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## Structural change and employment

Thomas Zuber

► **To cite this version:**

Thomas Zuber. Structural change and employment. Economics and Finance. Université Paris sciences et lettres, 2022. English. NNT : 2022UPSLE019 . tel-04482635

**HAL Id: tel-04482635**

**<https://theses.hal.science/tel-04482635>**

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**THÈSE DE DOCTORAT**  
**DE L'UNIVERSITÉ PSL**

Préparée à Ecole normale supérieure

# Structural change and employment

Soutenu par

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Le 23/03/2022

École doctorale n°465

**Economie**      **Panthéon-  
Sorbonne**

Spécialité

**Sciences économiques**

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# Remerciements

Ce travail n'aurait pas été possible sans l'aide et les encouragements de toutes les personnes qui m'ont accompagné pendant ces cinq années. Je commencerai par Gilles Saint-Paul, mon directeur de thèse, et Daniel Cohen, directeur du département d'économie de l'ENS, qui en plus de m'accueillir au sein du département, m'ont fait assez confiance pour me permettre d'assurer les cours de TD du M1 APE pendant ces 5 dernières années.

Mes co-auteurs sans qui cette thèse n'aurait pas la forme qu'elle a aujourd'hui. Luc Behaghel et Marc Gurgand qui ont pris le risque inconsidéré d'associer un macro-économiste de formation à l'élaboration d'une expérience aléatoire contrôlée ainsi qu'à l'enseignement dans le cursus PPD. Philippe Aghion qui m'a accueilli au 5ème étage du Collège de France et dont l'enthousiasme, les encouragements, les conseils ne m'ont jamais fait défaut pendant toutes ces années. Maxime Gravouille et Simon Margolin, co-locataires du bureau des doctorants du centre de recherche sur l'innovation. Sofia Dromundo avec qui je partage un amour un peu désuet de Stata (et Timothée Gigout-Magiorani qui est entièrement responsable de ce penchant). Eustache Elina qui a stoïquement enduré dans son salon mes divagations sur les externalités de demande agrégée. Yagan et Alexis, mes compagnons d'incertitudes.

Je tiens à remercier Nikhil Vellodi que mes problèmes de smooth pasting ont réussi à tenir éveillé la nuit et Axelle Ferrière qui, avec un certain bon sens, m'a fait remarquer que j'avais peu de chance, de mon vivant du moins, de trouver le maximum unique d'une fonction de plus d'un million de variables.

Ma gratitude va aussi à Antonin Bergeaud, Simon Bunel, Jean-Benoît Emeyoud, Céline Grislain-Létrémy, Pauline Lesterquy, Noémie Lisack, Gabriel Smagghue et Riccardo Zago qui m'ont guidé jusqu'au couloir M du SEPS, à la DEMS, aujourd'hui DECAMS, de la DGSEI, par l'entrée Driant, celle qui ferme à 19h, au siège de la Banque de France. Cette fin de thèse aurait été plus morne sans les fréquentes animations musicales réveillant en moi la nostalgie d'une Italie lointaine et dont Jean-Benoît a le secret.

Depuis les parois d'Aussois jusqu'aux salles de séminaire Philippe Mongin, fut pour

moi un guide et un exemple intellectuel. Je regrette la profondeur, l'étendue et la justesse de son savoir, sa volonté joyeuse de le transmettre, sa gentillesse.

Enfin tout cela n'aurait pas été possible sans l'aide de Laurence Vincent d'une part, et de Véronique Guillotin d'autre part, qui, régissant chacune d'un côté de la citadelle de Jourdan, ont suivi, orienté, réprimandé et réconforté toute une génération d'élèves à laquelle j'appartiens et qui leur doit de rendre accueillants les abords au demeurant austères d'une école d'économie.

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# Introduction

A mesure que les sociétés modernes s'enrichissent, la part de leur revenu directement issu de l'agriculture et de l'industrie a tendance à décroître (Kuznets, 1957; Boppart, 2014). La stabilité à long terme des taux d'intérêt réels, du rapport entre le stock de capital productif et le flux de production, comme celle des parts prises par les différents facteurs de production dans le revenu total (Kaldor, 1961) masquent les importants mouvements qui affectent en permanence la répartition des forces de production. Déséquilibre stable ou changement structurel, les équivoques du mouvement et de la stabilité, de la diversité et de l'unité apparaissent comme consubstantielles au processus même de la croissance moderne. Les causes de ces mouvements sont multiples.

D'un côté, des facteurs liés à la structure même de la production. La croissance de la productivité inégalement répartie entre grands secteurs de l'économie rend moins nécessaire l'usage relatif du travail dans l'industrie et a donc pour effet d'équilibre de déplacer la main d'oeuvre vers les services (Baumol, 1967; Ngai and Pissarides, 2007). L'accumulation de capital productif accentue ce phénomène dès lors que son usage est plus intensif dans l'industrie (Acemoglu and Guerrieri, 2008).

D'un autre côté, l'évolution de la répartition de la demande entre grands secteurs de l'économie participe de ce même mouvement tout en l'accentuant. Le statisticien allemand Ernst Engel remarquait dès 1857 que la part de l'alimentation dans le budget total des ménages tendait à se réduire lorsque ce budget augmentait. Cette loi microéconomique solidement établie (Houthakker, 1957) et étendue au cas des biens manufacturés, a d'importantes conséquences macroéconomiques. En effet, si les préférences des consommateurs changent à mesure que leur revenu individuel s'élève —cas des préférences dites non-homothétiques—, il suivra qu'une part de plus en plus importante du revenu total d'un pays sera consacrée aux dépenses de services plutôt que de biens manufacturés ou de produits agricoles (Kuznets, 1957; Kongsamut et al., 2001).

A quel point ces deux mécanismes — l'un du côté de l'offre, l'autre du côté de la demande — permettent-ils d'expliquer l'inexorable recul de la part occupée par l'industrie manufacturière dans la production totale des pays développés? En utilisant un modèle assez général pour intégrer ces deux mécanismes, Boppart (2014) montre

qu'ils contribuent chacun de manière sensiblement égale à l'essor des services et au déclin de la part de l'industrie dans le revenu national des Etats-Unis.

Cet exercice de quantification nous renseigne sur l'ampleur des forces en présence. Il demande néanmoins à être complété sur au moins quatre points : (i) les facteurs issus de l'offre et de la demande n'opèrent pas indépendamment l'un de l'autre mais interagissent entre eux en produisant des effets d'équilibre ; (ii) la matérialisation de ces effets dépend d'autres forces économiques indépendantes comme le degré de concurrence internationale ; (iii) une grande partie des mouvements induits par le changement structurel a lieu non seulement entre les grands secteurs de l'économie mais au sein même de ceux-ci ; (iv) la réallocation concrète des facteurs de production, qu'elle advienne entre firmes d'un même secteur ou entre firmes de secteurs d'activité différents, est conditionnée par les frictions auxquelles font face les agents économiques individuels.

Les changements de la structure de la demande et de la productivité n'opèrent pas de manière strictement indépendante. Dans la mesure où les progrès technologiques répondent aux incitations de marché (Aghion and Howitt, 1992; Romer, 1990), les déplacements de la demande entre secteurs devraient conduire à un progrès technologique asymétrique (Acemoglu, 2002), lui-même facteur d'amplification des déséquilibres de demande initiaux (Matsuyama, 2017). D'un point de vue méthodologique, la possibilité d'une interaction entre les deux sources principales du changement structurel souligne la difficulté d'identification et de quantification de mécanismes causaux univoques. Ceci est d'autant plus vrai que le changement structurel subit dans les faits l'influence de facteurs qui lui sont extérieurs.

Tout en produisant leurs effets propres les forces motrices du changement structurel peuvent être freinées ou amplifiées par les conditions extérieures dans lesquelles elles opèrent. Les événements historiques, irruptions de faits nouveaux dans le réseau des causalités existantes, pèsent parfois sur le déroulement tranquille des tendances économiques profondes. Au premier rang de ces événements, les évolutions récentes du commerce international répondent à leur propre logique économique et politique ont joué un rôle déterminant dans l'accélération du lent déclin de l'emploi manufacturier. En étudiant l'évolution de l'emploi à un niveau fin de granularité Autor et al. (2013) montrent que l'entrée de la Chine dans l'Organisation Mondiale du Commerce (OMC) en 2001 explique à elle seule près d'un quart du recul de l'emploi industriel que l'on observe aux Etats-Unis dans les années qui suivent. Ajoutant à la complexité du phénomène l'effet direct de la concurrence internationale sur les industries et les bassins d'emploi les plus exposés se double d'effets indirects potentiels. Confrontées à la compétition étrangère les entreprises domestiques peuvent réagir en réduisant leur volume de production sur les marchés les plus touchés mais aussi en déplaçant

la structure de leur production vers des biens moins exposés à la concurrence internationale, en exploitant les réductions éventuelles de coûts sur les biens intermédiaires importés, en innovant, ou en robotisant une partie des tâches anciennement accomplies par leurs salariés (Pierce and Schott, 2016; Autor et al., 2020a; Bloom et al., 2016). Une fois pris en compte ces différents canaux, l'effet net sur l'emploi de la concurrence internationale est a priori ambigu. Il dépendra en particulier du degré relatif d'exposition de chaque à firme à la concurrence directe sur ces produits et à une concurrence accrue sur les produits intermédiaires qu'elle utilisait jusqu'à présent comme intrants. Le premier chapitre de cette thèse revient sur le rôle joué par la concurrence internationale dans l'accélération du changement structurel et sur les mécanismes précis expliquant, par l'intermédiaire de la réaction des entreprises touchées, les effets observés sur l'emploi industriel. Cette étude contribue à la littérature existante en distinguant les effets de la compétition horizontale portant les bien produits par une entreprise, des effets de la compétition verticale portant sur les biens intermédiaires utilisés par une entreprise dans son processus de production. Nous trouvons que c'est essentiellement le premier type de compétition qui affecte de manière négative toutes les marges de réaction observées des entreprises manufacturières existantes. Au delà de ce résultat ce premier chapitre met aussi en évidence un point méthodologique important : alors que la plupart des études précédentes utilisent une variation sectorielle de l'exposition à la compétition internationale pour identifier les effets potentiels de celle-ci, l'utilisation de données précises sur la structure de production des entreprises nous permet de montrer que plus de 80% des différences d'exposition proviennent en réalité de différences intra-sectorielles.

L'exemple des interactions possibles entre changement structurel et compétition internationale montre par ailleurs que l'on ne peut pas se contenter d'appréhender ce phénomène au seul niveau des grands secteurs d'activité (Ding et al., 2019). Une des conséquences de la concurrence internationale est d'induire une sélection accrue des entreprises à l'intérieur même de secteurs d'activité restreints Melitz (2003); Foster et al. (2008). Par ces effets de sélection, le degré de mise en concurrence relative affecte le niveau de productivité des différents secteurs et, en définitive, la répartition agrégée des ressources de production (Hopenhayn, 1992; Melitz, 2003; Matsuyama, 2017). Dans les faits le changement structurel s'accompagne donc non seulement de mouvements de facteurs de production entre grands secteurs de l'économie (comme l'industrie et les services au sens large) mais aussi entre entreprises plus ou moins productives au sein de secteurs précisément définis, voire au sein d'une seule et même entreprise (Davis et al., 2006; Ding et al., 2019). Le deuxième chapitre de cette thèse revient sur ce mouvement de réallocation en étudiant la dynamique d'emploi des entreprises en situation de difficultés financières. Notre étude utilise les données d'ou-

verture de procédure de redressement judiciaire appariées à l’histoire individuelle d’un échantillon aléatoire de leurs salariés entre 2002 et 2012. Nous mettons en évidence une importante hétérogénéité individuelle dans les comportements de départ — à la fois dans le moment et le type de ce départ (licenciement ou départ volontaire). Cette hétérogénéité est influencée par des facteurs institutionnels comme les coûts de licenciement qui à l’équilibre orientent les comportements de séparation entre la firme et ses salariés. Aussi bien la dynamique que la nature des séparations que nous observons sont cohérentes avec un modèle simple du marché du travail impliquant des décisions de licenciement et de démissions qui en l’absence d’une indexation salariale sur le surplus net de l’appariement se révèlent être inefficaces. Contrairement aux modèles classiques du marché du travail frictionnel où chaque séparation est socialement efficace nous sommes donc en mesure de distinguer rigoureusement la nature de la contrainte qui s’exerce sur chacune des parties prenantes — firme et salarié — et qui, à l’approche d’une ouverture de redressement judiciaire, débouchent soit sur un licenciement soit sur une démission.

Pour une partie des salariés de ces firmes en difficulté, le changement structurel se traduira donc par des ajustements brutaux, de longues périodes de chômage, des pertes durables de revenu liées à des compétences insuffisamment valorisées, trop spécifiques ou rapidement dépréciées (Autor et al., 2014; Jacobson et al., 1993; Lachowska et al., 2020). Or les coûts associés à la réallocation de l’emploi entre entreprises et grands secteurs d’activité sont d’autant plus importants que les frictions sur le marché du travail sont grandes (Mortensen and Pissarides, 1994). L’équilibre frictionnel du marché de l’emploi se distingue de son équilibre compétitif dans la mesure où à chaque instant des postes vacants coexistent avec des chômeurs en recherche d’emploi. Les frictions empêchant l’appariement instantané des postes vacants aux travailleurs qui seraient prêts à les occuper sont traditionnellement reliées aux problèmes informationnels auxquels aussi bien les firmes que les travailleurs font face sur un marché fortement segmenté. Dans une situation de changement structurel où les besoins de réallocation de l’emploi entre firmes et grands secteurs de l’économie sont d’autant plus importants, les politiques publiques facilitant les appariements sur le marché du travail sont donc des outils essentiels pour éviter aux salariés licenciés de subir des pertes de revenu à long terme. La dernière partie de cette thèse vise donc à mieux comprendre les leviers qu’il est possible d’actionner pour amoindrir les frictions sur le marché du travail. Le troisième chapitre présente ainsi les résultats obtenus lors d’une évaluation d’un outil numérique d’aide à la recherche d’emploi —“La Bonne Boîte”— proposant aux demandeurs d’emploi de déposer des candidatures spontanées auprès d’entreprises n’ayant pas nécessairement déposé une offre à Pôle emploi. Les conseils de candidature spontanée prodigués par “La Bonne Boîte” reposent sur des prédictions de recrutement au niveau de l’ensemble des établissements français.

L'objectif affiché par "La Bonne Boîte" est de permettre aux demandeurs d'emplois inscrits à Pôle emploi de réintégrer le circuit "caché" des recrutements ne passant pas par le dépôt d'une offre auprès des services de Pôle emploi en anticipant sur les besoins de recrutements futurs des entreprises. Cette évaluation menée conjointement avec l'équipe de "La Bonne Boîte" et les services de Pôle emploi prend la forme d'une expérience aléatoire contrôlée de grande échelle qui a impliqué près d'un quart des bassins d'emploi de France métropolitaine (soit 1,2 millions de demandeurs d'emploi inscrits à Pôle emploi).

Dans le but d'évaluer l'intérêt global, aussi bien en termes de retour à l'emploi du côté des demandeurs d'emploi, qu'en termes d'aide au recrutement pour les firmes mises en avant par la plateforme "La Bonne Boîte", l'expérience que nous avons menée présente de plus l'originalité de réaliser un appariement doublement aléatoire entre firmes et demandeurs d'emploi. En plus de pouvoir comparer l'évolution du taux de retour à l'emploi des demandeurs tests encouragés à utiliser la plateforme avec ce même taux de retour à l'emploi chez les demandeurs témoins non inclus dans l'expérience, nous pouvons évaluer l'effet de la mise en avant systématique dans les résultats de recherches d'un ensemble de firmes tests elles aussi aléatoirement sélectionnées. Le caractère doublement aléatoire de cet appariement nous permet de mettre en évidence l'importance des frictions informationnelles qui grèvent le retour à l'emploi des chômeurs et le processus de recrutement des entreprises, mais aussi l'influence que les écarts de compétences inter-métier, où les contraintes de recherches liés au genre peuvent avoir dans le processus d'appariement sur le marché du travail.

De ce dernier chapitre se dégage finalement une conclusion optimiste. Malgré la faible ampleur de l'intervention mise en place nous observons un effet substantiel de redirection de l'effort de recherche des demandeurs d'emploi tests sur les entreprises mises en avant. Bien utilisées, les recommandations algorithmiques pourraient donc à l'avenir contribuer à l'amélioration du processus d'appariement sur le marché du travail.

# Chapitre 1

## Opposing firm-level responses to the China shock : horizontal competition versus vertical relationships ?

*Joint with P. Aghion (Collège de France and INSEAD), A. Bergeaud (Banque de France),  
M. Lequien (INSEE) and M. Melitz (Harvard).*

## 1.1 Introduction

The spectacular growth of China's exports following its accession to the WTO – the eponymous “China shock” – has induced substantial adjustments in the manufacturing sectors of developed economies. Most of the literature analyzing those adjustments starts with a measure of this shock (typically the growth rate of Chinese exports) at the sector-level. According to this measure, one of the most affected sectors is apparel. Consider two subsets of French firms classified in this sector from our sample in 1999. One set of firms produced women's jackets using woven polyester as intermediate input. The share of women's jackets imported from China (Chinese import penetration) increased by 30 percentage points (pp) between 2000 and 2007, whereas Chinese import penetration in woven polyester declined over the same period. Another set of firms produced embroidered clothes using women's trousers as intermediate input. Over that same 2000-07 period, Chinese penetration for embroidered clothes declined by 12pp, whereas Chinese penetration for women's trousers increased by 22pp. Both sets of firms were significantly impacted by the sharp rise in Chinese apparel, but in very different ways. The dominant component of the shock for the first set of firms is horizontal : a sharp increase in Chinese exports of products similar to those these firms are producing. On the other hand, the dominant component of the shock for the second set of firms is vertical : a sharp increase in Chinese exports of products used by this set of firms as an intermediate input. While the sales of the firms in the first set decreased markedly between 2000 and 2007, they increased for the firms in the second set over the same period.

In this paper, we disentangle the horizontal and vertical components of the Chinese import shock at the firm-level and analyze its effects on employment, sales, and innovation. We use French accounting records, customs, and patent information on a comprehensive firm-level panel dataset spanning the period 1994-2007 and show that those two components have opposite effects on French firms' outcomes in 2000-07. We find that exposure to horizontal trade competition is detrimental to firms' sales, employment and innovation. Moreover, this negative effect is concentrated among low-productivity firms. By contrast, we find a positive effect (although often insignificant) for the vertical component on firms' sales, employment and innovation.

More specifically, on the employment side, we find that including a separate control for the vertical component increases markedly the negative impact of the horizontal shock. However, part of this impact could stem from an industry aggregate trend – which could be driven either by trade competition or other correlated industry-level changes. Directly accounting for these industry-level trends, we show that the (within-industry) horizontal competition from Chinese goods does trigger a precisely

estimated downsizing of impacted French firms. On the innovation side and contrary to what we find for employment, no significant industry-wide trend emerges in the response of patenting to the China shock. After controlling for the vertical component of the shock, we do find a very strong and significant negative impact of increased horizontal competition on patenting by affected firms.

Our analysis relates to a growing literature on the effects of import competition and the China shock. Following the seminal work of [Autor et al. \(2013\)](#), a vast literature analyzes the effects of the China shock on those same employment, wage, and innovation outcomes. In particular see [Bloom et al. \(2016\)](#); [Iacovone et al. \(2011\)](#); [Autor et al. \(2020a\)](#); [Bombardini et al. \(2017\)](#). [Acemoglu et al. \(2016\)](#) show that import competition from China has increased after 2000 and has depressed US manufacturing employment and overall job dynamics through input-output linkages.

The same is true in France as shown by [Malgouyres \(2017\)](#). [Mion and Zhu \(2013\)](#) report that growth (resp. exit) rate of firms is negatively (resp. positively) associated with industry exposure to low-wage country imports, in particular because of China. [Dauth et al. \(2014\)](#) also document a negative impact on wage and employment in Germany due to the rising import competition with “the East” (including China).<sup>1</sup> But export oriented sectors experienced gains from trade liberalization. [Hombert and Ma-tray \(2018\)](#) discuss how firms escape trade-induced competition through innovation which allows them to increase product differentiation.

However, most of these papers use industry-level data and/or confound the horizontal impact of increased competition in output markets with the vertical impact of increased access to imported intermediates, thereby making it difficult to interpret their results. For instance, a positive effect of import shocks on domestic performance could be explained either by a positive escape competition effect from increased competition in output markets, or by an improved access to intermediate inputs.

[Acemoglu et al. \(2016\)](#) distinguish between downstream and upstream competition shocks like we do in this paper. Yet their analysis remains at industry level, and firms’ inputs are imputed from industry level IO matrices. Instead, we identify the impact of direct horizontal and vertical import competition using detailed firm-level output and input data. In a similar spirit ? looks at imports of intermediates versus final goods at

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1. [Dauth et al. \(2014\)](#) report that German firms were not only hit by a China shock but also by an Eastern Europe import shock. France, however is much less affected by this shock than Germany. To show this, Figure [A1.1](#) in Appendix [A1.2](#) shows the annual import shares for France and Germany from 9 Eastern European countries that are currently part of the EU, and compares those with the annual Chinese import shares. The pattern for the increase in Chinese imports is very similar for both France and Germany. However, the patterns are vastly different when it comes to the East-European imports : only Germany experiences a marked increase in Eastern European imports around the time of the “China shock”.

the local labor market level in Japan. Our use of product-firm level information allows us to be much more precise in the sense that we look at goods that are used as inputs by some firms but are produced as output by some others.

Industry-level analyses are fragile because the horizontal and vertical import shocks tend to be highly correlated across industries, so that regressions using only industry-level information will tend to confound these two shock components. Another issue with industry-level analyses is that relying only on industry-level variations makes it difficult to control for industry-level trajectories, regardless of a firm's exposure to either the horizontal or the vertical component of the China shock. Moving from industry- to firm-level analysis allows us both to separately identify the horizontal and vertical components of the China shock and to control for industry-level trends, and we find that more than 80% of the variance of the horizontal and vertical components of import competition stems from within industry differences.

