	-0.0418 (0.00377)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	43124	43509	42878	43411	43642	42885	43435	42819
F	9.597	11.48	3022.9	137246.7	9.894	9.267	10.47	53.42
Mean	0.0373	0.0408	0.0425	0.0364	0.0362	0.0416	0.0438	0.0345
P-value equality test	0.659		0.169		0.299		0.482	

Notes: Standard errors, displayed in parenthesis, are clustered at the agency level. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency and cohort are applied. The P-value of the wald test on the equality of the coefficients for each pair of sub-groups is displayed.

VI Conclusion

Previous economic research has found that intensive job search assistance improves the return to employment for the most vulnerable job-seekers. However it cannot be taken for granted that job search assistance will help the same way different types of job-seekers with different needs. This paper tackles for the first time job search assistance when conceived for the more autonomous job-seekers through the evaluation of the program “Activ'Emploi”.

This paper also contributes to the literature on the effectiveness of digital tools on job search Assistance. In contrast with previous studies, under this program, high frequency meetings are not required, job-seekers have access to a web platform through which they can contact counselors when they need and in a remote way (web-cam meetings, chat, etc.). Exclusive job offers and online courses among other services are also at their disposal.

This paper shows that for this population such a personalized and “soft” support, provided

remotely through a web platform, can improve the exit rates.

Our study analyzes the return to employment at different time horizons. Different take up rates between agencies are used to estimate an aggregated model at the agency and cohort of unemployment registration level. We use a difference-in-difference approach, which takes into account systematic differences between agencies and between cohorts that could be source of bias.

Activ'Emploi is found to have positive effects on the return-to-employment rates of its beneficiaries. Overall, these effects represent an increase of more than 20% in the probability of finding a job before 6 months. These effects are, however, lower than those observed in intensive support programs for jobseekers with severe difficulties. For example, [Behaghel et al. \(2014\)](#) obtain effects of the CVE intensive counseling program of about 50% on return to employment before 6 months. In addition, job-seekers who use the program also increase their chances of finding a permanent job. One of the limitations of this study is that we cannot identify what makes Activ'Emploi so effective since the study of the heterogeneity of the effects is not conclusive. We also lack information about the use of this program.

In France since 2013 job search assistance is organized in order to give priority to job-seekers with more difficulties. The more autonomous a job-seeker is the less help she receives as she will be allocated to a counselor with higher workload. Nor the causal effect of this policy neither the equilibrium effect if a job search assistance program such as Activ'Emploi was widened can be measured. However the results we find shed light on the fact that even a population that looks at first sight as not needing support can benefit from job search assistance. Furthermore they can benefit from a program that is not constraining for job-seekers, that does not require to mobilize many resources since it is delivered as a web platform and that consequently should not be very costly to implement.

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A Appendix

A.1 Institutional Setting

A.1.1 Descriptive Statistics of the different services provided by PES counselors to Job-seekers according to their modality

In the administrative data from the PES we can identify the main services provided by counselors to job-seekers.

For direct interactions between counselors and job-seekers we observe: meetings between counselors and job-seekers and also the vacancies counselors propose to job-seekers.

For programs counselors advise to job-seekers we observe training programs and programs related to the support of job-seekers through out their search. Among the latter we observe mainly: Outsourced Counseling programs that externalize the counseling of job-seekers to private placement operators; guidance for job-seekers to redefine their Professional Project and Research Strategies that help job-seekers improve their research in a practical way (workshops to improve their CV's and cover letters, to do mock interviews etc.).

Table [AI](#) shows the average amount of each of these services, provided to a given job seeker during her first 6 months in unemployment, according to the modality that was assigned to the job-seeker at registration.

Table A1: Balance Table of the average number of services provided by PES counselors to Job-seekers according to their modality during their first 6 months in unemployment.

	(1)	(2)	(3)
	Follow-Up	Guided	Reinforced
All Meetings	1.482	1.699	2.357
	(0.001)	(0.001)	(0.003)
Non-Mandatory Meetings	0.331	0.520	0.968
	(0.001)	(0.001)	(0.002)
Mandatory Meetings	1.151	1.179	1.389
	(0.001)	(0.001)	(0.002)
Vacancies	1.882	1.985	2.125
	(0.0734)	(0.0702)	(0.0408)
Total Nb of Support Services	0.163	0.256	0.382
	(0.000)	(0.001)	(0.000)
Firm Creation	0.000	0.000	0.001
	(0.000)	(0.000)	(0.000)
Outsourcing	0.013	0.029	0.087
	(0.000)	(0.000)	(0.000)
Professional Project	0.014	0.032	0.065
	(0.000)	(0.000)	(0.000)
Search Strategies	0.123	0.173	0.189
	(0.001)	(0.000)	(0.000)
Other	0.013	0.022	0.041
	(0.000)	(0.000)	(0.000)
Training	0.288	0.338	0.487
	(0.001)	(0.001)	(0.001)
N	1,225,742	1,764,980	743,967

Notes: Standard Errors are included in parentheses. All the differences across groups are statistically significant at the 1% level. This table includes all job-seekers registered between the 1st of June 2014 and the 31 of December 2014 (i.e. by construction unable to benefit from Activ'Emploi during their first 6 months in unemployment.)

Table A2: Comparison of the content of Activ'Emploi's web-platform for 4 different private placement operators

	AddViseo (Aksis)	Anuvéol	Portail Avenirs (Ingéus)	Plateforme AEM (Onlineformapro)
Navigating the platform	On the principle of gaming: validation of steps rewarded by obtaining "badges".	Classic navigation: a home page that presents the different entries of the website	Navigation according to two spaces: a personal space and an e-learning space	Platform organized around a dashboard (scheduled interviews, action plan, group workshops, monitoring of applications ...) and various tabs that allow the JS to open spaces to organize every aspect of her job search.
Mode of interaction with the provider	Online messaging: « Hotline »	Online messaging: « Hotline » + « Practical information » tab: which provides information on the functioning of the service.	No contact mode identified on the platform	Different modes of interaction with the referring counselor: mail, online messaging, video, forum + « Interviews » space: follow-up of the exchanges with the speaker.
Methodological support for job search	A dashboard that shows the actions to be prioritized given the "badges" obtained + Methodological guides + "Prepare" tab: possibility to store CVs, download CV and Cover Letter templates, and simulate interviews online	Section "My organization and my procedures": help on how to use of the Internet and other technological tools, tracking of job application + Section "my project and my tools: CV, Cover Letter, "present yourself in 3 minutes", "qualities and defects" ...	Section "My action plan": planning of the actions decided with the advisor + Section "My applications" allows the user to follow their progress (date of application, follow-up, interview) + CV and documents section: possibility to download CV and Cover Letter	« Action Plan » section: calendar of tasks to be completed + An agenda automatically filled in with regard to the completed sections + « Applications » section: allows the JS to add job offers or unsolicited applications and to fill in their status + « CV » section: allows the JS to create an online CV whose format is prepared + « Documents » section: allows the JS and her referent to submit documents
Provision of information and tools on job search	«Your job blog» tab: job search techniques tips, tricks, quizzes and games	«My job search strategy» section: information on training, courses and types of contracts	E-learning space: distance learning content produced by the providers (videos, quizzes, serious games, fact sheets)	«Training» space: allows JS to follow the proposed training courses (basic knowledge, packoffice, job search techniques...)
Provision of information and tools on the labor market	«Be informed and take action» tab: sheets by sector that list resource players, firm's news + Link to the Corporama firm's listing	« News » tab: links to Netvibes, a web portal that allows its users to customize their IT usage	«Find a firm» space via a firm directory developed by Ingéus, which offers a search by activity and geographical location + News on employment and the different sectors of activity.	«Employment News » Space + Directory of firms in the Franche Comté area
Provision of job offers	Job offers made available via the Search! Partner (compilation of several Jobboards) + « Live offers », collected by the provider available in the «Our flash info» tab.	«Job offer» tab: gives access to the PES website and the Alixio Mobilité platform (of the provider). Possibility to fill in your profile online to receive offers and apply for jobs.	Space «my new offers» lists the personalized offers taking into account the user profile + Space «search for an offer » : search browser, in partnership with Indeed (Jobboard)	Search engine compiling data from several Jobboards (The PES, Kelijob, meteojob), with regard to the occupational code of the job sought + Forum with other JS as a support for uploading job offers.

Notes: This table was translated and adapted from the qualitative evaluation made by the consulting firm Geste for the PES in April 2017.

A.2 Data and Descriptive Statistics

Figure A1: Histogram of the month of registration in Activ'Emploi among the beneficiaries of the program.

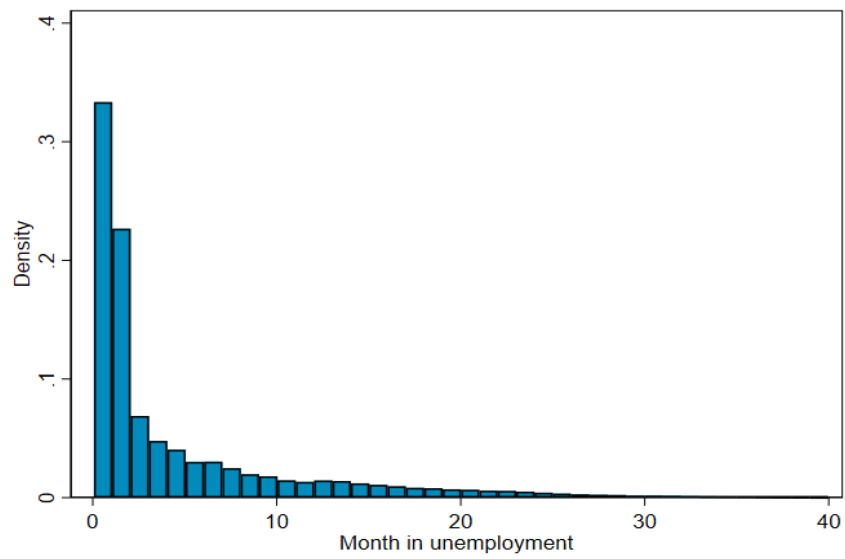


Table A3: Comparison of observable characteristics by treatment status.

	Sample average								
	Treated 1st month	Not treated 1st month	Diff (1-2)	Treated before the 3rd month	Not Treated before the 3rd month	Diff (4-5)	Treated before the 6th month	Not Treated before the 6th month	Diff (7-8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
reason for registration									
<i>lay-off</i>	0,351	0,346	0,005 ***	0,379	0,312	0,067 ***	0,393	0,341	0,052 ***
<i>end of contract</i>	0,369	0,381	-0,012 ***	0,357	0,418	-0,060 ***	0,356	0,383	-0,027 ***
<i>back from inactivity</i>	0,194	0,143	0,051 ***	0,187	0,127	0,060 ***	0,174	0,142	0,032 ***
<i>others</i>	0,087	0,130	-0,043 ***	0,077	0,144	-0,067 ***	0,077	0,134	-0,057 ***
sex (male)	0,471	0,495	-0,024 ***	0,465	0,500	-0,035 ***	0,468	0,497	-0,029 ***
age	33,480	35,163	-1,683 ***	34,318	34,442	-0,124 ***	34,715	35,135	-0,419 ***
number of children	0,607	0,751	-0,144 ***	0,661	0,712	-0,051 ***	0,687	0,751	-0,064 ***
marital status									
<i>single</i>	0,531	0,485	0,046 ***	0,502	0,505	-0,003 ***	0,491	0,486	0,005 ***
<i>divorced</i>	0,060	0,071	0,011 ***	0,064	0,070	-0,006 ***	0,066	0,071	-0,005 ***
<i>married</i>	0,407	0,438	0,031 ***	0,431	0,419	0,011 ***	0,439	0,436	0,002 ***
<i>widowed</i>	0,003	0,006	0,003 ***	0,004	0,006	-0,002 ***	0,004	0,006	-0,002 ***
RSA (1)	0,087	0,104	0,017 ***	0,085	0,116	-0,031 ***	0,088	0,105	-0,017 ***
French nationality	0,914	0,914	0,000 ***	0,921	0,914	0,007 ***	0,922	0,913	0,009 ***
level of education									
<i>no education</i>	0,007	0,028	-0,021 ***	0,008	0,027	-0,018 ***	0,009	0,030	-0,020 ***
<i>secondary school</i>	0,022	0,065	-0,043 ***	0,025	0,070	-0,045 ***	0,028	0,067	-0,040 ***
<i>masters degree and more</i>	0,283	0,158	0,124 ***	0,272	0,147	0,125 ***	0,260	0,153	0,107 ***
<i>third and fourth year university level</i>	0,168	0,118	0,050 ***	0,164	0,114	0,050 ***	0,158	0,116	0,042 ***
<i>second year university level</i>	0,206	0,158	0,048 ***	0,205	0,157	0,048 ***	0,203	0,155	0,048 ***
<i>high school</i>	0,165	0,216	-0,051 ***	0,166	0,223	-0,057 ***	0,170	0,219	-0,049 ***
<i>technical high school</i>	0,148	0,255	-0,107 ***	0,160	0,262	-0,102 ***	0,172	0,260	-0,088 ***
qualification level									
<i>not specified</i>	0,002	0,007	-0,005 ***	0,006	0,004	0,002 ***	0,007	0,007	0,000 ***
<i>unqualified laborer</i>	0,020	0,046	-0,026 ***	0,021	0,048	-0,027 ***	0,022	0,047	-0,025 ***
<i>skilled laborer</i>	0,044	0,083	-0,039 ***	0,046	0,091	-0,045 ***	0,049	0,085	-0,036 ***
<i>unqualified employees</i>	0,083	0,134	-0,051 ***	0,083	0,133	-0,050 ***	0,084	0,138	-0,054 ***
<i>qualified employees</i>	0,475	0,478	-0,003 ***	0,469	0,482	-0,013 ***	0,470	0,479	-0,009 ***
<i>technicians</i>	0,167	0,115	0,052 ***	0,165	0,115	0,050 ***	0,162	0,112	0,050 ***
<i>executive</i>	0,208	0,137	0,071 ***	0,210	0,127	0,083 ***	0,206	0,132	0,073 ***
looking for permanent contract	0,946	0,883	0,063 ***	0,943	0,884	0,059 ***	0,944	0,879	0,064 ***
looking for a full time job	0,949	0,892	0,056 ***	0,938	0,897	0,041 ***	0,935	0,890	0,045 ***
operational category									
<i>immediately available and looking for a full-time permanent job</i>	0,903	0,787	0,116 ***	0,892	0,788	0,103 ***	0,890	0,781	0,108 ***
<i>immediately available and looking for a part-time permanent job</i>	0,041	0,076	-0,035 ***	0,049	0,075	-0,025 ***	0,052	0,077	-0,025 ***
<i>immediately available and looking for a limited-term job</i>	0,054	0,117	-0,062 ***	0,057	0,116	-0,058 ***	0,056	0,120	-0,064 ***
<i>in search of employment but not immediately available</i>	0,000	0,014	-0,014 ***	0,001	0,015	-0,015 ***	0,001	0,015	-0,014 ***
<i>employed, looking for another job</i>	0,001	0,006	-0,005 ***	0,001	0,006	-0,006 ***	0,001	0,006	-0,005 ***
profession of the job wanted									
<i>agriculture and fisheries</i>	0,017	0,031	-0,014 ***	0,018	0,032	-0,014 ***	0,018	0,032	-0,014 ***
<i>art and shaping art works</i>	0,005	0,006	-0,001 **	0,005	0,006	0,000 ***	0,005	0,006	-0,001 ***
<i>bank insurance and real estate</i>	0,031	0,022	0,009 ***	0,031	0,022	0,009 ***	0,030	0,022	0,008 ***
<i>trade sale and large distribution</i>	0,160	0,148	0,012 ***	0,160	0,150	0,011 ***	0,162	0,147	0,015 ***
<i>communication and media</i>	0,056	0,032	0,024 ***	0,052	0,031	0,021 ***	0,050	0,031	0,018 ***
<i>construction building and public works</i>	0,055	0,075	-0,020 ***	0,054	0,080	-0,026 ***	0,055	0,076	-0,021 ***
<i>restaurant services tourism and leisure</i>	0,057	0,088	-0,031 ***	0,057	0,095	-0,037 ***	0,058	0,090	-0,032 ***
<i>industry</i>	0,097	0,074	0,023 ***	0,096	0,073	0,022 ***	0,095	0,073	0,022 ***
<i>installation and maintenance</i>	0,034	0,036	-0,002 ***	0,035	0,037	-0,002 ***	0,035	0,036	-0,001 ***
<i>health</i>	0,038	0,055	-0,017 ***	0,039	0,053	-0,015 ***	0,038	0,056	-0,018 ***
<i>personal community services</i>	0,127	0,192	-0,065 ***	0,134	0,191	-0,057 ***	0,135	0,196	-0,060 ***
<i>show</i>	0,008	0,017	-0,009 ***	0,008	0,017	-0,009 ***	0,008	0,018	-0,010 ***
<i>support to the company</i>	0,264	0,159	0,104 ***	0,262	0,150	0,112 ***	0,259	0,153	0,105 ***
<i>transport and logistic</i>	0,049	0,064	-0,014 ***	0,050	0,064	-0,014 ***	0,053	0,064	-0,011 ***
years of experience in the profession	6,690	7,023	-0,332 ***	7,083	6,811	0,271 ***	7,242	6,983	0,259 ***
reservation wage	2090,4	1687,8	402,60 ***	2090,0	1599,1	490,82 ***	2077,1	1664,0	413,2 ***
maximal duration of benefit (days)	474,40	470,65	3,742 ***	499,70	460,49	39,217 ***	511,46	466,38	45,082 ***
daily amount of the benefit (€)	57,41	53,31	4,094 ***	60,03	52,24	7,791 ***	61,15	52,65	8,500 ***
entitled to unemployment benefits	0,773	0,785	-0,012 ***	0,793	0,807	-0,014 ***	0,809	0,782	0,027 ***
number of services undertaken	0,236	0,349	-0,112 ***	0,241	0,320	-0,079 ***	0,271	0,352	-0,081 ***
number of services related to counseling and monitoring undertaken	0,065	0,097	-0,032 ***	0,064	0,089	-0,025 ***	0,072	0,098	-0,026 ***
Nobs	80108	1851079		159126	1772061		189595	1741592	

Notes: Significance at the 1%, 5% and 10% level, respectively, is indicated by ***, **, *. Only individuals registered as unemployed after January 2015 are included.

A.3 Empirical Strategy

Table A4: Correlation between exits during the first month and treatment rates during the first month by agency and cohort.

	(1)
	P(a=1 c,k)
$\hat{\varepsilon}_{ck}^1$	-0.0400 (0.0109)
N	20423
F	13.53
Mean	0.0528
Adjusted R2	0.500

Notes: Standard errors clustered at the agency level are displayed in parenthesis. This equation concerns cohorts from July 2015 and thus cohorts where individuals could have been treated during the first month. It includes agency and region*cohort fixed effects.

A.4 Results

Table A5: Effect of Activ'Emplei on the probability of finding a job at different horizons (OLS)

	% of Job-Seekers finding a job during month T in unemployment by agency and cohort									
	T=2	T=2	T=3	T=3	T=4	T=4	T=5	T=5	T=6	T=6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
% of JS treated before T by agency*cohort	0.0198 (0.00642)	0.00983 (0.00636)	0.0279 (0.00402)	0.0225 (0.00400)	0.0166 (0.00353)	0.0121 (0.00348)	0.0127 (0.00308)	0.0109 (0.00309)	0.0145 (0.00381)	0.0137 (0.00388)
$\hat{\varepsilon}_{ck}^1$	-0.0668 (0.00809)	-0.0796 (0.00795)	-0.0730 (0.00643)	-0.0766 (0.00613)	-0.0709 (0.00637)	-0.0725 (0.00613)	-0.0607 (0.00503)	-0.0601 (0.00467)	-0.0621 (0.00568)	-0.0576 (0.00445)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	44251	44246	44251	44246	44251	44246	44251	44246	44251	44246
F	40.07	21.28	90.09	14.89	75.99	14.45	82.95	13.19	60.57	11.49
Mean	0.0958	0.0958	0.0667	0.0667	0.0525	0.0525	0.0437	0.0437	0.0391	0.0391
Adjusted R2	0.271	0.287	0.274	0.284	0.160	0.173	0.191	0.208	0.317	0.348

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency for each cohort are applied.

Table A6: First stage: Enrollment rate into Activ'Emploi before a given month on the enrollment rate during the first month in unemployment.

% of Job-Seekers treated before month T in unemployment by agency and cohort								
	T=2	T=2	T=3	T=3	T=4	T=4	T=5	T=5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
% of JS treated during 1st month by agency*cohort	1.013 (0.00832)	1.008 (0.00825)	1.002 (0.00982)	0.995 (0.00977)	0.990 (0.0106)	0.984 (0.0105)	0.980 (0.0110)	0.973 (0.0110)
$\hat{\varepsilon}_{ck}^1$	-0.0177 (0.00406)	-0.0176 (0.00407)	-0.0223 (0.00470)	-0.0222 (0.00470)	-0.0276 (0.00513)	-0.0276 (0.00514)	-0.0311 (0.00544)	-0.0310 (0.00545)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	44251	44246	44251	44246	44251	44246	44251	44246
F	7418.2	372.0	5203.1	258.3	4397.5	222.8	3971.0	201.4
Mean	0.0337	0.0337	0.0377	0.0377	0.0407	0.0407	0.0434	0.0434
Adjusted R2	0.880	0.881	0.863	0.864	0.854	0.855	0.849	0.850

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency for each cohort are applied.

Table A7: Reduced Form: Effect of enrolling in Activ'Emploi during the first month on the probability of finding a job at different horizons

	% of Job-Seekers finding a job during month T in unemployment by agency and cohort									
	T=2 (1)	T=2 (2)	T=3 (3)	T=3 (4)	T=4 (5)	T=4 (6)	T=5 (7)	T=5 (8)	T=6 (9)	T=6 (10)
% of JS treated during 1st month by agency*cohort	0.0198 (0.00642)	0.00983 (0.00636)	0.0234 (0.00505)	0.0175 (0.00503)	0.0179 (0.00467)	0.0130 (0.00458)	0.0104 (0.00413)	0.00866 (0.00409)	0.0149 (0.00449)	0.0135 (0.00461)
$\hat{\varepsilon}_{ck}^1$	-0.0668 (0.00809)	-0.0796 (0.00795)	-0.0736 (0.00643)	-0.0771 (0.00614)	-0.0712 (0.00636)	-0.0728 (0.00612)	-0.0611 (0.00503)	-0.0605 (0.00467)	-0.0625 (0.00571)	-0.0580 (0.00446)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	44251	44246	44251	44246	44251	44246	44251	44246	44251	44246
F	40.07	21.28	75.59	14.34	70.68	14.37	78.13	12.89	61.49	11.56
Mean	0.0958	0.0958	0.0667	0.0667	0.0525	0.0525	0.0437	0.0437	0.0391	0.0391
Adjusted R2	0.271	0.287	0.273	0.284	0.160	0.173	0.191	0.208	0.317	0.348

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency for each cohort are applied.

Alternative Specification: We allow for treatment during a given period to have an effect on the same period and the following.

Table A8: Alternative Specification: Effect of Activ'Emploi on the probability of finding a job at different horizons (2SLS)

	% of Job-Seekers finding a job during month T in unemployment by agency and cohort									
	T=2 (1)	T=2 (2)	T=3 (3)	T=3 (4)	T=4 (5)	T=4 (6)	T=5 (7)	T=5 (8)	T=6 (9)	T=6 (10)
% of JS treated during T or before by agency*cohort	0.0196 (0.00634)	0.00975 (0.00631)	0.0233 (0.00504)	0.0176 (0.00506)	0.0181 (0.00473)	0.0132 (0.00466)	0.0106 (0.00422)	0.00890 (0.00421)	0.0153 (0.00460)	0.0140 (0.00477)
$\hat{\varepsilon}_{ck}^1$	-0.0665 (0.00809)	-0.0794 (0.00796)	-0.0731 (0.00643)	-0.0768 (0.00614)	-0.0707 (0.00636)	-0.0724 (0.00612)	-0.0608 (0.00504)	-0.0602 (0.00467)	-0.0620 (0.00569)	-0.0576 (0.00447)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	44251	44246	44251	44246	44251	44246	44251	44246	44251	44246
F	40.04	50.24	75.63	552.1	70.62	35.36	78.13	28.10	61.49	3896.2
Mean	0.0958	0.0958	0.0667	0.0667	0.0525	0.0525	0.0437	0.0437	0.0391	0.0391

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency and cohort are applied.

Table A9: Alternative Specification: Effect of Activ'Emploi on the probability of finding a job at different horizons (OLS)

	% of Job-Seekers finding a job during month T in unemployment by agency and cohort									
	T=2 (1)	T=2 (2)	T=3 (3)	T=3 (4)	T=4 (5)	T=4 (6)	T=5 (7)	T=5 (8)	T=6 (9)	T=6 (10)
% of JS treated during T or before by agency*cohort	0.0114 (0.00500)	0.00230 (0.00500)	0.0255 (0.00386)	0.0203 (0.00385)	0.0143 (0.00346)	0.00985 (0.00342)	0.0117 (0.00305)	0.00994 (0.00306)	0.0137 (0.00383)	0.0129 (0.00388)
$\hat{\varepsilon}^1_{ck}$	-0.0668 (0.00809)	-0.0797 (0.00796)	-0.0730 (0.00643)	-0.0766 (0.00614)	-0.0709 (0.00637)	-0.0726 (0.00613)	-0.0607 (0.00503)	-0.0601 (0.00467)	-0.0621 (0.00568)	-0.0576 (0.00445)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	44251	44246	44251	44246	44251	44246	44251	44246	44251	44246
F	38.12	21.09	87.79	14.81	73.34	14.40	82.14	13.20	60.41	11.48
Mean	0.0958	0.0958	0.0667	0.0667	0.0525	0.0525	0.0437	0.0437	0.0391	0.0391
Adjusted R2	0.271	0.287	0.274	0.284	0.160	0.173	0.191	0.208	0.317	0.348

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency for each cohort are applied.

Table A10: Alternative Specification First stage: Enrollment rate into Actif'Emploi before a given month on the enrollment rate during the first month in unemployment.

	% of Job-Seekers treated during month T or before by agency and cohort									
	T=2 (1)	T=2 (2)	T=3 (3)	T=3 (4)	T=4 (5)	T=4 (6)	T=5 (7)	T=5 (8)	T=6 (9)	T=6 (10)
% of JS treated during 1st month by agency*cohort	1.013 (0.00832)	1.008 (0.00825)	1.002 (0.00982)	0.995 (0.00977)	0.990 (0.0106)	0.984 (0.0105)	0.980 (0.0110)	0.973 (0.0110)	0.974 (0.0114)	0.967 (0.0113)
$\hat{\varepsilon}_{ck}^1$	-0.0177 (0.00406)	-0.0176 (0.00407)	-0.0223 (0.00470)	-0.0222 (0.00470)	-0.0276 (0.00513)	-0.0276 (0.00514)	-0.0311 (0.00544)	-0.0310 (0.00545)	-0.0339 (0.00570)	-0.0338 (0.00570)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	44251	44246	44251	44246	44251	44246	44251	44246	44251	44246
F	7418.2	372.0	5203.1	258.3	4397.5	222.8	3971.0	201.4	3686.3	187.6
Mean	0.0337	0.0337	0.0377	0.0377	0.0407	0.0407	0.0434	0.0434	0.0456	0.0456
Adjusted R2	0.880	0.881	0.863	0.864	0.854	0.855	0.849	0.850	0.847	0.848

Notes: Standard errors clustered at the agency level are displayed in parenthesis. Agency and a cohort*region fixed effect are included. The controls used are the average per agency and cohort of the following variables: age, sex, number of children, French nationality, marital status, benefiting from the RSA, reason for registration, looking for a permanent contract, looking for a full time job, operational category (availability), qualification level, profession of the job wanted, years of experience, level of education, number of support services undertaken by the past, number of support services related to counseling and monitoring undertaken by the past, entitlement to unemployment benefits, duration and amount of the benefits and reservation wage. Weights for the number of individuals registered in each agency for each cohort are applied.

Table A11: Naive Difference in differences estimate

	(1) P(T \leq 6)
Diff-in-diff	0.010 (0.002)
Observations	719,430
Mean control t(0)	0.350
Mean treated t(0)	0.359
Diff t(0)	0.00936
Mean control t(1)	0.346
Mean treated t(1)	0.365
Diff t(1)	0.0191

Chapter 2

Do Job Counselors Matter?

Measuring Counselors value-added in Job Search

Do Job Counselors Matter?