Also related to our analysis in this paper is a literature identifying a positive impact of increased access to imported intermediate inputs on firm performance. [Amiti and Konings \(2007\)](#) show that a 10 percentage points fall in input tariffs leads to a productivity gain of 12 percent for firms that import their inputs. In the same vein, [Amiti and Khandelwal \(2013\)](#) show that a reduction in import tariffs has a positive impact on product quality for varieties close to frontier and [Gopinath and Neiman \(2014\)](#) show that the devaluation of the Argentinian currency – which amounted to a negative shock for imported capital goods – had a negative impact on aggregate productivity.<sup>2</sup> We contribute to this literature by conducting a firm-level analysis on the impact of the vertical component of the China shock in regressions where we include the horizontal component of the shock and where we also control for industry-wide trends.

Other firm-level analyses on trade and innovation include [Lileeva and Trefler \(2010\)](#); [Bustos \(2011\)](#); [Aw et al. \(2011\)](#); [Aghion et al. \(2018, 2021\)](#). With French firm-level data, [Aghion et al. \(2018\)](#) show how an exogenous increase in the export market size induces innovation, in particular at the most productive firms; [Aghion et al. \(2021\)](#) further highlight the knowledge spillovers generated by exporting French firms on firms in the export destination countries. Here we analyze how the China import shock impacts employment and innovation, distinguishing between the horizontal and vertical components of the increased competition induced by that shock.<sup>3</sup>

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2. See also [Goldberg et al. \(2010\)](#); [Topalova and Khandelwal \(2011\)](#); [Bas and Strauss-Kahn \(2014, 2015\)](#) and [Bas \(2012\)](#).

3. The literature has also explored the reverse channel of how domestic technology adoption can generate import shocks. [Malgouyres et al. \(2019\)](#) shows for example how access to broadband internet has led to an increase in firm-level import.

Our paper is also connected with the theoretical literature on trade, innovation and growth (see [Grossman and Helpman, 1991a,b](#), [Aghion and Howitt, 2009](#), chapter 13) which analyzes the role of innovation decision in explaining firm dynamics in global economies. More recently, [Burstein and Melitz \(2013\)](#) reviews a rich literature that studies how firms' innovation responds to trade liberalization and [Akcigit et al. \(2018\)](#) develops a dynamic general equilibrium growth model with endogenous innovation in an open economy. The theoretical literature has also considered the heterogeneous impact of the China shock : [Caliendo et al. \(2019\)](#) builds a theoretical model allowing for both a horizontal component and a vertical component, which they calibrate using industry-level measures of the shock and of input-output connections.

The remaining part of the paper is organized as follows. Section [2.3](#) describes our data, shows some descriptive statistics, and outlays our estimation equations. Section [1.3](#) presents our results. Section [2.8](#) concludes.

## 1.2 Data, measurement, and empirical strategy

### 1.2.1 Data

We merge different sources of information at the firm-level. First, the administrative and tax dataset *FICUS* from Insee-DGFiP provides us with sales, employment, profit, and detailed sector information for the universe of French firms from 1994 to 2007. Second, the French Customs database provides us with firm-level information on exports and imports over a range of more than 5000 product categories (HS6 product-level).<sup>4</sup> This information is completed by *BACI*, from Cepii, which provides us with product level bilateral trade information for all country pairs. Finally, *PATSTAT* from the European Patent Office provides us with patenting information, which we match with firms' administrative identifiers using the matching algorithm developed by [Lequien et al. \(2019\)](#). This firm-level matching provides us with very precise information on total patent applications and the subset of triadic applications.<sup>5</sup>

Our various data sources run from 1994 to 2007. We use information over 1994-1999, our pre-sample period, to construct firms' exposure measures (the "share" part of our "shift-share" variables) as well as firm-level controls ; and information over the 2000-2007 period to construct our shocks (the "shift" part of our shift-share shocks) and

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4. (*Statistiques du Commerce Extérieur de la Direction Générale des Douanes et Droits Indirects*)

5. Triadic patent families are sets of patent applications filed at the European Patent Office (EPO), the United States Patent and Trademark Office (USPTO) and the Japan Patent Office (JPO) that share a same priority application.

analyze their impacts on firm-level outcomes. We restrict our firm sample to privately managed manufacturing firms : (i) which record positive sales in 1999 ; (ii) which have 10 employees or more at least once over our whole sample period ; (iii) which report export sales or imports to customs prior to our base year 1999.

Table 1.1 shows the mean values for our main outcome variables in 1999. Going from left to right in the table, we increasingly restrict the set of French firms we consider. The first column covers all privately owned firms. The second column focuses on manufacturing firms. The third column restricts attention to the subset of manufacturing firms which report exports or imports to customs in 1999. And the fourth column further restricts our sample to firms with at least one patent between 1993 and 2007. Moving from the universe of privately owned firms to the subset of manufacturing firms that both trade and patent, we see that average firm size, whether measured by sales, employment, or value added, systematically increases. In addition to showing larger sales and employment, patenting firms also display above average levels of value-added per worker, patent flows, export to sales ratios, and of the number of exported and imported products, while showing a slightly lower than average labor share.

These findings are consistent with the export and innovation premia reported in Aghion et al. (2018). They are also consistent with existing studies emphasizing a negative correlation between firms' productivity and labor share (see e.g. Autor et al., 2020b; Aghion et al., 2019), and a positive correlation between firm size and the extensive margin of trade (number of exported products, e.g. Bernard et al., 2014, 2019b for the U.S. and Mayer et al., 2014 for France).

As of 2007, 27% of manufacturing firms present in our sample in 1999 have disappeared from our fiscal files. This amounts to a yearly attrition rate of 3.8%. This rate most likely overestimates the true exit rate due to the death of the firm. If we restrict our exit count in year  $t$  to firms with either negative recorded value added in  $t - 1$  or with a drop of more than 30% in employment between  $t - 2$  and  $t - 1$ , the yearly average exit rate of manufacturing firms falls down to 1.8% (14% over the entire sample period). Finally, column (4) shows that, among manufacturing firms, those that engage in innovation and patenting exhibit lower exit rates (e.g. Bernard et al., 2006).

In the remaining part of the paper, we will focus our attention on firms that engage in international trade, i.e. on the subset of firms described in the last two columns of Table 1.1. Those are the firms for which we are able to construct our firm-level trade shocks.

TABLE 1.1 – DESCRIPTIVE STATISTICS

	All mean	Manufacturing mean	Customs mean	Patenting mean
Sales	8358.75	13592.21	17266.54	60233.90
Employees	40.44	60.22	81.25	259.28
Value added	2220.25	3236.57	4450.29	15881.26
Value added per worker	44.26	41.47	45.43	54.28
Labor share	0.58	0.60	0.59	0.52
Export intensity	0.05	0.13	0.13	0.21
Exported products	1.23	5.17	7.87	19.14
Imported products	1.99	8.38	12.75	27.90
Patent applications	0.00	0.25	0.37	2.96
Triadic patents	0.00	0.01	0.02	0.15
Exit	0.25	0.27	0.27	0.10
Death	0.14	0.14	0.14	0.06
Observations	243056	57764	37956	4710

**Notes** : Unweighted mean of descriptive variables by firm group in 1999. All columns exclude firms recorded with less than 10 employees over all our sample period. Going from left to right we step by step restrict the set of French firms. The first column covers privately owned firms, regardless of their industry. The second column only keeps privately owned manufacturing firms. The third column only keeps all privately owned manufacturing firms that can be matched to customs data in 1999. Finally the fourth column further restricts our sample to firms that are observed in patent data at least once between 1993 and 2007. Sales and value added are expressed in thousands of euros, value added per worker in thousands of euros per worker. We use a fractional count to define firms' total patent applications and triadic patent applications in 1999. Firm exit stands for missing fiscal identifiers as of 2007 while death stands for exit combined with negative recorded value added prior to exit and/or a 30% drop in firm employment in the 2 years preceding exit. Observations provide the number of firms.

## 1.2.2 Measuring trade shocks

For each firm, we construct both a horizontal trade shock and a vertical trade shock. The horizontal shock is constructed using the firm's export data at the detailed product-level to measure its exposure to increased Chinese import penetration on its *outputs* markets. The vertical shock is constructed using the firm's import data at the same detailed product-level to measure its exposure to the same Chinese import penetration on its *inputs* markets.

Formally, let  $x_{f,i,t_0}$  and  $m_{f,i,t_0}$  denote firm  $f$ 's exports and imports of product  $i$  in our base year  $t_0 = 1999$ . And let  $S_{i,t}$  denote the share of total French imports of good  $i$  originating from China in year  $t > t_0$ . Our baseline empirical specification will regress firm  $f$ 's outcome on long differences in the firm's horizontal and vertical exposures to Chinese import penetration. These are defined respectively by :

$$H_{f,t} = \sum_i \frac{x_{f,i,t_0}}{\sum_j x_{f,j,t_0}} S_{i,t} \quad \text{and} \quad V_{f,t} = \sum_i \frac{m_{f,i,t_0}}{\sum_j m_{f,j,t_0}} S_{i,t}.$$

We then define the shift-share long-run differences corresponding to measured changes

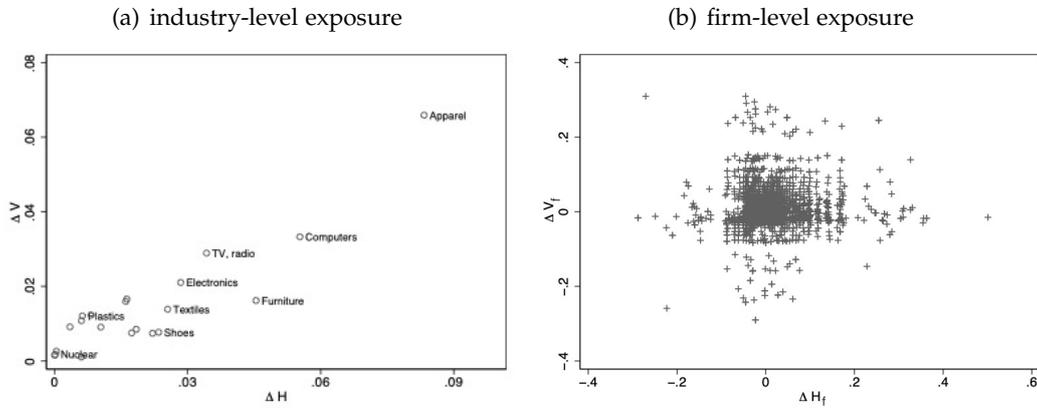
in horizontal and vertical exposure to Chinese import competition as :

$$\Delta H_f = \sum_i \frac{x_{f,i,t_0}}{\sum_j x_{f,j,t_0}} \Delta S_i \quad \text{and} \quad \Delta V_f = \sum_i \frac{m_{f,i,t_0}}{\sum_j m_{f,j,t_0}} \Delta S_i$$

where  $\Delta S_i$  is the 2007/2000 long run difference in the share of total imports of good  $i$  originating from China.<sup>6</sup> In order to match trade flows to customs data, we translate all product-level variables into the HS2002 classification at the 6-digit level.

Figure 1.1 plots the long-run differences over the 2000-2007 period for the horizontal and vertical exposure variables ; at the industry-level in Figure 1.1(a), and at the firm-level controlling for industry fixed effects in Figure 1.1(b). The horizontal and vertical exposures to Chinese import penetration are clearly correlated at the industry-level. This in turn implies that the firm-level variation displayed in Figure 1.1(b) is key for identifying the separate effects of horizontal and vertical trade competition for firm-level outcomes (controlling for industry trends). A simple variance decomposition of our firm-level horizontal and vertical shocks shows that only 10% of the overall variance can be explained by the 2-digit industry variation. The remaining variation is exhibited across firms *within* those industries.

FIGURE 1.1 – BETWEEN AND WITHIN INDUSTRY EXPOSURE TO TRADE COMPETITION



**Notes :** Panel (a) displays a scatter plot of the average long difference of the horizontal ( $\Delta H$ ) and vertical ( $\Delta V$ ) shocks by 2-digit manufacturing industries. Panel (b) displays a scatter plot of the residual long difference of firm-level horizontal ( $\Delta H_f$ ) and vertical ( $\Delta V_f$ ) once 2-digit industries fixed effects have been controlled for. For statistical secrecy reasons we discretize each shock's residuals into 100 bins and plot mean values of our residualized shocks for 2,997 groups each containing at least 5 firms. All long differences are taken over the 2000/2007 period.

6. The validity of this specification comes from an identification based on the exogenous assignment of the shocks. ? discuss at length the case of the China shock and argue that the associated specification can indeed reasonably be viewed as leveraging exogenous shock variations.

**Discussion about the horizontal and vertical shocks** By construction, the horizontal shock  $\Delta H_f$  captures a direct competition shock that directly impacts firm  $f$  at their position in the production chain. A positive  $\Delta H_f$  means that there is more production from China of the same goods that firm  $f$  produces. This is true regardless of whether firm  $f$  produces intermediate, final goods or both. The vertical shock  $\Delta V_f$  can be seen as an input-supply shock. A positive  $\Delta V_f$  means that there is an increasing production and export from China of goods that firm  $f$  uses as inputs. We expect such a positive shock to improve firm  $f$ 's access to upstream resources.

Even though firm-level measures of exposure to horizontal and vertical trade competition improve upon industry-level measures, Figure 1.1(b) also displays a positive correlation between  $\Delta H_f$  and  $\Delta V_f$ .<sup>7</sup> In our data this correlation arises from the fact that firms tend to export and import within the same detailed product category. This echoes Bernard et al. (2019a)'s finding that firms often export products that they did not themselves produce. To take into account this positive correlation between exports and imports at the firm-level, a second empirical specification developed in the Appendix ?? splits our horizontal and vertical shocks between : (i) a net export shock on exports that are not imported; (ii) a net import shock on imports that are not exported; (iii) a common export/import shock. Our results are robust to using this alternative specification.<sup>8</sup>

### 1.2.3 Empirical specification

Our baseline specification seeks to separately identify the causal impact of increased firm-level exposure to Chinese imports along the horizontal ( $\Delta H_f$ ) and vertical ( $\Delta V_f$ ) dimensions. The regression equation is :

$$\tilde{\Delta}Y_f = \alpha + \beta_H \Delta H_f + \beta_V \Delta V_f + \gamma' X_{f,t_0} + \eta_{s(f)} + \varepsilon_f, \quad (1.1)$$

where (i)  $\tilde{\Delta}Y_f$  is the growth rate of firm  $f$ 's outcome of interest between 2000 and 2007; (ii)  $X_{f,t_0}$  is a collection of firm-level pre- $t_0$  controls, with  $t_0 = 1999$ ; and (iii)  $\eta_{s(f)}$  are 2-digits industry fixed effects. The time window 2000-2007, which corresponds to the spectacular increase in China's influence in international trade, is very commonly

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7. The correlation between  $\Delta H_f$  and  $\Delta V_f$  when controlling for 2-digit industry fixed effects is 0.26 in our sample.

8. Aghion et al. (2018) shows that exports shock induce an innovation response by French firm. In our main specification, we consider the impact of the vertical and horizontal shock on firm patenting activities which could potentially be explained by the export channel if the export and import shocks are correlated. Our results are however unchanged when we control for the evolution of export over the time period considered.

used and allows our results to be comparable with previous studies of the effects of the China shock.

In all our specifications,  $X_{f,t_0}$  includes pre-1999 firm-specific levels and 5-years trends in employment and sales, as well as the dummies for the firm's export/import status.<sup>9</sup> Our regressions with patenting as the outcome variable further control for pre-1999 initial stocks and average yearly patenting rates in the relevant patent category.

We treat our raw dependent variables  $Y_f$  in three different ways. First, when  $Y_f$  is a flow variable such as sales or employment we use its "Davis-Haltiwanger" (DH) growth rate between  $t - k = 2000$  and  $t = 2007$  defined as :

$$\tilde{\Delta}Y_f = 2 \frac{Y_{f,t} - Y_{f,t-k}}{Y_{f,t} + Y_{f,t-k}}.$$

Second, when looking at patenting outcomes, we first compute firm's  $f$  1993-1999 and 2000-2007 average yearly flows of patents. We then define our dependent variable of interest  $\tilde{\Delta}Y_f$  as the DH growth rate of these two average yearly patent flows. Third, we treat binary outcomes such as industry switching or firm exit using a simple linear probability model.

We address potential biases on the estimated  $\beta_H$  and  $\beta_V$  coefficients arising from unobservable domestic shocks by instrumenting  $\Delta H_f$  and  $\Delta V_f$  by their counterparts constructed using product-level Chinese import penetration measures aggregated over six advanced countries excluding France (which is similar to [Autor et al., 2013's](#) identification strategy).<sup>10</sup>

## 1.3 Results

### 1.3.1 Comparing industry- and firm-level evidence

Before presenting our full results, we show in Table 1.2 how the measured responses to increased trade competition of employment and patenting vary when : (a) we move from industry-level shocks to firm-level shocks ; (b) we move from the overall universe of manufacturing firms to the subset of trading firms with available customs data.

Our dependent variables are the 2007/2000 DH long difference of employment and

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9. Controlling for export/import dummies amounts to controlling for the sum of "shares" in our shift-share shocks, which in turn is required when using an incomplete shift-share setting as explained in ?.

10. Our instrument are the counterparts of our horizontal and vertical shocks computed with import penetration measures from Canada, Germany, Italy, Japan, the United Kingdom and the United States.

TABLE 1.2 – COMPARING INDUSTRY- AND FIRM-LEVEL SHOCKS

	Employment							
	Industry			Employment		Firm		Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Horizontal	-0.728*** (0.213)	-0.467* (0.272)	-1.012*** (0.386)	-2.310*** (0.792)	-2.703*** (0.765)	-0.872*** (0.197)	-0.367*** (0.167)	-0.0130 (0.0311)
Vertical				1.868* (1.075)	1.833* (1.003)	-0.0214 (0.189)	0.136 (0.179)	-0.0208 (0.0312)
F		131.6	119.6	17.66	14.00	160.1	142.2	142.2
Mean	-0.0657	-0.0657	-0.0657	-0.0657	-0.108	-0.108	-0.108	0.0416
N	42323	42323	42323	42323	27884	27884	27883	27883
	Triadic patents							
	Industry			Triadic patents		Firm		Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Horizontal	-0.195 (0.560)	-0.781 (0.735)	-1.074 (0.775)	-1.564 (1.572)	-1.589 (1.565)	-1.513*** (0.494)	-1.312*** (0.487)	0.210 (0.374)
Vertical				0.748 (2.209)	0.844 (2.161)	0.114 (0.490)	-0.179 (0.482)	-0.335 (0.359)
F		165.4	84.84	20.90	20.40	131.4	141.8	96.39
Mean outcome	0.101	0.101	0.101	0.101	0.100	0.100	0.100	0.0960
N	5005	5005	5005	5005	4710	4710	4710	4710
Firm controls	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE								
Sample	All Mfg	All Mfg	All Mfg	All Mfg	Trading Mfg	Trading Mfg	Trading Mfg	Trading Mfg
Shocks	3-dgt industry	3-dgt industry	4-dgt industry (from customs)	4-dgt industry (from customs)	4-dgt industry (from customs)	Firm (from customs)	Firm (from customs)	Firm (from customs)

Notes : This table compares different specifications and sources of identification when taking the 2000/2007 DH growth rate of employment and the 1993-1999 versus 2000-2007 DH growth rate of average yearly triadic patent flows as the outcome variables of interest. Columns (1) to (4) look at the universe of privately owned manufacturing firms with more than 10 employees while columns (5) to (8) restrict this sample to firms with available trade data. Columns (1) and (2) use trade shocks directly defined at the 3-digit industry. Columns (3) to (5) use product information aggregated from firm-level data to construct 4-digit industry shocks. Finally columns (6) to (8) use our preferred firm-level shocks. Column (8) is a placebo test which takes the pre-1999 DH growth rate of employment and triadic patents as our dependent variables. The detail of each specification is given in the main text. Standard errors clustered at the 4 digit industry-level are in parentheses. \*\*\*, \*\* and \* indicate p-value of the Student test of null coefficient below 0.01, 0.05 and 0.1 respectively.

DH growth rate of yearly average triadic patent flows over the 2007-2000 period versus the 1993-1999 period. The first industry shock is defined as the increase in Chinese import penetration in each firm’s initial 3-digit NACE industry. We report the OLS and shift-share IV estimates associated with this first industry shock in columns (1) and (2), respectively. As reported in several previous studies using comparable sources of identification (e.g. Autor et al., 2013; Malgouyres, 2017; Autor et al., 2020a), the employment effect of increased industry-level competition appears to be large and negative.

To assess the differences that may exist between direct industry-level measures of trade competition and our product-level approach, we build a second industry shock by aggregating our firm-level weights within each 4-digit industry. This aggregation procedure allows us to compute both a horizontal and a vertical measure of industries’ exposures to increased trade competition. We start in column (3) by reporting the shift-share IV estimate of the horizontal component without controlling for its industry level vertical counterpart. The difference between columns (2) and (3) shows that compared to product-based measures, direct industry-level measures of exposure to trade competition miss an important part of the negative horizontal effect on employment growth. This can be attributed both to measurement error in the pure industry-level specification of column (2) and to the fact that industry-level measures tend to aggregate the vertical and horizontal components of trade competition. The

difference between columns (3) and (4) indeed shows that failing to account for the positive effect of vertical relationships leads to an upward bias on the coefficient associated to horizontal trade competition (omitted variable bias).

Before switching to our preferred firm-level specification, we check in column (5) that the employment effects from both vertical and horizontal shocks measured in column (4) on the universe of manufacturing firms do not change significantly when we restrict our sample to the subset of trading manufacturing firms. Those are the firms for which we can compute our firm-level shocks.

From column (6) onward, Table 1.2 reports firms' responses to those firm-level shocks on that subset of trading manufacturing firms. The estimated negative effect of the horizontal shock is divided by 3 when we switch from the industry trade measure (column (5)) to the more accurate firm-level trade measure (column (6)) on that same sample of firms. In addition there are other potential industry-level characteristics that are correlated with a high Chinese export growth rate. We account for these industry trends in column (7) by adding 2-digit industry fixed effects to our baseline specification. Column (7), which is our preferred specification, shows the within-industry impact of horizontal and vertical China shocks. Controlling for industry trends is particularly important if we try to isolate the impact of horizontal competition on employment : this impact is reduced by more than half when moving from column (6) to column (7), yet it remains economically and statistically significant. All regressions in the remaining part of the paper reproduce the setting of column (7) and include 2-digit industry fixed effects as well as the usual firm-level controls. Finally, the placebo test in column (8) shows no response from the pre-1999 employment growth rate to both shocks.

The bottom half of Table 1.2 shows that moving from the industry-level to our new firm-level measures of the China shocks also makes a big difference when assessing the impact of the China shock on innovation (new firm patents). The negative response of innovation to the horizontal competition shock only becomes significant once we use our firm-level shock and separately control for the vertical shock. On the other hand, controlling for the industry-level trends does not have a major impact on the negative economic magnitude of the innovation response to the shock : this response is only slightly reduced when these controls are introduced. We view this result as a strong argument in favor of switching to firm-level evidence whenever possible, and separating out the horizontal and vertical components of the China shock.

### 1.3.2 Main firm-level outcomes

Table 1.3 extends our preferred column (7) specification from Table 1.2 to additional left-hand-side firm outcome variables. The first set of variables captures additional dimensions of the firms' "current" status beyond employment : sales, the labor share (in value added), exit from manufacturing (firm remains active), and firm death. We also add a broader measure of innovation captured by the average flow of all patent applications (not just triadic patent applications). Lastly, we add a set of variables that capture changes to the firms' exported product mix (we do not observe product-level details for domestic sales). We measure the fraction of new and discontinued products (entry/exit of an exported HS6 product between 1999 and 2007). And we quantify the extent to which French firms in our sample shift their production towards products where France had a comparative advantage relative to China in 1999.<sup>11</sup> This variable is only defined for firms with available export data for both 1999 and 2007.

Our main findings can be summarized as follows : Only the horizontal shock negatively and significantly affects sales, employment, the firm's labor share, and patenting ; the vertical shock has no significant effect on these variables ; moreover, the vertical shock induces exit from manufacturing, conditional on the firm's survival. This last result suggests that the access to cheaper inputs allows firms to move away from production tasks and concentrate instead on service activities outside of manufacturing. For those firms that maintain their manufacturing activities in France, the vertical shock induces them to stick with their current set of products : these firms are far less likely to introduce new products. On the other hand, the horizontal shock induces a strong response in firms' product mix : the affected firms switch to products where France's relative comparative advantage is stronger. Evidently, firms that benefit from increased access to Chinese imported inputs find it profitable to continue producing/exporting products where France's comparative advantage is weak.

Our findings are consistent with Autor et al. (2020a) and Pierce and Schott (2016) who both find that increased exposure to trade competition leads U.S. firms to reduce sales, employment and to shift their production away from labor intensive and high labor share production tasks into service activities. Our contribution is to show that the negative impact of the increased Chinese exposure on sales, employment, labor share, and domestic innovation is tightly linked to the horizontal component of the trade shock. Finally, the direction of the effects of the shock on almost all firm-level

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11. We compute this firm-level measure of relative comparative advantage as an average across the set of exported products. For each HS6 product, we measure France's comparative advantage relative to China as the 1999 ratio of France's exports to the world over China's exports to the world. We then define firm-level comparative advantage as the average product-level comparative advantage over a firm's product mix, at all dates  $t \geq 1999$ .

outcomes is reversed when moving from the horizontal to the vertical component of the shock.

TABLE 1.3 – MAIN FIRM-LEVEL OUTCOMES

	Main outcomes					Patents		Products		
	(1) Sales	(2) Employment	(3) Labor share	(4) Exit mfg	(5) Death	(6) Triadic	(7) Appln	(8) Discontinued	(9) New	(10) Comp Adv
Horizontal	-0.417** (0.197)	-0.367** (0.167)	-0.255** (0.106)	0.0104 (0.0751)	0.0707 (0.0798)	-1.312*** (0.487)	-1.488* (0.854)	0.196* (0.117)	0.191 (0.161)	0.637*** (0.155)
Vertical	0.0653 (0.186)	0.136 (0.179)	0.136 (0.114)	0.301*** (0.0890)	-0.0765 (0.0931)	-0.179 (0.482)	0.412 (0.945)	-0.133* (0.0738)	-0.488*** (0.112)	-0.288* (0.151)
F	142.2	142.2	133.2	142.2	169.9	141.8	141.8	131.3	162.0	148.2
Mean outcome	0.0704	-0.108	-0.0236	0.0745	0.160	0.100	0.289	0.815	0.472	0.00161
N	27883	27883	24999	27883	33203	4710	4710	24232	17307	16090

**Notes :** This table reports our main results when regressing firm-level outcomes on our firm-level horizontal and vertical shocks. Columns (1) to (5) gather results for variables taken from French fiscal and administrative files. Columns (6) and (7) present results for triadic patents and patent applications. Columns (8) to (10) use exported products to construct measures of changes in a firms' product scope. We use DH growth rate for continuous variables and a simple linear probability model for dummy variables in columns (4) and (5). The share of discontinued products (8) is defined for firms with export data in 2000. The share of new products (9) is defined for firms with export data in 2007 and the DH growth rate of the relative comparative advantage content of a firm's exports (10) is defined for firms with available exports both in 2000 and 2007. The baseline sample includes all manufacturing firms with positive sales in 1999, which can be matched to customs data in 1999 and are recorded with at least 10 employees once between 1994 and 2007. Columns (6) and (7) restrict this sample to firms observed with at least one patent in our time window while columns (8) to (10) are by construction restricted to exporting firms. All models control for initial 5-years trends and level of sales and employment, export/import dummies as well as 2-digit industry fixed effects (NAF rev. 1 classification). We add 1999 stock of patents and pre-1999 trend in patenting activity for models involving patenting outcomes. Standard errors clustered at the 4 digit industry-level are in parentheses. \*\*\*, \*\* and \* indicate p-value of the Student test of null coefficient below 0.01, 0.05 and 0.1 respectively.

### 1.3.3 Comparing domestic input and sales with imports and exports

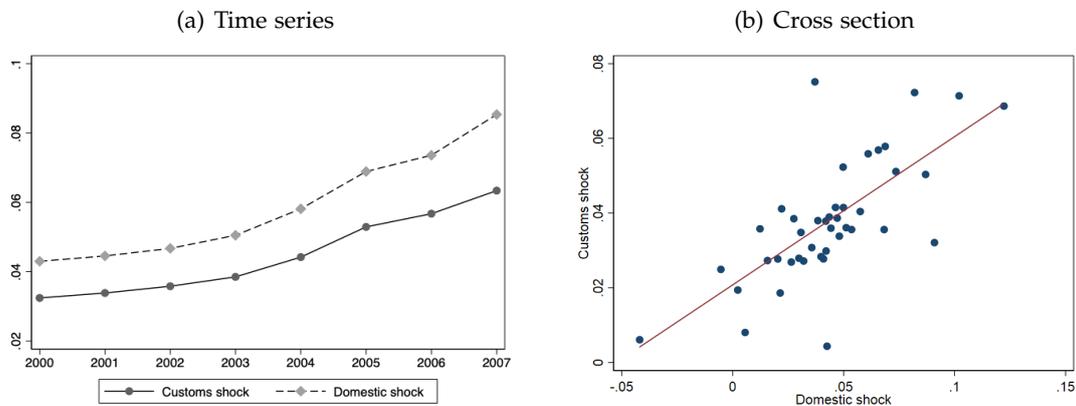
The construction of the shocks relies on international trade data at the firm level to assess precisely the set of products that are used as input and sold as output by French firms. This strategy has the advantage of relying on very detailed customs data which provides very granular details about the set of products exported and imported by each firm (a classification that contains more than 5,000 items). However, this requires us to restrict the analysis to firms participating in international trade.

In Table 1.2, columns (4) and (5), we have already shown that there is no significant change when we go from the sample of all manufacturing firms (including firms that do not trade) to our main sample of trading firms when we use industry level measures of exposure to Chinese competition in order to obtain a proxy of the exposure for the non-trading firms.

A related concern could be that the shocks affecting non-trading manufacturing firms differ systematically from the shocks that we observe for the set of trading manufacturing firms. To investigate this question, we leverage an additional data set, the "EAE Industry" (*Enquête Annuelle d'Entreprises dans l'industrie*). The EAE records some information on sales by operating industries for a large sample of manufacturing firms comprised of both trading and non-trading firms. The EAE data (4-digit French NAF nomenclature) is substantially less detailed than the product-level data that is available from customs data for the trading firms (700 product codes versus 5,000) and

does not exhibit enough within-industry variation for our main analysis with industry fixed effects. Nonetheless, this new data set allows us to construct an alternate measure for the firm-level horizontal shocks for both trading and non-trading firms, which we label “domestic”.<sup>12</sup> The timeline for the average “domestic” shock is displayed in Figure 1.2(a) below, along with our preferred “customs” shock that we used so far. As expected, restricting our analysis to the exported products observed in the customs data leads to an under-measurement in the *level* of Chinese import penetration. However, there is no discernible difference in the *changes* in Chinese import penetration over time that we exploit in our analysis : The two lines in Figure 1.2(a) are parallel. Similarly, we plot in Figure 1.2(b) the cross sectional correlation between the two shocks after having taken away a sector fixed effect.

FIGURE 1.2 – DOMESTIC VS CUSTOMS SHOCKS



**Notes :** The left-hand side graph plots average firm level Chinese horizontal import competition over the 2000/2007 period using (i) our main measure of horizontal exposure to trade competition taken from firm level customs data and (ii) an alternative exposure measure constructed from the industry decomposition of firms’ total sales (domestic and exported) as reported in the EAE survey dataset. The right-hand side graph plots the cross sectional relationship between these two different computation of the horizontal shock after absorbing a sector fixed effect. Resulting data points have been binned into 50 categories.

We now further investigate the differences between the customs shock that we used in the manuscript and this alternate domestic shock for our regression results. For completeness, we also report differences due to changes in the underlying sample of firms – the trading firms in the customs data and the sample of firms (both trading and non-trading) in the EAE data. These regressions are reported in Table 1.4, and should be compared to our main results reported in columns (6) – without industry fixed effects – and column (7) – with industry fixed effects in Table 1.2 of our manuscript. Those regressions are reproduced in the first column (columns 1 and 5) of each panel (employment/patents and with/without fixed effects) of Table 1.4. As

12. We can only use the EAE data to compute a version of the horizontal shock. We cannot use it to compute a vertical shock since it does not contain any information on inputs at the product level.

we previously mentioned, we cannot construct a vertical shock using the EAE data – and we therefore drop this additional regressor throughout. However, switching to this alternative construction of the horizontal shock barely impacts the coefficients as shown in columns (1) and (5) (which should be compared to columns (6) and (7) of Table 1.2).

TABLE 1.4 – DOMESTIC VS CUSTOMS : EMPLOYMENT AND TRIADIC PATENTS

EMPLOYMENT								
Without industry FE (column 6 of Table 2)				With industry FE (column 7 of Table 2)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Horizontal	-0.879*** (0.194)	-1.017*** (0.227)	-0.897*** (0.304)	-0.894*** (0.301)	-0.328** (0.160)	-0.434** (0.190)	0.214 (0.435)	0.243 (0.437)
Shocks	Customs	Customs	EAE	EAE	Customs	Customs	EAE	EAE
Sample	Customs	Customs and EAE	Customs and EAE	EAE	Customs	Customs and EAE	Customs and EAE	EAE
Firm controls	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE					✓	✓	✓	✓
F	310.9	161.7	89.00	75.34	232.0	141.1	104.2	96.15
Mean outcome	-0.108	-0.182	-0.182	-0.183	-0.108	-0.182	-0.182	-0.183
N	27884	12864	12864	14438	27883	12863	12863	14437

TRIADIC PATENTS								
Without industry FE (column 6 of Table 2)				With industry FE (column 7 of Table 2)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Horizontal	-1.465*** (0.492)	-1.789*** (0.569)	-1.740** (0.749)	-1.740** (0.749)	-1.382*** (0.483)	-1.470*** (0.545)	-1.913** (0.908)	-1.913** (0.908)
Firm controls	✓	✓	✓	✓	✓	✓	✓	✓
Sector FE					✓	✓	✓	✓
Shocks	Customs	Customs	EAE	EAE	Customs	Customs	EAE	EAE
Sample	Customs	Customs and EAE	Customs and EAE	EAE	Customs	Customs and EAE	Customs and EAE	EAE
F	176.0	130.4	155.1	155.1	159.2	128.8	149.7	149.7
Mean outcome	0.100	0.110	0.110	0.110	0.100	0.110	0.110	0.110
N	4710	3510	3510	3510	4710	3509	3509	3509

Notes : This table tests the specifications described in columns (6) and (7) of Table 1.2, both for employment (top panel) and triadic patents (bottom panel). Columns (1) and (5) reproduce these specifications but omit to control for the vertical shock constructed from our customs data. Columns (2) and (6) narrow the sample of firms to the subset of trading firms present in the EAE data. Columns (3) and (7) keep this sample but switch the horizontal shock from the customs to the EAE one. Finally columns (4) and (8) keep the EAE shock but extend the sample to include all firms of the EAE sample (not just the intersection of customs and EAE firms). All models control for pre-1999 5-years trends and level of sales and employment and export/import dummies. Standard errors clustered at the 4 digit industry-level are in parentheses. \*\*\*, \*\* and \* indicate p-value of the Student test of null coefficient below 0.01, 0.05 and 0.1 respectively.

Focusing on the left-hand-side panels (without fixed effects), we see that switching either the shock measure (domestic versus customs) or the firm sample (customs versus EAE) does not affect our main results (columns 1-4). The only noticeable difference is that the point estimate of the patent response using the EAE shock is larger, although substantially less precisely estimated. Focusing on the right-hand-side panels, we then notice some more substantial differences between the results using the EAE and customs shocks. Most notably, the employment response becomes insignificant with the EAE shock. This is driven by the much coarser measure of product aggregation that is available in the EAE data relative to customs : there is no longer enough within-industry variation to be able to measure the employment response while controlling for industry fixed effects. Only 36% of the variation in the EAE shock is within-industry. The comparable variation for the customs shock within-industry is substantially higher at 88%. In terms of the patenting response, we notice the same pattern as the one we had described without industry fixed effects : the patenting response with the EAE shock is larger although again much less precisely estimated.

Taken together, these additional results confirm that our main reported results for the impact of the horizontal China shock (columns 6-7 in Table 1.2) are not specific to our sample restriction to trading firms. This allows us to use the much more detailed product classification available in the customs data while controlling for industry fixed effects; and crucially also allows us to measure the impact of the vertical China shock for intermediate inputs.

### 1.3.4 Introducing firm heterogeneity

The average firm behavior as described in Table 1.3 may hide heterogeneous responses across different groups of firms. Therefore we group the firms according to their *initial labor productivity* measured as sales per worker in 1999. More specifically, we introduce below-median ( $q = 1$ ) and above-median initial productivity ( $q = 2$ ) dummies, which we interact with the vertical and horizontal shocks. Table 1.5 reproduces the results from Table 1.3 but separating the response of each of these two groups of firms to the horizontal and vertical China trade shocks.

TABLE 1.5 – EVIDENCE OF HETEROGENEOUS RESPONSE

	Main outcomes					Patents		Products		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Sales	Employment	Labor share	Exit mfg	Death	Triadic	Appln	Discontinued	New	Comp Adv
Horizontal*(q=1)	-0.409* (0.247)	-0.489** (0.206)	-0.244* (0.127)	-0.0326 (0.0648)	0.0349 (0.116)	-1.259** (0.516)	-1.888* (1.058)	0.0189 (0.0926)	-0.0368 (0.192)	0.578*** (0.208)
Horizontal*(q=2)	-0.403 (0.264)	-0.0778 (0.204)	-0.263 (0.168)	0.117 (0.127)	0.0442 (0.0888)	-1.159 (0.838)	-0.904 (1.372)	0.411** (0.184)	0.377** (0.178)	0.694*** (0.178)
Vertical*(q=1)	0.0185 (0.204)	-0.207 (0.200)	-0.0181 (0.128)	0.220*** (0.0740)	0.126 (0.110)	-0.0668 (0.481)	0.255 (1.139)	-0.0925 (0.0853)	-0.415** (0.172)	-0.327 (0.213)
Vertical*(q=2)	0.117 (0.328)	0.488* (0.282)	0.348* (0.188)	0.371** (0.162)	-0.322** (0.143)	-0.341 (0.901)	0.428 (1.622)	-0.224* (0.120)	-0.577*** (0.156)	-0.264 (0.194)
F	70.32	70.32	66.66	70.32	83.93	32.23	32.30	65.32	51.80	49.59
Mean outcome	0.0704	-0.108	-0.0236	0.0745	0.160	0.100	0.289	0.815	0.472	0.00161
N	27883	27883	24999	27883	33203	4710	4710	24232	17307	16090

**Notes :** This table reproduces the exact specifications described in Table 1.3 but interacts our horizontal and vertical shocks with below ( $q = 1$ ) and above ( $q = 2$ ) median dummies of sales per worker as measured in 1999. In addition to the controls described in Table 1.3 all models also control for the direct effects of the above/below median dummies. All models control for pre-1999 5-years trends and level of sales and employment, export/import dummies as well as 2-digit industry fixed effects (NAF rev. 1 classification). On the patent side we further add the initial stock of patents, the pre-1999 average patenting rate in the relevant patent category. Standard errors clustered at the 4 digit industry-level are in parentheses. \*\*\*, \*\* and \* indicate p-value of the Student test of null coefficient below 0.01, 0.05 and 0.1 respectively.

The negative effects of the horizontal shock highlighted in Table 1.3 on sales, employment, the labor share, triadic patents, and patent applications turn out to be concentrated on “laggard” firms with below median initial productivity. Consistent with this finding, the existing literature on competition and innovation points to a more negative effect of competition on innovation in firms far behind the technological frontier (Aghion et al., 2005).

Columns (2) and (3) also show that the effects of the vertical shock on employment and the labor share are positive and significant for the initially most productive firms :

these firms appear more able to enhance their competitive advantage following an increase in Chinese penetration on their inputs markets. Consistent with this observation, these more productive firms have a lower probability of exit (column (5)).

Columns (8), (9), and (10) document how firms also respond to the China shock through product turnover and shifts in their product mix. When facing higher competition on their output markets, frontier firms adjust their product mix : they stop exporting some of their products and start exporting new ones (columns 8 and 9). In contrast, when facing more intense competition in their input markets, both frontier and laggard firms introduce fewer new products. This suggests that improved access to cheaper inputs offsets the need to switch to new products. Finally, column (10) shows that both frontier and laggard firms respond to increased horizontal competition by strongly shifting their product mix towards products where France has a comparative advantage relative to China.<sup>13</sup>

### 1.3.5 Magnitude of the effects

From our baseline regression (1), following [Malgouyres \(2017\)](#) we can compute a counterfactual employment growth  $\tilde{\Delta}Y_f^H$  without the China shock (what would have happened with  $\Delta H_f = 0$ ) :

$$\frac{E_{f,2007}^H - E_{f,2000}}{\frac{E_{f,2007}^H + E_{f,2000}}{2}} = \tilde{\Delta}Y_f^H = \tilde{\Delta}Y_f - \beta_H \Delta H_f$$

Summing over all firms  $f$  in our regression sample, we can contrast the observed employment  $E_{f,2007}$  with the counterfactual employment  $E_{f,2007}^H$ . This quantifies the importance of the horizontal shock for the growth rate of employment over 2000-2007. While overall French manufacturing employment decreases by 7.6% in that period, we predict that it would only have decreased by 6.8% absent the horizontal China shock. This implies that 9.3% of the total manufacturing employment decline can be attributed to the horizontal China shock.<sup>14</sup> This share accruing to the China shock is consistent with the direct 10% contribution that [Acemoglu et al. \(2016\)](#) report for the US manufacturing sector over the period 1999-2011.

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13. This echoes the findings of [Bernard et al. \(2006\)](#) for the U.S.

14. The vertical component mitigates that overall negative impact. However, that coefficient for the vertical shock is not significant, so any quantification using that estimate should be taken with caution. Nonetheless, we can still proceed with a similar computation. In that case, the magnitude of the overall “China shock”—combining both the horizontal and vertical components—would be cut in half.

## 1.4 Conclusion

We use comprehensive firm-level panel data to analyze the effect of Chinese import shocks on sales, employment and innovation. We separately identify firms' responses to the horizontal and vertical components of the China shock. The horizontal shock is detrimental to firms' sales, employment, and innovation. Moreover, this negative effect turns out to be concentrated in low-productivity firms. The horizontal shock also strongly induces firms to switch their product mix towards products where France's comparative advantage relative to China is stronger. In contrast, the direction of those responses are reversed regarding the vertical shock.

At the industry-level, the horizontal and vertical shocks are highly correlated. Our results confirm that to correctly identify the impact of import competition, these two components must be disentangled at the firm-level and industry-wide trends must be controlled for.

Our results relate to the debate on tariffs as a policy response to import competition, in particular to recent papers on the welfare implications of the Trump tariffs that factor in the effects of these tariffs on American firm's input prices (see [Fajgelbaum et al., 2020](#); [Amiti et al., 2019](#); [Flaen et al., 2020](#)). By considering the impact on innovation, our findings suggest that the best response to the Chinese import shock is not to increase tariffs uniformly but rather to encourage investment in innovation while reallocating resources and jobs from less productive to more productive firms.

# A1 Appendix

## A1.1 Controlling for the common component of firms' export/import flows

In this Appendix we split our horizontal and vertical shocks between : (i) a net export shock on exports which are not imported; (ii) a net import shock on imports which are not exported; (iii) a common export/import shock. More formally :

— let  $\tilde{x}_{f,i,t_0}$  denote firm  $f$ 's **net exports** of product  $i$  in base year  $t_0$  :

$$\tilde{x}_{f,i,t_0} = \max(x_{f,i,t_0} - m_{f,i,t_0}, 0)$$

— let  $\tilde{m}_{f,i,t_0}$  denote firm  $f$ 's **net imports** of product  $i$  in base year  $t_0$  :

$$\tilde{m}_{f,i,t_0} = \max(m_{f,i,t_0} - x_{f,i,t_0}, 0)$$

— let  $\tilde{c}_{f,i,t_0}$  denote firm  $f$ 's **import/export intersection** of product  $i$  in base year  $t_0$  :

$$\tilde{c}_{f,i,t_0} = \min(m_{f,i,t_0}, x_{f,i,t_0}).$$

We shall then define firm  $f$ 's horizontal, vertical, and common Chinese shift-share shocks, respectively, by :

$$\Delta\tilde{H}_f = \sum_i \frac{\tilde{x}_{f,i,t_0}}{\sum_j \tilde{x}_{f,j,t_0}} \Delta S_i, \quad \Delta\tilde{V}_f = \sum_i \frac{\tilde{m}_{f,i,t_0}}{\sum_j \tilde{m}_{f,j,t_0}} \Delta S_i \text{ and } \Delta\tilde{C}_f = \sum_i \frac{\tilde{c}_{f,i,t_0}}{\sum_j \tilde{c}_{f,j,t_0}} \Delta S_i.$$

Our extended specification which splits our horizontal and vertical shocks between a net export shock on exports which are not imported, a net import shock on imports which are not exported, and a common export/import shock, is summarized by the regression equation :

$$\Delta_{t-k}^t Y_f = \alpha + \beta_H \Delta_{t-k}^t \tilde{H}_f + \beta_V \Delta_{t-k}^t \tilde{V}_f + \beta_C \Delta_{t-k}^t \tilde{C}_f + \gamma' X_{f,t_0} + \eta_{s(f)} + \varepsilon_f. \quad (\text{A1.1})$$

Table [A1.1](#) reports the results of this exercise and confirms the main messages conveyed in Table [1.3](#).