Measuring Counselors value-added in Job Search

Sofia Dromundo and Antton Haramboure *

Abstract

It is generally accepted that job search assistance programs shorten periods of unemployment, but little is known about the role played by job counselors. Using exhaustive administrative data from the French Public Employment Service (PES), we provide unbiased estimates of counselors value-added in the Parisian region. We find that having a counselor one standard deviation higher in the distribution of value-added translates into an increase of around 8.4% (13.2%) in the probability of finding a (stable) job within 6 months. We document the types of practices that make a high value-added counselor, exploiting rich data on meetings, proposed vacancies, training, and job search assistance programs for jobseekers. We find that depending on the objective pursued (fast or stable exits) and the jobseeker's characteristics, high value-added counselors are not the same and do not offer the same services. This points to a potential trade-off between practices that foster a rapid exit from unemployment and those that favor better quality matches.

Keywords: Labor, Public Policy, Job Search, Active Labor Market Policies, Unemployment

Jel classification : J64 J68

*Paris School of Economics, 48 Boulevard Jourdan, 75014 Paris, France. Emails: sofia.dromundo@psemail.eu and antton.haramboure@yahoo.fr

I Introduction

Job search assistance has shown promising results in helping jobseekers return to work (Card et al., 2010, 2018). However much still needs to be understood to explain such success. Despite their central role in the implementation of active labor market policies the role of job counselors remains an area in need of investigation. Notably, an interesting and little investigated aspect of is the role of job counselors who are at the heart of their implementation. In many OECD countries (such as France, UK, Switzerland and Germany), national employment agencies use job counselors for three main missions: i) To assist the unemployed in their job search, ii) monitor them throughout this process and iii) orient them towards active labor market programs. This role has considerable power to shape jobseekers' trajectories. While the literature provides analyses of the impact of labor market policies, little is known about the role played by job counselors.

To help bridge this gap, this paper answers the following questions : Do individual job counselors matter in helping unemployed finding (good) jobs? What tools do job counselors use? Are they more important for certain types of jobseekers? From a policy perspective, answering these questions could help define better human resources strategies for counselors as well as shape more efficient active labor programs.

To tackle these questions we exploit the rich administrative databases from the French Public Employment Service (PES). We track around 5000 counselors in charge of more than a million jobseekers in the Parisian region over 5 years. Through this data we can follow jobseekers from their registration in the PES to their potential re-employment. We have very detailed information on the interactions between counselors and jobseekers as well as the labor market programs they recommend.

The identification of causal counselor value-added takes advantage of the quasi-randomness in the matching process between jobseekers and counselors within agencies. We adapt the value-added estimation procedure developed for teachers in education (Koedel et al., 2015; Kane and Staiger, 2008; Chetty et al., 2014) to the context of active labor market policies. We use counselors who switch agencies to separately estimate the counselor value-added of interest from agency fixed effects. (Abowd et al., 1999, 2002). We conduct several checks to show that our value-added estimates are not driven by sorting on jobseeker's observable characteristics.

First we show that jobseeker trajectories are different depending on the counselor that advises them. Being followed by a counselor one standard deviation higher in the distribution translates to an increase on finding a job within 6 months of 8.4% and on finding a stable job within 6 months of 13.2% (a stable job is defined as not re-registering in unemployment in the 6 months that follow the exit). High value-added counselors are not necessarily the same

for the two outcomes. Being productive at placing jobseekers fast does not imply placing jobseekers in good quality jobs.

Second, we document the characteristics and practices of high value-added counselors. We build measures of different tools counselors use and show that use of these tools varies greatly across counselors. This underlines the considerable leeway job counselors have in their roles. We then correlate those measures to the counselor value-added measures previously estimated.

High value-added counselors tend to meet and share vacancies with their clients more often. They are however less likely to propose training or career reorientation that may have a negative impact in the short term (lock-in effect) but a positive long term impact. We do not find the use of sanctions or counselor characteristics to be related to counselor quality. When comparing counselors that have a high-value added in different outcomes, we observe that their preferred practices differ. Counselors that are more productive at fostering quality jobs than at fostering rapid exits set aside some tools. They less frequently contact jobseekers remotely (e-mail, phone etc.) and share job offers with them. They also less frequently use tools that the literature shows are not efficient for finding stable employment such as as outsourced counseling (Behaghel et al., 2014). Altogether, our findings raise a potential trade-off between practices that foster a rapid exit from unemployment and others that favor the acquisition of new skills and better quality matches, both of which may be crucial in the long term.

Finally, we explore if our results are heterogeneous across different types of jobseekers. We compute counselor effects on different sub-groups of jobseekers and we find that counselors seem to matter more for individuals less likely to find a job according to their set of observable characteristics.

This paper contributes to the still limited but growing literature on job counselors. By exploring separately some of the channels through which counselors act, these contributions shed light on the strong impact these individuals can have on jobseekers' trajectories.

Counselors play a role through the activation of search effort and information acquisition of jobseekers. This dimension is fundamental when we learn the importance of face to face meetings with counselors (Schiprowski, 2020) and the positive effects of increasing the number of vacancies proposed by counselors to jobseekers (Glover, 2019). These activation/information interventions often encompass a potential non-cognitive moral/psychological role of counselors which is difficult to disentangle (Arni, 2015).

In Switzerland Behncke et al. (2010b) finds that less cooperative counselors have better placement rates and Huber et al. (2017) further explore this effect to show that it is likely to be driven by counseling dimensions such as threats of sanctions and pressure to accept jobs.

Additionally counselors can act by assigning jobseekers to different active labor market programs, although Bolhaar et al. (2020) fail to detect heterogeneous treatment effects across counselors more or less likely to use certain programs.

Beyond the actual use of active labor market programs, [Arni et al. \(2020\)](#) show that counselors can influence job search through jobseekers' perception of their intended use of these tools (policy regime). Furthermore, they separate these job counselors policy regime from those of PES agencies.

Finally, shared characteristics of job counselors and their job-seeker clients can also have an impact. [Behncke et al. \(2010a\)](#) show that the chances of finding a job improve when jobseekers belong to the same social group (gender, age, education and ethnicity) as their referral counselors. In the specific context of the January 2015 "Charlie Hebdo" attacks in France, [Glover \(2019\)](#) shows that the actions of minority counselors offset the negative labor market shock experienced by minority jobseekers.

Our paper broadens these findings. We do not focus on an specific aspect of counseling but we assign a metric to the aggregate impact of job counselors' actions on job search outcomes. By doing so we are the first to give a global cardinal ranking to an entire population of counselors with respect to their value added. This results also relates to the broader and more established literature on the importance of individuals in the implementation of public policies ([Chetty et al., 2014](#); [Best et al., 2018](#)).

We also contribute to this literature by studying several potential roles played by counselors. This allows us to make a comprehensive picture of the many determinants of counselors efficiency and to compare them. Our work relates to the literature on how differences in the productivity of public service provision can be explained by differences in management practices ([Bloom et al., 2015b,a](#)).

Since we consider different job search outcomes we are able to asses how these determinants of counselors value-added change according to their objective (i.e., quantity vs quality of the placements).

In Section [III](#), we provide background information on the registration process and assignment of counselors to jobseekers. Section [III](#) presents the data. Section [IV](#) discusses the different objectives of counselors and presents the outcomes of interest. Section [V](#) provides the estimation of counselor effects, shows the results and tests the hypothesis behind this estimation. In Section [VI](#) we give insight about the practices and characteristics that make a productive counselor. In Section [VII](#) we explore the heterogeneity across different types of jobseekers. We conclude in Section [VIII](#).

II Institutional Setting: the French Public Employment Service (PES)

To receive unemployment benefits and job search assistance, jobseekers register at the PES. They register online or in person, and are assigned to the agency closest to their home. Agencies are the reference point of jobseekers. It is where they meet their counselors, receive

job search assistance and access materials to help them with their search such as computers, printers, phones, etc.

The first formality is to attend a mandatory registration interview. Since 2013, during this interview, among other things, jobseekers are assigned to one of three categories (see Table 1) depending on their level of autonomy in the job search. Counselors are specialized and only counsel jobseekers from one category.

Table 1: Three categories of counseling services at the French PES

Category	Workload	% of Unemployed	Autonomy Level
Follow-up	between 200 and 350 jobseekers	33%	highest level of autonomy
Guided	between 100 and 150 jobseekers	53%	need to be supported
Reinforced	up to 70 jobseekers	14%	need strong support

The more autonomous the jobseekers are, the less job search assistance they receive. For instance, in the Follow-up category, the number of clients is large but counseling is limited.

Individuals are categorized by an advisor that conducts the registration interview. The advisor is given guidelines that help her choice¹. It is important to keep in mind that "autonomy" does not depend only on the observable characteristics. The level of support a person needs is not necessarily related to their level of qualification or education. The advisor perceives many factors that are not apparent in the administrative data and that help her determine a jobseeker's level of autonomy. For example, an unskilled worker, in a region where the sector in which she wishes to work is developing and lacking manpower (i.e., a tight market), may be given the "follow-up" category. An executive who does not know how to use internet oriented tools to find a job could end up in a category for jobseekers who need more support.

Once the jobseeker's category is determined during the registration interview, counselors are randomly assigned to jobseekers. In each agency, a manager is responsible for assigning new entrants to counselors. The only information available to the managers is the jobseeker's category. Jobseekers should be assigned to the counselor with the greatest availability for their category in the agency which ensures that jobseekers' characteristics are balanced across different counselors. The fact that more productive counselors have greater turnover of jobseekers in their portfolios does not affect this balance. In section V.6.1 we validate this random allocation using an exhaustive set of jobseeker characteristics.

After the first match, jobseekers can change counselors during their unemployment. This change can be decided either by the jobseeker or the counselor. It can also be triggered

¹The tools for judging an applicant's autonomy are: the existence of job search instruments (updated CV, cover letter), whether the jobseeker has been actively looking for a job, whether the jobseeker faces major obstacles to employment (disabilities, childcare, etc.), the compatibility between the job wanted and the job market, and the quality and precision of the professional project.

by a change in agency or specialization of the counselor. In our sample, six months after registration less than 20% of jobseekers change counselor. Since the counselor change is likely to be endogenous we will focus on the first counselor attributed to the jobseeker for whom the assumption of a quasi random allocation should hold.

III Data

This project focuses on the Parisian Region (*Ile-de-France*) from July 2013 to December 2018. The study uses exhaustive administrative data from the PES.

jobseekers: The jobseeker's data contains their historical record in unemployment (*Fichier Historique*). It provides socio-demographic characteristics at registration (gender, age, level of education, qualification, experience in the desired occupation, etc.), information on previous unemployment (recurrence and duration of unemployment) as well as the elements related to their unemployment benefits (eligibility, duration, amount, etc.) and other welfare programs.

Employment: To track job finding we use the *Déclaration Préalable à L'Embauche* database. This database comes from the mandatory declaration made by employers for each new hire. It contains the date of hire, the hiring firm and the type of contract ².

Counselors: The PES information system *SISP* tracks the entry of jobseekers into counselors' portfolios. The data allows us to match counselors and jobseekers. It also provides information about the counselors practices: meetings and different types of contact with jobseekers, the connections of jobseekers with potential employers made by counselors, the services and programs recommended by counselors (counseling, training, workshops etc). Finally it provides some counselor characteristics: sex, years of experience and type of contract (open ended/fixed term, public/private).

As explained in detail in Appendix ^{A.1} restrict the sample. First, due to our empirical strategy, and the need of counselors moving across agencies in sufficient numbers (cf. section ^V), we focus on the "Guided" category which represents 53% of jobseekers. Second, because of data limitations, and due to a possible delay between registration and counselor assignment, we are confident on counselor assignments only after 30 days. Therefore we limit our sample to jobseekers who remained unemployed for at least one month and were assigned a counselor during this time frame. We identify a counselor value-added conditionally on staying unemployed at least one month. We believe such a restriction to be of little importance since the

²The DPAE cover the entire labor hiring with the exception of few jobs in the public sector, professional child care jobs, and independent employees. Further, this base can be linked to the jobseekers' database as long as the match happened within three years after the last registration in unemployment

influence of a counselor should be negligible for an individual finding a job before 30 days of unemployment. All together we obtain a final sample of :

- 151 agencies
- 4 956 counselors
- 1 010 446 jobseekers
- 1 485 710 unemployment spells

Due to the data limitation described in [A.1](#), estimating the stock of jobseekers followed by a counselor at a given time is not straightforward. We can however compute the number of jobseekers entering counselors' portfolios each quarter. In our sample, on average 32.9 new jobseekers (sd. 11.9) are registred with a counselor each quarter. This represent only a selected subset of the jobseekers followed by a counselor. Difference in portfolio size between counselors are likely to be driven by difference in hours worked and time spent on administrative task which are unevenly divided among counselors ³.

IV What does it mean to be a productive counselor? Choice of the outcomes of interest

The rise in the scope and the duration of unemployment since the 70's, led the French and many European governments to pursue a common objective: accelerate the return to employment.

In this context the counselor's job has become more demanding. They have to deal with a bigger pool of jobseekers with more diverse and complex profiles, and respond to the rise in efficiency requirements and results-based management ([Pillon, 2017](#)). Because of this contradictory features, counselors could face a tradeoff between placing people fast (ie., responding to the efficiency objectives) and placing people well (ie., taking into account the specificity of each case).

This trade-off raises the questions: What is a productive counselor? What is the objective a social planner should pursue? On the one hand, reducing unemployment, especially long-term unemployment, seems crucial. It has been shown that the longer jobseekers are unemployed the harder it is for them to find a job irrespective of the quality ([Kroft et al., 2013](#); [Eriksson and Rooth, 2014](#)). Therefore placing people fast helps avoid the long-term unemployment trap which can be very costly both for individuals but also for governments that have to sustain unemployment benefits. On the other hand completely neglecting jobseekers' individual profiles

³Until 2015, some counselors are notably working on several tasks at the same time : counsel to the jobseekers, calculation of unemployment benefits [Cours.des.Comptes \(2020\)](#) and services to firm [Algan et al. \(2020\)](#)

and perspectives on behalf of rapid placements does not seem optimal either. Placing people in jobs that do not correspond to their needs and desires can increase the separation rates which increase unemployment recurrence. People with multiple periods of unemployment or placing people in unsuitable jobs can also be very costly.

Determining the weight that should be given to each of these objectives goes beyond the scope of this paper. However to design public policies it is essential to understand the role of counselors in this context.

To measure the importance of counselor for each objective we study two different outcomes. First the placement rates within 6 months which corresponds to the objective of placing people quickly. Second, to include a quality dimension, we study the placement rate to stable jobs within 6 months. A stable job is defined as not re-registering in unemployment in the 6 months that follow the exit.

V Measuring ‘Counselor effects’

V.1 Estimation

Equation of Interest: The first purpose of this paper is to compute unbiased counselor value-added estimates. The statistical model of interest is the following:

$$Y_{it}^* = \beta X_{it} + \nu_{at} + v_{it}$$

where $v_{it} = \mu_j + \epsilon_{it}$

Where index a corresponds to the registration agency and t to the quarter of entry (cohort) of individual i into the portfolio of her counselor j .

The variable of interest Y_{it}^* corresponds to the placement rate and the quality of the placement (see Section ??). All shocks common to an agency (local market) and the composition of the cohort are neutralized by the parameter ν_{at} . Our model also controls for the following observable characteristics of jobseekers X_{it} : age, gender, nationality, number of children, marital status, occupation in the job desired, years of experience in that occupation, declared reservation wage, level of education, qualification, unemployment benefits entitlement status and length, etc. We are able to control for individual’s endowment with the ability to find a job through their history in unemployment (days spent unemployed last year, number of episodes of unemployment, etc.).

μ_j corresponds to a counselor’s value-added. To be interpreted causally, ϵ_{it} cannot be correlated with μ_j (**Assumption 1**). We test for the plausibility of this hypothesis in section V.6.1.

Our method allows us to compute such counselor effects by exploiting only within-agency variation. However ν_{at} and μ_j can only be separately identified if we observe counselors that

move across agencies during the period⁴ (Chetty et al. (2014); Abowd et al. (2002)). Since our sample contains a connected graph of 98.6% of the agencies (99,98% of jobseekers) linked by 11.4 "movers" on average⁵, we are able to separately identify those fixed effects and to obtain a global cardinal ranking of counselors.⁶ Therefore we can directly estimate the following value-added model:

$$Y_{it}^* = \beta X_{it} + v_{at} + \mu_j + \epsilon_{it} \quad (1)$$

Variance of the effect : We want to know how important and how heterogeneous counselors are in helping jobseekers find a job. We are therefore interested in the variance of μ_j . Yet, we know that the variance of the estimated counselor effects $Var(\hat{\mu}_j)$ is a biased estimator of the true variance. To solve for this, building on the teacher effects literature, we adapt the estimator proposed in Kane and Staiger (2008). We use the co-variance between the average residuals across all cohorts of jobseekers followed by the same counselors in different quarters.

Formally, we set :

$$\begin{aligned} Y_{it} &= Y_{it}^* - \beta X_{it} - v_{at} = \mu_j + \epsilon_{it} \\ \bar{Y}_{jt} &= \frac{1}{n} \sum_{i \in i:(i;t)=j} Y_{it} \end{aligned}$$

Under **Assumption 1** ϵ_{it} and μ_j are uncorrelated, we have:

$$Cov(\bar{Y}_{j,t}, \bar{Y}_{j,t'}) = Var(\mu_j) + Cov(\bar{\epsilon}_{j,t}, \bar{\epsilon}_{j,t'}) \quad \forall t \neq t'$$

Under **Assumption 2** $\bar{\epsilon}_{j,t}$ and $\bar{\epsilon}_{j,t'}$ are uncorrelated, we get:

$$Cov(\bar{Y}_{j,t}, \bar{Y}_{j,t'}) = Var(\mu_j) + 0$$

The plausibility of assumptions 1 and 2 are discussed in section V.6.1 and V.6.2 respectively.

We thus define our empirical estimator for the variance as the weighted⁷ covariance between the average residuals $\bar{Y}_{j,t}$ for all cohorts t and t' of jobseekers followed by the same counselor

⁴In our sample more than 17% of counselors move at least once. Younger counselors are overrepresented among these "movers".

⁵This number exceeds the average number of worker movers by firm, found in the employer-employee data in Europe and the US, which ranges from 2 to 11 movers per firm (Bonhomme et al., 2020).

⁶In the "Follow-up" ("Reinforced") category the largest connected graph is made of 80% (27%) of the agencies linked by 4.1 (1.92) "movers" on average. As a result many agencies are linked by only one "mover" in each group which could result in a limited mobility bias (Andrews et al., 2008, 2012; Bonhomme et al., 2020)

⁷The covariance calculation was weighted by the number of unemployed followed by each counselor each quarter.

j⁸ :

$$\hat{\sigma}_\mu = Cov(\overline{Y_{j,t}}, \overline{Y_{j,t'}})$$

Finally, in A.2 we propose a second estimator of the variance relying on a split sample procedure. Reassuringly, both estimators lead to very similar results.

Confidence intervals: We recover a standard deviation and a 95% Confidence Interval (CI) for the variance estimator using a bootstrap procedure. To limit the time cost, we follow the procedure proposed by Best et al. (2018). First, we estimate our value-added model (equation 1) and residualise the probability to find a job from the controls ($u_{it} = Y_{it}$).

We then randomly draw partial residuals, reassign them to each observation and re-estimate the counselor and agency effects. We repeat the procedure 100 times. To preserve the match structure of our data, we draw the partial residuals within the counselor*agency cells. This ensures the number of counselors who switch agencies and our connected set of agencies remain the same in each estimation.

Although this procedure allows us to recover a standard deviation in a reasonable time it is not without drawbacks. Using partial residuals instead of re estimating the full model does not allow for a correlation between counselors, agencies and controls. This should be of little concern as we show in section V.6.1 that the correlation between individual controls and counselor effect is very low.

V.2 Results

V.2.1 How important are Counselors?

The results of our main specification are plotted in Figure 1. Sub Figures 1(a) and 1(b) show the estimated distributions of counselors fixed effects (μ_j) for each of the outcomes of interest. These histograms adjust the variance to the unbiased estimate of the true variance described in the previous section ($\hat{\sigma}_\mu$).

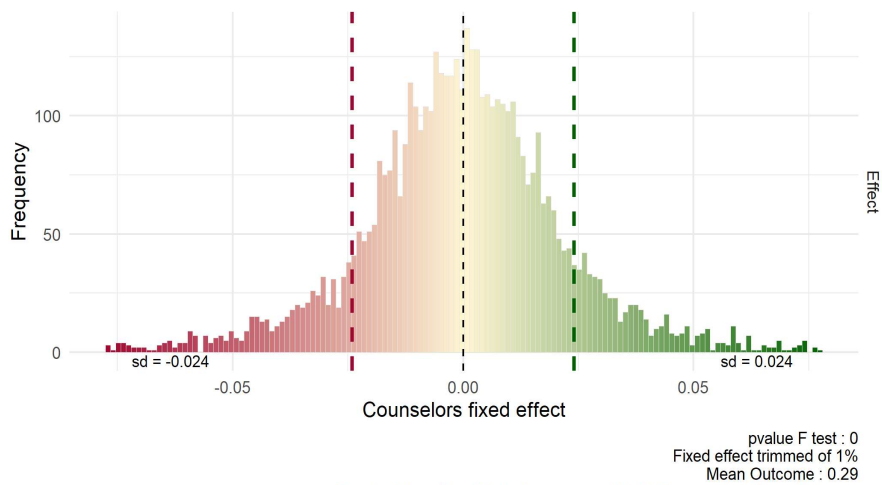
We observe a great variability in the efficiency of counselors. Thus, being followed by a counselor one standard deviation higher in the distribution translates to an increase of around 2.4 [0.8,3.9] percentage points in the probability of finding a job within 6 months and of 1.8 [0.7,3] percentage points in the probability of finding a stable job within 6 months. This corresponds to a sizable and economically relevant effect as it represents an increase of around 8.4% and 13.2% respectively.

The P-value of the F-test for the joint significance of counselor fixed effect is also reported in Figure 1 and is close to 0.

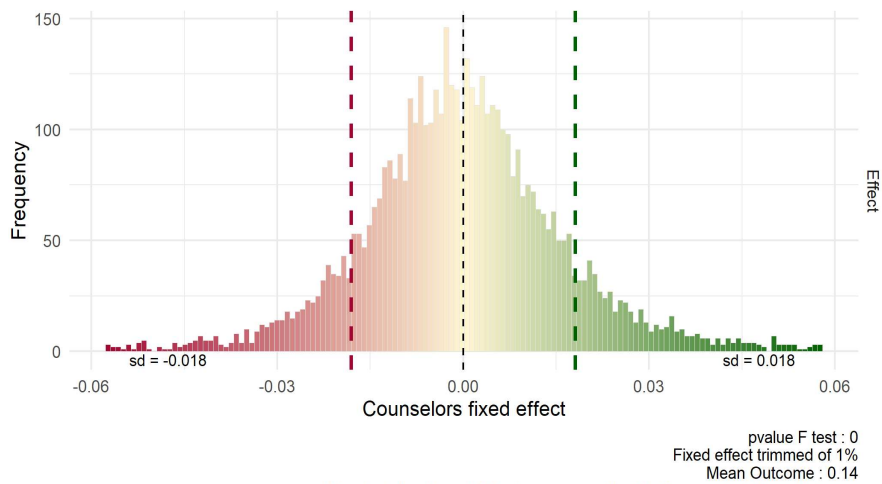
⁸Instead of all cohorts Kane and Staiger (2008) only use the adjacent cohort in t and t-1 of a teacher to estimate the variance of their effect. In section V.6.2 we show that the size of the estimated unbiased variance is not sensitive to which cohorts are used to compute it

By comparing these two figures it appears that the heterogeneity of counselors is slightly bigger, in relative terms, in Figure 1(b) (effect of 8.4% against 13.2%). Having a high value-added counselor matters therefore more when the stability of the job found is taken into account.

Figure 1: Distribution of counselor value-added on the probability of finding a job



(a). a job within 6 months from registration



(b). a stable job within 6 months from registration

V.3 Are high value-added counselors the same across different outcomes?

As discussed in Section IV placing jobseekers within 6 months is a short term objective while placing them in stable jobs takes match quality into account. It seems therefore necessary to understand if high value-added counselors are the same for both objectives (rapid and stable

exits) or if they differ considerably.

Naturally both of these outcomes are to some extent mechanically correlated: being productive at placing people in an stable jobs within 6 months is directly linked to being productive at placing people within 6 months. However, despite this feature, we still find considerable differences across counselor VA measures.

In Figure [A2](#) we plot the counselor value-added estimates for the two outcomes. We find a correlation coefficient of 0.56. This correlation is positive but still far from 1. Additionally we study the differences in the cardinal global rank of counselor VA across the two outcomes. Figure [A3](#) represents the distribution of the absolute value of this difference. On average the difference in the percentile rank of counselors is 19.3%. The bottom 10% of counselors have a difference in ranks below 2% while the top 10% have a difference above 44.4%.

Both the correlation and the differences in ranks underline that, although an important share of counselors have a similar value-added for both outcomes, significant differences remain. High value-added counselors who place jobseekers quickly are not necessarily equally productive at placing people in stable jobs.

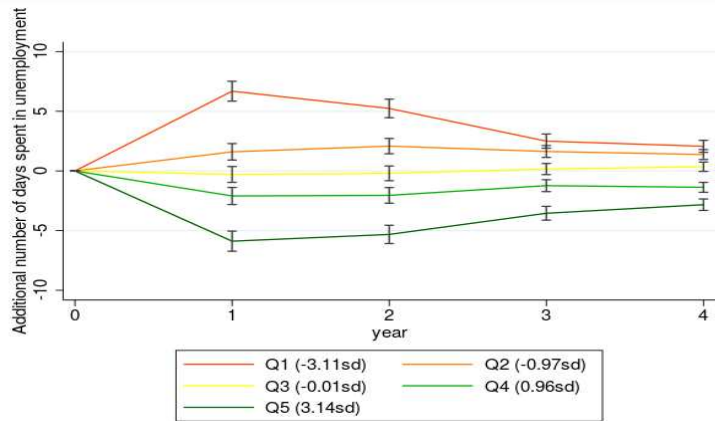
V.4 The prevalence of the effects

Having established that being advised by a productive counselor impacts considerably the exit rate of unemployed within 6 months, we would like to know the prevalence of these effects. We cannot estimate counselor value-added on long term outcomes, such as the exit rate within 4 years, since the sample of individuals we can observe at this horizon is not big enough for such a data demanding procedure. However we can descriptively check if individuals that were assigned more productive counselors at registration spend on average less time in unemployment in the long term. In Figure [2](#) we plot the additional ⁹ number of days spent in unemployment per year after registration by quintile of counselors' value-added. Quintile 5 corresponds to the most productive counselors at placing jobseekers within 6 months (with an average effect of 3.1 standard deviations). We restrict our sample to individuals we can follow for 4 years and therefore to individuals registered before June 2015.

This Figure is consistent with our previous results. The unemployed counseled by the most productive counselors (quintile 5), as measured through our model, spend around 6 fewer days in unemployment than the average in the year following their registration. Over the long run, we observe that the average number unemployed days of individuals guided by both productive and unproductive counselors converges. After 4 years the effect is smaller but remains significant. Therefore, the difference in unemployment trajectories induced by counselors slowly decreases over time but does not vanish.

⁹Additional with respect to the sample mean

Figure 2: Additional number of unemployed days, in a given year after registration, by quintile of counselor value-added



Note: The sample is restricted to individuals registered before January 2015. Average counselor effects for each quintile are displayed in parenthesis (expressed in standard deviations). The additional number of days spent in unemployment is computed as the difference of the average per quintile of this number with respect to the average in the entire sample. The 95% confidence intervals are displayed in brackets.

V.5 The distribution of counselors' value-added across the territory

We know of great spacial inequalities in unemployment trajectories in the Parisian region (Goillon et al., 2011). We wonder if these inequalities are reinforced by the spatial allocation of productive counselors. We therefore explore if there is a concentration of productive counselors in areas where jobseekers face more (or fewer) difficulties.