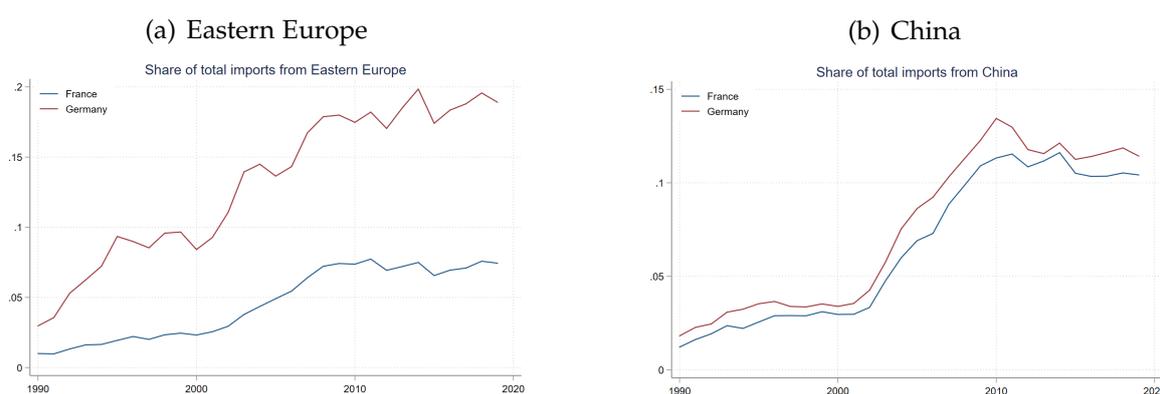
## A1.2 Additional Tables and Figures

TABLE A1.1 – MAIN OUTCOMES CONTROLLING FOR THE COMMON EXPORT/IMPORT COMPONENT

	Main outcomes					Patents		Products		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Sales	Employment	Labor share	Exit mfg	Death	Triadic	Appln	Discontinued	New	Comp Adv
Horizontal	-0.403** (0.195)	-0.374** (0.175)	-0.336*** (0.108)	0.0385 (0.0710)	0.0512 (0.0890)	-1.240** (0.553)	-1.967* (1.029)	0.279*** (0.102)	0.243 (0.164)	0.462*** (0.167)
Vertical	0.205 (0.202)	0.322* (0.191)	0.0808 (0.119)	0.269*** (0.0828)	0.0159 (0.0929)	-0.560 (0.457)	-1.040 (0.799)	0.0297 (0.0736)	-0.225* (0.129)	-0.00775 (0.141)
Common	-0.215 (0.222)	-0.215 (0.186)	0.140 (0.134)	0.0113 (0.0968)	-0.0563 (0.112)	-0.0744 (0.420)	1.104 (0.935)	-0.278*** (0.0714)	-0.288** (0.131)	-0.0332 (0.168)
F	88.05	88.05	79.67	88.05	118.6	71.79	71.79	105.4	123.2	125.9
Mean outcome	0.0704	-0.108	-0.0236	0.0745	0.160	0.100	0.289	0.815	0.472	0.00161
N	27883	27883	24999	27883	33203	4710	4710	24232	17307	16090

**Notes :** This table reproduces the results of Table 1.3 but adds the common shock to the original specification. Because we add the common component of the horizontal and vertical shocks, all results contained in this table control for a dummy indicating whether the firm both exported and imported in at least one HS6 product category. The definition of dependent variables and the exact specifications are otherwise unchanged. All models control for 2-digits industry fixed effects. Standard errors are clustered at the 2-digit industry-level. Standard errors clustered at the 4 digit industry-level are in parentheses. \*\*\*, \*\* and \* indicate p-value of the Student test of null coefficient below 0.01, 0.05 and 0.1 respectively.

FIGURE A1.1 – IMPORTS FROM CHINA AND FROM EASTERN EUROPE



**Notes :** Import to France and Germany from Eastern European countries (left-hand side) and China (right-hand side) as a share of total imports. Eastern European countries include BGR, CZE, EST, HUN, LTU, LVA, POL, ROU and SVK . Source : OECD.

## Chapitre 2

# The Anatomy of Worker Flows in Distressed Firms

*Joint with M. Gravouelle (PSE) and S. Margolin (Princeton).*

## 2.1 Introduction

Whereas an abundant literature has shown that workers are continually moving from declining to thriving firms (Postel-Vinay and Robin, 2002; Bartelsman et al., 2013; Haltiwanger et al., 2018), little is known on the actual timing and worker level determinants of these separations.

When confronted to diminishing profits due to a faltering level of demand, negative productivity shocks or an increase in competitive pressure, firms are bound to reorganize in order to restore their profitability level. Under downward wage rigidity this adjustment may go through a reduction in labor demand which mechanically increases the layoff risk faced by incumbent workers. Both because some workers are institutionally protected and because bankrupt firms should seek to retain their most valuable employees, this layoff risk will not increase homogeneously across workers. While firing costs and restrictions already constraint bankrupt firm's away from their optimal downsizing strategy, the endogenous response of workers seeking to escape the costs of job displacement by increasing their job-search effort will exert a further drag on distressed firms' future prospects.<sup>1</sup> On workers' side, a change in job-search behavior could lead to poorer job matches for panicked workers transitioning to a new firm from a bankrupt one. Overall, bankruptcy filing events should see large increases in worker-firm separations, both on the layoff and quits margins. By studying the timing, determinants and individual level consequences of worker/firm separations occurring in bankrupt firms, this paper opens the black box of workers' reallocation flows originating in firms' downsizing events.

Using linked employer-employee data matched to unemployment records as well as bankruptcy filings from France, this paper asks whether and how job flows react to the prospect of an upcoming financial turmoil. A first contribution is that our rich data sources allow us to decompose firms' employees outflows between job-to-job transitions and laid-off workers registering with public employment services and/or receiving unemployment insurance benefits. At the firm level we find that distressed firms' layoffs start to increase two years prior to the actual bankruptcy filing and are followed one year later by a surge in job-to-job moves from departing employees.

A second contribution of this paper is to study the individual determinants of worker-

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1. Since Jacobson et al. (1993)'s seminal study of displacement events in Pennsylvania, a long and ongoing stream of subsequent research repeatedly confirmed that displaced workers' earnings fall by more than 50% on impact and never fully recover in subsequent years. Going beyond earnings, displaced workers trapped in economically distressed areas face worsened social outcomes, falling marital rates, have to cope with poorer health, live shorter lives and tend to shift their political allegiance to candidates who pledge to rein in the negative effects of globalization (David et al., 2013; Autor et al., 2014; Dorn et al., 2020; Adda and Fawaz, 2020).

firm separations. We show that the timing and pattern of these separations are consistent with a simple model of quits and layoffs with heterogeneous workers and match specific stochastic productivity. We find that inherently productive individuals quit more frequently and sooner than less productive ones, that firm specific human capital decreases the probability of a layoff, and that firing costs play an important role in shaping bankrupt firms layoff policy. On departing workers' side, we find workers flying away from a distressed firm are willing to accept significant earnings cuts in order to escape the risk of a layoff, and that these earnings cuts are driven by lower hours worked in the destination firm. Finally we use a change in severance payment eligibility rules which occurred in France 2008 to assess the causal impact of firing costs on firms' downsizing strategy.

This paper relates to several strands of the literature. On the one hand we follow the steps of long series of papers studying the worker-level consequences of job displacement events. Starting with [Jacobson et al. \(1993\)](#) and [Farber et al. \(1993\)](#), this literature has shown that permanent loss of a long-term job leads to important and durable earnings losses ([Couch and Placzek, 2010](#); [Davis and Von Wachter, 2011](#); [Flaen et al., 2019](#); [Schmieder et al., 2020](#)), which can be decomposed into a significant drop in work hours that fades away gradually in the years following displacement and a more persistent reduction in wages ([Krolikowski, 2017](#); [Lachowska et al., 2020](#); [Jarosch, 2021](#)). This permanent loss in wages can in turn be split into the destruction of valuable match effects ([Lachowska et al., 2020](#)) as well as a higher probability of displaced workers transitioning toward firms paying lower wage premia ([Lachowska et al., 2020](#); [Fackler et al., 2021](#); [Brandily et al., 2020](#)). Finally [Schmieder et al. \(2020\)](#) show that the cyclical pattern in earning losses uncovered by [Davis and Von Wachter \(2011\)](#) can be directly traced back to these losses of firm premia experienced by displaced workers

A common shortcoming of early papers studying the consequences of job loss for workers' earnings is that ultimately displaced workers may differ in important dimensions from workers leaving the firm prior to the displacement event. In theory at least the correlation existing between the timing of separations and workers' individual characteristics is inherently ambiguous. While more productive workers are valuable to a distressed firm they also are the ones with better employment outside options and hence greater incentives to leave the firm before its final closure. From the firm's point of view, firing less productive workers may be seen as a preemptive strategy to forestall its own fall.

Among empirical studies accounting for the changing composition of worker flows around displacement events [Lengermann and Vilhuber \(2002\)](#) find that the composition of a firm's outgoing flows is tilted toward high skill workers in the quar-

ters leading up to displacement. They cannot however disentangle productive high wage workers quitting their distressed firm from costly tenured worker being laid-off. [Schwerdt \(2011\)](#) shows that, because of these composition changes, studies which exclude early leavers from their estimates of displacement effects will tend to overestimate the costs of job loss. These results, however, contrast with those of [Fackler et al. \(2014\)](#) who find that a failing firm's workforce tends to become increasingly skilled as the firm's exit date approaches. This finding is consistent with [Pfann \(2001\)](#) and [Pfann and Hamermesh \(2016\)](#) case study of a failing Dutch aircraft manufacturer showing that highly skilled workers remained on board longer than their low skill counterparts.

A related literature has examined the specific role of employment protection on job flows. Empirical studies have shown that firms face important labor adjustment costs ([Hamermesh, 1989](#); [Caballero et al., 1997](#)), and that separation costs were particularly high in France ([Abowd and Kramarz, 2003](#); [Kramarz and Michaud, 2010](#); [Cahuc et al., 2019](#)). Such costs are known to affect firms' hiring ([Kugler and Saint-Paul, 2004](#)), firing ([Bentolila and Bertola, 1990](#); [Saint-Paul, 1995](#)) and restructuring ([Dessaint et al., 2017](#)) policies. All three become critical as firms get close to bankruptcy.

This paper is also related to the literature which studies bankruptcy filings as a special case of job displacement events. Indeed, firm employment shrinks dramatically around bankruptcy filings ([Hotchkiss, 1995](#)), bond defaults ([Agrawal and Matsa, 2013](#)), and covenant violations ([Falato and Liang, 2016](#)). Anticipating this displacement risk, workers supply fewer labor ([Brown and Matsa, 2016](#)) and demand compensating differentials ([Abowd and Ashenfelter, 1981](#); [Topel, 1984](#); [Hamermesh and Wolfe, 1990](#); [Matsa, 2018](#); [Graham et al., 2019](#)) to employers with high financial leverage and thus higher risk of default. However, [Agrawal and Matsa \(2013\)](#) and [Baghai et al. \(2020\)](#) show that bankruptcy risk wage compensations are less sensitive to corporate leverage in high-protection institutional environments. In the relatively well protected environment we study, such risk compensations are less of a concern.

The rest of the paper unfolds as follows : section [2.2](#) outlines a simple model of worker/firm separations ; section [2.3](#) presents our data sources as well as some descriptive results on French firms' bankruptcies ; section [2.4](#) presents our main results on job flows by type of transitions ; section [2.5](#) studies the individual determinants of these separations ; section [2.7](#) show that firing costs are an important dimension shaping firms' layoff policies ; and section [2.8](#) concludes.

## 2.2 A Simple Model of Quits and Layoffs in Distressed Firms

In order to understand the forces determining the nature and timing of worker/firm separations we layout a simple continuous time partial equilibrium model of quit and layoff decisions under uncertainty. This model is similar in spirit to the model of layoffs under uncertainty outlined in Pfann (2006). Contrary to Pfann (2006) who models only a layoff decision, we explicitly take into account the possibility of quits and the strategic nature of the worker/firm relationship.

### 2.2.1 Set-up

**Environment** Time is continuous. Workers and firms are risk neutral and discount future outcomes at the market rate  $\rho$ . Consider the case of a firm combining one worker and one unit of capital in order to produce some output.

**Productivity** In the spirit of Jovanovic (1979) the flow product of a worker-firm match is  $y_t + \alpha$  where  $y$  is a match specific component evolving according to a geometric Brownian motion with match specific drift  $\mu$  and variance  $\sigma^2$  :

$$dy = \mu y dt + \sigma y dz$$

and  $\alpha \geq 0$  is the worker's idiosyncratic productivity type.

**Wage rule** We assume that in all active matches a fixed share  $\phi$  of the gross flow output  $y + \alpha$  is paid to the worker by its firm. Under this particular sharing rule wages are given as the sum of a time invariant worker specific component and time varying match specific component :

$$w_t = \phi \alpha + \phi y_t$$

While the shares of worker and match specific productivity paid to the worker need not be the same, we take a single sharing parameter  $\phi$  as our baseline case.<sup>2</sup> The fact that wages are negotiated out of the gross surplus  $\alpha + y_t$  rather than the familiar net

---

2. Close to our paper but considering a one-sided version of this model where workers are homogeneous and do not quit Cahuc et al. (2019) show that wages will be adjusted according to some fixed sharing rule of gross match productivity.

surplus as in [Mortensen and Pissarides \(1994\)](#) is fundamental to our approach. Indeed because at each point in time wages share the gross surplus of a match, separations will in general be inefficient. This property which may be undesirable in a general equilibrium setting is crucial to the modelling exercise that we propose. When separations are efficient, worker/firm matches are dissolved if and only if the net surplus from the match falls from zero. At this point neither the firm nor the worker want to stay in the match and both willingly depart to look for better production opportunities. In this sense efficient separations imply that all quits are desired by the firm while all layoffs are desired by the workers. While the efficient separation case is a very useful general equilibrium benchmark one can cast some doubts on the empirical relevance of such a setting where no meaningful distinction can be drawn between quits and layoffs. For this reason our model focuses on the partial equilibrium setting where workers' and firms' outside options are taken as given.

**Bankruptcy risk and exogenous separations** Because we are modeling firms and workers operating under the threat of a bankruptcy procedure we assume that exogenous separations may arise at the Poisson rate  $\delta$ .

**Endogenous separations** Productivity and worker types are observable by both parties and each side can decide to break off the match at any moment. A worker initiated separation is a "quit" whereas a firm initiated one is a "layoff". Upon quitting a worker receives utility  $Q(\alpha)$  which we assume to be increasing in a worker's type and which may or may not include the payment of a sunk cost. When a worker is laid off which can happen endogenously because its firm decides to fire him or exogeneously with Poisson probability  $\delta$  if the firm is liquidated, he receives utility  $U(\alpha)$  which we assume to be increasing in worker type  $\alpha$ . If the worker is laid off endogenously he also receives a severance payment  $F$  from the firm. On the opposite side of the market, firms may layoff workers in which case they get  $1/\rho$  in present discounted value out of their installed unit of capital but have to incur the severance payment  $F$ . If a worker quits the firm recovers  $1/\rho$ . Finally we assume that in case of liquidation the firm loses its unit of capital.

**Value functions** To get a simple closed form solution of quit/layoff decisions we assume that re-entry costs are infinite for both agents. Under these assumptions each workers' quit and firms' layoff unilateral strategies are easily characterized along the lines of [Dixit \(1989\)](#). See [Appendix A2.2](#) for a detailed treatment. Let  $J$  and  $E$  denote the firm's and worker's respective value functions. While a match is still productive  $J$  and  $E$  verify the following Bellman equations :

$$(\rho + \delta)E(y) = \phi(y + \alpha) + \delta U(\alpha) + \mu y E'(y) + \frac{\sigma^2}{2} y^2 E''(y) \quad (2.1)$$

$$(\rho + \delta)J(y) = (1 - \phi)(y + \alpha) + \mu y J'(y) + \frac{\sigma^2}{2} y^2 J''(y) \quad (2.2)$$

These Bellman equations cease to characterize  $E$  and  $J$  as soon as one of the two parties decides to leave the match.

### 2.2.2 Separation Regimes

We distinguish two different regimes which may arise in equilibrium. In the first regime the worker quits at a level of match productivity  $y_Q$  at which the firm would prefer not to let him go. In the second regime the opposite happens : the firm decides to layoff a worker when productivity reaches a level  $y_L$  at which the worker would rather stay on the job.

**Quit regime** In the quit regime, the worker freely chooses the quit threshold  $y_Q$ . This quit threshold and his value function  $E$  are simultaneously pinned down by (2.1) and the following value matching and smooth pasting conditions :

$$E(y_Q) = Q(\alpha) \quad \text{and} \quad E'(y_Q) = 0$$

In this case the firm's behavior is constrained and its value function  $J$  is pinned down by  $y_Q$ , the smooth Bellman equation (2.2), and the value matching condition :

$$J(y_Q) = \frac{1}{\rho}$$

**Layoff regime** In the layoff regime, the firm freely chooses the layoff threshold  $y_L$ . This layoff threshold and its value function  $J$  are simultaneously pinned down by (2.2) and the following value matching and smooth pasting conditions :

$$J(y_L) = \frac{1}{\rho} - F \quad \text{and} \quad J'(y_L) = 0$$

In this case the worker's behavior is constrained and his value function  $E$  is pinned down by  $y_L$ , the smooth Bellman equation (2.1), and the value matching condition :

$$E(y_L) = U(\alpha) + F$$

Proposition 1 characterizes the two equilibrium thresholds  $y_Q$  and  $y_L$ .

**Proposition 1.** *Under the assumption of infinite re-entry costs the thresholds  $y_Q$  in the quit regime and  $y_L$  in the layoff regime are given by :*

$$y_L = \max\left\{\frac{\gamma}{\gamma+1} \frac{\rho+\delta-\mu}{\rho+\delta} \frac{1}{1-\phi} [(\rho+\delta)\left(\frac{1}{\rho}-F\right) - (1-\phi)\alpha], 0\right\}$$

and

$$y_Q = \max\left\{\frac{\gamma}{\gamma+1} \frac{\rho+\delta-\mu}{\rho+\delta} \frac{1}{\phi} [(\rho+\delta)Q(\alpha) - \phi\alpha - \delta U(\alpha)], 0\right\}$$

where

$$\gamma = -\frac{\left(1 - \frac{2\mu}{\sigma^2}\right) - \sqrt{\left(1 - \frac{2\mu}{\sigma^2}\right)^2 + 4\frac{2(\rho+\delta)}{\sigma^2}}}{2}$$

While proposition 1 characterizes each agent optimal behavior in isolation it does not, however, answer question of knowing which (if any) of the two strategies will prevail in equilibrium. Knowing that the firms are willing to layoff workers at some positive productivity level or that workers are willing to quit at some other productivity level does not guarantee, however, that either of the layoff or quit strategies are an equilibrium. For either of these strategies to be an equilibrium we need to verify (i) in the layoff regime, that the worker will not quit before being laid-off, and (ii), in the quit regime, that the worker will not be laid-off before he decides to quit. Proposition 2 below characterizes the types of equilibria that may arise in this separation game according to the possible values of worker types  $\alpha$ . Before addressing this question directly in Proposition 2, a simple inspection of the two thresholds  $y_Q$  and  $y_L$  already yield some insights into the conditions that are necessary for either outcome to arise as an equilibrium of the separation game.

**Quit regime** Under what conditions can the quit regime arise? Would it be possible that  $y_Q$  reaches 0 so that in some cases workers never quit? The answer to this question depends on the shape of workers' two outside options  $Q$  and  $U$ . More precisely, there may or may not exist a cutoff value  $\alpha_Q > 0$  defined by

$$(\rho+\delta)Q(\alpha_Q) - \phi\alpha_Q - \delta U(\alpha_Q) = 0$$

such that workers of type  $\alpha \leq \alpha_Q$  never quit. However, under the assumption that the value of work relative to unemployment is weakly increasing in worker productivity ( $Q' - U' \leq 0$ ), that low type workers are at least indifferent between working and remaining unemployed, i.e.  $Q(0) - U(0) = B \geq 0$  and that  $Q$  is bounded away from

0 then  $\alpha_Q = 0$ . All worker types may be willing to quit if match productivity is low enough. We maintain these baseline assumptions throughout the rest of the paper.

**Layoff regime** Under what conditions can the layoff regime arise? Inspection of  $y_Q$  shows that if the worker's type  $\alpha$  is high enough

$$\alpha \geq \frac{\rho + \delta}{1 - \phi} \left( \frac{1}{\rho} - F \right)$$

then  $y_L = 0$  and the worker will never laid off.

Contrary to the quit case, however, where the constrained outside option of the firm is always higher than its unconstrained one ( $1/\rho > 1/\rho - F$ ), in the layoff regime some workers may always have an incentive to quit before being laid-off. This will be the case if the outside value of quit  $Q(\alpha)$  is preferable to how these workers fare upon getting fired, i.e. the value of unemployment  $U(\alpha)$  plus the value of severance payments  $F$ . In other words, the layoff regime *cannot* arise for all workers such that  $Q(\alpha) > U(\alpha) + F$ .

In order for layoffs not be altogether ruled out in the model, we assume from now on that  $Q(0) - U(0) = B \leq F$ , so that workers with low enough productivity may have an incentive not to quit before they are laid off. This assumption is reasonable insofar as one may plausibly think that lowest type workers have a value of work relative to unemployment close to zero ( $B \sim 0$ ). Combining this assumption with the assumption that  $Q - U$  is weakly increasing and further adding that  $\lim_{\alpha \rightarrow +\infty} Q(\alpha) - U(\alpha) > F$ , then there exists a unique  $\alpha_L > 0$  such that  $Q(\alpha_L) = U(\alpha_L) + F$ .