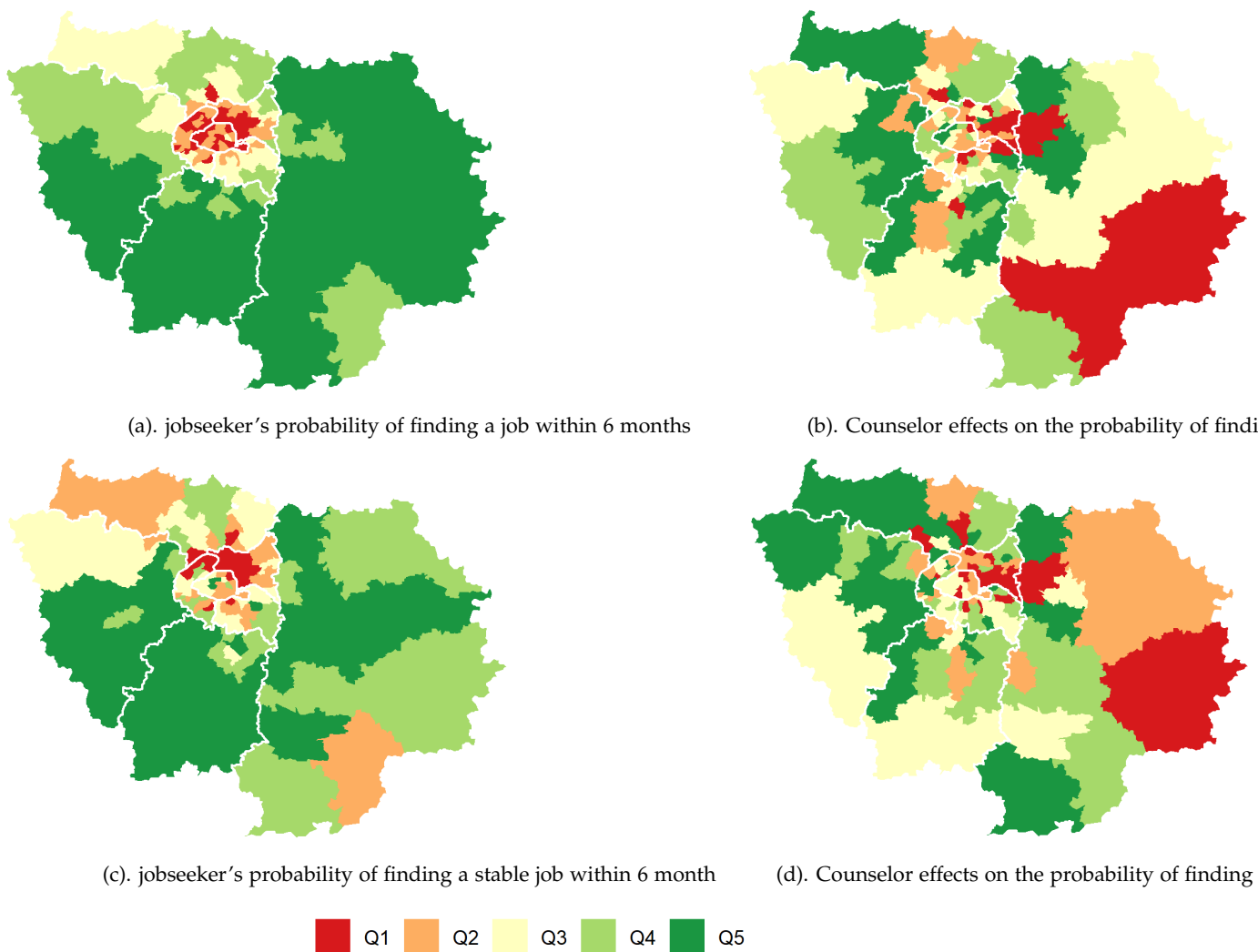
Figure 3 shows the geographical distribution of both jobseekers' probability to find a (stable) job and corresponding counselors' value-added across the Parisian region. Table A1 in the appendix presents the corresponding numerical correlations. Sub Figure 3(a) (3(c)) shows the mean probability for jobseekers to find a (stable) job, at the agency catchment area level classified by quintiles¹⁰. Similarly, Sub Figures 3(b) and 3(d) show the geographical distribution of counselors value-added estimates retrieved from our model for the two outcomes of interest.

In Sub Figure 3(a) we observe that jobseekers with fewer chances to find a job within 6 months (i.e., quintiles 1 and 2) are concentrated in the city of Paris and the adjacent suburbs. In suburbs farther from the city center jobseekers have on average a higher probability of finding a job quickly (i.e., quintiles 4 and 5). When looking at the probability of finding a stable job (Sub Figure 3(c)), the spatial distribution of jobseekers follows a similar visual pattern confirmed

¹⁰The catchment areas of the agencies are approximated. Most municipalities in the Parisian region are covered by a unique local PES Agency. To define agencies catchment areas we associate each municipality to the agency in which most of the municipality's jobseekers register. In our population of interest, 91% of jobseekers go to the main agency in their municipality. A notable exception are denser areas where municipality border do not always correspond to agency catchment area.

by a numerical correlation of 0.82. Although, in the city center of Paris, we observe areas from all the quintiles. When we look at the distribution of counselors allocated to these jobseekers (Sub Figures 3(b) and 3(d)) the picture changes. Productive job counselors are spread out across the territory. As a result, we observe a weak spatial correlation between jobseekers likelihood to find a job and counselors' value-added.

Figure 3: Geographical distribution of jobseekers' likelihood of finding a (stable) job and counselor effects by quintiles across the Parisian region



Note: This map displays the mean jobseekers' probability of finding a (stable) job as well as the corresponding estimated mean counselor's value added for the Parisian Region based on quintile classification at the agency catchment area level.

Overall our results show that counselors are heterogeneous and that being advised by a high value-added counselor substantially improves placements and placement quality. Additionally, we find that counselor effects seem to be prevalent and that more productive counselors,

unlike jobseekers, are spread out across the Parisian region. In the next section we discuss the credibility of the hypotheses that allow us to interpret counselor value-added estimates as causal.

V.6 Testing the validity of the assumptions

V.6.1 Assumption 1: Selection

If the PES guidelines are followed by the agency managers, allocation of jobseekers to counselors only depends on counselor availability at the time of registration, therefore the process is quasi-random. In this case counselor value-added estimates $\hat{\mu}_j$ can be interpreted causally. However, if jobseekers are selected into counselors' portfolios the causal identification of counselor value-added is challenged. For instance, if the most productive counselors are systematically assigned to the most disadvantaged jobseekers, the composition of the portfolios may bias our counselor value-added estimates downwards. Counselor value-added can therefore be estimated causally, through fixed effects $\hat{\mu}_j$, provided that our model controls for all factors that determine assignment of jobseekers to counselors within agencies¹¹.

This section assesses the relationship between counselor value-added and jobseeker observable characteristics to determine whether high value-added counselors are systematically assigned to certain types of jobseekers. A general way of measuring the extent of this selection is to compute the correlation between the estimated counselor value-added and the part of the outcome of interest that is determined by the observable characteristics of jobseekers in our model. We start by estimating equation (1) and isolating the part $P_{\hat{\beta X}}$ of Y_{it}^* predicted by all the observed individual characteristics:

$$Y_{it}^* = \beta X_{it} + v_{at} + \mu_j + v_{it} \rightarrow P_{\hat{\beta X}} = \hat{\beta} X_{it}$$

We estimate $P_{\hat{\beta X}}$ controlling for counselor fixed effects as in [Chetty et al. \(2014\)](#). If counselors quality is correlated with $X_{i,t}$, omitting μ_j would result in falsely attributing part of the counselor effect to individual characteristics.

We then correlate our counselor value-added estimates to this measure of the probability of finding a job as predicted by the observable characteristics in our model:

$$P_{\hat{\beta X}} = \alpha + \gamma \hat{\mu}_j + \kappa_{at} + \epsilon_{ijat}$$

¹¹As we include agency*cohort fixed effects in our main equation, we do not need the assignment of jobseekers to counselors to be quasi-random overall but only within agencies.

The significance and magnitude of γ will shed light on the degree of selection of jobseekers into counselors' portfolios within agencies:

- $\gamma > 0$: high value-added counselors receive low risk jobseekers
- $\gamma < 0$: high value-added counselors receive high risk jobseekers
- $\gamma = 0$: no sorting on individual observable characteristics

The blue line in Figure 4 plots the relationship between $P_{\beta X}$ and $\hat{\mu}_j$ non-parametrically, dividing the value-added estimates $\hat{\mu}_j$ into twenty equal-size groups (vingtiles) and plotting the mean value of $P_{\beta X}$ in each bin. The regression coefficient and standard error reported in this Figure are estimated on the micro data (not the binned averages), with standard errors clustered by agency-cohort.

The relationship between the predicted job finding rate and counselor value-added is nearly flat throughout the distribution. The γ coefficient obtained is 0,025 and significant at the 1% level. The significance of the coefficient does not allow us to conclude that selection does not occur. However, the magnitude of this coefficient is quantitatively very small and not controlling for individual characteristics would bias our estimate of only 2.5%. In other words, being advised by the most productive counselor of a given agency instead of the least productive, increases on average the probability of finding a job within 6 months of 11.6pp (40% of the mean probability). However, the most productive counselor is assigned individuals only 0.29pp (1%) more likely to find a job than the least productive counselor.

Table A2 in the Appendix displays a balance table of all observable characteristics of jobseekers according to the tercile of counselor VA. We observe that observable characteristics are overall well balanced across these terciles. Few differences appear as significant but remain small in magnitude.

Since we do control for individual characteristics in our main equation, our estimated counselor effects are not affected by selection on those characteristics. Nevertheless, selection on un-observables could remain even after controlling for individual characteristics. We are confident that this should not be an issue since, as shown in Table A2, our administrative data-set includes exhaustive information on jobseekers and almost all variables that have been shown by the literature to be predictive of unemployment outcomes: socio-economics characteristics such as qualification and level of education, previous history in unemployment, reservation wage, unemployment insurance entitlement, etc. Even if some crucial information is lacking, it is difficult to conceive that the selection of jobseekers into counselors' portfolios would be strongly correlated to it without being correlated with any of the individual characteristics we control for.

Figure 4 also plots in the same scale, the correlation between the estimated counselor value-added $\hat{\mu}_j$ and the outcome of interest Y^* (red line), and the correlation between counselor

value-added $\hat{\mu}_j$ and the job finding rate residuals Y (green line).¹² The counselor value-added estimates have a 1-1 relationship with job finding rate residuals throughout the distribution and are also positively correlated with the outcome Y^* . This is strong evidence that the relationship between $\hat{\mu}_j$ and job finding rate Y^* is not likely to be driven by persistent differences in jobseekers' characteristics across counselors (ϵ_{it}) and is instead likely to be driven by the causal impact of counselors in job placement (μ_j).

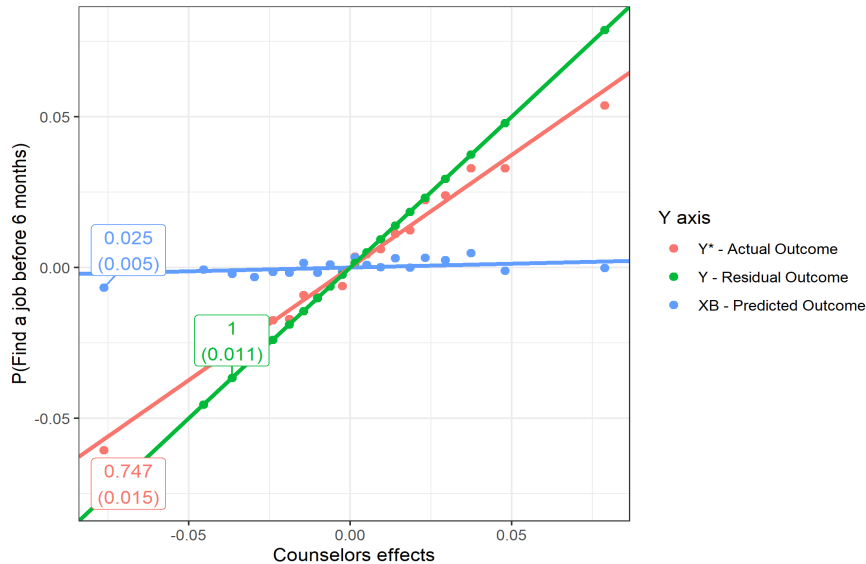
Finally, another simple test to assess selection is to compare the value-added measures retrieved from equation (1) estimated with and without controlling for individual characteristics. In case of substantial sorting, we would observe a large difference between the value-added estimates of the two equations as part of the effect would be captured by individual characteristics. The coefficient of correlation between the two sets of estimates is 0.97. This further argues for negligible sorting.

This section confirms that matching between jobseekers and counselors is quasi-random. We measure a negligible selection into counselors' portfolios on observed characteristics which we ultimately control for. This gives little room for selection on unobserved characteristics, which would need to be uncorrelated to the extensive set of observed characteristics. Given those results, we are confident in the causal interpretation of counselor value-added estimates.

¹²Remember that residuals Y are defined as follows:

$$Y_{it} = Y_{it}^* - \beta X_{it} - v_{it} = \mu_j + \epsilon_{it}$$

Figure 4: Effects of Counselor Value-Added on Predicted, Actual and Residual job finding rate within 6 months of unemployment



Note: This figure shows a binned scatter plot: it divides the counselor effects estimates μ_j into twenty equal-size groups (vingtiles) and plots the mean value of the Y-axis outcome in each bin. The regression coefficients and standard errors reported in this Figure are estimated within agency on the micro data (not the binned averages), with standard errors clustered by agency-cohort.

V.6.2 Assumption 2:

In the statistical model presented in section [V](#) the quality of counselors is fixed over time. This hypothesis, present in most of the VAM estimation in the literature, is equivalent to applying equal weight on the placement rate of the counselor in every period when forecasting counselor quality.

In the case of American teachers, [Chetty et al. 2014](#) show both a transitory and a permanent component in teacher quality. This can be seen in Figure [A4](#) in the appendix where they account non parametrically for the drift in quality by plotting the auto-correlation of test score residuals across classes taught in different years. In their context, the more recent test scores are better predictors of current teacher performance. Under the assumption of stationarity of Teacher VA and student achievement they compute time-varying fixed effects where adjacent periods are re-weighted using the auto-covariance vector.

In Figure [A5](#) in the appendix, we plot the square root of the auto-covariance of the placement rate residual, across cohorts from different quarters. The auto-covariance is roughly constant. We do not observe a drift in quality as in the case of American teachers. More recent periods do not seem to be better predictors of current counselor performance. This reassures us on the validity of the model. More importantly, the absence of such transitory patterns in the quality of counselors supports the validity of assumption 2 of Section [V](#) ($Cov(\epsilon_{it}, \epsilon_{i,t-s}) = 0$). In

our model the existence of a transitory component in counselor effects would be an omitted variable and error terms could be correlated over time. Overall this gives credence to the unbiasedness of our estimator of the true variance of counselor value-added ($Cov(\bar{Y}_{j,t}, \bar{Y}_{j,t-s})$).

This also means that our results are not sensible to the period chosen to define our estimator of the true variance. Using only the co-variance of adjacent periods as in Kane and Staiger (2008) would result in an estimated standard deviation of 0.026 of counselor value-added for the probability of finding a job within 6 months. This estimate is close to our preferred estimate of 0.024 which uses all possible periods.

VI What makes a high value-added counselor ?

VI.1 Estimation

The second contribution of this paper is to give insight about what is behind the value-added of counselors. To do so we exploit the rich administrative data set from the PES. For each counselor and jobseeker, we have information on their direct interactions and any programs and training recommended by counselors. We start by building measures of these different practices and services aggregated at the counselor level. We construct the vector P_j as the average number per jobseeker and per month of the different "services" provided by a counselor during the first 6 months of unemployment. We then regress our counselor value-added estimates on this measures as follows:

$$\hat{\mu}_j = \gamma + \delta P_j + \nu_a + \epsilon_j \quad (2)$$

We include agency fixed effects ν_a since ν_a and $\hat{\mu}_j$ can be correlated. For instance all productive counselors could be in the same agencies, as nothing in our framework states otherwise. By not controlling for agency fixed effects, we could therefore confound the effect of differences in counselor practices with differences in agency composition.

We carry both a univariate and a multivariate analysis. In the former we regress only one service at a time on the counselor value-added estimates. In the latter we control for all services simultaneously and bring a ceteris paribus interpretation.

In this analysis we investigate the practices of counselors that are correlated with their success, however as every aspect of job counseling cannot be measured in our data set, these results should not be interpreted as causal.

VI.2 Descriptive Statistics

In the administrative data we gathered, we identify 9 main counselor "practices".

For direct interactions between counselors and jobseekers we observe:

- Meetings between counselors and jobseekers
- Their remote contacts through email, web calls or phone calls
- Vacancies counselors propose to jobseekers
- Sanctions counselors impose if they believe jobseekers are not making enough efforts to find a job.

For programs counselors recommend to jobseekers, we group the data on 5 main types of programs:

- Outsourced Counseling: corresponds to programs that outsource jobseeker counseling to private placement operators.
- Training: While registered at the French PES, jobseekers follow different types of training programs to acquire new skills and access new employment opportunities.
- Professional Project: This type of program encourages jobseekers to redefine their professional project. It helps identify desirable occupations for a reorientation and guides jobseekers throughout the process.
- Research Strategies: This type of program helps jobseekers improve their search in a practical way. For instance, they can participate in workshops to improve their CV's and cover letters, or improve their interview skills.
- Firm Creation: Helps jobs-seekers interested in starting a new business to come up with new ideas, to conceive the business plan and financing, etc.

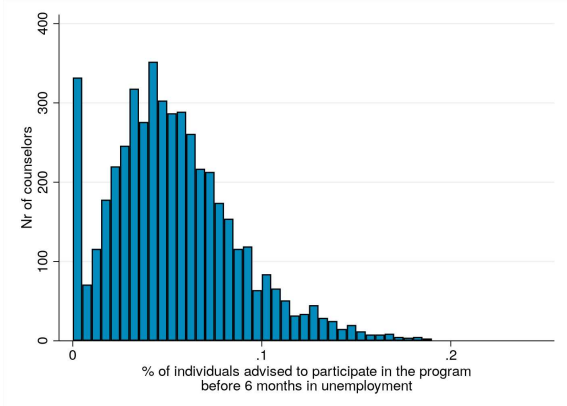
The literature on Active Labor Market policies has focused on many of these interactions and types of programs separately. These programs foster jobseekers through different channels: activation of the search effort and information acquisition (Outsourced Counseling, Research strategies, Vacancies, Meetings and Contacts), moral/psychological support (Meetings and Contacts), monitoring/retaliation (Sanctions) and mid/long term oriented programs that foster the acquisition of tools/skills (Training, Professional Project and Firm Creation).

This section describes how the use of these different "practices" varies across counselors. Figure 5 shows the distribution of counselors by the fraction of jobseekers they advise to participate in each type of program in the first 6 months of unemployment. We observe substantial dispersion between job counselors in how often they assign jobseekers to different types of programs. For example, some job counselors almost never use Research Strategies while others use it for almost half of the individuals they advise. Rates of use for Firm Creation are the lowest and many counselors never assign jobseekers to this type of program. Similarly,

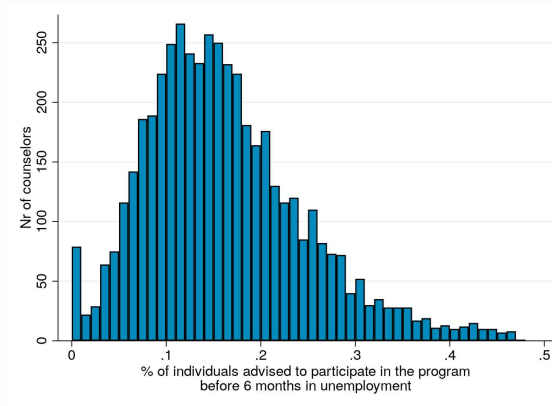
Figure [A6](#) (Appendix) shows the distribution of counselors according to the number of contacts (phone, email, etc.) and meetings they have, vacancies they propose, and sanctions they impose on average to a jobseeker in the first 6 months of unemployment. The recurrence of these different tools varies considerably between job counselors, except for sanctions that are almost never used by French job counselors.

Overall these figures show the high degree of discretion counselors have in our setting.

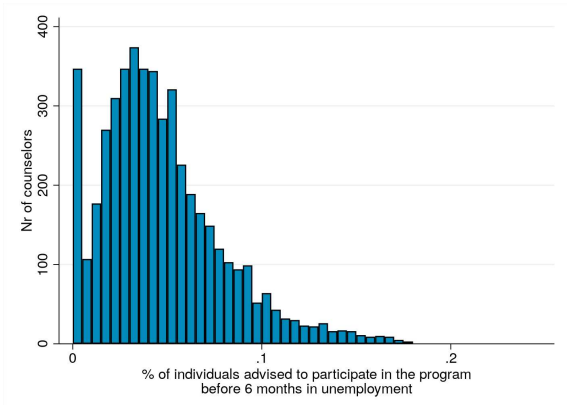
Figure 5: Distribution of counselors by the share of jobseekers they advised to participate in each selected type of program



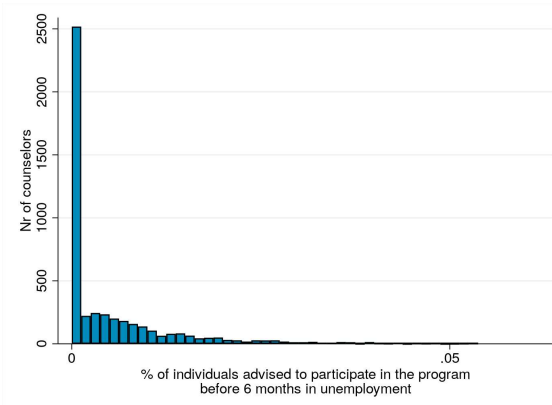
(a). Outsourcing



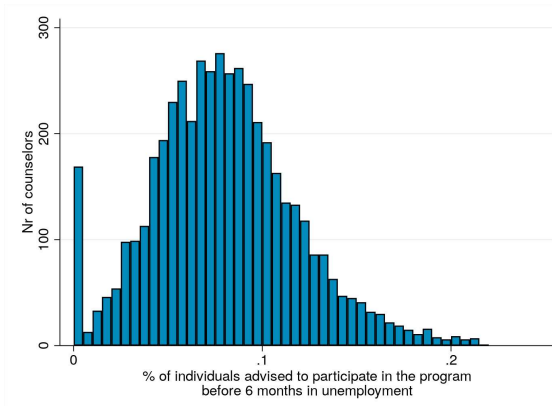
(b). Research Strategy



(c). Professional Project



(d). Firm Creation



(e). Training

VI.3 Results

Table 2 shows the results of regression (2) using both a multivariate (left panel) and univariate (right panel) analysis for each outcome of interest. The two analyses provide rather similar results¹³. The explanatory variables are the standardized number of "services" provided per jobseeker and per month by the counselors during the first 6 months of unemployment. Table A3 in the appendix provides the results of the same regressions but using non standardized explanatory variables. It allows us to obtain the direct effect of an additional "service" provided per jobseeker per month on counselor value-added.

Confirming results in the literature (Schiprowski, 2020), remote contacts and meetings are positively associated with counselors value-added on finding a job within 6 months. Table A3 in the appendix shows that a counselor who provides one more meeting per jobseeker per month increases a jobseeker's probability of finding a job within 6 months by around 2 percentage points (or 7%). The relationship between meetings and counselor value-added estimates is reduced but not erased when controlling for other services mainly proposed during meetings. As several of these services control for the activation/information channel, this suggests that the importance of meetings goes beyond this channel and could encompass potential moral/psychological support from counselors. While face-to-face meetings remain an explanatory variable of counselor value-added irrespective of the outcome of interest, remote contacts (email, phone, etc.) lose importance when considering the stability of the job found.

Proposed vacancies and outsourced counseling exhibit a similar pattern as they are the best predictors for productive counselors at placing people within 6 months. However, when considering placing people in stable jobs, the relationship is considerably reduced in the case of vacancies and entirely disappears for outsourced counseling¹⁴. However, proposing vacancies remains the most or one of the most important predictors of counselor value-added irrespective of the outcome studied, consistent with Bolhaar et al. (2020).

Research strategies are correlated with counselor value-added estimates but the effect is reduced when looking at the multivariate analysis. Moreover, Research Strategies gain importance when taking job stability into account.

Professional Projects and Training programs exhibit negative, albeit not consistently significant, correlation with counselor value-added estimates. These services aim at improving matches in the job market in the mid/long term (i.e., through the acquisition of human capital, career reorientation advice, etc.). However they have a potential lock-in effect in the short

¹³As presented in figure A7 in the appendix, this is explained by the relatively low correlation of provided services with each other.

¹⁴Since 2013 as the result of a large scale evaluation Crépon et al. (2013), the French PES has favored the use of private operators to outsource counseling and activation of most "autonomous" jobseekers to speed up their exit from unemployment. For an evaluation of this type of programs please see Dromundo and Gurgand (2021)

term (well documented in the case of training (Card et al., 2018; Biewen et al., 2014)).

Last, firm creation support programs and sanctions are not correlated with counselors value-added. This result seems natural as these two practices are the least used and exhibit the least variation in their use across counselors as shown in section VI.2.

Table A4 presents a similar analysis but using the exogenous characteristics of counselors instead of their practices. We include characteristics such as gender, experience or type of contract. None explain counselor value-added.

Table 2: Correlation of the counselor value-added estimates with counselor practices (in percentage points)

	Multivariate Analysis		Univariate Analysis	
	P(Job 6 M) (1)	P(stable Job 6 M) (2)	P(Job 6 M) (3)	P(stable Job 6 M) (4)
Contact	0.214*** (0.065)	0.025 (0.049)	0.191*** (0.062)	0.043 (0.047)
Meetings	0.153* (0.083)	0.129** (0.063)	0.268*** (0.068)	0.146*** (0.051)
Vacancies	0.377*** (0.069)	0.155*** (0.052)	0.36*** (0.065)	0.154*** (0.049)
Outsourced Counseling	0.279*** (0.067)	0.055 (0.05)	0.325*** (0.065)	0.086* (0.049)
Research Strategies	0.081 (0.081)	0.139** (0.061)	0.196*** (0.07)	0.16*** (0.053)
Professional Project	-0.266*** (0.067)	-0.172*** (0.051)	-0.138** (0.063)	-0.096** (0.047)
Firm Creation	0.056 (0.064)	0.08* (0.048)	0.049 (0.062)	0.073 (0.046)
Training	-0.085 (0.07)	-0.144*** (0.053)	0.003 (0.066)	-0.088* (0.05)
Subjective Sanction	0.001 (0.063)	0.037 (0.047)	0.024 (0.063)	0.043 (0.047)
Observations	4,784	4,758	4,784	4,758
R ²	0.238	0.234		
Adjusted R ²	0.212	0.208		
Residual Std. Error	0.041 (df = 4627)	0.031 (df = 4601)		

Note: This table shows the coefficients of the regression of counselor value-added estimates (obtained in (2)) on the standardized average number of services provided by counselors per jobseeker and per month during the their first six months in unemployment for the two outcomes of interest. The left panel present coefficients resulting from the regression using all the different provided services as explanatory variables simultaneously (multivariate). The right panel present coefficient for regression using successively each provided services as a unique explanatory variable (univariate). R square for each univariate regression are not reported. All regressions control for agency fixed effect. *p<0.1; **p<0.05; ***p<0.01

VI.4 Explaining differentials in counselor's VA across outcomes

In section [V.3](#) we show that high value-added counselors at placing people quickly are not necessarily also high value-added at placing them in stable jobs. Results from the previous section suggest that, on top of this, high value-added counselors might leverage a different set of practices according to the objective pursued.

In this section we establish whether counselors relative productivity differences across outcomes are linked to differences in practices. To do so, for each counselor we compute the difference between her rank in the distribution of the VA in the outcome "stable exits" and her rank for the outcome related to "rapid exits"¹⁵. We then regress this difference in ranks on the set of services counselors provide .

The results of this regression are shown in Table [A5](#) in the appendix. We observe some statistically significant coefficients. This validates the hypothesis that counselors better ranked at placing jobseekers in stable jobs do not use the same set of tools. Remote contacts that are more impersonal lose their importance, as well as outsourced counseling that has been shown by the literature to be inefficient at keeping individuals out of unemployment for long periods of time ([Behaghel et al., 2014](#)). Counselors better ranked in the "stable" outcome also propose vacancies less frequently.

All the significant coefficient are negative. Therefore it appears difficult to pin down what makes a counselor better at fostering quality jobs than fostering rapid exits.

We do not observe any difference in practices such as Meetings and Research Strategies. As shown in the previous section, these practices are positively correlated with both value-added measures. These results show that Meetings and Research strategies are equally important to foster faster and stable exits from unemployment.

Together these results show that the link between counselor practices and their value-added depend on the outcome studied. Productive counselors are not the same and do not propose the same services depending on the objective pursued. All in all these results bring to light the trade-off counselors could face between quantity oriented and quality oriented practices. Counselors who are better at fostering a rapid exit from unemployment might be doing so at the cost of lower job quality. This raises questions about the social optimum that goes beyond the scope of this paper.

VII Heterogeneity

In this last section we analyse the heterogeneity of our results. First, we investigate first if counselors matter more for certain types of jobseekers. Second, we explore the practices

¹⁵The outcome "stable exits" corresponds to finding a stable job within 6 months and the outcome "rapid exits" to finding a job within 6 months regardless of the quality.

that determine the value-added of counselors for jobseekers more or less likely to find a job according to their observable characteristics.

VII.1 Are counselors more important for certain types of jobseekers?