All in all, combining the conditions that  $y_Q > 0$  and that  $Q(\alpha) < U(\alpha) + F$  we have already shown that the layoff regime can only arise for workers of type :

$$\alpha \leq \min \left\{ \frac{\rho + \delta}{1 - \phi} \left( \frac{1}{\rho} - F \right), \alpha_L \right\} \quad (2.3)$$

where the first term of the minimum ensures that firms are willing to fire the worker at some low enough productivity level and the second term ensures that workers will not systematically quit well before reaching this low productivity level.

**Separation equilibria** Proposition 2 defines the different separation regions (quit or layoff).

**Proposition 2.** *What's more :*

- under the assumption that gain from re-employment is high enough for productive workers in the sense that :

$$\lim_{\alpha \rightarrow \infty} \frac{(\rho + \delta)Q(\alpha) - \delta U(\alpha)}{\phi} > \frac{(\rho + \delta)1}{1 - \phi} \frac{1}{\rho}$$

then there will exist a threshold  $\underline{\alpha}$  such that the quit strategy is an equilibrium for matches involving a type  $\alpha$  worker if and only if  $\alpha > \underline{\alpha}$ . The cutoff  $\underline{\alpha}$  will verify  $\underline{\alpha} \geq \alpha_Q$  where  $\alpha_Q$  is such that  $\phi\alpha + \delta U(\alpha) > (\rho + \delta)Q(\alpha)$  iff  $\alpha \leq \alpha_Q$ ;

- under the assumption that baseline unemployment benefits are not too high :

$$\rho U(0) < \frac{\phi}{1 - \phi} (\rho + \delta) \frac{1}{\rho} - \left( \frac{\phi}{1 - \phi} + 1 \right) (\rho + \delta) F$$

there will exist a threshold  $\bar{\alpha}$  such that the layoff strategy is an equilibrium for matches involving a type  $\alpha$  worker if and only if  $\alpha < \bar{\alpha}$ . The cutoff  $\bar{\alpha}$  above which the layoff strategy is not an equilibrium must verify :

$$\bar{\alpha} \leq \min \left\{ \frac{\rho + \delta}{1 - \phi} \left( \frac{1}{\rho} - F \right), \alpha_L \right\}$$

where  $\alpha_L$  is such that  $Q(\alpha) \geq U(\alpha) + F$  iff  $\alpha > \alpha_L$

We relegate the proof of Proposition 2 to Appendix A2.2. Concretely Proposition 2 shows that all workers of type  $\alpha > \max\{\bar{\alpha}, \underline{\alpha}\}$  will quit in equilibrium while all workers of type  $\alpha < \min\{\bar{\alpha}, \underline{\alpha}\}$  will be laid off. According to the relative value of the two thresholds two cases may arise. If  $\bar{\alpha} < \underline{\alpha}$  there exists a range of worker types where neither strategy is an equilibrium. If  $\bar{\alpha} > \underline{\alpha}$  there exists a range of worker types where both strategies are a possible equilibrium. While we do not have a general result on the ordering of  $\underline{\alpha}$  and  $\bar{\alpha}$ , a numerical resolution of the model shows that both the multiple equilibria and no-equilibria case may arise under plausible parameter values.

Because firing costs make firms reluctant to layoff all employees, an increase in firing costs affects negatively  $\underline{\alpha}$  : when firing costs are high, even low-type workers will be pushed to quit before the firm decides to fire them. On the layoff side, however, firing costs have offsetting effects on  $\bar{\alpha}$  the highest worker type that may be fired in equilibrium. At low levels of firing costs, severance payments are such that even low productivity workers prefer to quit before their firms get a chance to let them go. As firing costs increase (i) the incentive for higher type workers to wait for a layoff increases but (ii) at the same time the willingness of firms to fire these workers decreases. Under the first effect the  $\bar{\alpha}$  layoff boundary goes up, but starts to decrease after the second effect kicks in at high levels of firing costs. Figure A2.2 in Appendix A2.2

presents the results of a simple calibration where the counter-balancing effects of firing costs on the layoff behavior of firms and the quit strategies of workers give rise to a non-separation region as well as a multiple equilibria one for some intervals of worker types.

### 2.2.3 A Primer on Quit and Layoff Strategies

Albeit simple this partial equilibrium framework has reach implications for both the timing and nature of worker/firms separations. In this subsection we focus on the effects of (i) employment protection and (ii) productivity, as well as their interaction with the probability of a firm's bankruptcy.

**Employment protection** In this model firing costs have a timing and selection effect. Inspection of the layoff productivity thresholds  $y_L$  shows that, given a worker's type, higher firing costs  $F$  will delay a firm's decision to layoff a worker and hence make layoffs altogether less likely to occur.

Firing costs also have an influence on the type  $\alpha$  of workers who will be fired by the firm. The upper threshold for layoffs given in (2.3) shows that firing costs matter for worker selection through different channels. First the left term under the minimum of (2.3) decreases when firing costs increase : this is so because firms become more reluctant to fire low productive workers when faced with higher severance payments. Second, the right term under the minimum of (2.3) increases when firing costs increase : this happens because high firing costs mean high severance payments on the worker side so highly productive workers become reluctant to quit, making this constraint less likely to be binding. Overall, increases in firing costs increase labor hoarding of productive workers and lower the average productivity of laid-off workers by protecting incumbent workers further down the idiosyncratic productivity range. What's more the marginal effect of  $F$  on firms' selective layoff choices increases with the exogenous firm failure probability  $\delta$ .

Although perhaps not surprising, this conclusion may have important practical consequences when one considers how crucial selective layoff may be to help a declining firm reorganize and overcome negative shocks.

**Productivity** On the productivity side, match specific productivity  $\mu$  increases the option value of waiting for better times both for the worker and firm. An increase in  $\mu$  will hence delay both quits and layoffs. Finally while proposition 2 shows that worker idiosyncratic productivity  $\alpha$  directly influences the nature of the separation decision (quit or layoff), inspection of the quit thresholds  $y_Q$  shows that the non-zero

possibility of a firm's failure  $\delta > 0$  plays an important role in high ability workers self selection into an early departure strategy. To see why consider the simple case where  $U$  is constant and  $Q(\alpha) = c + \phi\alpha/\rho$  the present discounted value for worker  $\alpha$  of waiting a finite amount of time to be matched to a secure outside sector where the expected trend productivity is 0. In this case :

$$y_Q = \frac{\gamma}{\gamma+1} \frac{\rho + \delta - \mu}{\rho + \delta} \frac{1}{\phi} [(\rho + \delta)c + \delta\alpha - \delta U]$$

So that  $\delta > 0$  implies that, conditional on quitting, high type workers will quit sooner than their lower type co-workers. This would be the case in a "secure" firm where the probability of exogenous match destruction would effectively be zero — i.e.  $\delta = 0$ .

This simple case outlines a potentially important link between bankruptcy risk  $\delta$  and selective quits by highly productive workers. As the expression for  $y_L$  shows, firms value high type workers and are reluctant to let them go. If it were empirically verified this selective quits mechanism could potentially exert a further drag on bankrupt firms' survival probability thereby creating an endogenous feedback loop between bankruptcy risk  $\delta$  and the flight of most productive workers.

## 2.3 Institutional Background and Data

We briefly review the french bankruptcy system, the source of identification, and the data.

### 2.3.1 Institutional Background

**Firm bankruptcy** The French bankruptcy procedure is similar to other systems available in a wide range of countries (for example Chapter 7 and Chapter 11 in the United States). It is a compulsory—following a court order—entry under legal supervision because of a firm inability to service its debt. Two main procedures are available<sup>3</sup> : *liquidation*, during which the filing firm ceases its operation and all assets are sold, and *administration*, during which the filing firm is restructured. The main difference between them is that the latter targets firms which might be able to continue their activity in the long-run, while the former mostly applies to firm that should cease their activity.

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3. The liquidation procedure is similar to Chapter 7 in the United States, while the administration procedure is similar to Chapter 11.

We use firms in the administration procedure because they face a strong negative signal about their future prospects, but do not necessarily fail. We now review the timeline of this procedure. Within the first couple of months following the firm default, a legal court opens the administration procedure. It follows a short period of observation (up to 6 months<sup>4</sup>) during which an insolvency practitioner designated by the court assesses the firm's assets and liabilities. This practitioner can also engage in economic layoffs. Based on this evaluation, the court then decides to confirm the administration procedure, re-categorize the firm into liquidation or accept a disposal plan<sup>5</sup>.

**Firing costs** Upon layoff, workers are eligible to a severance package paid by the firm. Both the eligibility rules and the cash transfer of the package are based on seniority and a reference wage. A legal standard stating the minimal seniority and the minimal amount conditional on the reference wage is defined at the national level. Sectors, industries or even firms, might set better parameters than the national one. While we do not have the full information on the precise severance package for each worker and firm, we use the national standard as a lower bound of the severance package schedule. Our estimation strategy should then be interpreted as an intent-to-treat design.

We exploit a change in the eligibility criteria which occurred in France on the 27th of June, 2008. Figure 2.1 plots the national legal severance payments schedule before and after the reform, for a reference wage of 1500 euros per month. Two discontinuities should be noted. First, a jump in the first years of seniority where workers shift from zero to a positive amount due to the minimum seniority threshold kicking in. Following the reform, this threshold has been lowered from two years to one year.<sup>6</sup> Second, a change in the slope occurs at 10 years of seniority. After this point, workers are entitled to an increase in their severance payments. The slope remains constant after.

We recover a proxy for severance payments in our worker level data by inverting the relationship between the reference wage and total earnings in the last observed spell at a given tenure level whenever a worker's separation can be classified as a layoff (we explain in more detail how we classify layoffs and quits in section 2.3.3 below).

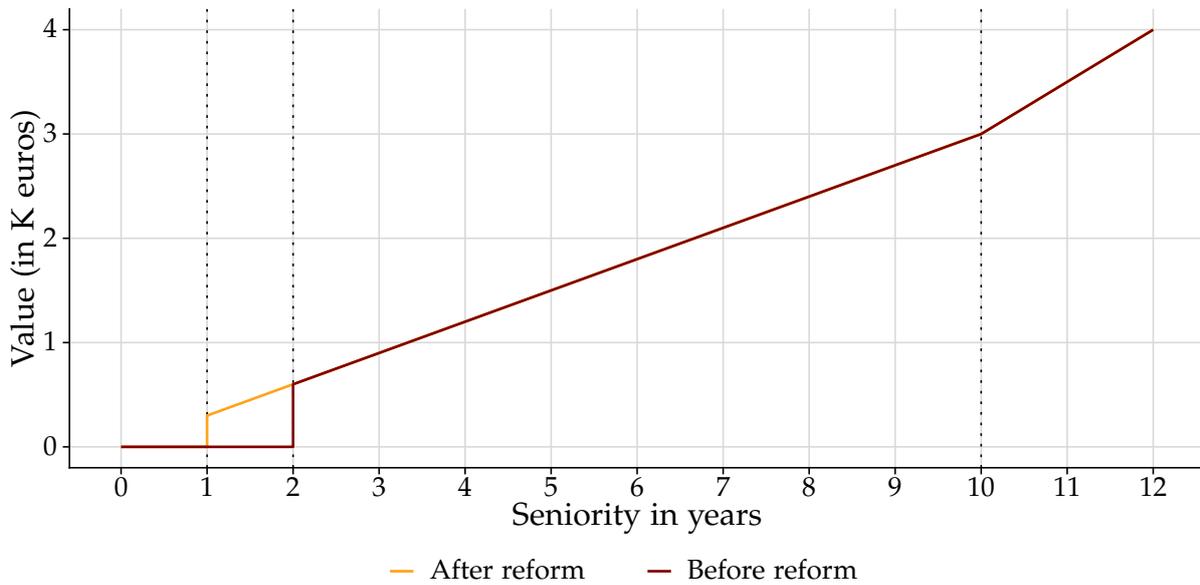
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4. This period can be renewed twice, but is in general lower than 6 months.

5. Firms can also directly exit the bankruptcy procedure. Still, this option remains unlikely.

6. All legal references are available at this [link](#).

FIGURE 2.1 – Severance payments schedule for 1500 euros of monthly wage



### 2.3.2 Firms

**Firm level data** Our first source of information on the firm side consists of publicly available bankruptcy data. We restrict our sample to firms filing for bankruptcy ("ouverture de redressement judiciaire") between 2008 and 2017. There 148,248 records of such bankruptcy filings in our sample, amounting to an average yearly flow of a little less than 15,000 firms. Our public available data allows us to observe not only bankruptcy filings but other subsequent events as well. In this this particular sample of firms, the next event after the bankruptcy filing itself took on average 7.44 months to materialize. All in all 72% of firms filing for bankruptcy were liquidated, 27.6% underwent a reorganization plan and 5.5% were subject to a disposal plan.<sup>7</sup> Within this original sample of bankruptcy filings we focus on firms which are privately owned, which can be matched to fiscal records in the years leading to their bankruptcy filing with non missing employment data. This selection trims our original sample down to 79,876 distinct firms filing for bankruptcy between 2008 and 2017. Compared to the mass layoffs literature which often focuses on large firms, in the year of their bankruptcy filing the firms we observe employ on average 7.87 workers, while median employment is just 3.

7. These categories are not mutually exclusive : a firm can enter a reorganization plan but may still end up being liquidated.

**Descriptive statistics** How do the bankrupt firms we observe compare to non-bankrupt firms? Table 2.1 below reports observed differences in the sample of firms filing for bankruptcy in 2012 and the universe of privately managed firms which do not file for bankruptcy between 2008 and 2017. In the year of their bankruptcy filing bankrupt firms are smaller, less productive and less profitable than their non-bankrupt counterpart. Reminiscent of on-going structural change bankrupt firms operate also more frequently in the manufacturing sector. Finally, although not surprisingly, bankrupt firms exit much more frequently than their non-bankrupt counterparts.<sup>8</sup>

TABLE 2.1 – BANKRUPT VS NON BANKRUPT FIRMS IN 2012

	Filing			No filing		
	mean	median	sd	mean	median	sd
Employment	9.02	3.5	30.2	11.7	2.5	307.5
Sales	1267.8	315.4	13139	2333.2	182.67	82926.1
Value added	272.3	93.7	1173.36	569.3	69.6	20211.2
Profit	-108.5	-16.3	947.6	136	9.6	9207.9
Manufacturing	0.123			0.075		
Exit	0.85			0.26		
Observations	8,610			1,436,718		

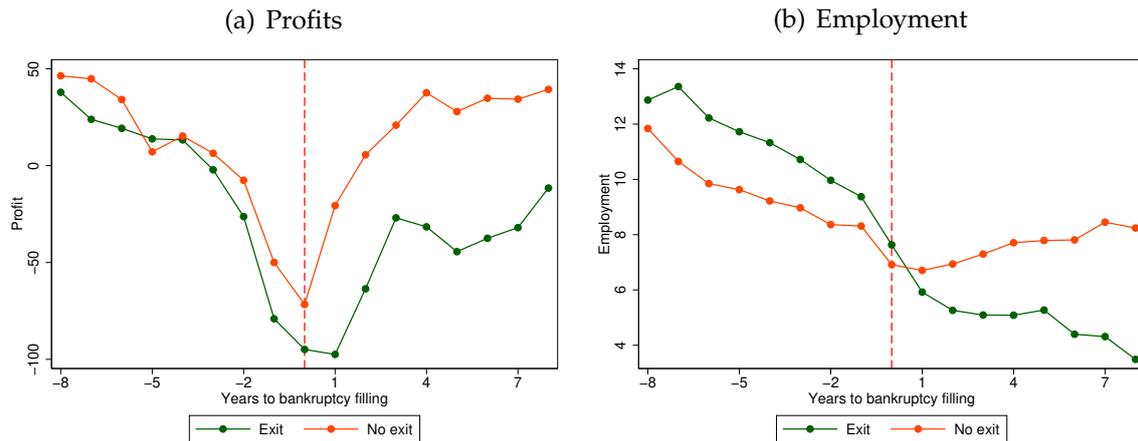
**Notes :** This table reports the 2012 mean, median and standard deviation of employment, sales, value added, profit and the mean of a manufacturing dummy for firms filing for bankruptcy in 2012 versus firms never filing for bankruptcy between 2008 and 2017. Sales, value added and profits are expressed in thousands of 2012 euros. Manufacturing is defined as the 10-33 range in the 2 digit NAF rev. 2 classification. Exit is defined as a firm’s fiscal identifier missing from our administrative records in 2018.

Our yearly fiscal data allows us to study bankrupt firms’ fate in the years leading to and following their bankruptcy filing. Figure 2.2 plots firms’ profits and employment in the years leading to and following a bankruptcy filing. What’s more Figure 2.2 distinguishes the outcomes of firms which survived to their bankruptcy procedure (orange) and firms which did not (green curve). In both groups firms’ profits and employment decline continuously in the years leading to a bankruptcy filing. Profits drop into negative territory 3 years prior to the filing date. These negative stream of profits appears to prompt a strong and swift downsizing of distressed firms : mean firm employment drops suddenly by about 30% in the two years around a bankruptcy filing. This drop is consistent with the increase in voluntary and involuntary separations which we observe at the firm level in our matched employer-employee data. In the years following the bankruptcy filing, survivors’ mean employment stabilizes at a lower level while profits fully recover and come back to positive levels. Compared to their pre-bankruptcy size, survivors’ long term employment and sales are shrunk by a factor of 2. In the other group, despite a continued decline in employment, non-

8. Here a firm’s "exit" is defined as a firm’s fiscal identifier missing from our administrative records in 2018.

survivor firms are unable to restore a positive profit rate. This long period of negative profits ultimately culminates in these firms' exit from the market. These dynamic patterns stand in contrast with the findings of [Fackler et al. \(2018\)](#) who point to the sudden nature of bankrupt firms' fall in sales and employment. In the French data at least, firms filing for bankruptcy appear to step on a path of slow decline several years before their actual exit.<sup>9</sup>

FIGURE 2.2 – FIRMS' DOWNWARD PATH TO BANKRUPTCY



**Notes :** These two graphs plot (a) mean firm profits (thousands of euros) and (b) mean firm employment in the years leading to and following each firm's bankruptcy filing. We distinguish between firms who survive after their bankruptcy procedure and firms exiting from the market in the years following their Bankruptcy filing. Sample : all firms which fill for bankruptcy between 2008 and 2017. The vertical red dashed line stands for the year in which each firm fills for bankruptcy.

### 2.3.3 Workers

**Worker level data** We gather information on workers' labor market outcomes across time using the FH-DADS panel. This panel matches linked employer-employee data to administrative unemployment records, if any. Workers in the panel represent a random 1/12th sample of the universe of French workers during the 2002-2012 period.

We construct our main sample of interest by focusing on all workers who can be matched to a firm filing for bankruptcy between 2008 and 2017. We apply several selection steps in order to construct a sample of workers for which we are able to reconstruct a well defined labor market history after they have left their bankrupt firm. Firstly we select workers who are last seen to work in a bankrupt firm 4 years before or 2 years after this firm's bankruptcy filing date. Secondly we only retain workers working under an indefinite duration contract ("CDI") when they are last observed in their bankrupt firm. In order to be able to reconstruct a robust labor market history

9. This finding is consistent with previous studies of firms' exits in the French manufacturing sector by [Bellone et al. \(2006\)](#).

we drop workers whose next observed spell begins more than 30 days after the end of their bankruptcy spell. We then classify our departing workers as job-to-job movers versus layoffs thanks to two different sources of information. First, we check whether or not each departing worker's next registered spell is an unemployment spell (worker registering with public employment services, "FH" part of the panel). Second, we check whether each departing worker received insurance payments within the year. These insurance payments are recorded separately on a yearly basis as an "ASSEDIC" line in our work history file ("DADS"). If either one of these conditions is met we classify a departing worker as a layoff.<sup>10</sup> Conversely, we define a job-to-job transition as a worker leaving a bankrupt firm to work in another firm within 30 days, without registering to public employment services nor claiming unemployment benefits within the year. We neither include in our analysis workers who exit the labor market for good (in the sense that their bankruptcy spell is their last observed spell in our employment or unemployment data) or who reappear after more than one month in our data. In order to discard workers whose contract is merely transferred to a new employer we also exclude workers who leave their origin firm after a disposal plan has been agreed on.

**Descriptive statistics** Our final sample of workers comprises 21,295 individuals whose 2002/2012 labor market history crossed paths with one of 12,981 firms filing for bankruptcy over the 2008/2017 bankrupt firm. In practice we start observing workers working in our set of bankrupt firms as soon as 2005. Table 2.2 below reports the mean, median and standard deviations of workers present in our main sample when last observed in their bankrupt firm.

In our baseline sample 51% of workers transition to another firm within 30 days while the other 49% receive unemployment insurance payments within the year or register to public employment services (i.e. "layoffs"). Among workers transitioning to another firms within 30 days, only 69.7% are hired on an indefinite duration basis. The average change in gross hourly wages associated to job-to-job transitions is -2.0%.

## 2.4 The ins and outs of worker flows in bankrupt firms

We start by documenting the pattern of separations taking place in distressed firms in the years leading up to and immediately following a bankruptcy filing. In order to do so at a quarterly rather than annual level we reconstruct separation rates from our

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10. In practice, only 8% of workers observed in our sample receive unemployment benefits without registering with public employment services within one month.

TABLE 2.2 – WORKERS WHEN LAST OBSERVED IN A BANKRUPT FIRM

	mean	median	sd	observations
Age	35.7	34	10.6	21,295
Male	0.66	1	0.47	21,295
Employer size	114.3	16	351.4	21,295
Manufacturing	0.14	0	0.35	21,295
Monthly wage	2433.2	1966.2	2222.2	21,252
Part time	0.20	0	0.40	21,295
Tenure (years)	2.91	1.64	3.95	19,969
Eligible to severance pay.	0.57	1	0.49	19,969
Potential severance pay.	2582.0	1503.2	4217.5	11,535

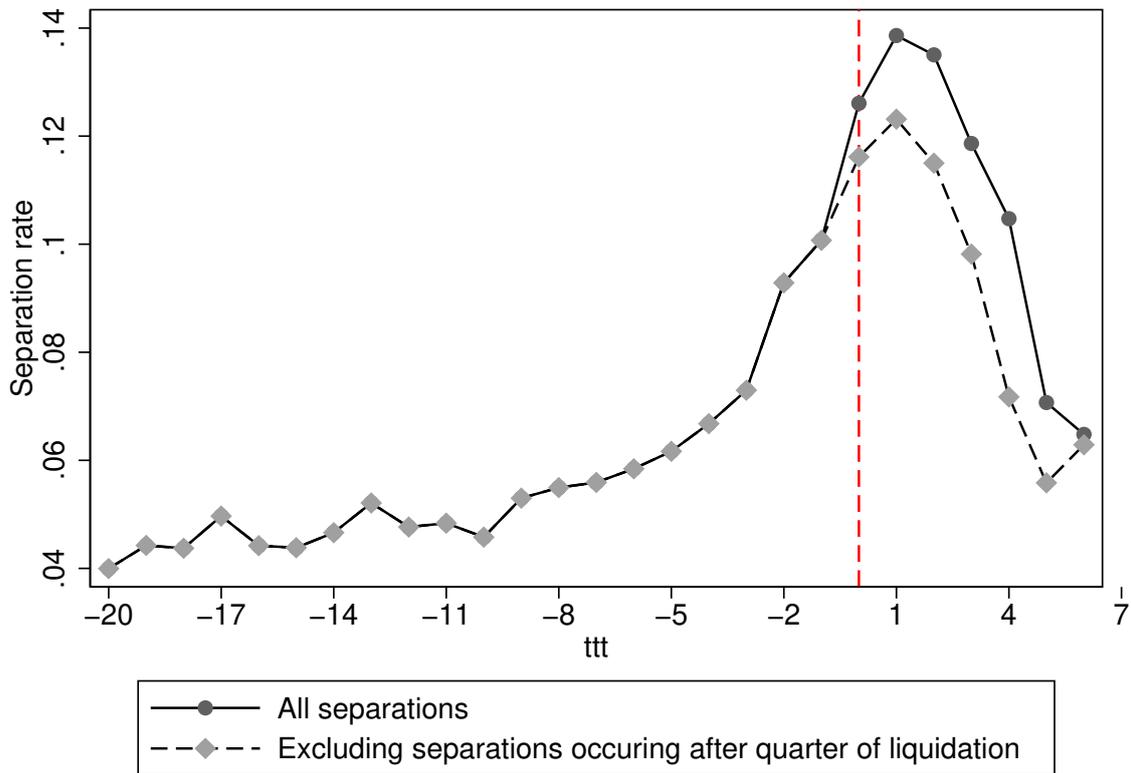
**Notes :** This table reports the mean, median and standard deviation of the monthly gross wage, age, tenure, dummy for male and size of bankrupt firm for workers who were last employed in one of our bankrupt firms on an indefinite duration contract. Gross monthly wages are computed as monthly equivalents of the gross hourly wage we observe in the data.

linked employer-employee data set. We start by selecting each worker's last observed spell in a bankrupt firm. As described in section 2.3.3 these spells extend over the 2002/2012 period and are restricted to workers employed by bankrupt firms under permanent "indefinite duration" contracts. We define quarterly firm-level separation rates as the ratio of the measured number of separations within the quarter over a firm's yearly average employment level. Figure 2.3 below displays the evolution of the mean separation rate in a time window comprising quarters 5 years prior and 2 years after each firm's bankruptcy filing. Separations start to rise 2 years prior to the filing, accelerate 1 year prior to the filing and peak on quarter after the bankruptcy filing date.

A potential concern, however, could be that part of these separations are occurring because firms are shutting down entirely. As always the definition of a firm's "exit" is not self evident. The advantage of our data source of bankruptcy filings is that we know whether and when the bankruptcy procedure was terminated and converted into a liquidation one. To check the extent to which the dynamic behavior of our measured separation rate comes from an extensive firm closure margin rather than the intensive margins of quits and layoffs, we exclude from our computations all separations taking place after each firm's liquidation date, if any. The result of this exercise is plotted as a gray dotted line alongside our main separation rate in Figure 2.3. By construction our main measure of separations and the corrected "intensive-only" one do not differ prior to the bankruptcy filing date. After the bankruptcy filing date we find that the extensive firm closure margin explains at most one sixth of the overall separation rate. This finding seems to indicate that most workers are laid off from or quit their firm prior to its final administrative closure date.

Although the observation that firms' separation rate increases dramatically around bankruptcy filing events does not come as a surprise, an advantage of our precise

FIGURE 2.3 – THE PATTERN OF SEPARATIONS IN DISTRESSED FIRMS



**Notes :** This graphs plot bankrupt firms’ mean separation rate prior to and after a bankruptcy filing event. The separation rate is defined as the number of separations occurring in a given quarter over a firm average size in the relevant year. While the first black curve keeps all separations, the second gray curve excludes separations occurring at firms which were liquidated.

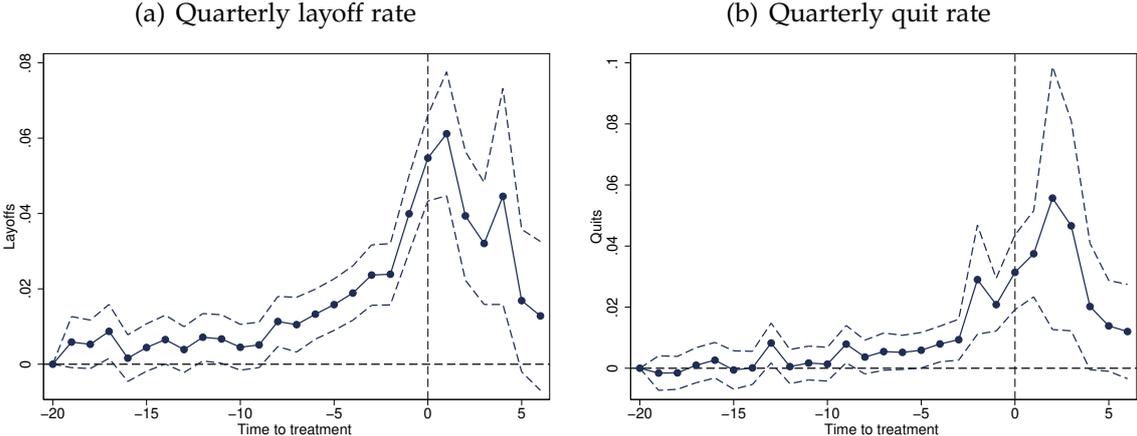
matched employer-employee and unemployment insurance data is that we are able to observe workers’ outcomes in the weeks, following their separation from bankrupt firms. We use this wealth of information to split our main measure of separations into two distinct categories : (i) job-to-job transitions, (ii) workers transitioning into unemployment. As explained in Section 2.3.3, we drop workers whose next observed spell (be it an employment or unemployment one) starts more than 30 days after the end of the last spell observed in the bankrupt firm. This selection criteria is strict (we drop 50% of observations) but ensures that the classification method we propose is reliable. Job-to-job transition are defined as workers leaving a bankrupt firm and finding a job in another firm within 30 days and without registering with Public Employment Services. Unemployed workers are workers leaving a bankrupt firm and registering with Public Employment Services within 60 days. Finally exits from the labor market are workers separating from a bankrupt firm before September 1st, 2012 and never reappearing in our data.

Unlike Baghai et al. (2020) who use a similar methodology to decompose job flows in distressed firms, we do not use the voluntary vs involuntary separations on purpose as some job-to-job transitions may in fact be closer to what could reasonably be called

an involuntary move than a voluntary one.

Given these definitions Figure 2.4 below decomposes the baseline separation rate of Figure 2.3 into its two sub-components. More precisely figures 2.4(a) and 2.4(b) present the result of a regression of firm level separation rates on quarters-to-the-bankruptcy dummies. Separation rates 5 years prior to each firm’s bankruptcy filing date are taken as the reference omitted category.

FIGURE 2.4 – FIRM LEVEL SEPARATIONS BY TYPE OF TRANSITION



**Notes :** These graphs plot (a) the mean quarterly layoff rate and (b) the mean quarterly quit rate observed at firms filing for bankruptcy. Layoffs are defined as separations followed by registration with public employment services within one month and/or UI payments. Quits are defined as separations followed by an employment spell in another firm starting within one month, without UI payment or prior registration with public employment services. A reference quarter is taken 5 years prior to the bankruptcy filing date and 95% confidence interval are displayed alongside each curve.

On the layoffs’ side, inspection of Figure 2.4(a) reveals that bankrupt firms start to fire workers well in advance of the the actual bankruptcy filing date. The timing of these preemptive layoffs which start approximately 2 years prior to the filing date, is consistent with the pattern of falling profits and employment displayed in Figure 2.2. Consistent with firms laying off workers because of their falling profit rate, the share of displaced workers registering with public employment services under the "economic layoff" category peaks at 80% within the quarter following the bankruptcy filing date (see Figure A2.1 in appendix).

Figure 2.4(b) displays the rate at which workers leave bankrupt firms in order to work in another firm. Contrary to layoffs, these job-to-job moves start to increase only one year prior to the actual filing date. The fact that job-to-job transition appear to lag the increase in layoffs at the firm level may indicate that workers (i) use their firm’s layoff policy as signal to form expectations on their firm’s survival probability, (ii) use this information to intensify their job search effort and (iii) that it takes workers of distressed firms a relatively long amount of time to find an alternative healthy firm.

These observations relate to Pfann and Hamermesh (2016)’s two-sided learning model. As firms downsize, they collect information on their employees’ loyalty, while

workers learn about their probability of being laid off. Contrary to their findings, Figure 2.4 suggests that workers learn from their employers' layoff policy and adapt their search behavior accordingly.

Note however that an important constraint placed upon any strategic reaction — on workers' side at least — is the inherently frictional nature of the labor market. As outlined in figure 2.4 by the difference in the timing of quits and layoffs in bankrupt firms, even if workers can increase their job search effort to avoid displacement, job finding itself will inevitably respond with a considerable lag.

## **2.5 The nature and timing of worker-firm separations : an empirical exploration.**

We bring these predictions to the data and test which worker level characteristics affect the timing and nature of worker separations in our set of bankrupt firms. To do this we first regress an early departure dummy equal to one for workers separating from a bankrupt firm before the bankruptcy filing. in quarters from the bankruptcy filing. In order to distinguish the relative influence on the timing of worker/firm separations of firms' layoff strategies on the one hand and workers' quit strategies on the other hand, we break up the early departure dummy into an "early quit" dummy and an "early layoff" one. We regress these three match separation outcomes on a set of worker level observable characteristics including sex, a dummy for full time work, a dummy for worker eligibility to severance payments in case of dismissal<sup>11</sup>, and the logs of age, tenure and hourly wage. Columns (1), (3) and (5) of Table 2.3 show that : (i) high-tenure workers tend to leave at a later date than their low-tenure co-workers and that this effect comes from a lower probability to be laid-off prior to the bankruptcy filing, (ii) there is no apparent gender pattern in firms' and workers' separation strategies, (iii) controlling for tenure, older workers are more likely to be fired early on, but less likely to quit, so that the overall effect on the timing of separations is not statistically different from zero, (iv) workers protected by firing costs are less likely to be fired early on and hence separate later, (v) high wage workers are more likely to quit but less likely to be laid-off prior to the bankruptcy filing so that the net effect of wages on the overall timing of separation is not statistically different from zero. earlier and are less at risk of being laid-off.

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11. Worker eligibility criteria to severance payments in case of dismissal changed during our sample period. Between 2002 and 2008, workers had to have at least two years of tenure in the firm to be eligible to a severance payment. In 2008 this tenure threshold was lowered to 1 year. In our definition of eligibility we use eligibility criteria at the time of a worker's departure from its bankrupt firm.

TABLE 2.3 – THE TIMING AND NATURE OF WORKER-FIRM SEPARATIONS

	(1) Early departure	(2) Early departure	(3) Layoff	(4) Layoff	(5) Quit	(6) Quit
Full time	-0.00925 (0.0116)	-0.0160 (0.0123)	0.00849 (0.0133)	-0.0135 (0.0142)	-0.0177 (0.0122)	-0.00250 (0.0131)
Male	0.0111 (0.00849)	-0.000102 (0.00907)	0.0124 (0.0118)	0.00881 (0.0128)	-0.00129 (0.00995)	-0.00892 (0.0112)
log(tenure)	-0.0152 (0.00478)	-0.0222 (0.00545)	-0.0143 (0.00603)	-0.0228 (0.00775)	-0.000860 (0.00591)	0.000544 (0.00745)
log(age)	-0.0153 (0.0137)	-0.0854 (0.0243)	0.0656 (0.0167)	0.00319 (0.0300)	-0.0809 (0.0158)	-0.0886 (0.0267)
Eligible to sev. pay.	-0.0435 (0.0150)	-0.0509 (0.0171)	-0.0473 (0.0180)	-0.0628 (0.0209)	0.00384 (0.0177)	0.0120 (0.0206)
log(hourly wage)	0.00879 (0.0136)		-0.105 (0.0146)		0.114 (0.0139)	
$\hat{\epsilon}_{i,t}$		-0.0321 (0.0204)		-0.184 (0.0223)		0.151 (0.0220)
$\hat{\alpha}_i$		0.0693 (0.0246)		-0.0145 (0.0275)		0.0839 (0.0302)
$\hat{\beta}'X_{i,t}$		0.301 (0.0930)		0.191 (0.121)		0.109 (0.103)
Obs	9109	7788	9109	7788	9109	7788
F	10.44	10.62	14.20	13.78	15.35	9.772
Mean	0.387	0.381	0.180	0.177	0.208	0.204
Adjusted R2	0.738	0.738	0.360	0.365	0.465	0.460

Notes : This table reports the results of six different OLS regressions. Models (1) and (2) take an early departure dummy (worker departing prior to the bankruptcy filing) as the dependent variable. Models (3) and (4) replace the early departure dummy by an early layoff one and models (5) and (6) by an early quit dummy. All models control for year, 2-digits occupations and origin firm fixed effects whose coefficients are not reported. Standard errors are clustered at the origin firm level and are reported in parentheses.

Whereas nothing warrants a causal interpretation of these estimates, they appear to be broadly consistent with the predictions of our model of worker-firm separations. While actual worker tenure is an equilibrium outcome depending on all the parameters of our model, it is strongly dependent on match specific trend productivity  $\mu$  which was predicted to increase match duration, both for layoffs and quits. The influence of the match specific trend in productivity should also be seen to operate through wages. However, the fact that high wage workers tend to quit more suggest that other wage component such as workers' idiosyncratic productivity may be at work in determining the nature of worker-firm separations. To understand which components of wages are driving the effects on quits and layoffs displayed in Table 2.3 we break down log hourly wages into a worker specific component reflecting workers' idiosyncratic productivity  $\alpha$  and a residual term incorporating any potential match specific component of wages.

To do so we recover worker wage fixed effects from a first step estimation of an

AKM type log-wage equation (Abowd et al., 1999).<sup>12</sup> We run this first step on the full linked employer-employee panel underlying our analysis, including workers and firms who were never associated to a bankruptcy event. We follow standard practice in estimating a log-wage equation with worker and firm fixed effects :

$$\log(w_{i,t}) = \gamma + \beta X_{i,t} + \alpha_i + \psi_{j(i,t)} + \epsilon_{i,t} \quad (2.4)$$

where  $w_{i,t}$  is worker's  $i$  wage at period  $t$ ,  $X_{i,t}$  contains all time varying controls including workers' age and tenure, year fixed effects as well as workers' 2 digit occupation,  $\alpha_i$  is a worker fixed effects and  $\psi_{j(i,t)}$  is the firm fixed effect associated to worker  $i$ 's employer  $j$  at time  $t$ .

We take each worker's estimated wage premium  $\hat{\alpha}_i$  as an empirical proxy for workers' idiosyncratic productivity component  $\alpha$  and use estimated residuals  $\hat{\epsilon}_{i,t}$  as measure of potential match effects.<sup>13</sup>

The results of this decomposition exercise are presented in columns (2), (4) and (6) of Table 2.3. Confirming the effect found on tenure the match component of wages now appears to be positively related to match duration (although not significantly so). Consistent with the main predictions of our model workers characterized by a high idiosyncratic component of wages tend to leave earlier and self-select into the "quit" regime. These findings are consistent with Lengermann and Vilhuber (2002), who observe a right-shift in the quality distribution of departing workers. Our results suggest that this right-shift is driven by high-type quits rather than layoffs.

## 2.6 Should I stay or should I go : Outgoing workers' worsened labor market outcomes

An obvious limitation of our bare-bones models of worker-firm separations is the absence of any form of dependence between the timing of separations and outgoing workers' outcomes. The pattern of quits analyzed in the preceding section supports the idea that workers who *can* leave *do* leave before their firm's final closure. Given the sizable earning losses incurred by displaced workers, it is possible that part of the self-selection of workers into an early-quit strategy can be attributed to deteriorating

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12. Because not all the workers of our main sample are in the AKM connected set, we are able to construct log-wage person effects for only 40.8% of our sample.

13. Under a match effect model the error term of equation 2.4 hides a match specific component potentially correlated to other independent variables as well as an error term. We use this imperfect and indirect approach to measure match specific components of wages because estimating a true match specific model would further reduce the sample due to the stronger identification requirements.

labor market outcomes as one gets closer and closer to the bankruptcy filing date. In other words the timing of quits itself could play an important role in shaping job-to-job movers' labor market outcomes through (i) a firm selection channel and/or (ii) a bargaining channel (departing workers settling for lower earning at a given firm). The first selection channel would arise if workers departing from distressed firms aimed at lower paying firms than they otherwise would have. The second bargaining channel would matter if potential employers were able to exploit incoming workers' deteriorating prospects at their origin firm to lower their recruitment wage. This would happen in any model involving wages set through bilateral Nash bargaining as in [Mortensen and Pissarides \(1994\)](#).

To test this hypothesis we look at the influence of late departures on job-to-job movers outcomes. As there is no ex-ante definition of what a "late departure" actually is, we use the change in bankrupt firms' layoff policy occurring two years prior to the actual filing date as our cut-off date. Table 2.4 below presents the results of different specifications (in rows) regressing job-to-job movers' outcomes (in columns) on a later departure dummy — where "late departure" is defined as job-to-job moves occurring less than two years prior to or anytime after the firm's bankruptcy filing date. Are control group comprises all workers that leave a bankrupt firm at two years or more before the firm's bankruptcy filing date.

Dependent variables are (1) the variation of log-daily earnings between each job-to-job mover's destination and origin firms, (2) the variation of log-hourly wages, (3) the variation of estimated firm wage premia estimated from 2.4, (4) variation of the log of hours per day, (5) variations in a log-hours per day firm specific premia, and dummies for (6) part time work and (7) change of occupation.

TABLE 2.4 – LATE DEPARTURE AND EARNING LOSSES

	Daily earnings	Wage rate		Hours worked		Occupation	
	(1) $\Delta \log(\text{daily earnings})$	(2) $\Delta \log(\text{hourly wage})$	(3) $\Delta \psi_w$	(4) $\Delta \log(\text{hours per day})$	(5) $\Delta \psi_{\text{hours/day}}$	(6) Part time	(7) Change of occupation
OLS	-0.0805 (0.0201)	0.00879 (0.00851)	0.00859 (0.00483)	-0.00910 (0.0187)	0.00433 (0.00728)	0.0336 (0.00833)	-0.00992 (0.0102)
OLS + Controls	-0.0640 (0.0199)	0.00456 (0.00812)	0.00571 (0.00450)	-0.0735 (0.0186)	0.00399 (0.00733)	0.0319 (0.00871)	0.00160 (0.0116)
Matching	-0.0447 (0.0196)	0.00970 (0.00812)	0.00348 (0.00451)	-0.0564 (0.0184)	0.00396 (0.00671)	0.0313 (0.00850)	-0.00599 (0.00997)
Obs	8829	8819	7752	8818	7608	8855	8676
Mean	0.0260	-0.0338	0.0209	0.0612	0.0235	0.196	0.306

Notes: This table reports the results of different specifications (in rows) regressing a late-departure dummy on different worker level outcomes in columns (see main text for details on the construction of the treatment dummy). While the first row reports the results of a series of simple OLS regressions, the second row adds detailed individual controls. Finally the third row uses nearest neighbor matching to control for selection on worker's observable characteristics. Standard errors in parenthesis are clustered at the origin firm level.

The first row of Table 2.4 reports raw OLS estimates using the late departure dummy as the independent variable. These raw estimates show that late movers incur substantial earning losses. These earning losses, however, are entirely driven by pressured workers moving to low-hours jobs (columns 4 and 5) rather than low-paying jobs (column 2) or firms (column 3). Running an AKM regression similar in every respect

to 2.4 but taking the log of daily hours as a dependent variable we test whether the observed variation in log-hours per day can be attributed to workers transitioning to firms offering lower hours on average. The result of this exercise reported in column (5) shows that this apparently not the case so that workers under the threat of a bankruptcy filing appear to settle for lower hours without changing their target destination firm. Looking at the proportion of workers accepting part time contracts in column (6) confirms the negative result on hours worked. Finally column (7) shows that workers do not appear to switch occupation more frequently when confronted to an increased risk of economic layoffs.

The fact that a deterioration in workers' perceived outside options leads to lower earnings through a drop in hours worked but no reduction in bargained wages is in itself of general interest. Indeed this result is consistent with the recent study of Jäger et al. (2020) who find that wages are insensitive to variations in worker's value of non-employment; as well as with Marinescu and Skandalis (2021)'s finding that exhaustion of UI benefits coincide with a drop in earnings which is mainly driven by a drop in hours worked (69%).

These OLS estimates obviously suffer from the self-selection of workers into a departure date. We try to partly account for this selection by (i) controlling for quantiles of worker level observed characteristics used in Table 2.3, and (ii) using nearest neighbor matching on the same set of covariates. These two estimates reported in the second and third rows of Table 2.4 confirm that workers which would incur potential earning losses when transitioning to another employer self-select into the late-departure category. Yet the matching estimator preserves half of the originally estimated drop in daily earnings, pointing toward a negative effect of late-departure on match quality.

## 2.7 Do firing costs really matter?

We conclude this analysis of worker-separations in distressed firm by asking whether or not the positive influence of firing costs on eligible workers' match duration and their negative influence on non-eligible workers' layoff probability is likely to capture a causal impact of firing costs on firms' downsizing process. As suggestive as they may be the results displayed Table 2.3 do not entirely settle the matter.

To this end we exploit a change in eligibility criteria which occurred in France on the 27th of June, 2008 which lowered the eligibility threshold from 2 years to 1 year of tenure. We use this change to isolate the effect of firing cost of firms' layoff policy using to distinct but complementary identifying strategies.