VII.1.1 Estimation Strategy

To determine the importance of counselors across different types of jobseekers we run a specification analogous to equation [1](#) where we interact counselor fixed effects with jobseekers type, denoted h , as follows:

$$Y_{it}^* = \beta X_{it} + \nu_{it} + \mu_{jh} + \epsilon_{it} \quad (3)$$

Running this equation we obtain, for every counselor and type of jobseeker h (i.e., women and men), a value-added estimate. We thus recover a distribution of counselor value-added for each subset h of jobseekers. By computing the unbiased estimator of the variance of each distribution [16](#), as in section [V](#), we are able to measure the importance of counselors for each sub-population. We do so in absolute terms but also relative to each sub-population mean probability of finding a job within 6 months.

VII.1.2 Confidence Interval

To test if counselor effects are different across jobseeker types, we build on the bootstrap procedure presented in section [V](#). For each dimension of heterogeneity, we recover a standard deviation for the counselor effects $\hat{\mu}_{jh_1}$ and $\hat{\mu}_{jh_2}$ for jobseekers of type h_1 and h_2 respectively. We further compute the standard deviation of the difference between the two counselors effects [17](#) and build a confidence interval. We do this both for the counselor effect expressed in absolute and relative terms.

VII.1.3 Results

We conduct the heterogeneity analysis for 3 main variables: sex, education and the probability $P_{\beta X}$ of finding a job within 6 months as predicted by observable characteristics. As explained in section [V.6.1](#), $P_{\beta X}$ is computed as the part of Y_{it}^* explained by all the observed individual characteristics ($\hat{\beta}X_{it}$) of our model which also controls for counselor and agency*time fixed effects. This variable is a measure of how likely it is for jobseekers to find a job without the intervention of the PES.

¹⁶For each h the unbiased estimator of the variance can be written: $\hat{\sigma}_{\mu,h} = Cov(\overline{Y_{h,j,t}}, \overline{Y_{h,j,t-s}})$

¹⁷ $Var(\mu_{jh_1} - \mu_{jh_2}) = Var(\hat{\mu}_{jh_1}) - 2cov(\hat{\mu}_{jh_1}, \hat{\mu}_{jh_2}) + Var(\hat{\mu}_{jh_2})$

Table 3 shows the results of this analysis. The outcome of interest is the probability of finding a job within 6 months. Each column corresponds to one of the dimensions of heterogeneity studied. The first line shows the standard deviation of counselor value-added in absolute terms while the fourth line expresses it relative to the mean outcome of each sub population. For each pair of subgroups we also display the difference in the standard deviation of counselor value-added and its confidence interval.

Having a counselor one standard deviation above in the distribution of counselor value-added increases the chance of finding a job by 8.6% and by 9.6% for high and low educated individuals respectively. This effect is of 8.3% for men, 10% for women, 12.6% for individuals less likely (below the median) to find a job and 7.3% for individuals most likely to find a job. However, the differences between pairs of subgroups are not statistically significant for the education and sex dimensions. Therefore we can only conclude for the "Predicted probability" dimension. Counselors matter more for individuals less likely to find a job according to their set of observable characteristics. This population has the most difficulties in accessing employment. It seems natural that they benefit more from counselor support.

Table 3: Heterogeneity of counselor quality across different types of job-seekers

	Outcome : Probability to find any job before 6 months					
	Education		Predicted Probability		Sex	
	Below High School	High School and above	Low $P_{\hat{\beta}X}$	High $P_{\hat{\beta}X}$	Men	Women
Sd FE (Counselor effect)	0.026	0.026	0.024	0.028	0.027	0.026
Difference	0.000		0.004		0.001	
IC 95 - Difference	[-0.003;0.003]		[0.001;0.007]		[-0.002;0.004]	
Sd FE / mean outcome	0.096	0.086	0.126	0.073	0.083	0.100
Difference	0.010		0.053		0.017	
IC 95 - Difference	[-0.001;0.021]		[0.041;0.065]		[-0.000;0.034]	
Mean Outcome	0.272	0.301	0.189	0.387	0.319	0.258
Correlation btw FE	0.269		0.244		0.231	
Num Obs	834,399	651,325	734,583	734,584	755,024	730,700

Note: This table shows the heterogenous counselor effect recovered from regressions in which counselor fixed effects were interacted with a dimension of heterogeneity. All regression control for counselors' agency fixed effect as well as individual jobseekers controls. The correlation between fixed effect is weighted by the number of jobseekers followed by the counselor

VII.2 The determinants of high value-added counselors for jobseekers more or less likely to find a job

VII.2.1 Estimation

To find the practices that explain the quality of counselors for each sub-group of jobseekers, we run the following equation:

$$\mu_{jh} = \gamma + \delta P_{jh} + \nu_a + \epsilon_j \quad (4)$$

This equation mirrors equation ?? but using both counselor value-added and practices specific to each jobseeker sub-group. Counselor value-added estimates μ_{jh} are retrieved from the previous section (equation 3). P_{jh} corresponds to the average number, per jobseeker and per month, of "services" provided by a counselor to jobseekers of a given sub-group h, during the first 6 months of unemployment.

VII.2.2 Results

Table 4 shows the results from equation 4 when the heterogeneity dimension studied is how likely jobseekers are to find a job within 6 months according to their observable characteristics. Columns 1 and 4 show results on the entire population of jobseekers (already presented in Table 2). Columns 2 and 5 exhibit the results for individuals with high (above the median) probability of finding a job. Columns 3 and 6 do so for individuals less likely to find a job.

We observe that counselors who are productive at placing job seekers with a high probability of finding a job privilege more Contacts and Vacancies. Counselors who are productive at placing jobseekers with more difficulties exiting unemployment recommend more Training programs (only significant in the uni-variate analysis). Similarly Professional Projects negative correlation is lower when looking at individuals with lower chances of finding a job.

Overall it seems that productive counselors placing jobseekers with high chances of finding a job privilege more activation oriented programs. By contrast, counselors that support jobseekers with more difficulties accessing employment are more likely to use long-term oriented programs.

Table 4: Correlation of the estimated counselor effects with counselor practices for jobseekers more or less likely to find a job within 6 months.

	Multivariate Analysis			Univariate Analysis		
	All Jobseekers (1)	High Predicted Probability (2)	Low Predicted Probability (3)	All Jobseekers (4)	High Predicted Probability (5)	Low Predicted Probability (6)
Contact	0.214*** (0.065)	0.598*** (0.101)	-0.102 (0.075)	0.191*** (0.062)	0.486*** (0.099)	-0.09 (0.071)
Meetings	0.153* (0.083)	0.046 (0.133)	0.13 (0.094)	0.268*** (0.068)	0.199* (0.106)	0.184** (0.078)
Vacancies	0.377*** (0.069)	0.77*** (0.104)	0.226*** (0.077)	0.36*** (0.065)	0.704*** (0.101)	0.268*** (0.074)
Outsourced Counseling	0.279*** (0.067)	0.245** (0.109)	0.24*** (0.075)	0.325*** (0.065)	0.286*** (0.104)	0.246*** (0.073)
Research Strategies	0.081 (0.081)	-0.029 (0.124)	-0.017 (0.092)	0.196*** (0.07)	0.12 (0.112)	0.069 (0.078)
Professional Project	-0.266*** (0.067)	-0.255** (0.104)	-0.136* (0.077)	-0.138** (0.063)	-0.164 (0.1)	-0.063 (0.072)
Firm Creation	0.056 (0.064)	-0.055 (0.099)	0.022 (0.069)	0.049 (0.062)	-0.062 (0.098)	-0.004 (0.067)
Training	-0.085 (0.07)	-0.168 (0.113)	0.104 (0.08)	0.003 (0.066)	-0.109 (0.106)	0.159** (0.076)
Subjective Sanction	0.001 (0.063)	-0.067 (0.098)	0.077 (0.062)	0.024 (0.063)	-0.037 (0.099)	0.08 (0.062)
Observations	4,784	4,270	4,272	4,784	4,270	4,272
R ²	0.238	0.234	0.225			
Adjusted R ²	0.212	0.208	0.198			

Note: This table shows the coefficients of the regression of different sets of counselor value-added for finding a job a 6 month on the standardized average number of services provided by counselors per jobseeker and per month during their first six months in unemployment. In column 1 and 4, counselor effects are issued from the main estimation (1). In the other columns, counselor fixed effects are recovered from the estimation of (2) where counselor value-added estimates are differentiated for jobseekers having a high or a low probability to find a job at 6 months. Standardized average number of services provided by counselors are computed for column 1 and 4 on the full population of jobseekers and for the others on the sub-population with high and low probability to find a job. The left panel presents coefficients resulting from the regression using all the different provided services as explanatory variables simultaneously (multivariate). The right panel presents coefficients of regressions using successively each provided services as a unique explanatory variable (univariate). R square for each univariate regression are not reported. All regression control for counselors' agency fixed effect. *p<0.1; **p<0.05; ***p<0.01

VIII Conclusion

In this paper we show that job counselors tasked with advising, monitoring and directing jobseekers towards active labor market policies are important sources of variation in job search outcomes.

We take advantage of the exhaustive administrative dataset from the PES that allows us to

match jobseekers to their referral counselor. We compute a value-added model exploiting that, within agencies, the allocation of jobs-seekers to counselors is quasi-random.

We find that being assigned to a counselor one standard deviation higher in the distribution of value-added translates into an increase of around 8.4% in the probability of being employed in any job within 6 months and of 13.2% in the probability of being employed in a stable job within 6 months.

After establishing job counselors' high level of discretion, we investigate which practices are correlated with positive counselor performance. We find a trade-off between short-term oriented practices that favor a rapid exit from unemployment and practices that favor better quality matches.

Finally, we find that high value-added counselors seem to matter more for individuals less likely to find a job according to their observable characteristics. Counselor practices also change according to the type of jobseekers they advise.

Our findings have important policy implications. First, they underline the importance of recruiting and retaining high value-added counselors. Training counselors and intentionally assigning job-seekers to counselors could also have an impact. Second, we show that to achieve the objective determined by the social planner (i.e., rapid or stable exits) the counselor actions are crucial. All in all, to build better active labor market policies, policy makers need to take into account the key role of counselors in charge of their implementation.

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A Appendix

A.1 Data Restrictions

1. As explained in section [V](#), to be able to have a global ranking of counselor value-added across agencies, we need to have counselors that switch and therefore link agencies over our period. It turns out that in our data set we have:
 - For the "Guided" category : 98,6% of agencies are connected by 11,44 links on average. This represents 99,98% of jobseekers in this category.
 - For the "Reinforced" category : at most 27,34% of agencies are connected by 1,92 links on average. This represents only 40,79% of jobseekers in this category.
 - For the "Follow-up" category : at most 80% of agencies are connected by 4,1 links on average. This represents only 89,11% of jobseekers in this category.

Consequently we will restrict our sample to the "Guided" category.

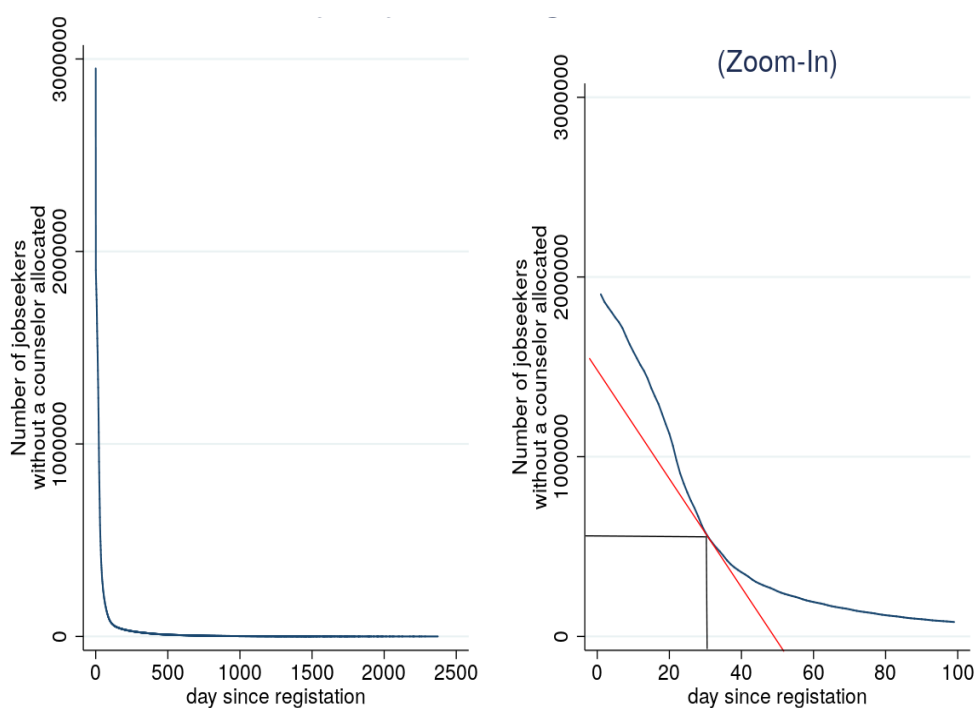
2. The data set that allows us to link jobseekers and counselors only gives us the date of entry of jobseekers into counselors' portfolios. The data therefore shows a jobseeker as being advised by a counselor from the date of allocation until she changes counselor, even if she found a job in between. This structure makes harder for us to identify who is the counselor in charge of the jobseeker at the beginning of the spell because there can be a delay between registration and the date a counselor is allocated to the jobseeker. Therefore, for instance, if in a second spell a jobseeker appears in the data as being counseled for the first 5 days by the same counselor than in her previous episode and then changes to another counselor, it is very likely than in reality she was not followed up by her previous counselor in this second spell. She may have been counseled only by the new counselor but after a delay of 5 days.

To solve for this , we plotted in figure [A1](#) the number of individuals registered for the first time that did not have a designated counselor, each day after registration. The figure shows an L shaped curve: most of the counselors are allocated during the first days in unemployment, the curve then reaches a plateau. By zooming in we identify that the inflection point of this curve is at around 30 days , at this point around 82% of jobseekers have been assigned to a counselor. We will therefore assume that after 30 days in unemployment we are certain of counselor allocation. This translates into 3 concrete sample corrections/restrictions:

- For individuals that have had a previous unemployment episode: if the previous counselor appears at the beginning of the spell but is replaced by another counselor within the first 31 days in unemployment, we keep the second counselor as being the counselor in charge.

- All the episodes where the first counselor is allocated after 31 days are dropped. This allows us to have more comparable individuals as they get exposed to counseling at more or less the same time in unemployment.
- For consistency all the episodes of less than 31 days are dropped. In fact, for individuals that stay less than 31 days in unemployment and appear as being with their previous counselor, we do not know if this counselor was really in charge of advising them or if another counselor would have appear in the data had they stay longer.

Figure A1: Number of jobseekers without a counselor allocated by day since registration



A.2 A second estimator of the variance

A second estimator of the variance can be computed from our data using a non parametric split sample approach (Finkelstein et al., 2016). We randomly split our sample of jobseekers in two, stratifying by agency*quarter*counselor cells. We then estimate equation 1 twice and recover two sets of counselor fixed effects ($k=1,2$). An estimator for the variance of counselor

value-added is the empirical covariance between the two estimates :

$$\hat{\sigma}_{\mu}^2 = Cov(\hat{\mu}_{j_1}, \hat{\mu}_{j_2}) \quad (5)$$

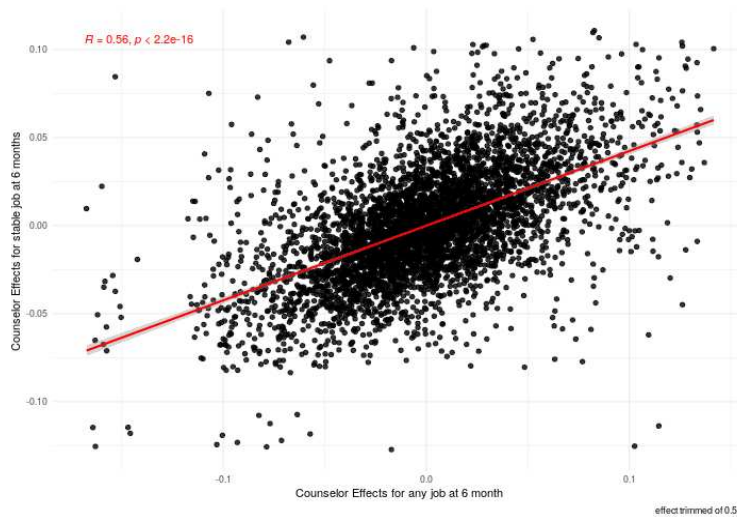
Such procedure would yield unbiased estimates under the assumption that the error term of the two estimates are uncorrelated. In the context of teacher value-added literature this fails due to the presence of a classroom effect. In our case jobseekers followed by a same counselor rarely meet and it is rather implausible that such group specific component exist.

This second estimator for the variance deliver very similar results than the first. One standard deviation in the distribution of counselor translate into 2.36 percentage points more chance to have found any job at 6 month and 1.76 percentage points to have found a durable job. Those estimates are very close to the 2.4 and 1.8 found with out favored estimator of the variance.

A.3 Estimation

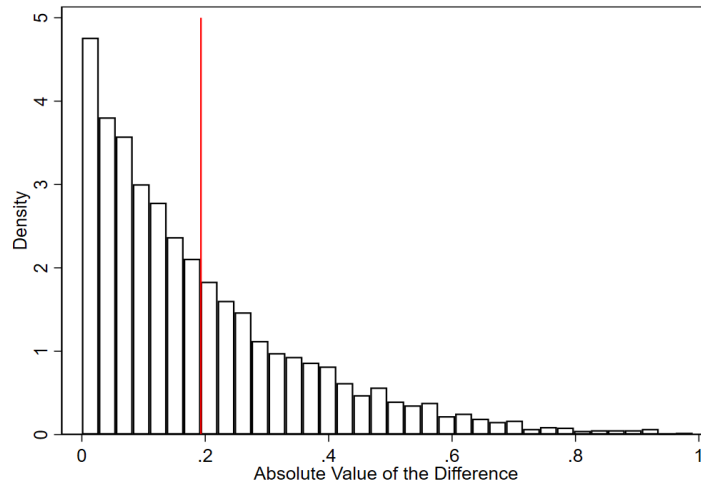
A.3.1 Counselor value-added correlation

Figure A2: Correlation between the counselor value-added for finding a job within 6 month and finding a stable job within 6 months



Note: this graph represents scatter plots of the value-added estimates retrieved from equation 1 where the outcomes of interest are: having found a job within 6 months (X-axis) and having found a stable job 6 months after registration (Y-axis). R is the Pearsons coefficient of correlation between the two sets of value-added estimates

Figure A3: Histogram of the difference in counselor vA percentile ranks across the two outcomes (in absolute value)



Note: This figure displays the histogram of the distribution of counselor’s difference in ranks across the two outcomes of interest. Ranks are expressed in percentiles. The difference is computed in absolute value. The red line indicates the average (0.193)

A.3.2 Geographical correlations

Table A1: Correlation matrix of jobseeker’s probability to find a (stable) job and counselor’s value added at the agency catchment area level

	JS probability of finding a job	JS probability of finding a stable job	Counselor FE on the probability of finding a job	Counselor FE on the probability of finding a stable job
JS probability of finding a job	1	0.82	0.29	0.33
JS probability of finding a stable job	0.82	1	0.29	0.28
Counselor FE on the probability of finding a job	0.29	0.29	1	0.62
Counselor FE on the probability of finding a stable job	0.33	0.28	0.62	1

Note: This table displays the correlation matrix between the average, at the agency catchment area level, in the Parisian Region of: jobseekers’ probability to find a job, jobseekers’ probability to find a stable job, counselor’s value added on the probability of finding a job and counselor’s value added on the probability of finding a stable job.

A.3.3 Estimation Robustness

Table A2: BALANCE TABLE OF OBSERVABLE CHARACTERISTICS BY TERCILE OF COUNSELOR VALUE-ADDED

Variable	(1) Bottom	(2) Middle	(3) (2)-(1)	(4) P-value (3)	(5) Upper	(6) (5)-(1)	(7) P-value (6)
Sex (male)	0.49	0.49	-0.00	0.813	0.49	-0.01	0.435
Age	34.77	34.52	-0.25	0.075	34.50	-0.27	0.101
N of children	0.90	0.88	-0.02	0.280	0.91	0.00	0.089
French Citizenship	0.66	0.67	0.01	0.540	0.69	0.02	0.330
Experience (years)	4.90	4.85	-0.05	0.188	5.03	0.13	0.618
<i>Marital Status</i>							
Single	0.52	0.53	0.01	0.025	0.53	0.00	0.055
Divorced	0.08	0.08	-0.00	0.002	0.08	-0.00	0.021
Married	0.39	0.38	-0.01	0.145	0.39	0.00	0.282
Widowed	0.01	0.01	-0.00	0.453	0.01	-0.00	0.246
Looking for a Permant job	0.93	0.93	-0.00	0.801	0.93	-0.00	0.003
Looking for a Full-time job	0.90	0.90	0.00	0.905	0.90	0.00	0.349
<i>Operational category (Availability)</i>							
Immediately available/Full-time permanent job	0.83	0.84	0.01	0.824	0.84	0.00	0.400
Immediately available/Part-time permanent job	0.09	0.08	-0.00	0.672	0.08	-0.00	0.054
Immediately /definite duration job	0.06	0.07	0.00	0.799	0.07	0.00	0.030
In search but not immediately available	0.01	0.01	-0.01	0.006	0.01	-0.00	0.790
Employed looking for another job	0.01	0.01	-0.00	0.873	0.00	-0.00	0.184
<i>Qualification level</i>							
Unqualified Laborer	0.05	0.04	-0.00	0.015	0.04	-0.00	0.311
Qualified Laborer	0.06	0.05	-0.00	0.115	0.05	-0.00	0.015
Unqualified Employee	0.25	0.25	-0.00	0.293	0.25	-0.00	0.837
Qualified Employees	0.50	0.51	0.01	0.620	0.49	-0.01	0.025
Technician	0.05	0.05	0.00	0.176	0.05	0.00	0.024
Executive	0.07	0.08	0.00	0.592	0.10	0.02	0.017
<i>Profession of the Job Wanted</i>							
Unknown	0.00	0.00	-0.00	0.766	0.00	0.00	0.330
Agriculture and Fisheries	0.01	0.01	-0.00	0.538	0.01	0.00	0.845
Art Works	0.01	0.01	-0.00	0.469	0.01	-0.00	0.841
Bank Insurance and Real Estate	0.02	0.02	0.00	0.858	0.02	0.00	0.819
Trade Sales and Large Distribution	0.16	0.16	0.00	0.068	0.16	0.00	0.158
Communication and Media	0.03	0.03	0.00	0.718	0.03	0.00	0.129
Construction, Building and Public Works	0.08	0.08	-0.00	0.462	0.07	-0.01	0.084
Catering Tourism and Leisure	0.11	0.11	-0.00	0.573	0.10	-0.01	0.567
Industry	0.03	0.03	-0.00	0.990	0.03	0.00	0.880
Installation and Maintenance	0.04	0.03	-0.00	0.047	0.03	-0.00	0.051
Health	0.03	0.03	0.00	0.087	0.03	-0.00	0.120
care/community services	0.22	0.21	-0.00	0.083	0.21	-0.01	0.036
Entertainment	0.01	0.01	0.00	0.155	0.01	-0.00	0.163
Support to the company	0.15	0.15	0.00	0.964	0.16	0.01	0.173
Transports and Logistics	0.11	0.11	0.00	0.034	0.11	0.01	0.659
Observations	429982	613714	1043696		442014	871996	
Nb Couns	1652	1652	3304		1652	3304	

Table A2 (continued): BALANCE TABLE OF OBSERVABLE CHARACTERISTICS BY
 TERCILE OF COUNSELOR VALUE-ADDED

Variable	(1) Bottom	(2) Middle	(3) (2)-(1)	(4) P-value (3)	(5) Upper	(6) (5)-(1)	(7) P-value (6)
<i>Education level</i>							
No Formal Education	0.08	0.08	-0.00	0.278	0.07	-0.01	0.232
Secondary School	0.14	0.13	-0.01	0.062	0.13	-0.01	0.121
Technical High School	0.23	0.23	-0.00	0.562	0.23	-0.00	0.062
High School	0.25	0.25	0.00	0.971	0.25	-0.00	0.137
Second Year University	0.11	0.12	0.00	0.723	0.12	0.01	0.855
Third/Fourth Year University	0.09	0.10	0.00	0.466	0.10	0.00	0.459
Masters Degree and More	0.09	0.10	0.01	0.0489	0.11	0.02	0.011
<i>Reason for Registration</i>							
Lay-off	0.21	0.21	0.00	0.248	0.22	0.02	0.321
End of Contract	0.29	0.30	0.01	0.109	0.30	0.01	0.000
Back from inactivity	0.21	0.21	-0.00	0.476	0.21	-0.00	0.419
Others	0.29	0.28	-0.01	0.439	0.27	-0.02	0.008
Reservation Wage (month)	1844.52	1865.57	21.06	0.853	1942.20	97.68	0.010
Entitled to UB	0.46	0.47	0.01	0.526	0.49	0.04	0.014
N of spells before	0.72	0.72	0.00	0.973	0.70	-0.02	0.024
Unemployed before	0.54	0.54	0.00	0.822	0.53	-0.01	0.050
Welfare program (RSA)	0.26	0.25	-0.01	0.115	0.23	-0.03	0.007
Observations	429982	613714	1043696		442014	871996	
Nb Couns	1652	1652	3304		1652	3304	

Note: This table displays the average observable characteristics by tercile of counselor Value-added. Columns (3) and (6) display the differences between the middle and bottom tercile and between the upper and bottom tercile respectively. The P-values of these differences are displayed in columns (4) and (7) and are issued from regressions including agency fixed effects and dummy variables indicating the belonging to either the middle (column (4)) or the upper tercile (column (7)).

Figure A4: Chetty et al. 2014 - Autocorrelation Vector in Elementary School for English and Math Test Scores

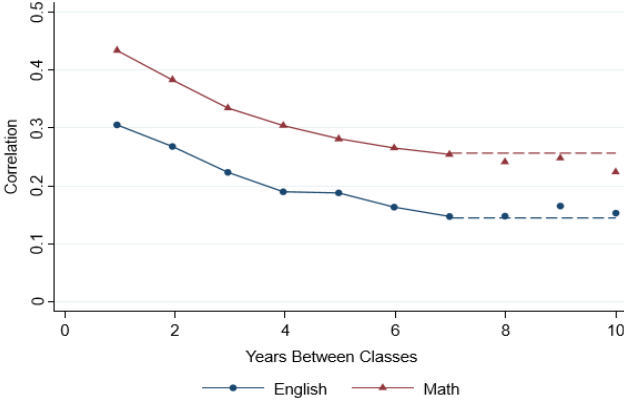
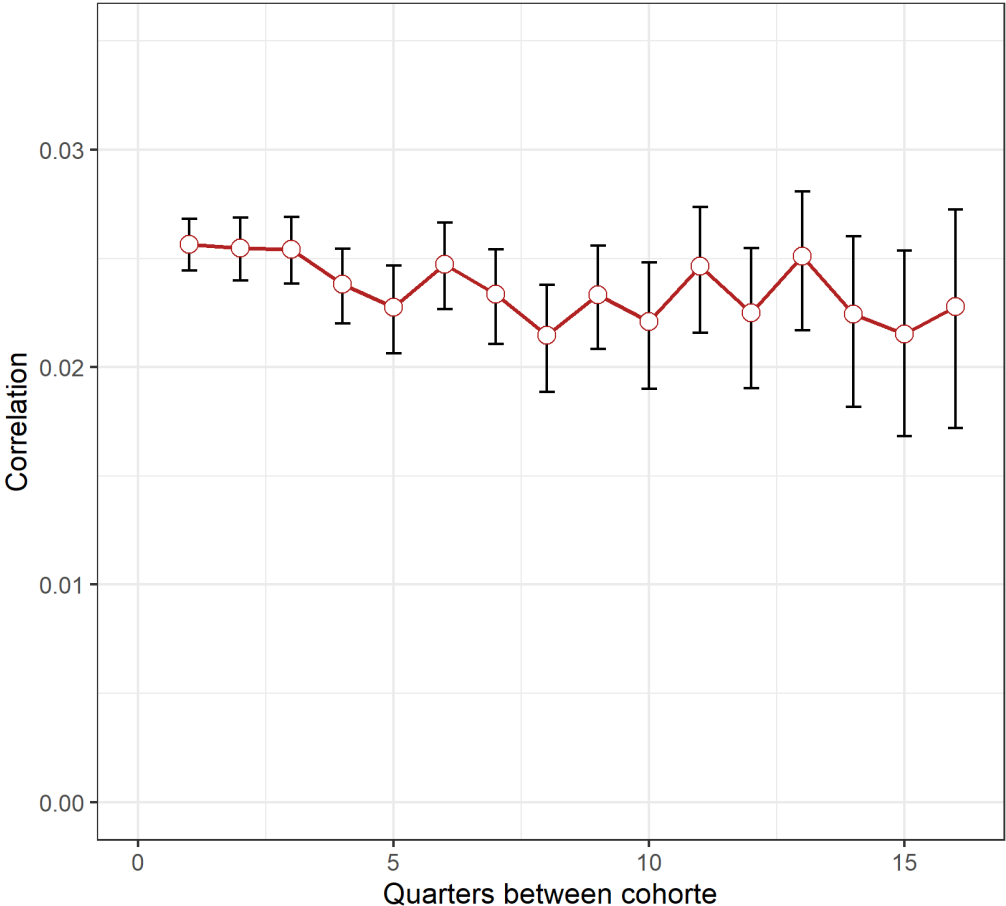


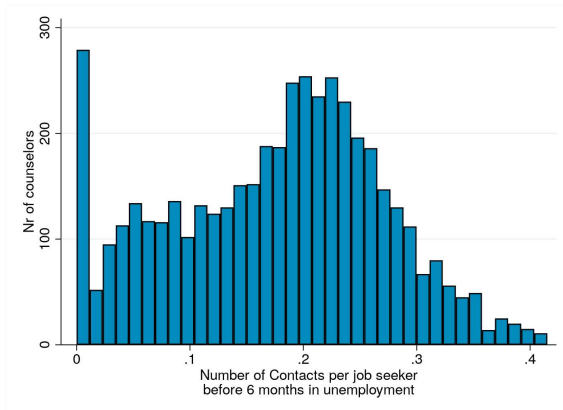
Figure A5: Autocovariance Vector for Placement rates within 6 months (Square root)



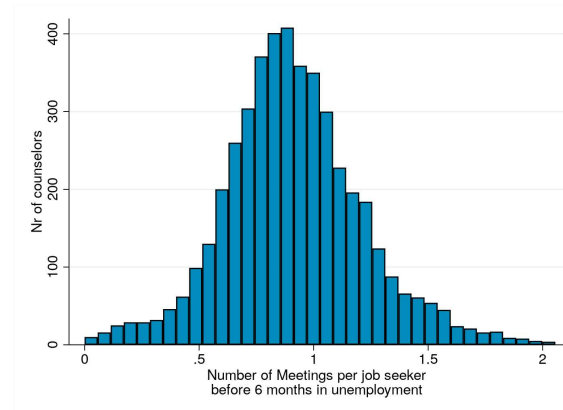
Note: These figures show the correlation between mean placement rate residuals across cohorts of jobseekers followed by the same counselors in different quarters. To calculate these vectors, we residualize the placement rate using jobseekers individual controls and agency fixed effects. We then calculate a (precision-weighted) mean test score residual across cohorts for each counselor-quarter. Finally, we calculate the autocorrelation coefficients as the correlation across quarter for a given counselor, weighting by the mean of jobseekers followed in the quarter taught in the two quarter. Confidence Intervals in brackets where computed by bootstrap.