First, we use a difference-in-differences using the change in eligibility criteria between

2007 and 2008 as our treatment of interest and two cross-sections of workers employed under an indefinite duration contract on the 27th of June in 2007 or 27th of June 2008 as our reference population. More specifically we take workers with between 0 and 1 year of tenure and employed as of the 27th of June 2007 or 2008 as our control group, and workers with between 1 and 2 years of tenure employed as of the 27th of June 2007 or 2008 as our treated group.<sup>14</sup> This first strategy has the advantage of exploiting the full [1,2] years of tenure range for which an eligibility change occurred in 2008 as our treatment variation.

Table 2.5 below reports the results of this difference-in-differences specification where  $D = (\text{tenure} \in [1,2])$  is a treatment dummy and  $T = (\text{year} = 2008)$  a treatment year one. We use a one month unemployment indicator as our dependent variable — where "unemployment" is defined in the same manner as in section 2.3.3 (i.e. an employment spell ending within one month and immediately followed by a registration with public employment services and/or unemployment insurance payments within the year.<sup>15</sup>

The raw difference-in-differences estimate reported in column (1) of Table 2.5 points to a 31% decrease in unemployment hazard associated to the change in treatment status of relatively more tenured employees in 2008. Adding worker level controls in column (2) as well as firm fixed effects in column (3) does change the order of magnitude of the estimate. To test whether bankrupt firms are more sensitive to firing costs than healthy ones, we fully interact our DID setup with a dummy variable equal to one for firms filing for bankruptcy in 2008. Consistent with our model of worker/firm separations, the coefficient on the triple interaction reported in column (4) points to a stronger impact, although imprecisely estimated, of firing costs on distressed firms' layoff policies.

A first draw back of the difference-in-differences approach, however, is that the parallel trends assumption will fail as soon as the unemployment hazard rate-tenure profile exhibits some degree of cyclicity. Another draw back of this approach is that as our bankruptcy records data starts in 2008 and not in 2007 we are unable to assess whether bankrupt firms react to firing costs in a specific manner when compared to other firms.

With these caveats in mind we next turn to a regression-discontinuity design exploi-

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14. Tenure is only defined for individuals with non-missing firm entry date. What's more, this entry date comes in a "year plus share of year" format We recover the starting day of each worker's association with his current firm as  $(\text{entry} - \lfloor \text{entry} \rfloor) * 365$ .

15. It may arise that separated workers receive unemployment insurance but do not appear as registered with public employment services. This case may result in measurement error in our dependent variable but is quantitatively rare.

TABLE 2.5 – LAYOFF RISK AND FIRINGS COSTS : DID ESTIMATE

	(1)	(2)	(3)	(4)
T	0.00442 (0.000400)	0.00432 (0.000420)	0.00604 (0.000567)	0.00429 (0.000419)
D	-0.00311 (0.000341)	-0.00289 (0.000343)	-0.00115 (0.000455)	-0.00286 (0.000343)
T × D	-0.00296 (0.000525)	-0.00278 (0.000527)	-0.00240 (0.000775)	-0.00275 (0.000526)
T × D × B				-0.00986 (0.00957)
Constant	0.00935 (0.000256)	0.00925 (0.000265)	0.00630 (0.000277)	0.00917 (0.000264)
Obs	524229	524226	428117	524226
F	138.6	120.1	66.59	54.45
Mean	0.00947	0.00947	0.00816	0.00947
Adjusted R2	0.000874	0.00200	0.0434	0.00210
Controls		✓	✓	✓
Firm FE			✓	

**Notes :** This table reports the results of a DID design where  $T = (year = 2008)$  is the time variable and  $D = (Tenure > 1year)$  is the treatment assignment variable. The dependent variable is a dummy for unemployment status within one-month. The sample is constructed taking workers employed under an indefinite duration contract on the 27th of June 2007 and 2008 and who had between 0 and 2 years of tenure. While column (1) presents the raw OLS estimate, columns (2) adds individual level controls (quantiles of age and wage, sex and 2-digit occupations) and (3) further adds firm fixed effects. Finally column (4) fully adds a full set of interactions with  $B$  a bankruptcy variable equal to one if the worker is employed by a firm filing for bankruptcy prior to 2008

ting the 1 year of tenure threshold for workers who were employed on the day in which the change in eligibility criteria took effect. Compared to the difference-in-differences strategy, the regression-discontinuity estimate is immune to violations of the parallel trends assumption but estimates the causal effect of firings costs only in a neighborhood of the the 1 year of tenure threshold. Figure A2.3 plots the density of the running variable (tenure in years) around the threshold. While large discontinuities exist away from the threshold because of increased seasonal hires in January, this should not impact our estimate which we recover from a locally linear polynomial approximation following the bandwidth selection method proposed by [Calonico et al. \(2014\)](#).

In the first panel of Table 2.6 column (1) reports our baseline RD estimate on the full sample of workers which were observed as working in a indefinite duration contract on the date of the eligibility change. This point estimate confirms the negative effect of firing costs on unemployment risk and is larger in magnitude than the DID one. The different magnitudes of the effect estimated through the RD and DID designs

TABLE 2.6 – LAYOFF RISK AND FIRING COSTS : RD ESTIMATE

	(1) All	(2) Healthy firm	(3) Bankrupt firm	(4) Placebo
<i>A - Without controls</i>				
RD at 1 year of tenure	-0.00494 (0.00203)	-0.00485 (0.00201)	-0.00696 (0.0229)	0.000792 (0.00146)
Obs	261249	258325	2924	264029
<i>B - With individual controls</i>				
RD at 1 year of tenure	-0.00468 (0.00200)	-0.00460 (0.00197)	-0.00686 (0.0229)	0.00133 (0.00143)
Obs	260723	257804	2919	263506

**Notes :** This table reports regression discontinuity estimates at the one-year of tenure threshold taking a one-month layoff dummy as the dependent variable. We select the sample of individuals employed in indefinite duration contracts on the 27th of June 2008 using a 2 years window around the threshold (0 to 2 years of tenure). We use a locally linear polynomial approximation and the two-sided optimal bandwidth selection proposed by [Calonico et al. \(2014\)](#). While panel A presents raw RD estimates, panel B adds individual level controls (wage and age quantiles, sex and occupation). Column (1) reports the overall estimate, columns (2) and (3) break this estimate between firms without any bankruptcy filing in 2008 and firms which filed for bankruptcy in 2008, column (4) presents an overall placebo estimate using a sample of workers 1 year prior to the change in eligibility rights. Standard errors in parenthesis.

between are not surprising insofar as the RD and DID estimand (i) do not use the same treatment variation (workers with 2 years of tenure exhibit a greater jump in firing costs) and (ii) the underlying treatment effects may be heterogeneous along the tenure dimension.

Columns (2) and (3) of Table 2.6 repeat the RD exercise but split the original sample of workers between workers working in a firm without any bankruptcy event and workers employed at a firm within one year of a bankruptcy filing event. While the point estimate in the bankruptcy sub-sample remains large and negative, indicating that firing cost may indeed affect the downsizing process of bankrupt firm, the small sample size does allow us to detect a potential heterogeneous effect. Finally column (4) presents a placebo test by running the same RD regression on the full sample of workers who were employed one year prior to the reform's date. Indeed one might worry that the one year tenure threshold may coincide with other institutional or behavioral discontinuities affecting firm's layoff policies. Reassuringly this placebo RD estimate is not significant at any meaningful confidence level and is in absolute value an order of magnitude below our baseline RD estimate. Panel B presents the exact same specifications but adds individual level controls. As shown in panel B of Table 2.6 these results are robust to adding a set of adds individual level controls (wage, sex, occupation, age).

The fact that firing costs' appear to influence firms' layoff choices confirms that the non-causal estimate of Table 2.3 (which amounted to a 10% decrease in layoff risk

for workers protected by severance payment eligibility) may be a first order issue when considering the downsizing process of distressed firms. Indeed the amount of firing costs paid by firms to displaced workers are mandated by law and are strongly related to (i) tenure and (ii) past wages. As already noted this dependence introduces a potentially sub-optimal correlation between firms' firing incentives and worker types. In our data the correlation coefficient between estimated worker types and severance payment is mildly positive at 0.06 suggesting that firing costs may entice firms to fire valuable workers further jeopardizing their survival probability.

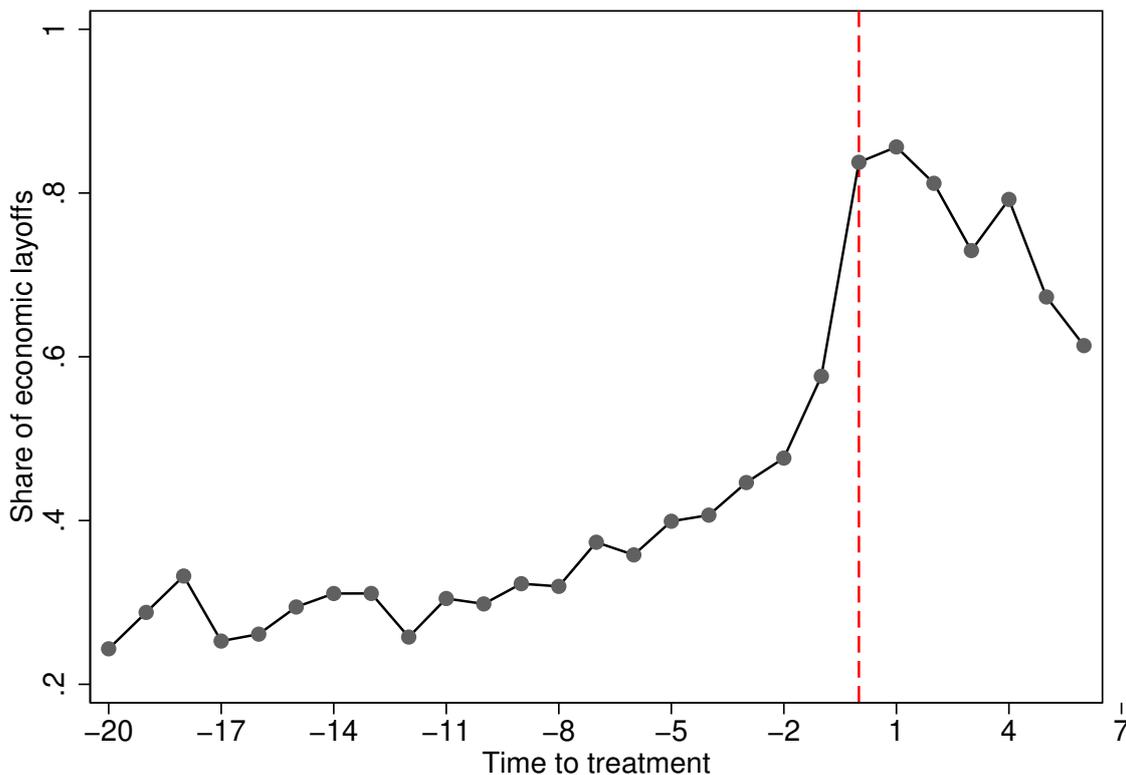
## 2.8 Conclusion

Using linked employer-employee data matched to unemployment records as well as bankruptcy filings from France we document that distressed firms' layoffs changes markedly two years prior to the actual bankruptcy filing and are followed one year later by a surge in job-to-job moves from departing employees. We show that micro-level patterns are consistent with a simple model of worker/firm separations. Both the data and the model are consistent with productive workers quitting their distressed firm early on and unproductive workers being fired by the firm in an attempt to restore profitability. On top of these heterogeneous patterns we find that firing costs impact distressed firms downsizing strategy by preventing the layoff of unproductive workers. What's more, the causal interpretation of this conclusion is supported by the analysis of a change in severance payment eligibility rights which occurred in France in 2008.

## A2 Appendix

### A2.1 Composition of separation flows

FIGURE A2.1 – THE PATTERN OF SEPARATIONS IN DISTRESSED FIRMS



Notes : This graph plots the share of public employment services registrations of workers coming from bankrupt firms which are categorized as economic layoffs in the broad sense (MOTINS variable of the FH file). We include in this category the economic layoff motive as well as other minor categories related to economic layoffs.

### A2.2 Model

#### Set-up

**Environment** Time is continuous and indexed by  $t$ . Workers and firms are risk neutral and discount future outcomes at the market rate  $\rho$ . To produce  $Y$ , a firm combines one worker and one unit of capital. We decompose  $Y$  between a match specific component  $y$  and the worker's idiosyncratic productivity type  $\alpha \geq 0$ . The law of motion of  $y$  is a geometric Brownian motion :

$$dy = \mu y dt + \sigma y dz$$

with match specific drift  $\mu$  and variance  $\sigma^2$ .

**Value function when matched** We now define the worker and the firm value function when producing. In our baseline model we assume a fixed share  $\phi$  of the gross flow output  $Y$  is paid to the worker by its firm. Note also that this type of sharing rule could be derived from an offer/counter-offer game as in [Cahuc et al. \(2006\)](#). A lay-off can either happen endogenously through the firm decision or exogenously under the threat of a bankruptcy procedure as a Poisson process with intensity  $\delta$ . When laid off, the worker receives utility  $U(\alpha)$  which we assume to be increasing in worker type  $\alpha$ . The worker value and firm value functions are :

$$\begin{aligned}\rho E(y_t) &= \phi(y_t + \alpha) + \delta[U(\alpha) - E(y_t)] + \frac{\mathbb{E}_t[dE(y_t)]}{dt} \\ \rho J(y_t) &= (1 - \phi)(y_t + \alpha) - \delta J'(y_t) + \frac{\mathbb{E}_t[dJ(y_t)]}{dt}\end{aligned}$$

Using Ito's Lemma, we expand  $dE(y)$  and  $dJ(y)$  :

$$\begin{aligned}dE(y) &= E'(y)dy + \frac{1}{2}E''(y)(dy)^2 \\ dJ(y) &= J'(y)dy + \frac{1}{2}J''(y)(dy)^2\end{aligned}$$

Taking the expectation and dividing both sides by  $dt$ , we find the usual Bellman equations associated to an underlying diffusion process :

$$(\rho + \delta)E(y) = \phi(y + \alpha) + \delta U(\alpha) + \mu y E'(y) + \frac{\sigma^2}{2} y^2 E''(y) \quad (\text{A2.1})$$

$$(\rho + \delta)J(y) = (1 - \phi)(y + \alpha) + \mu y J'(y) + \frac{\sigma^2}{2} y^2 J''(y) \quad (\text{A2.2})$$

**Value function when quitting** The worker quits when  $y < y_Q$  and the worker and firm value matching conditions are defined by :

$$E(y_Q) = Q(\alpha) \quad (\text{A2.3})$$

$$J(y_Q) = \frac{1}{\rho} \quad (\text{A2.4})$$

**Value function when laid off** The firm lay off the worker when  $y < y_L$  and transfers an amount  $F$  of firing costs to the worker. The worker and firm value matching conditions are then defined by :

$$E(y_L) = U(\alpha) + F \quad (\text{A2.5})$$

$$J(y_L) = \frac{1}{\rho} - F \quad (\text{A2.6})$$

### Equilibrium regimes

We want to determine the worker's quit threshold  $y_Q$  under what we call the "quit regime" and the firm's layoff threshold  $y_L$  under the "layoff" regime.

**Quit regime** Using equation bellman equations (A2.1)-(A2.2), value matching conditions (A2.3)-(A2.4) and the smooth pasting condition  $E'(y_Q) = 0$ , we can solve anatically for  $y_Q$  :

$$y_Q = \frac{\gamma}{\gamma + 1} \frac{\rho + \delta - \mu}{\rho + \delta} \frac{1}{\phi} [(\rho + \delta)Q(\alpha) - \phi\alpha - \delta U(\alpha)]$$

where :

$$\gamma = -\frac{(1 - \frac{2\mu}{\sigma^2}) - \sqrt{(1 - \frac{2\mu}{\sigma^2})^2 + 4\frac{2(\rho+\delta)}{\sigma^2}}}{2}$$

is the absolute value of the negative root associated to the two Bellman equations' characteristic polynomial. See Dixit (1989) for a full treatment.

We now need to verify the following boundary condition on the firm side : the unique solution for  $J$  verifying  $J(y_Q) = 1/\rho$  also verifies that for all  $y > y_Q$ ,  $J(y) \geq \frac{1}{\rho} - F$ . Intuitively, it means that the firm has no interest in laying off a worker above the threshold for which the worker will quit. In this case, it means that the "quit" strategy is valid. For all  $y \geq y_Q$  the value of the firm can be written as :

$$J(y) = B_J y^{-\gamma} + J_P(y)$$

where  $B_J$  is a constant and

$$J_P(y) = \frac{1 - \phi}{\rho + \delta - \mu} y + \frac{(1 - \phi)\alpha}{\rho + \delta}$$

is the permanent value associated to match productivity  $y$ . The value function  $J$  is pinned down to the continuity condition :

$$J(y_Q) = \frac{1}{\rho}$$

Combining these two expressions we can solve for  $B_J$  as :

$$B_J = y_Q^\gamma \left( \frac{1}{\rho} - J_P(y_Q) \right)$$

which pins down our candidate  $J$ . We now have to check under which condition  $J(y) > \frac{1}{\rho} - F$ . This is trivially verified for  $y \leq y_Q$  where  $J(y) = \frac{1}{\rho}$ . On  $]0, +\infty[$ , generic solution to the firm's Bellman equation will start to decrease, reach a minimum and increase to infinity as long as  $\phi < 1$ . A sufficient condition for  $J$  to be greater than the firm's layoff outside option hence is that :

$$J'(y_Q) \geq 0$$

This will be the case if and only if :

$$-\gamma B_J y_Q^{-\gamma-1} + \frac{1-\phi}{\rho+\delta-\mu} \geq 0$$

$$-\gamma y_Q^\gamma \left( \frac{1}{\rho} - J_P(y_Q) \right) y_Q^{-\gamma-1} + \frac{1-\phi}{\rho+\delta-\mu} \geq 0$$

$$\frac{1-\phi}{\rho+\delta-\mu} y_Q \geq \gamma \left( \frac{1}{\rho} - J_P(y_Q) \right)$$

$$\frac{1-\phi}{\rho+\delta-\mu} y_Q \geq \gamma \left( \frac{1}{\rho} - \frac{1-\phi}{\rho+\delta-\mu} y_Q - \frac{(1-\phi)\alpha}{\rho+\delta} \right)$$

$$\frac{1-\phi}{\rho+\delta-\mu} y_Q \geq \frac{\gamma}{(1+\gamma)} \left( \frac{1}{\rho} - \frac{(1-\phi)\alpha}{\rho+\delta} \right)$$

$$y_Q \geq \frac{\gamma}{(1+\gamma)} \frac{\rho+\delta-\mu}{1-\phi} \left( \frac{1}{\rho} - \frac{(1-\phi)\alpha}{\rho+\delta} \right)$$

$$\frac{\gamma}{\gamma+1} \frac{\rho+\delta-\mu}{\rho+\delta} \frac{1}{\phi} [(\rho+\delta)Q(\alpha) - \phi\alpha - \delta U(\alpha)] \geq \frac{\gamma}{(1+\gamma)} \frac{\rho+\delta-\mu}{1-\phi} \left( \frac{1}{\rho} - \frac{(1-\phi)\alpha}{\rho+\delta} \right)$$

$$\frac{1}{\phi} [(\rho+\delta)Q(\alpha) - \phi\alpha - \delta U(\alpha)] \geq \frac{1}{1-\phi} [(\rho+\delta)\frac{1}{\rho} - (1-\phi)\alpha]$$

Which simplifies to :

$$\frac{(\rho + \delta)Q(\alpha) - \delta U(\alpha)}{\phi} \geq \frac{(\rho + \delta) 1}{1 - \phi \rho}$$

As long as  $\lim_{\alpha \rightarrow \infty} \frac{(\rho + \delta)Q(\alpha) - \delta U(\alpha)}{\phi} > \frac{(\rho + \delta) 1}{1 - \phi \rho}$  there will be an  $\alpha_Q$  such that the sufficient condition  $J'(y_Q)$  will be verified for all  $\alpha > \alpha_Q$ .

**[Now need a condition which says that for some  $\alpha$  low enough the minimum of  $J$  will go below the firm's layoff outside option.]**

The minimum of a solution to the firm's Bellman equation is attained for  $y_J$  such that  $J'(y_J) = 0$  :

$$y_J = \left[ \frac{1 - \phi}{\rho + \delta - \mu} \frac{1}{\gamma B_J} \right]^{\frac{-1}{1 + \gamma}}$$

What's more we know that for  $\alpha$  small enough (i.e.  $\alpha < \alpha_Q$ ) then  $y_J > y_Q$  and value of this minimum is  $\underline{J}$  :

$$\underline{J} = B_J \left[ \frac{1 - \phi}{\rho + \delta - \mu} \frac{1}{\gamma B_J} \right]^{\frac{\gamma}{1 + \gamma}} + J_P \left( \left[ \frac{1 - \phi}{\rho + \delta - \mu} \frac{1}{\gamma B_J} \right]^{\frac{-1}{1 + \gamma}} \right)$$

As  $y_Q \mapsto 0$  when  $\alpha \mapsto 0$ , so does  $B_J$  and so does  $\underline{J}$ . Hence, there exists  $\underline{\alpha}$  such that for all  $\alpha < \underline{\alpha}$  the quit equilibrium is not sustainable — i.e. such that  $J(y) < \frac{1}{\rho} - F$  for some  $y > y_Q$ .

**Layoff regime** Using equation bellman equations (A2.1)-(A2.2), value matching functions (A2.5)-(A2.6) and the smooth pasting condition  $J'(y_L) = 0$ , we can solve anatically for  $y_L$  :

$$y_L = \frac{\gamma}{\gamma + 1} \frac{\rho + \delta - \mu}{\rho + \delta} \frac{1}{1 - \phi} \left[ (\rho + \delta) \left( \frac{1}{\rho} - F \right) - (1 - \phi) \alpha \right]$$

where :

$$\gamma = - \frac{\left( 1 - \frac{2\mu}{\sigma^2} \right) - \sqrt{\left( 1 - \frac{2\mu}{\sigma^2} \right)^2 + 4 \frac{2(\rho + \delta)}{\sigma^2}}}{2}$$

is the absolute value of the negative root associated to the two Bellman equations' characteristic polynomial.