A.4 Descriptive Statistics on Practices

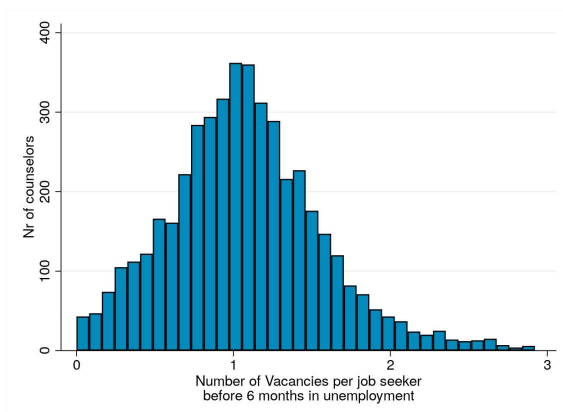
Figure A6: Distribution of counselors by the number of contacts (phone, email, etc.) and meetings they have, vacancies they propose, and sanctions they impose on average to a jobseeker.



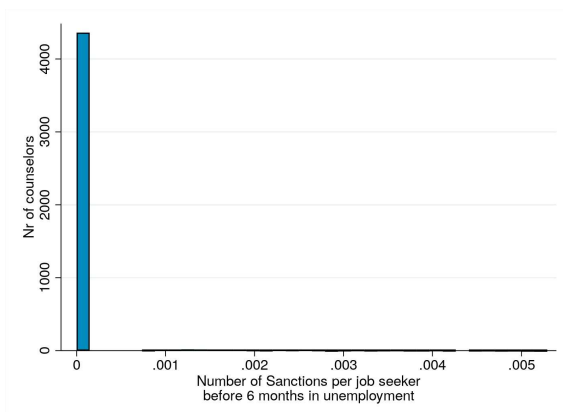
(a). Contacts



(b). Meetings



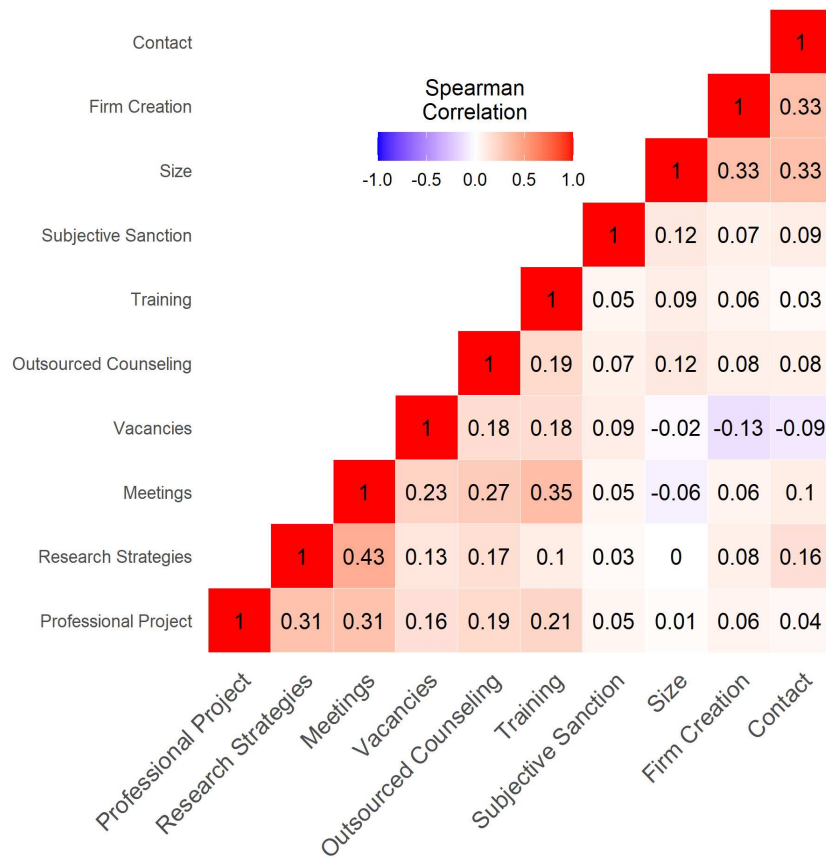
(c). Proposed Vacancies



(d). Sanctions

A.5 Results on Practices

Figure A7: Heat map of the correlation between different services offered by counselors



Note: This figure show the Spearman correlations between each of the services proposed by counselors. The number of service correspond to the counselor average number of time the services is proposed per jobseeker per month of unemployment in the first 6 months the spell

Table A3: Correlation of the estimated counselor value-added with counselor practices

	Multivariate Analysis		Univariate Analysis	
	P(Job 6 M) (1)	P(stable Job 6 M) (2)	P(Job 6 M) (3)	P(stable Job 6 M) (4)
Contact	8.677*** (2.626)	1.022 (1.976)	7.753*** (2.53)	1.728 (1.897)
Meetings	1.884* (1.021)	1.586** (0.769)	3.304*** (0.836)	1.796*** (0.628)
Vacancies	2.841*** (0.519)	1.166*** (0.391)	2.716*** (0.489)	1.159*** (0.368)
Outsourced Counseling	21.776*** (5.235)	4.332 (3.933)	25.38*** (5.092)	6.732* (3.816)
Research Strategies	2.311 (2.315)	3.964** (1.741)	5.599*** (2.017)	4.59*** (1.512)
Professional Project	-24.636*** (6.216)	-15.961*** (4.687)	-12.787** (5.836)	-8.94** (4.377)
Firm Creation	20.494 (23.14)	28.974* (17.349)	17.803 (22.574)	26.586 (16.843)
Training	-5.703 (4.721)	-9.696*** (3.561)	0.222 (4.46)	-5.887* (3.344)
Subjective Sanction	4.492 (301.208)	175.799 (225.769)	114.78 (303.23)	208.124 (226.228)
Observations	4,784	4,758	4,784	4,758
R ²	0.238	0.234		
Adjusted R ²	0.212	0.208		
Residual Std. Error	0.041 (df = 4627)	0.031 (df = 4601)		

Note: Coefficient have been multiplied by 100. This table shows the coefficients of the regression of counselor value-added estimates (obtained in (2)) on the average number of services provided by counselors per jobseeker and per month during the their first six months in unemployment for the two outcomes of interest. The left panel present coefficients resulting from the regression using all the different provided services as explanatory variables simultaneously (multivariate). The right panel present coefficient for regression using successively each provided services as a unique explanatory variable (univariate). R square for each univariate regression are not reported. All regression control for counselors' agency fixed effect. *p<0.1; **p<0.05; ***p<0.01

Table A4: Correlation of the estimated counselor value-added with counselor characteristics (in percentage points)

	Multivariate Analysis		Univariate Analysis	
	P(Job 6 M) (1)	P(stable Job 6 M) (2)	P(Job 6 M) (3)	P(stable Job 6 M) (4)
Women	0.184 (0.145)	0.012 (0.109)	0.124 (0.147)	-0.025 (0.11)
Open ended contract	-0.103 (0.253)	-0.313 (0.191)	-0.348 (0.246)	-0.504*** (0.184)
Less than 2 years Exp	0.174 (0.258)	0.142 (0.194)	0.118 (0.258)	0.095 (0.193)
More than 10 years Exp	-0.202 (0.267)	-0.201 (0.201)	-0.198 (0.263)	-0.224 (0.196)
More than 5 years Exp	-0.276 (0.285)	-0.159 (0.214)	-0.327 (0.285)	-0.141 (0.213)
Civil Servant Contract	0.001 (0.185)	-0.017 (0.139)	-0.205 (0.167)	-0.216* (0.126)
Observations	4,537	4,516	4,537	4,516
R ²	0.235	0.237		
Adjusted R ²	0.209	0.210		
Residual Std. Error	0.040 (df = 4383)		0.030 (df = 4362)	

Note: This table shows the coefficients of the regression of counselor value-added estimates (obtained in (2)) on counselor characteristics provided for the two outcomes of interest. The left panel present coefficients resulting from the regression using all the different provided services as explanatory variables simultaneously (multivariate). The right panel present coefficient for regression using successively each provided services as a unique explanatory variable (univariate). R square for each univariate regression are not reported. All regression control for counselors' agency fixed effect. *p<0.1; **p<0.05; ***p<0.01

Table A5: Correlation between the difference in ranking across our two VA measures and counselor practices

	(1) Difference between counselor's VA ranking (="stable exits" ranking - "rapid exit" ranking)
Contact	-0.00968** (0.00487)
Meetings	0.00195 (0.00588)
Vacancies	-0.0111** (0.00442)
Outsourced Counseling	-0.0102** (0.00499)
Research Strategies	0.00330 (0.00579)
Professional Project	0.00440 (0.00392)
Firm Creation	0.00213 (0.00393)
Training	-0.00764 (0.00536)
Subjective Sanction	0.00379 (0.00327)
Constant	0.000271*** (0.0000678)
N	4756
R2	0.255

Note: This table displays the coefficients of the regressions of the difference in ranking across our two VA measures and counselor practices. More precisely, the outcome of interest is the difference between the rank of counselors in the VA measure for the outcome related to "stable exits" (finding a stable job within 6 months from registration) and the rank of counselor's in the VA measure for the outcome related to "rapid exits" (finding a job within 6 months regardless of the quality). These rank measures are expressed in percentiles. The explanatory variables of interest are the standardized average number of services provided by counselors. The regression includes controls

for agency fixed effects and a constant. Standard errors are displayed in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$

Chapter 3

Directing job search: a large scale experiment

Directing job search: a large scale experiment.*

Luc Behaghel[†] Sofia Dromundo[‡] Marc Gurgand[§] Yagan Hazard[¶]
Thomas Zuber^{||}

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Abstract

We analyze the employment effects of directing job seekers' applications towards establishments likely to recruit, building upon an existing Internet platform developed by the French public employment service. Our two-sided randomization design, with about 1.2 million job seekers and 100,000 establishments, allows to precisely estimate supply- and demand-side effects. We find a 2% increase in job finding rates among women, while establishments advertised on the website increase their hirings on indefinite duration contracts by 3%.

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[†]Paris School of Economics, INRAE, luc.behaghel@psemail.eu

[‡]Paris School of Economics sofia.dromundo@psemail.eu.

[§]Paris School of Economics, CNRS, marc.gurgand@psemail.eu

[¶]Paris School of Economics, yagan.hazard@psemail.eu

^{||}Paris School of Economics, thomas.zuber@psemail.eu

I Introduction

Matching frictions are at the heart of equilibrium unemployment theory. In addition to their consequences on job search, they are hypothesized to be a key driver of hiring costs which contribute to the determination of job creation by firms (Pissarides, 2000). However, while there is a rich micro-econometric literature on job search, there is limited micro evidence to quantify the firms' response to variation in hiring frictions (Oyer and Schaefer, 2011). Despite major changes in matching and hiring technologies with the arrival of the Internet, it is not fully clear to what extent firms' hiring costs have decreased, and to what extent this has spurred job creations. This striking empirical gap concerning a key element of a standard theory of unemployment can be explained by the lack of credible sources of variation in hiring costs that are needed to identify effects on firms' recruitment decisions.

This paper provides early evidence on workers' and firms' reactions to an attempt to reduce matching frictions by providing targeted match recommendations. Leveraging an existing platform run by the French public employment service (PES), we conduct a two-sided randomized experiment involving about 1.2 million job seekers and 100,000 establishments. The job seekers' sample comprises all unemployed job seekers registered at the PES in 94 local labor markets (about one fourth of the French labor market). The establishments are selected by the platform called "La bonne boîte" ("the good firm", henceforth LBB), based on an algorithm predicting hirings at the firm \times occupation level. The goal of the PES with this service is to provide job seekers with access to the so-called "hidden market" of firms that recruit without posting job ads. On the business-as-usual mode, the LBB website directs each job seeker toward a list of firms most likely to hire him according to the location and occupation criteria he enters. During the experiment, while the platform remains available to all, we introduce two experimental treatments. First, we randomly select a subset of firms among those short-listed by the LBB algorithm. During four weeks, those "treated" firms are displayed in priority in response to job seekers' requests on the website, while the remaining "control" firms are not displayed (or displayed at the bottom of the list if there are too few treated firms satisfying the search criteria). Second, we randomly draw two thirds of the 1.2 million job seekers to receive two or four emails pushing the LBB service, with specific, individualized, recommendations towards up to eight of the treated firms. This two-sided randomization design provides random variation to study the supply and demand responses to targeted matching recommendations. Specifically, the compar-

¹Relevant literature regarding the impact of the Internet on the labor market and job search includes Autor (2001); Kuhn and Skuterud (2004); Kuhn and Mansour (2013); Kroft and Pope (2014). Algan et al. (2018) provides one of the few pieces of evidence on the effect of decreased hiring costs on job creations. Horton (2017) is, to the best of our knowledge, the only paper focusing on the effect on firms' hirings of platform-mediated algorithmic recommendations of potential candidates. See Kircher (2020) for a review of ongoing work in the field.

ison across experimental groups of job seekers allows us to study the labor supply response to customized recommendations. In addition, as long as job seekers respond to the emails or to the listings posted on the LBB website by sending more applications to treated firms, our design provides unique variation to study the labor demand response to changes in the number and type of spontaneous applications received by firms.

On the job seekers' side, we find that receiving emails with targeted recommendations slightly increases job finding rates. This impact is however small, and concentrated among women: the probability that they start a new job within 4 months increases by 0.2 percentage point (a 2% increase from a baseline level of 12.9%). Despite the large sample size, we are unable to detect any statistically significant effect on men. On the firms' side, we find a marginally significant increase in hiring rates. Importantly, while the increase in exits to jobs is concentrated among women and for definite duration contracts, the additional hirings by firms are not particularly driven by women and concern indefinite duration contracts. This suggests that the effect on firms is driven by an additional inflow of applicants caused by the systematic display of treated firms on the LBB website, rather than by the targeted recommendation in the emails. Importantly, we find that the predictions of the LBB algorithm are overall correct: firms that are predicted to hire more do hire more. However, they only marginally hire more when advertised by LBB. The first contribution of the paper is thus to show that the advertising of firms likely to hire but not necessarily ready to post job ads has some limited effects on recruitment outcomes.

The second contribution of our empirical design is to provide evidence on occupational search. Our empirical design indeed includes additional sub-treatment arms: in a first arm, workers searching for a given occupation are recommended to apply to firms that are predicted to hire in the same occupation or in a very close one; in the second treatment arm, workers are recommended to apply to firms likely to hire in neighboring occupations. Symmetrically, in a first arm firms are selected to receive workers searching in the occupation they are predicted to hire from; in a second arm, firms are signaled to candidates further away in the occupational space. This allows us to investigate how broadening job search to nearby occupations allows to reduce occupational mismatch, a question that has triggered significant interest in the recent literature (Marinescu and Rathelot, 2018; Belot et al., 2018). Here again, our two-sided randomization design allows us to assess the consequences of extending the occupational distance in proposed matches both from the firms' and the workers' perspective. In theory, two opposite forces are at play: extending the distance between proposed matching parties allows the firm (resp. the worker) to access a broader choice set, but it may also increase screening costs and reduce the expected productivity of the proposed matches. Empirically, the two aspects tend to offset each other: on average, we do not find firms (or workers) directed to closer matches to be more likely to match.

In Section [II](#), we provide background information on LBB's job search platform. Section [III](#) presents the experimental design. Results are given in Section [IV](#) and Section [V](#) concludes.

II Context

II.1 "La Bonne Boîte": an online job search platform

"La Bonne Boîte" (LBB) is a digital tool put in place by the French Public Employment Service (PES) in 2016. It aims to help job seekers in their search by encouraging them to make unsolicited (spontaneous) applications.

On this platform, job seekers indicate a geographical area and an occupation of search (see Figure [A1](#)) and, using an algorithm based on past recruitment data, LBB proposes a list of firms likely to hire them (see Figure [A2](#)). Once they "click" on a firm of interest an email address and/or phone number to contact the firm directly is given (see Figure [A3](#)). Importantly, LBB predictions use the universe of French firms, so that recommendations are not restricted to firms advertising a position or to firms in contact with the PES. Therefore the goal of LBB is to highlight the hidden job market by reducing informational frictions.

In order to propose firms likely to hire for a specific area and occupation, LBB uses establishment/occupation hiring predictions. These predictions are derived from establishment level predictions which are then mapped into establishment/occupation hiring prediction using a sector/occupation crosswalk.² LBB then defines for each occupation a specific predicted hiring threshold above which an establishment is deemed a "hiring firm" for this specific occupation.³ If there is no such establishment, LBB's search engine will suggest to extend the search to a wider geographical area.

We do not have a leeway on the algorithm used to predict hiring, and take it as given. However, we are confident in the quality of LBB's prediction for our purpose: their prediction does explain realized hirings. Figure [A6](#) plots the relationship between the log of firms' average predicted hiring, within twenty equal-size groups, and the log of realized average hiring in each of those groups of firms. The Figure also plots the linear correlation between the logs of predicted hiring and realized hiring, estimated on the individual data. The correlation coefficient is 0.89, with an R-squared of 0.37, and significant at the 1% level.

²This crosswalk is based on the share of each occupation hirings within each sector. This share was computed for registered unemployed exiting unemployment between the 02.03.2016 and 31.03.2017 (<https://www.data.gouv.fr/fr/datasets/nombre-dembauches-par-code-ape-et-code-rome/>).

³As a consequence, a given establishment can be considered as a "hiring firm" for one occupation but not for another.

II.2 Measuring occupational distance

One of the potential advantages of internet job search tools like LBB is to allow job seekers to expand the occupational breadth of their job search effort, if the platform directs job seekers to nearby occupations. The measure of occupational distance used to do so builds on the 532-occupation classification⁴ used by the PES when asking job seekers their desired occupation, and by LBB to compute hiring predictions. In addition, we take advantage of PES' expert knowledge on possible transitions to build a simple measure of occupational distance. More precisely, for every single occupation, the PES lists a set of neighbor occupations which are deemed close enough in terms of required skills for job seekers to transition to without any further training. We use these neighboring occupations to build an occupational graph where each occupation is connected to its listed neighboring occupations. As the closeness of occupations is not necessarily symmetric (occupation A neighboring occupation B does not entail that occupation B neighbors occupation A), the underlying occupational graph is a directional one. Finally we use this occupational graph to measure the relative closeness of any two occupations. To do this we compute the shortest path linking any two occupations and take this shortest path as our main measure of occupational distance. With this methodology 6.20% of occupations end up isolated, the average occupational distance between any two connected occupations, measured by the number of intermediary nodes, is 7.11 and occupations are on average connected to 3.34 immediate neighbor occupations. As shown in Figure A5 of Appendix A.2, our measure of occupational distance correlates well with occupational transitions observed in the French data over the 2008/2012 period. Importantly, by limiting ourselves to PES' original definition of "close" occupations we only would have covered 15% of observed transitions. By extending our measure of occupational distance to pairs which were not previously ranked we are able to cover 83% of observed occupational transitions, hence giving a much more comprehensive view of the underlying occupational structure of the French labor market.

III The Experiment

III.1 Experimental design

Unlike previous work which tended to focus either on supply or the demand side effects of job-search assistance programs, our design aims at uncovering both effects simultaneously. To do so, we implement a two-sided randomization, on the firms' and job seekers' sides.

The experimental treatments are assigned within commuting zones⁵. Our experimental sample

⁴Both the PES and LBB use the same 532-occupations ROME classification ("Répertoire Opérationnel des Métiers").

⁵When assigning treatment within a commuting zone, we do not distinguish across job seeker and establishment pairs by their geographical distance. Indeed, the existing evidence suggests that spatial mismatch is second order

covers 94 out of the 404 French commuting zones,⁶ representing a pool of 1,209,859 job seekers and 98,366 hiring establishments. We randomly draw 806,437 and 38,810 treated job seekers and establishments in their respective treatment group. We now describe the randomization design.

III.1.1 Basic Design

The basic experimental treatment consists in increasing treated firms' and treated job seekers' exposure to LBB's job search services. First, we randomly select a subset of firms among those short-listed by LBB's algorithm. We stratify the random selection of treated firms within 5-digits sectors and above median/below median predicted hiring bins. During four weeks, selected "treated" firms are displayed in priority in response to job seekers' requests on the website, while the remaining "control" firms are not displayed (or displayed at the bottom of the list if there are too few treated firms satisfying the search criteria). Second, we randomly draw two thirds of the 1.2 million job seekers to receive two or four emails pushing the LBB service, with specific recommendations toward up to eight of the treated firms. We stratify the random selection of treated job seekers within desired occupations and above median/below median bins of a linearly predicted exit rate out of unemployment.

Even though the random selection of a pool of treated job seekers and a pool of treated establishments tells us which job seekers and which establishments will enter our pairwise recommendations, it does not tell us which specific pairwise recommendations will be formed. Indeed, once we have proceeded with the random selection of treated job seekers and treated establishments we are left with a two-sided assignment problem. Given that we should recommend a particular set of treated establishments to a particular set of treated job seekers, *which* establishment should we recommend to *which* job seeker?

Furthermore, this assignment has to take into account the additional random variation in the amount of recommendations and their occupational distance. Next sections explain how we solved for this assignment problem.

compared to occupational mismatch (Marinescu and Rathelot, 2018). The role of geographical distance can however be analyzed ex post based on remaining non-experimental variation; this is kept for further analysis.

⁶We randomly selected these 94 Commuting Zones out of all the 404 possible commuting zones. We stratified this random selection of treated commuting zones within tightness and size quintiles. For more details on Commuting Zones and local labor markets see Appendix Section A.3.

III.1.2 Introducing random variations in the number of recommendations and their occupational distance

Beyond the first order effectiveness of tailored job-search recommendations, there are two important unknowns that underlie our experiment. Firstly, we do not a priori know (a) *how many* recommendations job seekers and establishments should receive for these recommendations to have an effect. Secondly, we do not a priori know (b) *how far* in the occupational space we should advise job seekers and establishments to look for jobs and employees. In order to get a sense for (a) and (b) we build into our experimental design a further level of randomness by distributing 4 possible treatment status among treated job seekers and establishments, using a factorial design.

Hence while among treated job seekers some will receive **many** recommendations, others will only receive a **few**. At the same time some treated job seekers will be recommended to establishments hiring **far away** in the occupational space while others will be recommended to establishments hiring **close to** their own occupation. Similarly, while some establishments will be recommended to large pool of job seekers *conditional* on their level of predicted hiring some other establishments will only be recommended to few job seekers. And while some establishments will be recommended to occupationally close-by job seekers, others will be recommended to job seekers far away in the occupational space. We sum up the structure of our experimental design and the distribution of the different treatment status for job seekers and establishments in Table [1](#).

Table 1: TREATMENT ARMS AND RECOMMENDATIONS TYPES

Job-seekers				Establishments			
Treated		Control		Treated		Control	
	<i>Few</i>	<i>Many</i>		<i>Few</i>	<i>Many</i>		
<i>Close</i>	201,589	201,812	403,422	<i>Close</i>	9,716	9,614	59,556
<i>Far</i>	201,525	201,511		<i>Far</i>	9,792	9,688	

III.1.3 Drawing pairwise recommendations

Given each agent's treatment status how do we form the specific job seeker/establishment pairwise recommendations that will be used in our intervention? In practice job seekers who were assigned the **few** status received 4 recommendations while job seekers who were assigned the **many** status received 8. Knowing how many recommendations should be received by each job seeker we need to move to the other side of the market and distribute these recommendations among all treated establishments. We solve this potentially complex problem through an algorithm designed to allocate pairwise recommendations optimally. The inputs of this algorithm are the number of establishments that should be recommended to each job seeker. This number

is fixed at the individual level by each job seeker's treatment status. Our allocation algorithm then fills these recommendations with particular treated establishments so as to (a) equalize the number of recommendations per predicted hiring among establishments and (b) minimize the occupational distance of recommendations. While accomplishing this task our algorithm is constrained by each agent's non-random occupational location and each agent's random treatment status.

In the end, on both sides of the market, each agent's treatment status determines how many recommendations he will receive and how far these recommendations will be in the occupational space. Hence, while our pairwise recommendations partly reflect the non-random empirical distribution of job seekers and predicted vacancies across the occupational space, they also incorporate a random component linked to each agent's specific treatment status which will allow us to identify the effect of the number of recommendations and their occupational distance.

III.1.4 Drawing pairwise recommendations

In practice, our experiment consisted in emailing treated job seekers with links to LBB's contact information of specific establishments. Job seekers interested in the establishment that we recommended could use this information to contact the firm and make an unsolicited application. Importantly this contact information usually consisted of a location, an email or a telephone number. When no contact information is available for a given establishment LBB allows its user to directly search for this information on Google. What's more, in some cases LBB allows job seekers visiting its pages to directly send an application through public employment services' online application tool. When this tool was available, and as can be seen in Figure [A3](#) in appendix, job seekers just needed to click on a "Send an application" (in French "Postuler") icon which appeared on the right hand side of the contact information page.

As can be seen in Table [2](#) below or Figure [A4](#) in appendix, the emails we used to direct job seekers to specific establishments contained the following information: the job seeker's name, general information on the hiring behavior of firms - and in particular on the fact that a considerable share of hirings stem from unsolicited applications -, general information on LBB, each job seekers desired occupation, at most two links to the LBB page of recommended establishments and, finally, a general purpose link directing toward LBB's search engine. Apart from the job seeker's name and search occupation the only specifically individual content of these emails were the links to the contact information of recommended firms. Importantly these links were job seeker/establishment specific so that by tracking job seekers' clicks we could record their interest in some specific establishment. How were this links formed and dispatched into different emails? As previously explained we drew within the pool of nearby treated establishments as

many establishments, i.e. either 4 or 8, as each job seeker’s treatment status required. Once these 4 or 8 recommendations had been drawn for each job seeker we distributed them respectively into either 2 or 4 different emails. Each email thus contained at most two links directing to the contact information of at most two distinct establishments. When a single establishment ended up appearing twice in a single email we collapsed the two links into one single link. Finally we distinguished between establishments hiring in a job seeker’s own occupation and establishments hiring in another occupation by explicitly acknowledging one of the two cases when introducing each link. Establishments hiring in one’s own occupation were introduced as such while establishments hiring in a neighboring occupation were framed as "hiring in an occupation not far from yours". After the specific links to recommended establishments’ contact information, the email concluded with a general purpose link directing to LBB’s search engine. The content of our emails is summed up in Table 2 below.

Table 2: AN EMAIL’S SCHEMATIC CONTENT

Dear Mr./Mrs. [X],

You are currently registered with the public employment services and are looking for a job as a [X’s occupation].

Did you know that 7 out of 10 firms take into consideration unsolicited applications before actually posting a job-offer?

"La Bonne Boîte", an online platform linked to public employment services, has selected for you a few firms which might be interested in your profile.

Here is one that is likely to be interested in [your profile/a profile close to yours]:

- [Link to recommended establishment 1]

And another one that is likely to be interested in [your profile/a profile close to yours]:

- [Link to recommended establishment 2, if any]

You can send them your application.

By clicking on [this link/these links] you will be able to contact [this firm/these firms] thanks to the coordinates that will appear or by using PES’ online application tool if it is available.

You may also search for other firms on LBB’s website [general purpose link]

Yours sincerely,

III.2 Randomization in practice

III.2.1 Job seekers

On the job seeker side, we exploit exhaustive administrative data from the PES. It includes detailed information on the past and current unemployment spells as well as the socio-demographic

characteristics (gender, age, level of education, qualification, desired occupation, experience in the desired occupation, etc.) of all registered unemployed job seekers. This data source will also provide the main outcome of interest: exit from unemployment (date and type of contract) obtained through previous employment declarations filled by the employer ("DPAE").

We use this data set to recover the list of job seekers who were unemployed in the selected Commuting Zones during the month prior to the start of the experiment.⁷ After dropping all job seekers whose desired occupation is missing (274, 662), all job seekers for whom we were unable to get a valid email address (198, 510) and all job seekers listed as currently unavailable for active work (609, 547), we obtain a final sample of 1,209, 859 active and registered unemployed job seekers. In our sample, 47% are male, 61% hold at least one diploma, the average age is 37.7, the average work experience 6.6 years and the average unemployment spell at the time of the experiment is of 21 months⁸

We proceed to the random selection of treated job seekers within our 94 treated commuting zones in the following way. On the job seekers' side treatment status assignment probability is 2/3 within strata jointly formed by commuting zones, desired occupation and an above median/below median measure of the predicted exit rate out of unemployment.⁹ We select an unbalanced 2/3 treatment assignment probability in order to leave room for the four distinct treatment arms which will receive different types of recommendations. At the upper treated/control level we end up with 403, 422 job seekers in the control group and 806, 437 job seekers in the treatment group. The balance of job seekers' observable variables across treatment and control groups is presented in Table 3. Furthermore this table presents the p-values associated to an F-Test of the regressions of each observable on four indicator variables corresponding to the four job seekers' treatment arms. Note that our ex-post measure of job-finding indicates that about 34% of ini-

⁷While our experiment started on the 19/11/2019 we could only access administrative data which had been updated with an accurate unemployment status on the 30/09/2019. While proceeding with the design and randomization of our experiment we were left in the dark about the actual employment outcome of job seekers between the 30th of September and the 19th of November.

⁸The reason behind the high average unemployment duration is the fact that the experiment concerns the stock of unemployed and not the flow. Long term unemployed are thus present in our sample and drive this average upwards. While the averages is of 21 months the median is only of around 13 months.

⁹We aim at measuring the effect of our intervention on the job finding rate of job seekers. Therefore, we stratify on important predictors of job seekers' job finding rate in order to improve the statistical power of our analysis. Commuting zones and job seekers' occupation are such important predictors, hence our choice to stratify on these features. Then, we predict the exit rate out of unemployment within six month for each job seeker trough a simple LPM on job seekers' observables (gender, age, level of education, qualification etc.) in an historic version of our administrative data set which encompasses the job finding history of all registered unemployed job seekers between 2016 and 2018. We use the predictions of this model in our sample as a synthetic index on which we stratify further. This allows us to reduce the number of stratification variables while still improving the balance between control an treatment group (and consequently statistical power).

tially registered job seekers found a job prior to the start of our experiment. This pre-treatment attrition rate appears to be well balanced across treatment and control groups.

Table 3: BALANCE TABLE FOR JOB SEEKERS IN TREATED CZ.

Variable	(1) C	(2)	(3) T	(4)	(5) T-C	(6)	(7) F-test
Male	0.474	(0.499)	0.475	(0.499)	0.000	(0.001)	0.69
Age	37.684	(11.972)	37.720	(11.962)	0.036	(0.023)	0.95
Diploma	0.615	(0.487)	0.615	(0.487)	-0.000	(0.001)	0.63
Experience (y)	6.630	(7.915)	6.633	(7.915)	0.003	(0.015)	0.25
Unemployment spell (m)	21.359	(25.926)	21.399	(25.917)	0.041	(0.050)	1.02
Predicted exit rate	0.213	(0.071)	0.213	(0.071)	0.000	(0.000)	0.69
Predicted tightness	0.397	(0.657)	0.397	(0.658)	0.000	(0.001)	1.04
Present at treatment	0.661	(0.473)	0.662	(0.473)	0.000	(0.001)	0.84
Observations	403,422		806,437		1,209,859		1,209,859

Note: Standard errors are displayed in parentheses. Column (7) presents the F-Test p-values for the regressions the variable listed in the first column on four indicator variables corresponding to the four job seekers' treatment arms.

III.2.2 Establishments

On the establishment side, we use LBB's data which includes the number of predicted hirings per occupation and establishment, an indicator of the fact that the firm is identified as a "hiring firm", its size and its location (Zip Code).

As the foremost purpose of our experiment is to evaluate LBB's effectiveness as a job-finding tool we decide to keep only firms that are predicted to hire above the "hiring firm" threshold in at least one occupation. Finally, since LBB maps establishment level hiring predictions into establishment/occupation ones, we choose, within our sample of hiring establishments, to keep all occupations with positive predicted hirings regardless of whether or not these establishment/occupation specific hirings are above LBB's "hiring firm" threshold. All in all, our sample of establishments/occupations predicted hirings consists of all occupations with positive predicted hirings within establishment which have at least one occupation above the "hiring firm" threshold. We obtain a final sample of 98,366 hiring firms.

Given this sample of hiring establishments we begin by randomly dividing commuting zones into two distinct groups with different treatment assignment probabilities. In the first group establishments will have a 20% chance of being drawn for treatment. In the second group this probability is 60%. We decide to work with such heterogeneous treatment probabilities in order

to create commuting zones where establishments will be exposed to a more or less intensive treatment. Indeed establishments from commuting zones with a 20% treatment rate will on average be recommended to three times as many job seekers as establishments from commuting zones with a 60% treatment rate. Given these commuting zone specific treatment probabilities for establishments we proceed to draw treated establishments within each commuting zones and strata formed by establishment's 5-digits sector as well as an above median/below median measure of predicted hirings.¹⁰ Consistent with the fact that the average treatment probability across commuting zones is 40% we end up with 59,556 establishments in the control group and 38,810 establishments in the treatment group. As it was the case for job seekers, treated establishments will also be distributed into four different treatment arms. The balance of establishments' observables across treatment and control groups is presented in Table 4. Our sample appears to be balanced for all firms observable characteristics: number of hirings predicted by LBB, email availability, establishment level tightness as predicted by LBB,¹¹ hirings realized during the semester prior to the start of our experiment and whether the firm had job offers posted at the PES or not. This balance test however assumes that the relationships between treatment status and pre-determined variables are linear. When we allow for a non-parametric relationship between initial hirings and treatment status we find slight but potentially important unbalances given initial hirings' explanatory power on our main outcomes of interest. Our baseline establishment level results hence control for quantiles of initial hirings as explained in subsection IV.2.2 and appendix A.8.

¹⁰We aim at measuring the effect of our intervention on firm's hiring decisions. Therefore, we choose to stratify on important predictors of hiring in order improve our statistical power. Detailed (5-digits) sectors happen to be an important predictor of hiring and seasonality of it, hence our first stratification choice. Then, we take advantage of LBB's prediction of future hiring, a natural candidate for stratification that summarizes the predictive power of other observable firms' characteristics for hiring.

¹¹We define occupation*CZ predicted tightness measures as the number of predicted hirings over the number of registered job seekers. We use these occupation*CZ predicted tightness measures to build an establishment level predicted tightness measure which we compute as the average of predicted tightness measures over an establishment occupational structure.

Table 4: BALANCE TABLE FOR ESTABLISHMENTS IN TREATED CZ.

Variable	(1) C	(2)	(3) T	(4)	(5) T-C	(6)	(7) F-test
Predicted hirings	4.909	(0.065)	4.856	(0.073)	-0.053	(0.098)	0.772
Contact email available on LBB	0.476	(0.002)	0.471	(0.002)	-0.004	(0.003)	0.630
Predicted tightness	0.538	(0.002)	0.538	(0.002)	-0.001	(0.004)	0.999
Initial hirings (all)	36.104	(2.129)	32.693	(2.068)	-3.410	(2.969)	0.342
Initial hirings (indefinite)	3.862	(0.057)	3.770	(0.092)	-0.092	(0.108)	0.759
Initial hirings (definite)	32.242	(2.125)	28.923	(2.062)	-3.319	(2.961)	0.331
Posted offer at PES	0.492	(0.002)	0.494	(0.002)	0.002	(0.003)	0.177
Observations	59556		38810		98366		98366

Note: Standard errors are displayed in parentheses. Regressions are weighted by inverse treatment status probability. Column (7) presents the F-Test p-values for the regressions the variable listed in the first column on four indicator variables corresponding to the four establishments' treatment arms.

III.2.3 Treatment

The actual experiment took place in between November 19th 2019 and December 4th 2019. During this period we sent more than 2,400,000 emails to the pool of treated job seekers. These emails were sent in four different batches and contained all the job seeker/establishments pairwise recommendations formed according to each agent's treatment status. We give below descriptive statistics on the precise quantitative and qualitative nature of these recommendations.

As can be seen in Table 5, on average job seekers belonging to the "Few" treatment arm received recommendations to 3.19 distinct establishments while job seekers belonging to the "Many" treatment arm, received recommendations to 5.62 distinct establishments. In both the "Few" and "Many" treatment arms, the relative occupational distance of these recommendations varied according to each job seeker's "Close" or "Far" treatment status. Whereas job seekers bound to receive "Close" recommendations were kept at a 0.55 average distance, job seekers in the "Far" treatment arm were set recommendations on average 1.28 occupations away from their original search occupation.

Table 5: JOB SEEKERS' REALIZED TREATMENT

Variable	Group	Mean	Sd	Min	Max	Obs
Distinct rec.	Few	3.19	1.07	1	4	399821
	Many	5.62	2.34	1	8	399938
Occupational dist.	Close	0.55	1.19	0	15	400504
	Far	1.28	1.56	0	15	399705

Note: This table gives descriptive statistics for the number of distinct recommended firms in the "Few" versus "Many" job seekers' treatment arms as well as the average occupational distance of job seekers' recommended establishments in the the "Close" versus "Far" treatment arms.

On the establishments' side the same treatment arm pattern can be read from Table 6. In the case of establishments, however, the relevant statistic for the "Few"/"Many" treatment arms is the number of distinct job seekers *per-predicted* hiring (as explained earlier we allowed the number of recommendations by establishment to vary conditional on an establishment's predicted hirings). Establishments belonging to the "Many" treatment arm were recommended to twice as many job seekers per-predicted hiring when compared to the establishments belonging to the "Few" treatment arm (63.9 versus 27.8). Finally, establishments belonging to the "Far" treatment arm were on average recommended job seekers farther away in the occupational space than establishments belonging to the "Close" treatment arm (0.64 versus 0.09).

Table 6: ESTABLISHMENTS' REALIZED TREATMENT

Variable	Group	Mean	Sd	Min	Max	Obs
Rec./pred. hiring	Few	27.8	41.4	0.03	1295	18742
	Many	63.9	93.5	0.02	2277	18725
Occupational dist.	Close	0.09	0.15	0	3.12	18633
	Far	0.64	0.72	0	10.5	18834

Note: This table gives descriptive statistics for the number of distinct recommended job seekers per predicted hirings in the "Few" versus "Many" establishments' treatment arms as well as the average occupational distance of establishments' recommended job seekers in the "Close" versus "Far" treatment arms.

IV Results

In this section we present our preliminary results on the response of treated job seekers and establishments. We restrict our descriptive statistics and analysis to job seekers who were still

unemployed when our experiment began (19/11/2019).¹² This means that we exclude from our computations every job seekers who either exited PES' registers and/or took up a job before 19/11/2019. We do not allow job seekers exiting our sample prior to the start of the experiment because of short term contracts to re-enter it when their contract is (presumably) terminated. As could be seen in Table 3 above, the pre-intervention attrition rate is 34% and not significantly different in the treatment and control groups.

IV.1 Job seekers

IV.1.1 Take-up

Table 7 presents our main take-up measures on the job seekers' side. These measures are (1) opening at least one email and (2) clicking on at least one link. While some emails were lost due to invalid email addresses a vast majority of job seekers received at least one email (96%). Overall 64% of job seekers opened at least one email and 25% clicked on at least one link. Conditional on clicking on at least one link job seekers clicked on average 2.98 times on 1.95 distinct links.

Table 7: TAKE-UP MEASURES

	mean	sd	count
Received email	0.96	0.19	533557
Opened email	0.64	0.48	533557
Click	0.25	0.43	533557
Click if opened email	0.36	0.48	340777
Total clicks if click	2.98	3.02	130810
Distinct clicks if click	1.95	1.09	130810
Application if click	0.28	0.45	7423

Sample restricted to the set of 533,695 job seekers who were still unemployed in the treatment group as of 19/11/2019. The "Application if click" variable is only defined for job seekers who clicked on at least one link while being logged in their PES' online account.

Whereas we could perfectly track the reception/opening of emails as well as each job seeker's clicks on our recommendation links we could only keep track of job seekers' subsequent applications if these applications were made through PES' online application tool. Online applications were only possible for a subset of establishments and job seekers. In particular, job seekers had to be connected to PES' online services in their browser before or just after clicking on the link in order to be able to use PES' online application tool. We could therefore measure applications

¹²Because of delay with which job-finding is observed in administrative data we were not able to exclude job seekers finding a job between 30/09/2019 and 19/11/2019 prior to our randomization. As shown in 3 we do not detect any significant unbalance in our treatment/control groups with respect to this particular dimension.

conditional on click only for a small subset of about 7,500 job seekers. For this subset 28% of clicking job seekers followed through with an online application to a recommended establishment. Taking this application rate at face-value and knowing that there were about 130,000 clicking job seekers still unemployed at the time when our experiment began, we could infer that our intervention generated about 35,000 applications. On the establishment side, given that there were about 39,000 treated establishments, this would amount to a bit less than 1 application per treated establishment. Of course this measure stems from the application rate of a highly selected set of workers.¹³ What's more, assuming that different application tools (online, personal email, mail, phone calls) are substitutes, this would be an upper bound for the applications our intervention generated.

IV.1.2 Reduced form results

Overall reduced form results

In this section we present our baseline reduced form results on the job seekers' side. Our main dependent variable is access to employment as registered by PES, over a period of four months since treatment. More specifically we know each job seeker's return to employment status, type of contract, the date at which this contract is set to start and, for definite duration contracts, the date at which this contract will be terminated.

The main equation we estimate is the following:

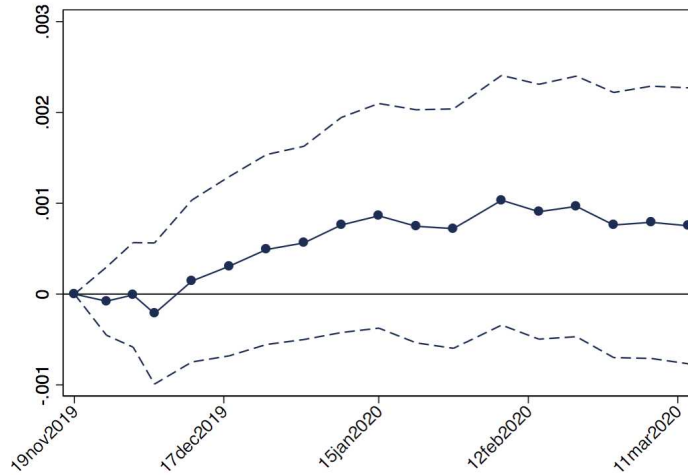
$$y_{ic} = \alpha_1 + \beta_1 Z_{ic} + \epsilon_{ic} \quad (1)$$

Where index c corresponds to the commuting zone of job-seeker i . The dependent variable of interest y_{ic} corresponds to the job finding status of individual i at a given point in time, and the type of contract found (finite or indefinite duration). Z_{ic} is a dummy equal to 1 if the job-seeker received an email. We compare treated and control individuals from treated commuting zones, β is therefore our "intention to treat" estimate from the job-seeker's side.

Figure 1 presents this baseline intention to treat regression at different time horizons pooling together all types of contract. Each point depicts the result of a separate regression of access to employment before some date on our intention to treat status for the set of job seekers who were still unemployed when our intervention began. Going from left to right, the time horizon widens so that the overall graph depicts the cumulative effect of our treatment on job-finding. Despite this cumulative effect being positive and increasing over time, it remains small, less than 0.1% compared to the mean 14% employment rate at the end of our time window. What's more this is not statistically different from zero at the 5% confidence level.

¹³Among treated workers who clicked the particular set of workers which were connected to PES' online application service while clicking on our links were 18.7% more likely to find a job within three months.

Figure 1: JOB-FINDING ITT ESTIMATES

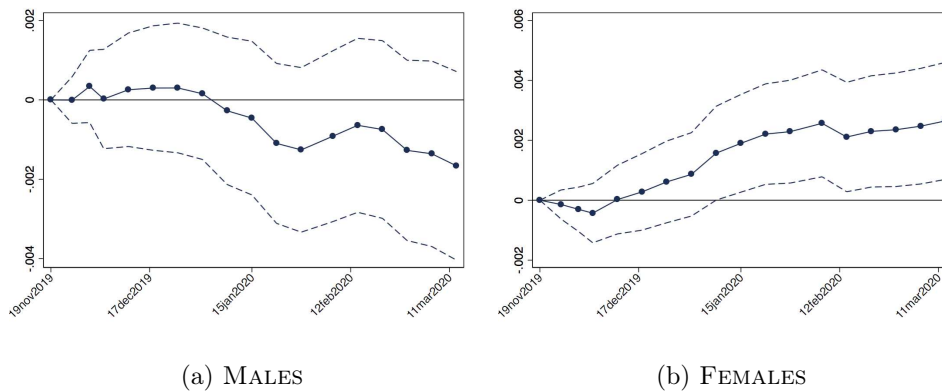


Note: This graph presents the ITT estimates for job finding at different time horizons. Sample restricted to job seekers who were still unemployed as of 19/11/2019. Standard errors are clustered at the labor market (Occ.*CZ) level and associated 95% confidence intervals are displayed.

Gender differences in job seekers' responses

Hidden under the general picture given by Figure 1, the respective responses of males and females to our intervention differ markedly. As can be seen in Figure 2 which depicts the counterpart of Figure 1 for both genders taken separately, whereas the overall response of men is zero, women's response after two months since the beginning of our intervention is positive and significant.

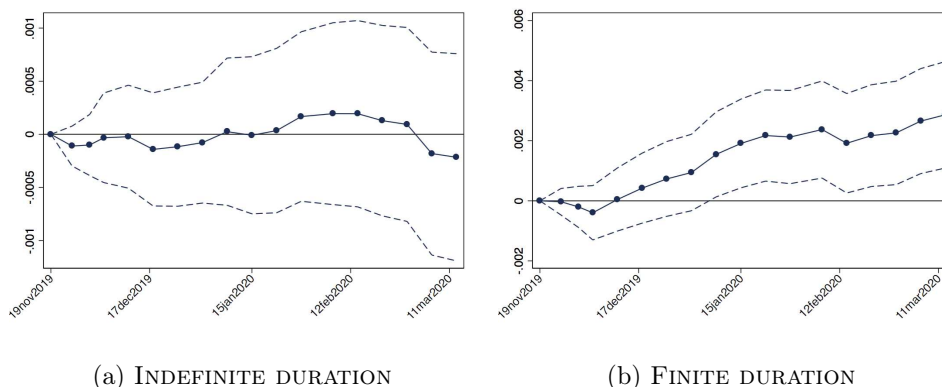
Figure 2: JOB-FINDING ITT ESTIMATES BY GENDER



Note: ITT estimates for job finding at different time horizons for (a) males and (b) females. Sample restricted to job seekers who were still unemployed as of 19/11/2019. Standard errors are clustered at the labor market (Occ.*CZ) level and associated 95% confidence intervals are displayed.

Further decomposing women’s response into access to indefinite as opposed to definite duration employment (Figure 3), we find that the positive effect of our intervention is driven by a rise in treated women’s return to definite duration employment.¹⁴

Figure 3: JOB-FINDING ITT ESTIMATES BY CONTRACT TYPE FOR FEMALES



Note: ITT estimates for job finding of (a) indefinite duration and (b) finite duration contracts at different time horizons. Sample restricted to female job seekers who were still unemployed as of 19/11/2019. Standard errors are clustered at the labor market (Occ.*CZ) level and associated 95% confidence intervals are displayed.

Women’s and men’s responses to tailored job-search advice appear to be strikingly different. Could this difference be driven unbalances in the gender distribution across observables and labor markets? In other words, are women reacting more to our treatment because they differ in some observable way from men or because they work in occupations that tend to respond more strongly to the provision of tailored job-search advice. To check this, we interact our intention-to-treat status with a male/female dummy and control for the interaction of our treatment with a set of observables, including a full set of labor market fixed effects. We present the results of these robustness checks for definite duration hirings in Table 8. The different response of men and women stays remarkably robust for all the interacted controls and interacted labor market fixed effects we include, indicating that the gender differences in the response to our provision of tailored job search recommendations do not appear to be driven either by individual level observables being correlated to gender differences or by labor market differences.

¹⁴A further decomposition between "long term" (i.e. more than six months) definite duration contracts and short term (i.e. less than six months) definite duration contracts shows that this effect is driven by short term definite duration contracts.

Table 8: ITT ESTIMATES ON JOB FINDING ON FINITE DURATION CONTRACTS BY GENDER

	(1)	(2)	(3)
Male # ITT	-0.0420 (0.135)	-0.0367 (0.135)	-0.221 (0.149)
Female # ITT	0.287 (0.108)	0.309 (0.110)	0.257 (0.130)
Controls	No	Yes	Yes
Labor Market FE	No	No	Yes
Observations	800297	800237	793103
Mean	0.154	0.154	0.154
Adjusted R2	0.00201	0.0203	0.109

Note: This table displays the results of a regression of finite duration job-finding on the interactions of our treatment with a dummy for males and a dummy for females. Column (1) does not add any control, column (2) controls for the direct and interacted effects of the centered value of age, a diploma dummy, experience and unemployment spell duration. Finally column (3) adds the direct and interacted effect of centered labor market (Occ.*CZ) fixed effects calculated through a first stage regression. Sample restricted to job seekers who were still unemployed as of 19/11/2019. Standard errors in parentheses are clustered at the labor market (Occ.*CZ) level. Coefficients and standard errors in percentage points.

IV.1.3 Potential mechanisms underlying gender differences

Differences in take-up

To investigate which potential mechanisms underlie the gender differences we find in job seekers' responses to our intervention we try to follow gender differences along the causal chain that eventually links our intervention to the hiring of a job seeker. This causal chain starts with opening of emails, then goes on with clicking on links, applying to firms, being called for an interview, receiving an offer, accepting it. We start from the beginning by first looking at gender differences in initial take-up measures. To do so we regress our main take-up measures, opening at least one email and clicking on at least one link, on a male/female dummy. Table 9 shows that men are 6% less likely to open the emails we sent them. This big difference in take-up passes through to subsequent clicks and remains large when we include detailed individual level controls as well as labor market fixed effects. The fact that women are 25% more likely than men to click on the recommendation link we sent them cannot, however, fully account for the gender differential we see on final outcomes. The initial variation in take-up must hence be complemented by other differences involving latter stages of the hiring process. Unfortunately

we were not able to track applications and interviews of all treated and control job seekers. One possibility could for instance be that men and women react differently to suggestions to widen the occupational breadth of their job-search effort — we investigate this possibility in the following subsection exploiting our web survey.

Table 9: GENDER DIFFERENCES IN TAKE-UP (in percentage points)

	Opened email			Clicked on link		
	(1)	(2)	(3)	(4)	(5)	(6)
Male	-6.733	-6.645	-3.982	-5.957	-5.796	-3.458
	(0.294)	(0.250)	(0.189)	(0.258)	(0.253)	(0.174)
Controls		Yes	Yes		Yes	Yes
Fixed effects			Yes			Yes
N	533557	533557	525702	533557	533557	525702
Mean	0.639	0.639	0.639	0.245	0.245	0.245

Standard errors in parentheses

Note: Regression of (1,2,3) opening at least one email and (4,5,6) clicking on at least one link on male female dummy. We add individual level controls in columns (3,4,5,6) as well as labor market fixed effects in columns (3,6). Sample restricted to treated job seekers who were still unemployed as of 19/11/2019. Standard errors are clustered at the labor market (Occ.*CZ) level. Coefficients and standard errors in percentage points.

Evidence from survey data on intermediary outcomes

To get some insights on job seekers' reactions to the emailing campaign, we ran a short web survey on a sample of 11,741 job-seekers, 2/3 of which are treated job-seekers. In order to increase the chances for treated job-seekers to respond to our survey, we over-represented among this population, job-seekers that had clicked on at least one link of the intervention email. More precisely, among treated individuals we surveyed, 80% clicked on at least one link while only 20% did not.

The exact questions asked during the survey and the comparative statistics of not-surveyed, surveyed and respondent job-seekers can be found respectively in Tables [A4](#) and [A5](#) in the appendix.

Outcomes are measured about two months after the emails were sent. Table [10](#) displays intention-to-treat effects, pooling the different job seekers' treatment arms together, but distinguishing women from men. Panel A shows limited reactions of job seekers to the emails: the only statistically significant effect is an increased usage of the LBB platform, in similar proportions for men and women (5-6 percentage points, equivalent to a 25% increase). Other search activities do not seem to be affected: the use of Internet and the number of types of Internet website used (in

a list of five), the number of responses to job ads, the number of spontaneous applications, the probability to apply outside of one's preferred occupation, and the overall time dedicated to job search are not significantly impacted. The only exception is the decrease in the probability that male job seekers apply for jobs outside of their preferred occupation (a 10 percentage point, or 20% decrease), which contrasts with a small, non significant increase for women. The difference between the two effects is statistically significant (p-value=0.02), suggesting that men and women used LBB differently, with men substituting applications they would have made outside of their preferred occupation with applications to firms predicted to hire in their preferred occupation.

Panel B of Table 10 shows the impact of the emailing on interviews and job offers. While the sample size does not allow to detect the small effect on job finding rates among women shown by the administrative data, it is reassuring that the two sources yield similar rates in the control group (about 15%). More importantly, the survey complements the administrative data with information on interviews. As a result of the treatment, men witness a decrease in the number of job interviews (p-value=0.05) while women witness a non-significant increase. The difference in impact is marginally significant (p-value=0.06). Taken together, the results of the two panels suggest that treated men and women increased their use of the LBB platforms, but in different directions: while men used it to focus their search on their preferred occupations, women kept searching outside of their preferred occupation as before. This helped them close the gap with men in terms of job interviews. In turn, this may explain the small positive impact of the emailing on women's exit toward finite duration contracts shown by the administrative data.

The survey results must however be taken with caution. As shown in Panel C of Table 10 and as is common with such web surveys, response rates to the job seekers' survey are low (around 25%) so that results may not be representative of the population. The different lines of the table also show the progressive erosion of the sample as respondents move from one question to the next, with a rapid decrease of the number of observations across outcomes.¹⁵ In addition, response rates are unbalanced between treatment and control for women: treated women are significantly less likely to respond to the survey (-6 percentage points, compared to 31% for control women). Such differential attrition may bias the estimates. Appendix Table A6 uses the bounding methods proposed by Lee (2009) and Behaghel et al. (2015) to correct for possible sample selectivity bias.¹⁶ Overall, the bounding approaches provide evidence that the results of

¹⁵The lines of the table follow the survey order, with the exception of the number of hours searched, which came as the last question.

¹⁶Lee bounds trim the sample of control women using worst-case and best-case scenarios; the width of the identified set is proportional to the share of "marginal respondents", i.e. those that respond when they are not treated but would not have responded otherwise. Behaghel et al. (2015) provide tighter bound by making use of information on the number of survey rounds needed to get the job seekers to respond: as shown in Appendix Figure A8, four rounds of survey were sufficient to reach the same response rates among control women as among other groups. Under a monotonicity assumption, Behaghel et al. (2015) show that those "early responders" are comparable to the responders in the other three groups.

Table 10 are not driven by sample selectivity. In particular, the confidence intervals obtained following Behaghel et al. (2015) are quite close to those obtained by ignoring non-response. The difference in occupational search between men and women found in Table 10 is therefore a possible explanation for the differential effect of the intervention on job finding rates by gender.

Table 10: IMPACT OF EMAILING ON INTERMEDIARY OUTCOMES

	Women			Men			p-value different ITT				
	Mean control	ITT	(se)	p-value	N	Mean control		ITT	(se)	p-value	N
A. Job search											
Used Internet for job search	0.85	0.01	(0.02)	0.57	2447	0.87	0.01	(0.03)	0.67	1583	0.94
# Internet search channels used	2.43	-0.01	(0.09)	0.95	2447	2.53	0.04	(0.11)	0.74	1583	0.76
Used LBB	0.20	0.06	(0.02)	0.02	2421	0.21	0.05	(0.03)	0.12	1570	0.88
Responded to job ads	0.53	-0.01	(0.03)	0.69	2374	0.55	-0.02	(0.04)	0.63	1546	0.92
# job ads responded	4.35	-0.31	(0.57)	0.59	2313	5.40	-0.40	(0.87)	0.65	1498	0.93
Made spontaneous application	0.49	-0.01	(0.03)	0.65	2280	0.54	0.00	(0.04)	0.97	1468	0.80
# spontaneous applications	3.54	0.33	(0.55)	0.55	2256	4.23	-0.09	(0.73)	0.90	1449	0.64
Applied in other occupation	0.48	0.02	(0.03)	0.54	2213	0.52	-0.10	(0.04)	0.02	1412	0.02
# hours searched per week	7.92	-0.04	(0.75)	0.96	2057	9.28	0.27	(0.98)	0.78	1327	0.80
B. Interviews and job offers											
Called for interview	0.34	0.03	(0.03)	0.40	2202	0.42	-0.06	(0.04)	0.10	1403	0.07
# calls for interviews	0.80	0.08	(0.13)	0.53	2191	1.15	-0.30	(0.15)	0.05	1397	0.06
Received offer	0.24	0.00	(0.03)	0.99	2188	0.22	0.02	(0.04)	0.55	1394	0.64
Accepted offer	0.15	0.01	(0.02)	0.62	2077	0.14	0.03	(0.03)	0.34	1334	0.65
C. Response to survey											
Responded to survey	0.31	-0.06	(0.01)	0.00	6587	0.22	0.01	(0.02)	0.40	5154	0.00

Note: Each line displays results from separate estimations for men and women.

Source: Survey see [A4](#)

Treatment arms and gender comparisons

In our attempt to understand the origin of the gender differential we see on final outcomes, we also investigated potential differences in the reaction of males and females to the different treatment arms. The results are presented in Table [II](#). Among males (Panel A in Table [II](#)), none of the four variations of the treatment are found to have any significant treatment effect. However, the picture is quite different for females (Panel B), as two treatment arms ("Few/Close" and "Many/Far") stand out as the main drivers of the differential treatment effect observed between males and females on the return to employment in definite duration contracts.

The efficiency of the "Many/Far" treatment arm seems in line with the results obtained in the analysis of the survey, as this treatment is the one with the strongest encouragement to broaden the job search. The fact that it turns out to be one of the treatment arms with the largest gaps in treatment effect between males and females suggests once again that, when encouraged to broaden their job search, females were more responsive than males and this translated in a larger access to employment through definite duration contracts. The effect for the "Few/Close" treatment on women is even stronger, but is more difficult to explain on the basis of the survey analysis. However, one should probably not over-interpret the differences in treatment effects between this arm and the others, as most pairwise differences are not statistically significant given the large confidence intervals.^{[17](#)}

¹⁷One might still wonder why the "Few/Close" treatment arm would perform better than the "Many/Close" one, as this difference is one of the few that is statistically significant and is puzzling. A tentative explanation is that when we were increasing the number of recommendations made, the average quality of those recommendations was decreasing — in the sense that we were more likely to recommend firms recruiting farther away from the initial occupation stated by job seekers. Given that the recommendations were then sent in a random order — as opposed to some sorting by quality — it might be that treated individuals in the "Many/Close" arm were disappointed by the recommendations we made in the first e-mails, and stopped paying attention to our next e-mails.

Table 11: IMPACT OF TREATMENT ARMS ON EMPLOYMENT, BY GENDER

A - MALES				
	(1)	(2)	(3)	(4)
	Few/Close	Many/Close	Few/Far	Many/Far
All contracts	0.0693 (0.206)	-0.160 (0.205)	-0.276 (0.207)	-0.291 (0.202)
Indefinite duration	-0.00895 (0.109)	-0.105 (0.105)	-0.108 (0.106)	-0.272 (0.103)
Definite duration	0.0783 (0.189)	-0.0543 (0.186)	-0.167 (0.192)	-0.0193 (0.187)
Observations	179549	179793	179743	179485

B - FEMALES				
	(1)	(2)	(3)	(4)
	Few/Close	Many/Close	Few/Far	Many/Far
All contracts	0.526 (0.166)	0.0382 (0.169)	0.188 (0.170)	0.239 (0.170)
Indefinite duration	0.00571 (0.0834)	0.110 (0.0853)	-0.147 (0.0822)	-0.0619 (0.0829)
Definite duration	0.520 (0.153)	-0.0716 (0.153)	0.335 (0.156)	0.301 (0.154)
Observations	219576	219632	219215	219725

Note: This table reports treatment arms specific ITT estimates of different job finding outcomes for (A) males and (B) females separately. Each reported coefficient stands for a separate regression of one of the three possible outcomes (all contracts hirings, indefinite duration hirings, definite durations hirings) restricting the treated group to each one of the four treatment arms successively. Sample restricted to job seekers who were still unemployed as of 19/11/2019. Standard errors are clustered at the labor market (Occ.*CZ) level. Coefficients and standard errors in percentage points.

The differential effects we find on job-finding, treatment take-up and search behavior across genders echo the results found by [Arni et al. \(2021\)](#). In their paper they show that women react to an information intervention that encourages job search via social contact by substituting their effort towards more social search, increasing their search efficiency and consequently finding employment faster and in more stable jobs. Therefore it appears that women react more and in a more constructive way to job-search information interventions than men.

Furthermore the result on women's occupational search patterns complements the geographical

findings of [Le Barbanchon et al. \(2019\)](#). Women’s broader occupational search may be linked to the tighter geographical constraint they face in their job search strategies.

IV.2 Establishments

In this section we present our main reduced form results on the establishments’ side. Unlike job seekers whose treatment we could fully monitor, establishments’ ex-post treatment was partly determined by treated job seekers application behavior. We first start by describing in more details establishment’s ex-post treatment and then go on to present our reduced form results.

IV.2.1 Ex-post treatment

Recall that treated establishments were affected in two ways by our intervention. On the one hand, as we virtually erased control establishments from LBB’s search results during a whole month after the start of the experiment, treated establishments were mechanically affected by an increased exposure in LBB’s search results. This first aspect of our intervention possibly resulted in an increased number of applications stemming both from treated and control job seekers, who would be using LBB’s search engine independently from the experiment, as well as job seekers outside our sample (non-registered job-seekers). On the other hand, unlike control establishments, treated establishments were specifically recommended by email to treated job seekers. This second aspect of establishments’ treatment possibly resulted in an increased number of applications stemming specifically from treated job seekers.

Fortunately we were able to measure the relative strength of both aspects of establishments’ treatment by keeping track of (1) the overall number of clicks on each establishment’s contact information in LBB’s general search results and of (2) the overall number of clicks on our specific recommendation links. We sum up this information in [Figure A7](#) and [Table 12](#). [Figure A7](#) shows the distribution of clicks per establishments generated by our recommendations links. On average our specific recommendation links resulted in establishments’ contact information being clicked on 13.8 times by 9.1 distinct job seekers. Assuming the subsequent application rate to be around 0.27 (see [Section IV.1.1](#)) and given that on average job seekers clicked on the recommendation links of 2 distinct establishments this would result in a bit more than one application per treated establishment.¹⁸

How does the number of clicks stemming from our recommendation links compare to the overall increase of treated establishments’ exposure in LBB’s search results? To answer this question we look at the number of clicks per establishment that are not originating directly from one of our links. [Table 12](#) compares this overall number of regular clicks per establishment in the treated

¹⁸Note that our data on clicks on the firm side includes both job seekers who were still unemployed as of 19/11/2019 and job seekers who left our sample of interest before that, hence overestimating the number of effective clicks by 38%.

and control groups in (1) the month before our experiment began, (2) the month during which our experiment took place and (3) the two months after our experiment ended. We see that while there was no significant difference between our treatment and control groups in the pre-intervention period, the overall number of clicks on treated establishments was more than twice as large as their control counterpart during our intervention. Further, this difference disappears in the two months following the end of our intervention.

Tables [A2](#) and [A3](#) in the appendix show these same results separately according to the treatment assignment of Commuting Zones. As expected, we observe that in commuting zones with a 20% firms treatment rate the increase is larger. During the intervention, treated firms have 133% more clicks than their control counterparts while in commuting zones with a 60% treatment rate this number decreases to 97% . Pulling together clicks stemming from recommendations and clicks stemming from treated establishments' increased exposure, our experiment generated on average 15.6 more clicks per treated establishment, 89% of which stemmed directly from our recommendation links.

Table 12: OVERALL NUMBER OF CLICKS BY ESTABLISHMENTS

	(1)	(2)	(3)
	Pre intervention	During intervention	Post intervention
ITT	0.0171 (0.0734)	1.802 (0.0702)	0.0526 (0.0408)
Constant	3.600 (0.0806)	1.563 (0.0375)	1.700 (0.0411)
N	98366	98366	98366
Mean	3.608	2.469	1.726

Note: ITT of the overall number of clicks by establishments for (1) the pre-intervention period, (2) while the intervention is going on and (3) in the month following the end of our intervention. This excludes the clicks on the links provided in our email campaign. Regressions are weighted by inverse treatment status probability. Standard errors are clustered at the labor market (Sector*CZ) level.

IV.2.2 Reduced form results

We now present our main reduced form results on the establishments' side. Symmetrically to job-seekers, the PES data allows to recover the main outcomes of interest on the establishment side. More specifically we are able to access not only hiring declarations related to registered job seekers but the universe of hiring declarations by French establishments ("DPAE") over our period of interest. For each hiring declaration we know: the hiring establishment's fiscal identifier (SIRET), the starting date of the contract, the type of contract and whether or not the hired

employee was registered as a job-seeker at the PES. Thanks to this rich set of information we are able to measure establishment level hirings over time for different types of contracts (definite duration and indefinite duration) and different types of job-seekers (registered and non-registered at the PES). Finally, because unlike registered job-seekers, hiring declarations of non-registered job seekers are not associated to an individual identifier¹⁹, we are not able to distinguish hirings of different non-registered job seekers from the re-hiring of a single non-registered job seeker. For consistency reasons our count of hirings per establishment will hence include all hiring declarations even when they can be traced back to a single registered individual — in other words job seekers that are hired twice in our time window will be counted as two different hires in our establishment level hiring data.

Because of the small but significant imbalances between treatment and control firms with respect to pre-treatment establishments' hirings demonstrated in Table A8, all the results we present include control dummies for hirings that occurred between May 1st, 2019, the earliest date for which we obtained data on individual level hirings, and November 19th, 2019, the beginning of our intervention. Controlling for this covariate is crucial for the estimation of firm-level treatment effects as past hirings have a large predictive power on current levels of hirings.²⁰ Therefore, (i) slight imbalances on the allocation of treatment status with respect to past hirings can lead to confounding when estimating average treatment effects, and (ii) including past hirings as a control reduces the residual variance left unexplained in our regression models, therefore improving our statistical power (which we view as a side benefit). Since past hirings level is a continuous covariate, we used a data-driven way of creating bins of past hirings levels (using regression trees), and use the corresponding dummies as controls. We describe the construction of these bins in Appendix A.8. As recommended by Athey and Imbens (2016) we include in our baseline specification centered bins of pre-intervention hirings as well as their interactions with treatment.

More precisely we estimate:

$$N_{jc} = \alpha_2 + \beta_2 T_{jc} + \gamma \theta_{jc} + \kappa(\theta_{jc} * T_{jc}) + \omega_{jc} \quad (2)$$

Where index c corresponds to the commuting zone of establishment j . The dependent variable of interest N_{jc} corresponds to the total number of hires of establishment j at a given point in time, for different types of contracts (definite duration and indefinite duration) and different types of job-seekers (registered and non-registered at the PES). θ_{jc} includes a set of dummies: a dummy for each centered bin of past hirings levels. T_{jc} is a dummy equal to 1 if the establishment is treated: put in priority on LBB's web-platform and recommended by email to treated job seekers. β_2 is therefore our "intention to treat" estimate at the establishment level. As shown

¹⁹All non-registered job seekers are identified by the same individual identifier.

²⁰When regressing post-intervention levels of hirings on bins of pre-intervention ones (for all, indefinite and definite contracts), we find an (adjusted) R^2 of (respectively) 0.828, 0.527 and 0.831.

by [Athey and Imbens \(2016\)](#), this specification is equivalent to running split sample regressions on each bin and aggregating back to the average treatment effect using each bin’s sample share.

Keeping in mind that the upper bound for the number of recommendation-related applications received by treated establishments is low, we do not expect to see huge effects on establishment level hirings. Indeed, [Table 13](#) shows that the intention-to-treat estimate (equation(2)) pooling all types of contracts together is small and not significantly different from zero (Column 1).

When we consider indefinite duration contracts and definite duration contracts separately (Columns 2 and 3), however, the picture is quite different. [Table 13](#) shows that while definite duration contracts hirings are not affected by our intervention, we pick up a positive and significant effect on indefinite duration hirings. This effect is small, close to 0.06, but not negligible as it amounts to a 3.2% increase over establishments’ mean hirings of indefinite duration contracts (1.9 in our sample). Notice that we reach similar conclusions when estimating these effects through a doubly robust estimation strategy, using random forest as implemented by the R package `grf` ([section A.10](#) presents these results).

Table 13: ESTABLISHMENTS’ ITT ESTIMATES FOR TOTAL HIRINGS BY CONTRACT TYPE

	(1)	(2)	(3)
	All	Indefinite	Definite
ITT	-0.0199 (0.556)	0.0616 (0.0356)	-0.00507 (0.546)
N	98366	98366	98366
Mean	18.35	1.923	16.43
Adjusted R2	0.829	0.530	0.833

Standard errors in parentheses

Note: This table presents the ITT for different types of hirings since 19/11/2019 controlling for quantiles of pre-19/11/2019 hirings. Regressions are weighted by inverse treatment status probability. Standard errors are clustered at the labor market (Sector*CZ) level.

Finally, to investigate the effect of treatment intensity we look at our ITT on indefinite duration hirings separately in commuting zones where the share of treated firms was respectively 20% and 60%. We find that the average effect on indefinite duration hirings appears to be driven by commuting zones where we concentrated treatment on a lower share of treated firms: the intention to treat estimates are 0.0761 and 0.0493 respectively. These split-sample point estimates, however, are not significantly different from each other (see [table A9](#) in appendix).

IV.3 Heterogeneity Results

Heterogeneity according to the previous relationship between firms and the PES

Results so far have shown that increasing the pool of candidates that send unsolicited applications to firms translates into an increase of hires on permanent contracts. This result might be expected to vary according to whether or not firms had a pre-existing relationship with public employment services. If the PES holds relevant information on firms' needs and expectations or if firms already used the PES to advertise their vacancies, we would expect the effect of LBB to be reduced. In fact firms that are used to work with the PES are more likely to receive candidates through the formal channel: candidates that apply either directly through a posted vacancy or following the recommendation of their referral counselor.

In order to explore this potential heterogeneity dimension we distinguish firms which used PES to post at least one job add in the two years preceding our experiment from firms which did not do so. This definition splits our sample in two roughly equally sized sub-samples of firms. Table 14 shows our ITT estimates on hirings on permanent contracts according to whether the firms posted a job add in the past (column 2) or not (column 3). We find that job creations linked to our intervention are concentrated among firms which did not previously post job adds using PES. The ITT point estimate for this sub-sample is significant at the 10% level and is twice as large as the estimate on firms already affiliated to PES.

Whereas Algan et al. (2018) show that a reduction in screening costs can lead to increased job creation by firms that already have a relationship with the PES, the intervention they implement aims at improving the matching efficiency of the "visible" market to which both firms and job seekers already have access. Complementary to these findings, we show that job-creation can also be fostered through the hidden market. LBB reduces hiring costs by directing job seekers' search efforts toward firms that do not usually post vacancies through PES and that are thus also less likely to be known to PES counselors.

Table 14: ESTABLISHMENTS' ITT ESTIMATES FOR HIRINGS ON INDEFINITE DURATION CONTRACTS BY THE PRE-EXISTING RELATIONSHIP RELATION WITH THE PES

	(1)	(2)	(3)
	All	Posted offer	No posted offer
ITT	0.0616	0.0483	0.0816
	(0.0356)	(0.0615)	(0.0447)
N	98366	48527	49839
Mean	1.923	2.566	1.297
Adjusted R^2	0.530	0.542	0.582

Standard errors in parentheses

Note: This table presents the ITT for hires on indefinite duration contracts according to whether a firm posted (column 2) or did not post (column 3) a job add at PES during the two years preceding our intervention. We control for quantiles of pre-19/11/2019 indefinite durations hirings. Regressions are weighted by inverse treatment status probability. Standard errors are clustered at the labor market (Sector*CZ) level.

Heterogeneity according to firm size

As hiring channels and practices are likely to vary with firm size, our intervention may have affected small and larger firms differently. On the one hand one could think that smaller firms facing more matching frictions than larger firms, being less visible and having access to a smaller social network of current and former workers may respond more strongly to a sudden increase in the flow of applications they receive. On the other hand, it is possible that large firms receive more applications, because unemployed workers anticipate that the odds of a recruitment is larger in large firms (and LBB does provide firm size information as soon as on the first page of results). If this expectation is correct, receiving CVs will indeed fasten large firms' recruitment decisions. Splitting our sample between firms with less or more than 10 employees, Table [15](#) shows that our treatment is driven by the latter group of relatively large firms. The effect is rather large, as it represents a +5% increase in hirings. If those firms received one more CV on average under treatment, it would imply that one CV out of 20 is successful; but large firms likely receive more CVs than average.

Table 15: ESTABLISHMENTS' ITT ESTIMATES FOR INDEFINITE DURATION
HIRINGS BY ESTABLISHMENT SIZE

	(1)	(2)	(3)
	All	Small	Large
ITT	0.0616 (0.0356)	0.00541 (0.0312)	0.167 (0.0854)
N	98366	60387	37979
Mean	1.923	1.030	3.353
Adjusted R^2	0.530	0.462	0.562

Standard errors in parentheses

Note: This table presents the ITT for hires on permanent contracts according to firm's size. Small firms are defined as firms having less than 10 employees, large firms as firms having more than 10 employees. We control for quantiles of pre-19/11/2019 indefinite durations hirings. Regressions are weighted by inverse treatment status probability. Standard errors are clustered at the labor market (Sector*CZ) level.

IV.4 Pulling together results from the two sides of the market

At first sight our results on firms hires may appear to contradict our initial estimates for job seekers which showed a zero effect on indefinite duration hirings and a positive effect on definite duration hirings. A plausible explanation for this surprising finding is twofold. On the one hand, the fact we do not see a surge in definite duration hirings on the establishments' side must hence mean that part of the increase in definite duration hirings of female job seekers was offset by the displacement of some control job seekers. On the other hand, the fact that we see an increase in indefinite duration hirings on the establishments' side but none on the job seekers' may be linked to the twofold nature of our treatment on the establishments' side. In this section we test these two hypothesis.

Crowding out effect on job-seeker's side

First, we want to test if the absence of effect on female definite duration hirings on the establishments' side is due to a displacement effect from treated to control job-seeker's (Crépon et al., 2013).

To do so we exploit the fact that we randomly treated only 1/4 of France's commuting zones and left 3/4 of them untreated. We use these super-control commuting zones in order to compare those who were assigned to control in an area in which some were treated on the one hand, and all those who were in areas where no one was treated on the other hand.

More specifically we run:

$$y_{ic} = \alpha_3 + \beta_3 Z_{ic} P_c + \delta_3 P_c + v_{ic} \quad (3)$$

Where P_c is a dummy for being in any treated commuting zone. In this specification, β_3 is the difference between those assigned to treatment and those who are in treatment zones but are not themselves assigned to treatment and δ_3 is the effect of being untreated in a treated commuting zone compared to being untreated in an untreated zone. δ_3 is thus the estimate that informs us about the existence of crowding-out effects.

Table 16 runs this regression only for females, with y_{ic} corresponding to job finding on definite duration contracts. We observe that δ_3 is not significant and thus we cannot conclude that non treated female job-seekers in treated commuting zones have less chances to find a definite duration contract than their counterparts in non-treated commuting zones. Moreover the confidence interval at 95% of our crowding out effect does not include -0.258 ([-0.125;0.815]). Therefore, even if we might lack statistical power to detect potential crowding out, it seems unlikely that a displacement sufficiently large to compensate our main effect is taking place.

Table 16: JOB-SEEKERS' CROWDING OUT EFFECT FOR FEMALE IN DEFINITE DURATION CONTRACTS

	(1)
	Definite Duration Contract
Treated (β)	0.258 (0.108)
In a Treated Commuting Zone (δ)	0.345 (0.240)
Constant	0.135 (0.00112)
N	1978410

Note: This table presents the estimates of equation (3) for definite duration hirings. Sample restricted to female job seekers who were still unemployed as of 19/11/2019. Standard errors are clustered at the labor market (Occ.*CZ) level (clustering only at the CZ level does not change the significance of our estimates). Coefficients and standard errors in percentage points.

The effect on firms driven by the increased exposure on the website and not by the emails.

Second, we want to test if the absence of effect on indefinite duration hirings on job-seekers' side is linked to the twofold nature of our treatment. Indeed, it is perfectly possible that the increase in establishments' hirings of indefinite duration contracts was entirely driven by treated establishments' increased exposure in LBB's general search results and not by our pairwise job seeker/establishment recommendations. If this were the case, the indefinite duration hirings caused by our intervention should be almost equally distributed across treated and control job

seekers thereby explaining the zero ITT effect on indefinite duration hirings on the job seekers' side²¹.

We indirectly test this hypothesis by looking at establishments' indefinite duration hirings intention-to-treat estimate according to the type of individuals hired. If our pairwise recommendations had played a significant role in establishments' hirings on indefinite duration contracts we would expect to see an effect only among treated job-seekers.

Table 17 shows this decomposition. It displays the hires of the designated Treated (column 1) and Control (column 2) job-seekers; the sum of both which corresponds to the hires of Present job-seekers (column 3); the hires of Not Present Job-seekers (column 4) who are job-seekers that registered between a month and a half before and the beginning of the experiment but that were not part of the mailing campaign (because the data in hand was not updated enough at the time of the experiment); the sum of the hires of all job-seekers (column 5); the hires of individuals not registered as job-seekers (column 6) and the sum of all the hires for the overall effect (column 7).

Most importantly, we observe that hires of Treated and Control job-seekers contribute equally to our main effect. The effects on hires of both types of individuals are of the same magnitude. This result is consistent with the zero effect on indefinite duration hirings on the job seekers' side.

Similarly the effect on hires of Not Present job-seekers, although not statistically significant, is close in magnitude to the effect on hires of Present job-seekers.

This results confirm that the effect we find on indefinite duration hirings is driven by treated establishments' increased exposure to LBB's regular users rather than from our recommendation links.

Additionally, we observe that our effect on indefinite duration contracts is mostly driven by hires of registered Job-seekers (rather than not Job-seekers) who are the target of LBB.

²¹Because of our intervention treated job seeker's are more likely to use LBB than control job seekers (see Table 10). This difference however, does not seem strong enough for treated job seekers to be hired more in indefinite duration contracts than control job seekers (see Figure 3).

Table 17: ESTABLISHMENTS' ITT ESTIMATES FOR INDEFINITE DURATION
HIRINGS ACCORDING TO THE TYPE OF INDIVIDUAL HIRED

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treated JS	Control JS	Present JS	Not Present JS	JS	Not JS	All
ITT	0.0106 (0.00386)	0.0114 (0.00646)	0.0220 (0.00829)	0.0275 (0.0215)	0.0495 (0.0253)	0.0121 (0.0206)	0.0616 (0.0356)
N	98366	98366	98366	98366	98366	98366	98366
Mean	0.102	0.187	0.289	0.928	1.217	0.707	1.923
Adjusted R2	0.119	0.167	0.207	0.405	0.427	0.421	0.530

Standard errors in parentheses

Note: This table presents the ITT for hires on permanent contracts according to the type of individual hired. We control for quantiles of pre-19/11/2019 indefinite durations hirings. Regressions are weighted by inverse treatment status probability. Standard errors are clustered at the labor market (Sector*CZ) level.

V Conclusion

Building upon an existing service developed by the French public employment service, this paper has provided experimental evidence on the employment effects of a machine learning algorithm harnessed by an Internet platform to reduce informational frictions. These effects are local and small. First, women seem to be more responsive to the recommendations pushed by emails, and see a small increase in job finding rates (limited to definite duration contracts). Second, establishments put forward on the website marginally increase their hirings (into indefinite duration contracts). The fact that the effect on women in definite duration contracts is only found on the job seekers side suggests that treated women crowd out control ones (or control/treated men). A similar caveat applies to the effect on hirings in indefinite duration contracts: it may still be the case that treated establishments crowd out control ones.

Importantly, our experimental treatment on the job seekers' side is only incremental: the LBB platform has been in place for more than five years, and 20% of control job seekers visit it on the business-as-usual operating mode (over two months of observation). The experiment increases that share to 25% in the treatment group, and the results show that the local average treatment effect of the emailing campaign on the 5% of compliers is limited. Our experiment does not identify the effect on the 20% of "always takers" who may well have self-selected to use the platform because they need the information on hiring firms most, and therefore have larger effects. However, a previous, rough evaluation of LBB detected similarly small effects on 6-month job finding rates, at a time (end of 2015) when baseline usage of the platform was quite low, so that the compliers in this early evaluation resembled today's always takers.

Given the limited effect on job seekers, one might be surprised to detect any effect on firms. Note however that the experiment on the establishment side makes a stronger difference than on the worker side: a subset of firms is systematically advertised on the LBB website during four weeks (for treated and control job seekers) and by emails sent in four waves during two weeks (for treated job seekers). The fact that this advertising increases hiring rates provides unique evidence that matching frictions play a role in limiting labor demand, as standard unemployment equilibrium models posit. Yet, this role appears quantitatively limited.

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A Appendix

A.1 Context

Figure A1: LBB'S HOME PAGE



Figure A2: LBB'S RESEARCH RESULTS PAGE

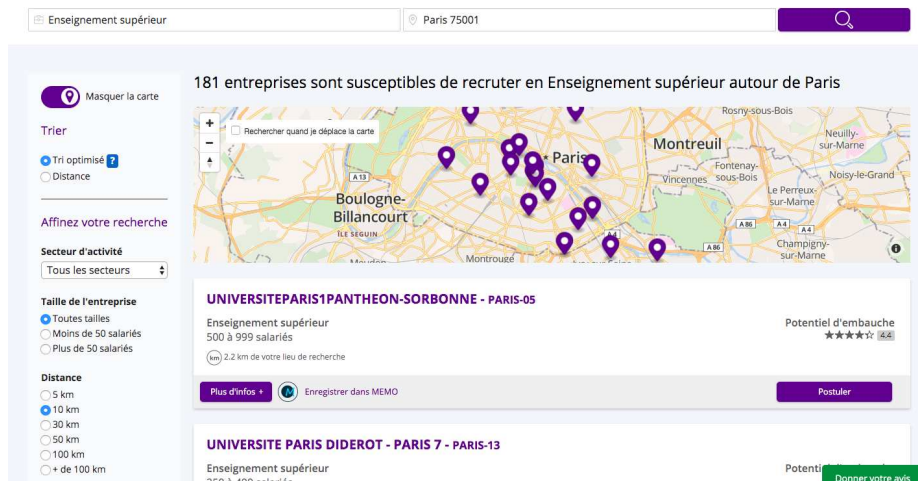


Figure A3: LBB's FIRM CONTACT INFORMATION PAGE

UNIVERSITEPARIS1PANTHEON-SORBONNE - PARIS-05

Enseignement supérieur
500 à 999 salariés
2.2 km de votre lieu de recherche

Potentiel d'embauche
★★★★☆

Plus d'infos Enregistrer dans MEMO Postuler

Raison sociale
UNIVERSITE PARIS 1 PANTHEON
SORBONNE

Contact
racbiatss@univ-paris1.fr
[0144077918](tel:0144077918)

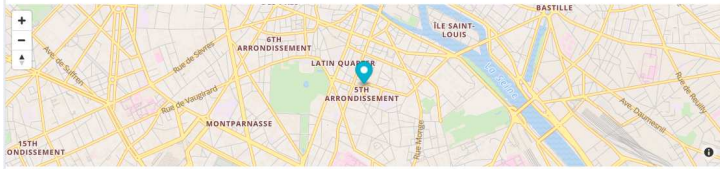
Mode de contact à privilégier
Envoyer un CV et une lettre de motivation

Informations supplémentaires
[Google](#)
[Kompass](#)
SIRET : 19751717000019

Enseigne
UNIVERSITEPARIS1PANTHEON-SORBONNE

C'est mon entreprise!
Modifier ces informations

Adresse
Service des ressources humaines
12 PLACE DU PANTHEON
75005 PARIS-05



ormations-entreprise/19751717000019 Télécharger la fiche en PDF Donner votre avis

Figure A4: EMAIL SENT TO TREATED JOB SEEKERS

Bonjour M. Zuber,

Vous êtes inscrit à Pôle emploi et avez déclaré rechercher un emploi dans la catégorie : « Sommelierie ».

Savez-vous que 7 entreprises sur 10 examinent des candidatures spontanées avant de se décider à publier une offre d'emploi ?

La Bonne Boite, un service de Pôle emploi, a repéré des entreprises que votre profil pourrait intéresser.

En voici une susceptible de rechercher un profil proche du vôtre :

- [GSF MERCURE](#)

Vous pouvez leur envoyer une candidature spontanée.

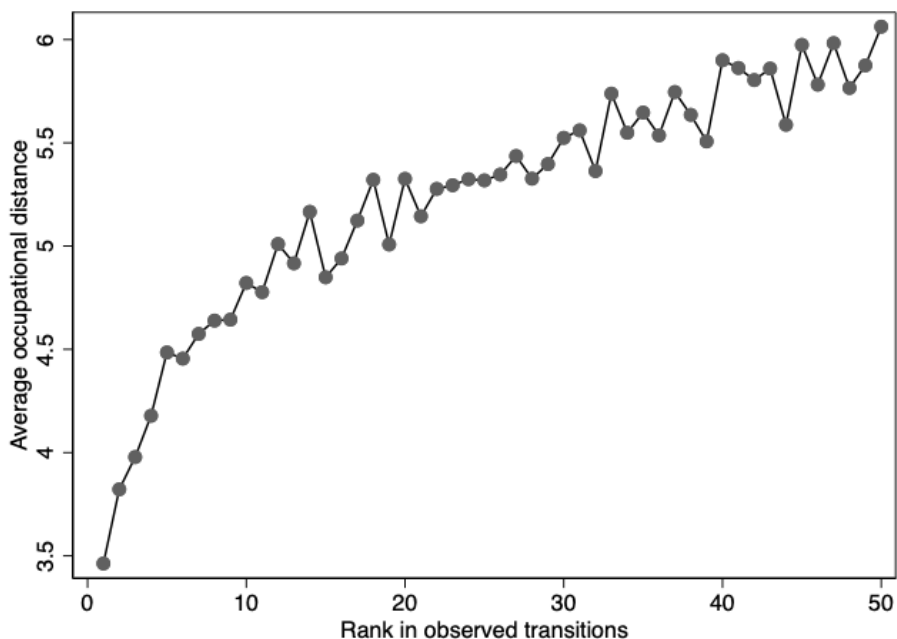
En cliquant sur ce lien, vous pourrez contacter l'entreprise grâce aux coordonnées qui s'affichent ou en utilisant l'outil de candidature en ligne « **postuler** » lorsque celui-ci est disponible.

Vous avez également la possibilité de retrouver d'autres entreprises sur le site [La Bonne Boite](#)

En vous souhaitant une pleine réussite dans votre recherche d'emploi.

A.2 Occupational distance and observed transitions

Figure A5: MEAN OCCUPATION DISTANCE VS OBSERVED RANK IN OCCUPATIONAL TRANSITIONS



Note: This graph constructed by ranking occupational transitions according to their frequency within each origin occupation and then computing the mean occupational distance of these transition in each rank category. In other words, across all origin occupations, destination occupation ranked first in terms of transitions were located at an average occupational distance of 3.5. Data on occupational transitions are constructed from the FHDADS panel covering the 2008-2012 period. We are constrained to this rather short period because prior to 2008 the DADS did not record a 4-digit occupation. An occupational transition from A to B is defined as a job-seeker looking for a job in occupation A finding a job in occupation B. While the search occupation A is coded in the ROME classification, the destination occupation B is coded according to the PCS classification used in DADS files. We translate the PCS classification into the ROME one by using the ROME-FAP-PCS matching provided by the French unemployment agency as well as each ROME's distribution of educational attainments among job seekers observed in our pre-treatment data. In total this graph is constructed from 1,092,233 individual transitions over the 2008-2012 period

A.3 Commuting zones and local labor markets

A.3.1 Commuting Zones

For administrative purposes the PES divides the french territory into 404 commuting zones ("bassins d'emploi"). A commuting zone is a geographical space where most of the population lives and works. In other words, most people do not leave this area to go to their place of work. Both job seekers and firms are thus mapped to an specific commuting zone through their zip code. These areas have an average population of 160,000 and are spread over an average radius of 20.3km.²² Finally, and consistent with France's unemployment rate, there are on average 13,467 job seekers in each commuting zone.

For this experiment 94 commuting zones out of the 404 initial ones were selected. We leave the 310 remaining commuting zones untouched for a future experiment guided by the learnings of this one. Nevertheless this experiment remains a large-scale experiment with more than 1.2 million job seekers and 750 thousand firms involved. The 94 commuting zones of our interest are randomly selected from the pool of commuting zones. Table A1 shows the main characteristics of commuting zones selected for the experiment (column 1) and commuting zones not selected for the experiment (column 2). We observe that characteristics between those groups are balanced and therefore our sample is representative of the entire France.

Table A1: COMMUTING ZONES' STATISTICS

Variable	(1) Selected Zone	(2) Non Selected Zone	(3) (2)-(1)
Surface (m2)	182507.453 (423423.031)	150871.219 (200091.297)	-31636.240 (31,679.127)
Population	154650.000 (133044.750)	161688.672 (196349.313)	7,038.673 (21,628.875)
Number of Unemployed	12,870.830 (12,109.896)	13,648.951 (17,855.393)	778.122 (1,966.694)
Unemployment Ratio	0.079 (0.017)	0.081 (0.019)	0.002 (0.002)
Number of Hiring Firms	7,985.681 (9,362.619)	8,512.371 (15,645.074)	526.690 (1,699.878)
Tightness	0.623 (0.402)	0.585 (0.241)	-0.038 (0.034)
Observations	94	310	404

Standard errors in parenthesis.

²²We miss data for one commuting zone which regroups Saint-Martin and Saint-Barthélemy.

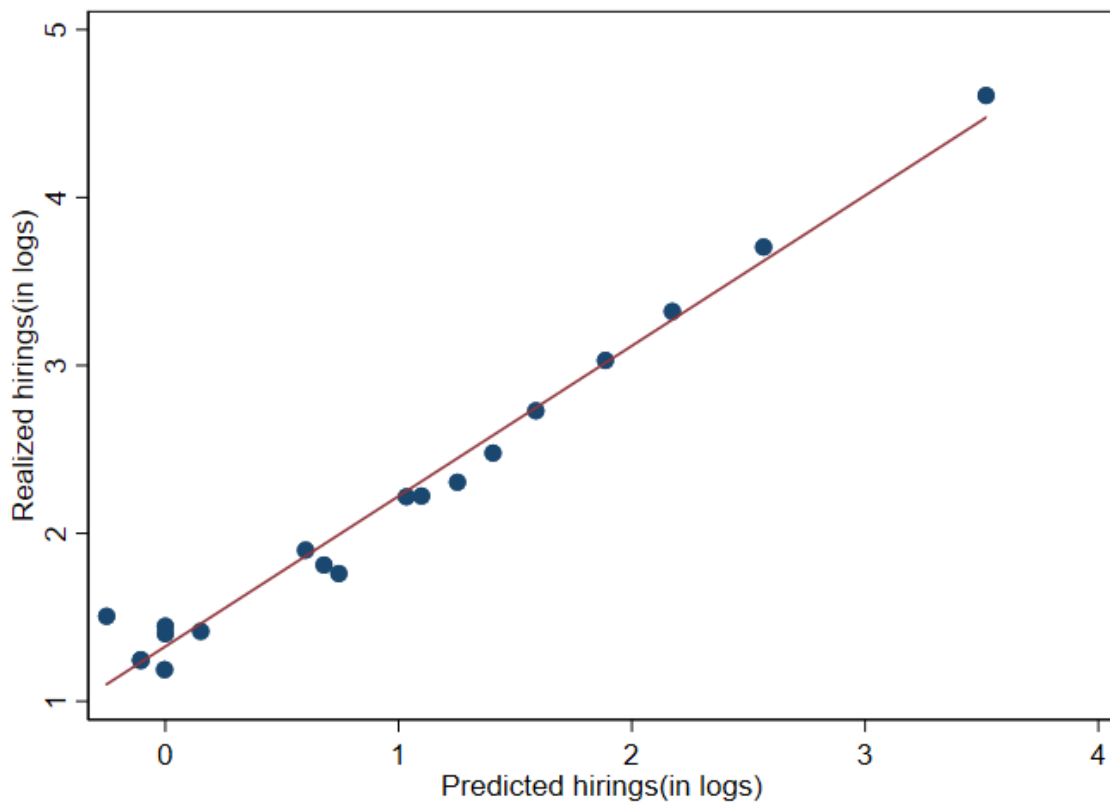
A.3.2 Local Labor Markets

Upon registering with public employment services, job seekers are asked to fill in a certain number of personal information including their desired occupation. As one's desired occupation is not, however, a required information we drop job seekers whose search occupation appears as missing in our data. Job seekers who choose to register a desired occupation can select one occupation from the 532 options given in the "ROME" classification of occupations used by french unemployment services²³). We define a local labor market as the intersection between commuting zones and occupations. In France there are 404 CZ and 532 occupations, which makes $404 \times 532 = 214928$ local labor markets. Among these potential labor market only 174733 turn up with a least one job seeker or one active establishment. On average a local labor market is populated by 31 job seekers and 19 establishments which total 12 predicted hirings. The mean predicted hirings to job seekers ratio is 0.31. This ratio can be thought of as the predicted tightness of our local labor markets.

²³ROME stands for "Répertoire opérationnel des métiers": Operational directory of occupations.

A.4 Correlating predicted and realized hirings

Figure A6: REALIZED HIRINGS AMONG UNEMPLOYED JOB SEEKERS OVER THE 30/09/2019-13/03/2020 PERIOD VS LBB'S PREDICTED HIRINGS AS OF 11/08/2019 (IN LOGS)

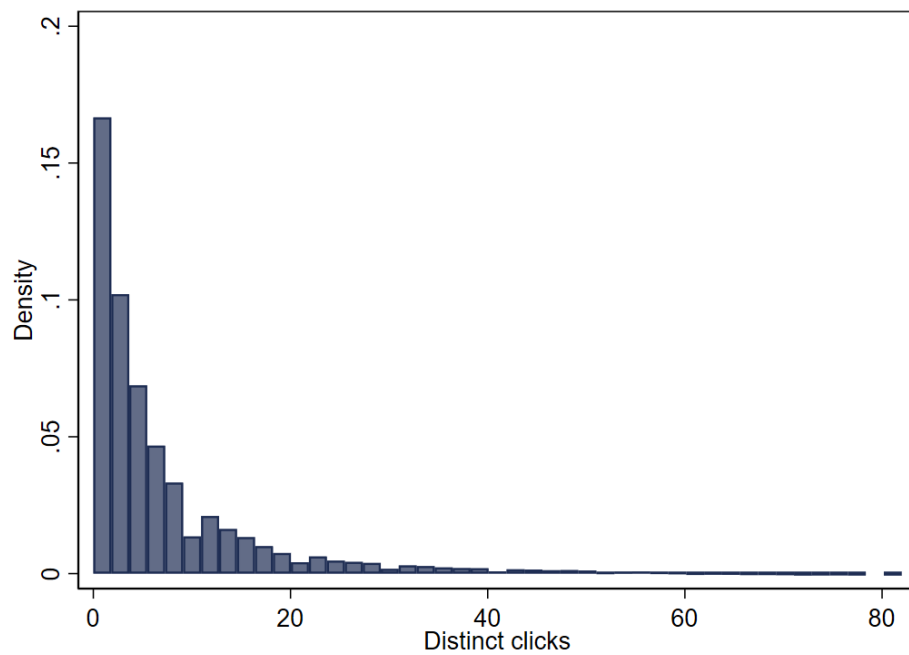


Note: Correlation of the number of predicted hirings per establishment and the number of realized hirings.

$$\text{LOG}(\text{REALIZED HIRINGS}) = 1.33(0.0053) + 0.89(0.0039) \times \text{LOG}(\text{PREDICTED HIRINGS}), R^2 = 0.37$$

A.5 Ex-post treatment

Figure A7: NUMBER OF DISTINCT CLICKS BY TREATED ESTABLISHMENT



Note: Distribution of the number of distinct clicks (one per job seeker) per establishment. The displayed distribution is cut above the 99th percentile. The average number of distinct clicks per establishment is 9.1

Table A2: OVERALL NUMBER OF CLICKS FOR ESTABLISHMENTS IN COMMUTING ZONES WHERE 60% OF FIRMS WERE TREATED

	(1)	(2)	(3)
	Pre intervention	During intervention	Post intervention
ITT	0.0124 (0.0908)	1.539 (0.0761)	0.0211 (0.0547)
Constant	3.912 (0.143)	1.590 (0.0635)	1.864 (0.0751)
N	47305	47305	47305
Mean	3.920	2.516	1.877
Adjusted R^2	-0.0000208	0.0100	-0.0000182

Note: ITT of the overall number of clicks for establishments in commuting zones with a 60% treatment rate during (1) the pre-intervention period, (2) while the intervention is going on and (3) in the month following the end of our intervention. Regressions are weighted by inverse treatment status probability. Standard errors are clustered at the labor market (Sector*CZ) level.

Table A3: OVERALL NUMBER OF CLICKS FOR ESTABLISHMENTS IN COMMUTING ZONES WHERE 20% OF FIRMS WERE TREATED

	(1)	(2)	(3)
	Pre intervention	During intervention	Post intervention
ITT	0.0221 (0.114)	2.044 (0.114)	0.0820 (0.0601)
Constant	3.311 (0.0849)	1.539 (0.0422)	1.548 (0.0399)
N	51061	51061	51061
Mean	3.315	1.951	1.565
Adjusted R^2	-0.0000185	0.0206	0.0000337

Standard errors in parentheses

Note: ITT of the overall number of clicks for establishments in commuting zones with a 20% treatment rate during (1) the pre-intervention period, (2) while the intervention is going on and (3) in the month following the end of our intervention. Regressions are weighted by inverse treatment status probability. Standard errors are clustered at the labor market (Sector*CZ) level.

A.6 Survey Design

Table A4: THE CONTENT OF THE SURVEY

For Everyone

[Q1] In the past four weeks, have you used the following Internet services for your job search?

- The PES website (including *emploistore* ²⁴)
- Temporary employment agency websites
- Sites specialized in job offers (monster, keljob, appec...)
- "Leboncoin.fr" website ²⁵
- Professional social networks (Viadeo, LinkedIn...)

[Possible answers: No / Less than 1 hour per week / 1 to 3 hours per week / More than 3 hours per week]

[Q2] In the last four weeks, have you used the "La Bonne Boîte" service from the PES?

[Possible answers: No / Yes]

[Q3] In the last four weeks, have you responded to any job offers?

[Possible answers: No / Yes]

[Q3b] <if yes> How many?

[Q4] In the last four weeks, have you made unsolicited applications?

[Possible answers: No / Yes]

[Q4b] <if yes> How many?

[Q5] <if at least one application (unsolicited or not: Q3=yes OR Q4=yes)> When you registered with the PES, you had declared that you were looking for a job in the category <occupation sought>. During the last four weeks, did you make any applications (unsolicited or not) for other types of jobs?

[Possible answers: No / Yes]

[Q6] In the past four weeks, have you had a job test or interview?

[Possible answers: No / Yes]

[Q6b] <if yes> How many?

[Q7] In the past four weeks, have you received a job offer?

[Possible answers: No / Yes]

[Q7b] <if yes> Did you accept it?

[Q8] On average over the past four weeks, how many hours per week did you spend on your job search? - open field

Only For Treated Job-Seekers

Over the past four weeks, the PES service "La Bonne Boîte" has sent you emails inviting you to apply to firms that may be recruiting your profile or profiles similar to yours. We would like to know if these recommendations were useful to you.

(Loop on recommended firms "i")

[Q9i] Did you contact the firm <XXX>?

[Possible answers: No / Yes]

[Q10i] <if Q9i="no"> Why didn't you contact them?

- The firm's activity did not correspond to the field I was looking for
- The firm was too far from my home
- I had other priorities
- I was no longer looking for a job
- I don't remember receiving this offer.
- Other

[Q10ib] <if Q10i="Other"> Specify - open field

[Q11i] <if Q9i="yes"> What action did the <XXX> firm take on your application?

- Invited me to a test or interview
- Said they were holding my application in reserve for possible future hires
- They declined my application
- I have not received a reply for the moment
- Other

[Q11ib] <if Q11i="Other"> Specify - open field

[Q12i] <if Q11i = test or interview> Have you received a job offer from the firm <XXX>?

[Possible answers: No / Yes/ Pending]

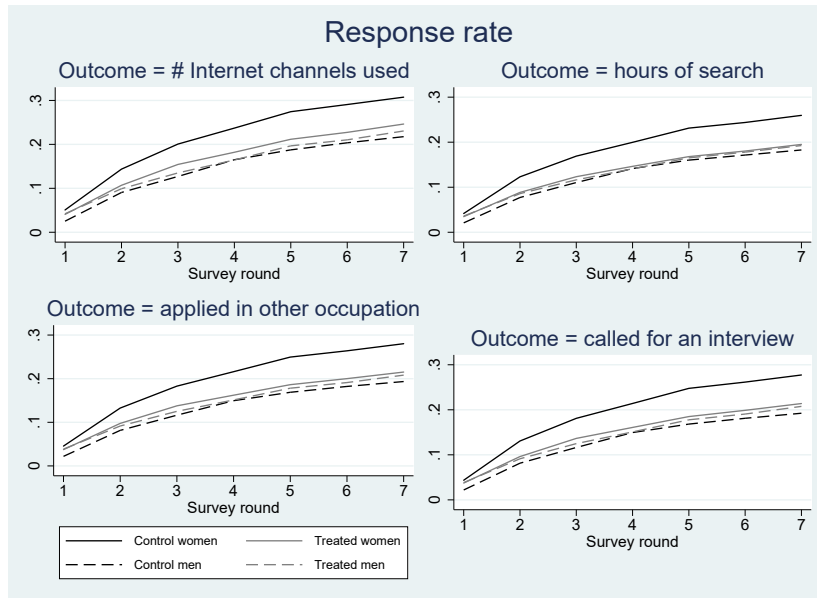
Table A5: COMPARISON OF OBSERVABLE CHARACTERISTICS BY JOB SEEKER'S SURVEY STATUS.

Variable	(1) Not Surveyed	(2)	(3) Surveyed	(4)	(5) (3)-(1)	(6) Responded	(7)	(8) (6)-(1)
Male	0.475	0.000	0.472	0.007	-0.003	0.429	0.013	-0.046***
Age	37.701	0.011	37.479	0.176	-0.222	40.085	0.304	2.384***
Diploma	0.615	0.000	0.612	0.007	-0.002	0.681	0.012	0.067***
Experience (y)	6.628	0.007	6.502	0.106	-0.126	7.714	0.214	1.086***
Unemployment spell (m)	21.386	0.024	21.216	0.355	-0.170	22.429	0.720	1.043
Predicted exit rate	0.213	0.000	0.213	0.001	-0.000	0.214	0.002	0.001
Predicted tightness	0.397	0.001	0.399	0.008	0.002	0.389	0.015	-0.007
Present at treatment	0.661	0.000	0.665	0.007	0.003	0.719	0.012	0.058***
Observations	1198118		11741		1209859	4191		1202309

Note: Standard errors are displayed in columns (2), (4) and (7). Significance at the 1%, 5% and 10% level, respectively, is indicated by ***, **, * in columns (5) and (8). Weights are included in order to take into account the over-sampling, among the treated, of individuals that clicked on at least one link in the intervention email.

A.7 Survey evidence on job seekers' response

Figure A8: RESPONSE RATE BY SURVEY ROUNDS



Note: Cumulative response rate at the end of the different survey rounds, by job seekers' gender and treatment status. Treated group pools job seekers receiving two and four emails.

Source: Survey of job seekers.

Table A6: IMPACT OF EMAILING ON INTERMEDIARY OUTCOMES: ROBUST-NESS TO DIFFERENTIAL NON-RESPONSE

	Women			Men		
	No correction	Call again	Lee	No correction	Call again	Lee
A. Job search						
Used Internet for job search	[-0.03 ; 0.06]	[-0.04 ; 0.03]	[-0.15 ; 0.08]	[-0.04 ; 0.06]	[-0.04 ; 0.05]	[-0.03 ; 0.16]
# Internet search channels used	[-0.19 ; 0.18]	[-0.31 ; 0.24]	[-0.73 ; 0.67]	[-0.19 ; 0.26]	[-0.24 ; 0.29]	[-0.39 ; 0.49]
Used LBB	[0.01 ; 0.11]	[0.01 ; 0.11]	[-0.03 ; 0.33]	[-0.01 ; 0.12]	[-0.01 ; 0.11]	[-0.08 ; 0.12]
Responded to job ads	[-0.08 ; 0.05]	[-0.09 ; 0.07]	[-0.21 ; 0.16]	[-0.09 ; 0.06]	[-0.10 ; 0.08]	[-0.12 ; 0.09]
# job ads responded	[-1.42 ; 0.81]	[-1.55 ; 1.38]	[-2.50 ; 3.60]	[-2.10 ; 1.31]	[-1.93 ; 1.59]	[-4.59 ; 1.29]
Made spontaneous application	[-0.08 ; 0.05]	[-0.10 ; 0.05]	[-0.22 ; 0.19]	[-0.08 ; 0.08]	[-0.11 ; 0.06]	[-0.11 ; 0.12]
Applied in other occupation	[-0.04 ; 0.08]	[-0.06 ; 0.08]	[-0.19 ; 0.25]	[-0.17 ; -0.02]	[-0.19 ; -0.01]	[-0.22 ; 0.01]
# hours searched per week	[-1.51 ; 1.43]	[-1.98 ; 1.80]	[-3.78 ; 5.78]	[-1.65 ; 2.20]	[-1.62 ; 2.63]	[-5.27 ; 2.47]
B. Interviews and job offers						
Called for interview	[-0.04 ; 0.09]	[-0.05 ; 0.10]	[-0.13 ; 0.30]	[-0.14 ; 0.01]	[-0.14 ; 0.04]	[-0.20 ; 0.03]
# calls for interviews	[-0.17 ; 0.34]	[-0.16 ; 0.36]	[-0.35 ; 0.89]	[-0.59 ; -0.01]	[-0.63 ; 0.03]	[-1.04 ; 0.04]
Received offer	[-0.05 ; 0.06]	[-0.10 ; 0.05]	[-0.13 ; 0.33]	[-0.05 ; 0.09]	[-0.04 ; 0.09]	[-0.12 ; 0.09]
Accepted offer	[-0.04 ; 0.06]	[-0.10 ; 0.05]	[-0.07 ; 0.18]	[-0.03 ; 0.09]	[-0.02 ; 0.08]	[-0.11 ; 0.08]

Note: 95% confidence intervals of the ITT are reported with three treatments of non-response: assuming ignorability (no correction); following [Belaghel et al. \(2015\)](#) (call again); following [Lee \(2009\)](#) (Lee).

Source: Survey of job seekers.

A.8 Imbalances in hirings levels prior to the intervention

In this appendix we describe how we construct the initial hirings bins that we use in our establishment level baseline specification as controls.

For each each dependent variable (total hirings, definite duration hirings, indefinite duration hirings, etc) we use a separate regression tree (using R package `rpart`) to predict post-intervention hirings as a function of pre-intervention hirings. While doing so we exclude treated firms from our sample. The relevant initial hirings thresholds are reported in Table [A7](#). In Table [A8](#), we show the regression of the firm treatment dummy on the pre-intervention hirings dummies corresponding to those bins. We do observe that there is imbalance at the top of the pre-hirings distribution (bins 6 and 7) for definite duration hirings, and, as a result, also for all hirings. To account for this, we control for this set of hiring dummies in firms regressions.

Table A7: PRE-INTERVENTION HIRING BINS THRESHOLDS

Bin thresholds	(1)	(2)	(3)	(4)	(5)	(6)
All hirings	6.47	258.1	892	2100	5540	10230
Definite duration	4.7	246.4	885.8	2107	5521	10230
Indefinite duration	0.82	6.24	22.81	74.76	284.8	
Indefinite duration (registered job seekers)	0.59	3.53	7.27	16.73	40.55	139.5
Indefinite duration (non registered job seekers)	0.31	3.28	10.24	21.07	45.68	133.6

Note: This table reports the initial hirings thresholds recovered from our regression trees for each dependent variable. The first bin for total hirings is defined as establishments which hired more than 0 and less than 6.47 workers prior to 19/11/2019, the second bin as establishments which hired more than 6.47 but less than 258.1 workers prior to 19/11/2019, etc.

Table A8: IMPACT (IMBALANCE) OF PRE-INTERVENTION LEVEL OF HIRINGS ON TREATMENT STATUS

	(1)	(2)	(3)
	All	Indefinite	Definite
Bin 2	-0.00716 (0.00381)	0.00562 (0.00393)	-0.00613 (0.00397)
Bin 3	-0.00463 (0.0161)	-0.00554 (0.00631)	0.00507 (0.0162)
Bin 4	0.00741 (0.0307)	-0.0265 (0.0118)	-0.00201 (0.0312)
Bin 5	-0.0258 (0.0484)	-0.0229 (0.0292)	-0.0321 (0.0487)
Bin 6	-0.270 (0.113)	0.114 (0.112)	-0.269 (0.113)
Bin 7	0.0368 (0.122)		0.0376 (0.122)
Constant	0.506 (0.00232)	0.501 (0.00289)	0.505 (0.00222)
N	98366	98366	98366
F	1.602	2.381	1.450
Adjusted R ²	0.0000539	0.000104	0.0000418

Note: Regressions of treatment status of establishments on bins of pre-intervention levels of hirings (all, indefinite, and definite contracts). Bins were created by a regression tree predicting the level of hirings post-intervention using pre-intervention levels, on the subsample of control establishments.

A.9 Results on firms' effects on indefinite duration contracts by treatment arm

Table A9: Establishments' ITT estimates for Indefinite Duration Contacts by treatment arm

	(1)	(2)	(3)
	All	20%	60%
ITT	0.0616 (0.0356)	0.0761 (0.0528)	0.0493 (0.0503)
N	98366	51061	47305
Mean	1.923	1.828	2.026
Adjusted R2	0.530	0.489	0.570

Standard errors in parentheses

Note: This table presents the ITT for Indefinite Duration Contacts according to the different treatment arms since 19/11/2019 controlling for quantiles of pre-19/11/2019 indefinite durations hirings on Indefinite Duration Contacts. Standard errors are clustered at the labor market (Sector*CZ) level.

A.10 Robustness check: doubly robust estimation of ITT on firms' hirings

In this subsection, we present the results of a doubly robust estimation of average treatment effects on firms' hirings (using random forests, as proposed in the R package `grf`). We view this as a robustness check, and a way to convince further the reader of the absence of any data mining in the way we introduce the control for past hirings. Indeed, this estimation strategy "debias" both the dependent variable (post-intervention hirings) and the treatment using a prediction of a prediction based on a random forest built using the control variables. Therefore, in this specification, we simply include past hirings in the list of control variables, and let the random forest algorithm create splits. Table [A10](#) reports the result for the three main dependent variables considered in the paper (hirings in all, indefinite and definite contracts).

Table A10: DOUBLY ROBUST ESTIMATION OF ITT ON FIRMS' HIRINGS

	All	Indefinite	Definite
ITT	0.224 (0.698)	0.106 (0.045)	0.337 (0.689)

Standard errors in parentheses

Note: We use the `causal_forest()` and `average_treatment_effect()` from R package `grf`, with 100 trees grown for each specifications.