We now need to verify that, at this threshold, given the value matching condition we impose on the worker side, the boundary condition on  $E$  holds : the solution for  $E$

pinned down by  $E(y_L) = U(\alpha) + F$  also verifies that for all  $y > y_L$ ,  $E(y) \geq Q(\alpha)$ . Intuitively, it is not profitable for the worker to quit the firm before the firm laying off the worker, such that the "layoff" strategy is valid. For all  $y \geq y_L$  the value of the worker can be written as :

$$E(y) = B_E y^{-\gamma} + \frac{\phi}{\rho + \delta - \mu} y + \frac{\phi\alpha + \delta U(\alpha)}{\rho + \delta}$$

where  $B_E$  is a constant.  $E$  is subject to the value matching condition :

$$E(y_L) = U(\alpha) + F$$

Combining these two expressions we can solve for  $B_E$  as :

$$B_E = y_L^\gamma (U(\alpha) + F - E_P(y_L))$$

where

$$E_P(y_L) = \frac{\phi}{\rho + \delta - \mu} y_L + \frac{\phi\alpha + \delta U(\alpha)}{\rho + \delta}$$

is the permanent value associated to match productivity  $y_L$ . The condition  $E'(y_Q) \geq 0$  now writes :

$$-\gamma B_E y_L^{-\gamma-1} + \frac{\phi}{\rho + \delta - \mu} \geq 0$$

$$\frac{\phi}{\rho + \delta - \mu} \geq \gamma B_E y_L^{-\gamma-1}$$

$$\frac{\phi}{\rho + \delta - \mu} \geq \gamma y_L^\gamma (U(\alpha) + F - E_P(y_L)) y_L^{-\gamma-1}$$

$$\frac{\phi}{\rho + \delta - \mu} y_L \geq \gamma (U(\alpha) + F - E_P(y_L))$$

$$\frac{\phi}{\rho + \delta - \mu} y_L \geq \gamma (U(\alpha) + F - \frac{\phi}{\rho + \delta - \mu} y_L - \frac{\phi\alpha + \delta U(\alpha)}{\rho + \delta})$$

$$\frac{\phi}{\rho + \delta - \mu} y_L \geq \frac{\gamma}{1 + \gamma} (U(\alpha) + F - \frac{\phi\alpha + \delta U(\alpha)}{\rho + \delta})$$

$$y_L \geq \frac{\gamma}{1+\gamma} \frac{\rho + \delta - \mu}{\phi} (U(\alpha) + F - \frac{\phi\alpha + \delta U(\alpha)}{\rho + \delta})$$

$$\frac{\gamma}{\gamma+1} \frac{\rho + \delta - \mu}{\rho + \delta} \frac{1}{1-\phi} [(\rho + \delta)(\frac{1}{\rho} - F) - (1-\phi)\alpha] \geq \frac{\gamma}{1+\gamma} \frac{\rho + \delta - \mu}{\phi} (U(\alpha) + F - \frac{\phi\alpha + \delta U(\alpha)}{\rho + \delta})$$

$$\frac{\gamma}{\gamma+1} \frac{\rho + \delta - \mu}{\rho + \delta} \frac{1}{1-\phi} [(\rho + \delta)(\frac{1}{\rho} - F) - (1-\phi)\alpha] \geq \frac{\gamma}{1+\gamma} \frac{\rho + \delta - \mu}{\rho + \delta} \frac{1}{\phi} ((\rho + \delta)(U(\alpha) + F) - \phi\alpha - \delta U(\alpha))$$

$$\frac{1}{1-\phi} [(\rho + \delta)(\frac{1}{\rho} - F) - (1-\phi)\alpha] \geq \frac{1}{\phi} ((\rho + \delta)(U(\alpha) + F) - \phi\alpha - \delta U(\alpha))$$

Which simplifies to :

$$\frac{(\rho + \delta)(\frac{1}{\rho} - F)}{1-\phi} \geq \frac{\rho U(\alpha) + (\rho + \delta)F}{\phi}$$

Let  $\rho U(0)$  the flow level of utility reached by lowest type workers when they become unemployed. Under the assumption that these baseline unemployment benefits are not too high :

$$\rho U(0) < \frac{\phi}{1-\phi} (\rho + \delta)(\frac{1}{\rho} - F) - (\rho + \delta)F$$

then there will exist an  $\alpha_L > 0$  such that the sufficient condition for a layoff equilibrium to exist  $E'(y_L) \geq 0$  will be met for all  $\alpha < \alpha_L$

**[Now need a condition which says that for some  $\alpha$  high enough the minimum of  $E$  will go below the worker's quit outside option so that the .]**

The minimum of a solution to the worker's Bellman equation is attained for  $y_E$  such that  $E'(y_E) = 0$  :

$$y_E = \left[ \frac{\phi}{\rho + \delta - \mu} \frac{1}{\gamma B_E} \right]^{\frac{-1}{1+\gamma}}$$

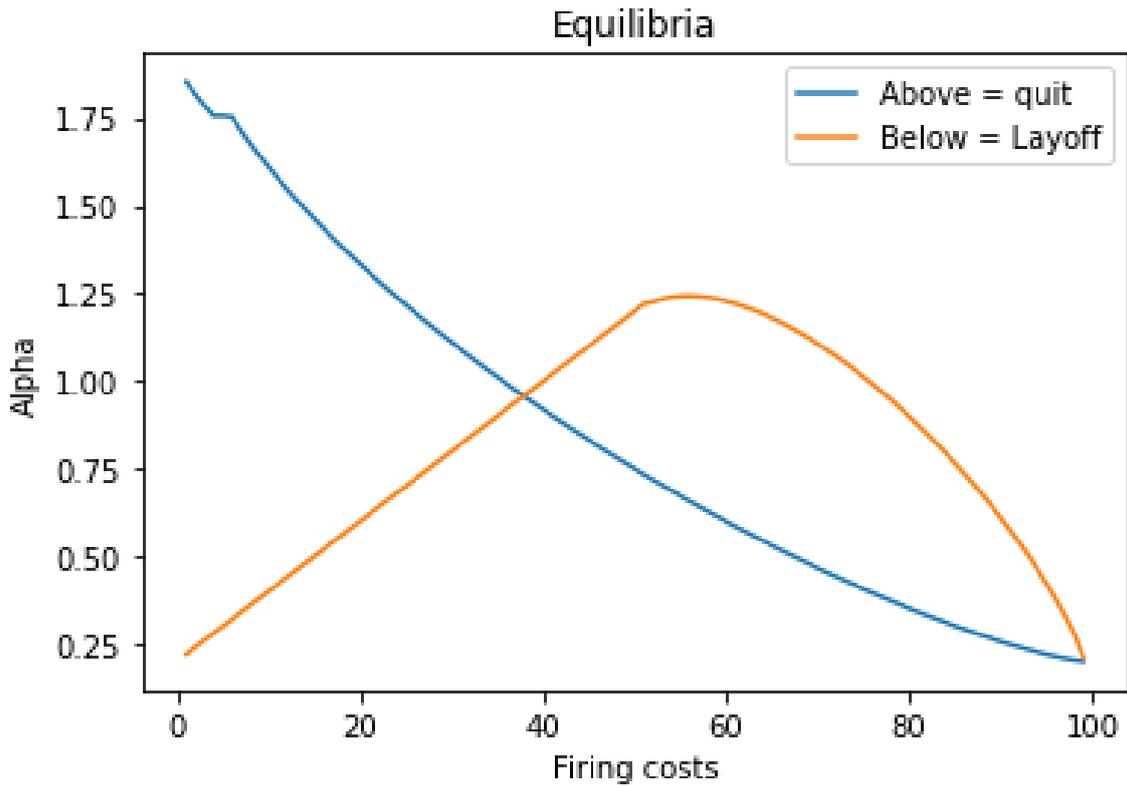
What's more we know that for  $\alpha$  high enough (i.e.  $\alpha > \alpha_L$ ) then  $y_E > y_L$  and the value of this minimum is  $\underline{E}$  :

$$\underline{E} = B_E \left[ \frac{\phi}{\rho + \delta - \mu} \frac{1}{\gamma B_E} \right]^{\frac{\gamma}{1+\gamma}} + E_P \left( \left[ \frac{\phi}{\rho + \delta - \mu} \frac{1}{\gamma B_E} \right]^{\frac{-1}{1+\gamma}} \right)$$

As  $y_L$  reaches 0 when  $\alpha \mapsto (\rho + \delta)(\frac{1}{\rho} - F)/(1 - \phi)$ , so does  $B_E$  and so does  $\underline{E}$ . Hence, there exists  $\bar{\alpha}$  such that for all  $\alpha > \bar{\alpha}$  the layoff equilibrium is not sustainable — i.e. such that the worker would rather quit  $E(y) < Q(\alpha)$  at some  $y > y_L$  instead of waiting to get fired.

## Calibration

FIGURE A2.2 – POSSIBLE EQUILIBRIA AS FUNCTIONS OF FIRING COSTS



**Notes :** This graph illustrates the possible equilibria that may arise under a simple calibration of our model at different levels of firing costs. More specifically we plot  $\underline{\alpha}$  (blue) and  $\bar{\alpha}$  (orange) for values of firing costs ranging from 0 to 100. At any given level of firing costs  $F$ , worker types above the blue line may quit in equilibrium, while all worker type below the orange line may be fired as an equilibrium outcome. We take  $\rho = 0.01$ ,  $\mu = 0.01$ ,  $\phi = 0.5$ ,  $\delta = 0.1$ ,  $Q(\alpha) = \phi\alpha/\rho$  and  $U = 10$  constant.

## A2.3 Empirical Strategy

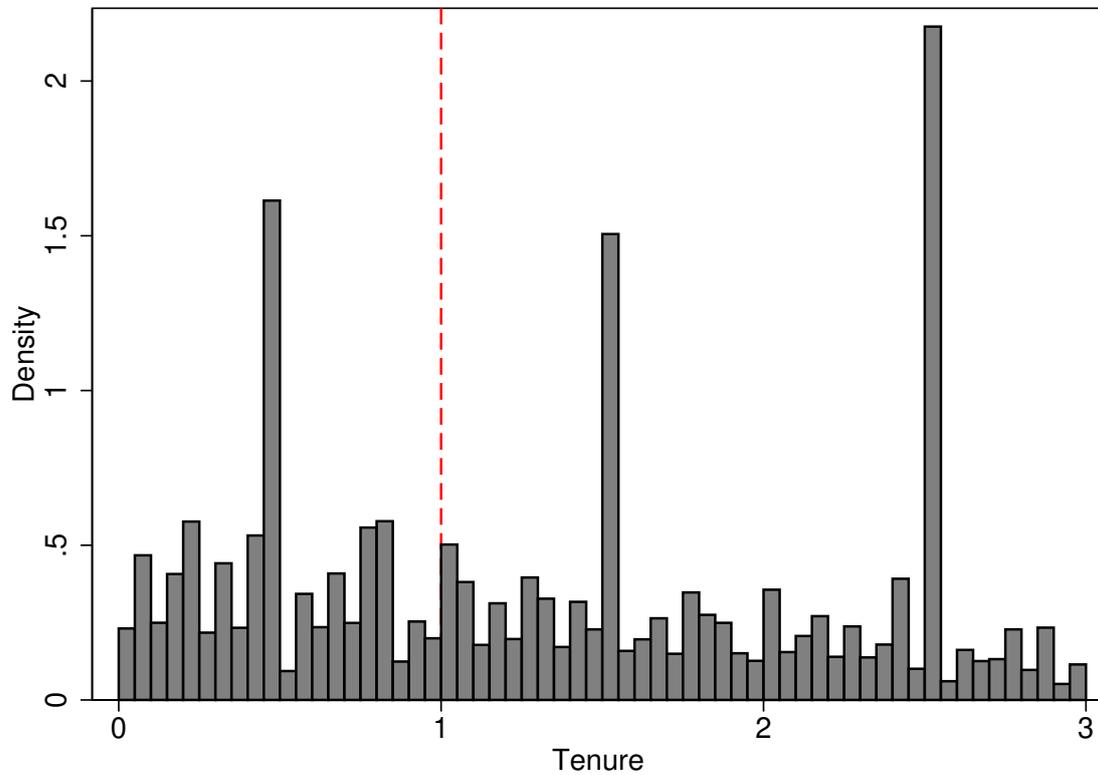
## A2.4 Difference in discontinuity

$$Y_{it} = \alpha_i + \alpha_t + D_{it} [tenure \in [1,2]] + \sum_{h=-5}^4 \beta_h \cdot 1 [t_0 + h = t] \cdot D_{it} [tenure \in [1,2]] + \varepsilon_{it}$$

restricting to the population with  $tenure \in [0,2]$ .

## A2.5 Regression discontinuity

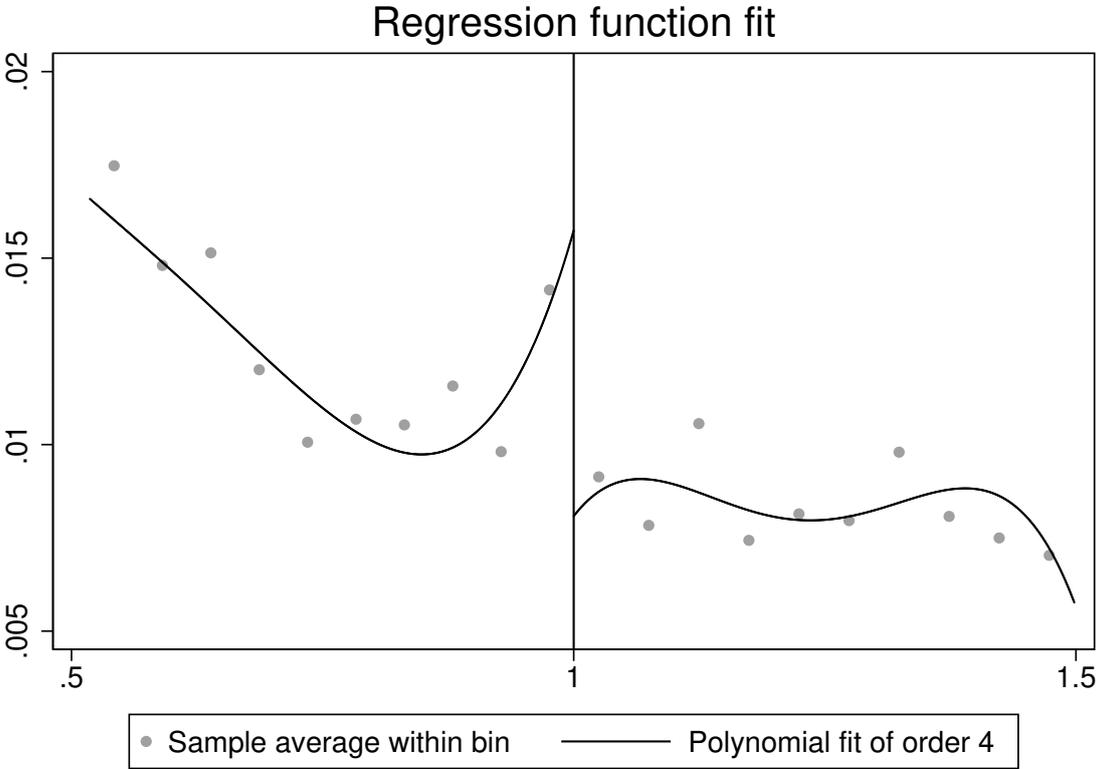
FIGURE A2.3 – DISTRIBUTION OF THE RUNNING VARIABLE AROUND THE ONE YEAR THRESHOLD



Notes : This graph plots the density of tenure, our running variable, for workers employed under an indefinite duration contract on June 27th 2008. The spikes at 0,5, 1,5 and 2,5 come from contracts beginning on the first of January.

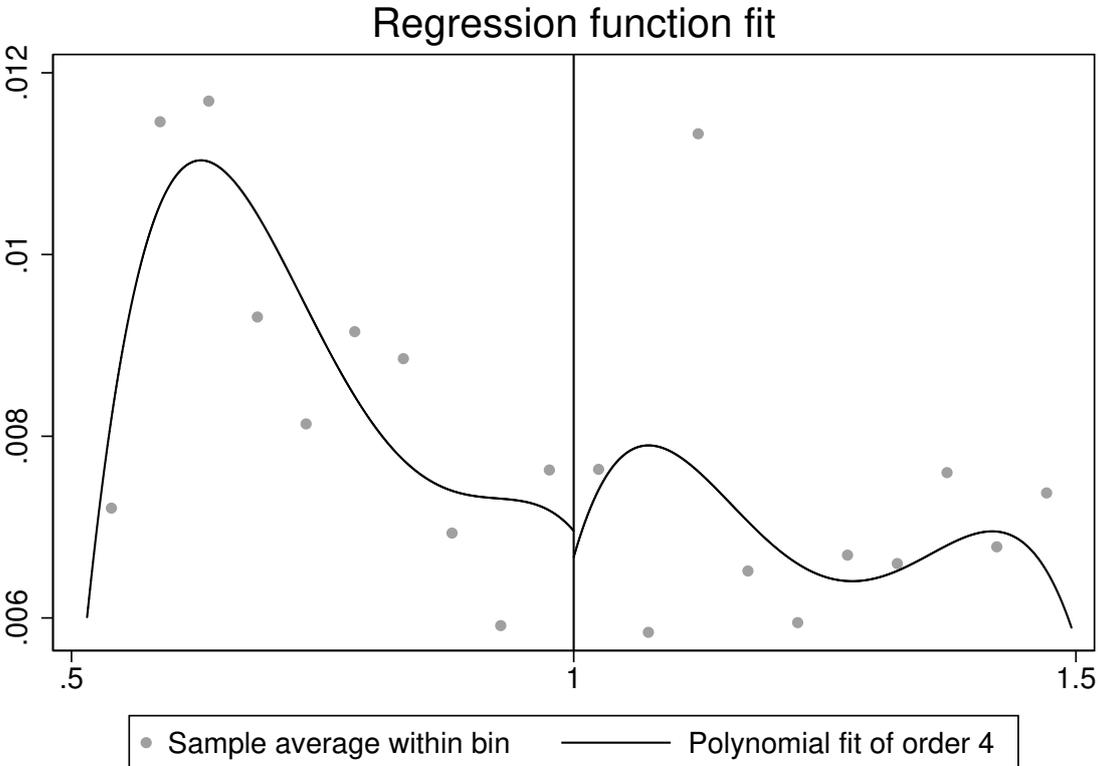
## A2.6 The timing of quits and layoffs : no firm FE

FIGURE A2.4 – REGRESSION DISCONTINUITY : UNEMPLOYMENT HAZARD RATE VS TENURE.



Notes : This graph plots one month unemployment rate against tenure for workers employed under an indefinite duration contract on June 27th 2008 as well as separate fourth order polynomial fits on the left and right of the one year tenure threshold.

FIGURE A2.5 – PLACEBO REGRESSION DISCONTINUITY : UNEMPLOYMENT HAZARD RATE VS TENURE.



Notes : This graph plots one month unemployment rate against tenure for workers employed under an indefinite duration contract on June 27th 2007 as well as separate fourth order polynomial fits on the left and right of the one year tenure threshold.

TABLE A2.1 – THE TIMING AND NATURE OF WORKER-FIRM SEPARATIONS : NO FIRM FE

	(1)	(2)	(3)	(4)	(5)	(6)
	Early departure	Early departure	Layoff	Layoff	Quit	Quit
Compleat	-0.00838 (0.0125)	-0.00484 (0.0129)	0.0396 (0.00976)	0.0158 (0.0114)	-0.0480 (0.0106)	-0.0206 (0.0112)
Male	0.0327 (0.0102)	0.0354 (0.0115)	0.00937 (0.00920)	0.00761 (0.0106)	0.0233 (0.00848)	0.0278 (0.00954)
log(tenure)	-0.0568 (0.00495)	-0.0693 (0.00624)	-0.0391 (0.00432)	-0.0543 (0.00563)	-0.0178 (0.00423)	-0.0150 (0.00534)
log(age)	-0.0977 (0.0153)	-0.236 (0.0285)	0.0119 (0.0130)	-0.0930 (0.0257)	-0.110 (0.0131)	-0.143 (0.0258)
Eligible to sev. pay.=1	-0.0917 (0.0148)	-0.0962 (0.0174)	-0.0605 (0.0133)	-0.0637 (0.0159)	-0.0312 (0.0127)	-0.0325 (0.0152)
log(hourly wage)	-0.00747 (0.0129)		-0.123 (0.0109)		0.116 (0.0102)	
$\hat{\epsilon}_{i,t}$		-0.0368 (0.0204)		-0.244 (0.0196)		0.207 (0.0163)
$\hat{\alpha}_i$		0.0675 (0.0219)		-0.0199 (0.0180)		0.0874 (0.0179)
$\hat{\beta}'X_{i,t}$		0.496 (0.102)		0.308 (0.0989)		0.188 (0.0943)
Obs	15722	12134	15722	12134	15722	12134
F	115.1	78.76	86.32	62.62	71.68	53.98
Mean	0.477	0.466	0.235	0.222	0.242	0.244
Adjusted R2	0.208	0.216	0.0640	0.0733	0.150	0.163

**Notes :** This table reports the results of six different OLS regressions. Models (1) and (2) take an early departure dummy (worker departing prior to the bankruptcy filing) as the dependent variable. Models (3) and (4) replace the early departure dummy by an early layoff one and models (5) and (6) by an early quit dummy. All models control for year and 2-digits occupations fixed effects. Standard errors are clustered at the origin firm level and are reported in parentheses.

## Chapitre 3

# Directing job search : a large scale experiment.

*Joint with L. Behaghel (PSE), S. Dromundo (OECD), M. Gurgand (PSE) and Y. Hazard (PSE).*

## 3.1 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.<sup>1</sup> 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

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1. Relevant 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.

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 comparison 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 3.2, we provide background information on LBB's job search platform. Section 3.3 presents the experimental design. Results are given in Section 3.4 and Section 3.5 concludes.

## 3.2 Context

### 3.2.1 "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 A3.1) and, using an algorithm based on past recruitment data, LBB proposes a list of firms likely to hire them (see Figure A3.2). Once they "click" on a firm of interest an email address and/or phone number to contact the firm directly is given (see Figure A3.3). 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.<sup>2</sup> LBB then defines for each occupation a specific predicted hiring threshold above which an establishment is dee-

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2. This 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/>).

med a "hiring firm" for this specific occupation.<sup>3</sup> 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 A3.6 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.

### 3.2.2 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 classification<sup>4</sup> 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 A3.5 of Appendix A3.2, our measure

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

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

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.

## 3.3 The Experiment

### 3.3.1 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 zones.<sup>5</sup> Our experimental sample covers 94 out of the 404 French commuting zones,<sup>6</sup> 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.

#### 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

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5. When 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 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.

6. We 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 A3.3.

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.

### **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 recommen-

ded 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 3.1.

TABLE 3.1 – TREATMENT ARMS AND RECOMMENDATIONS TYPES

		<b>Job-seekers</b>				<b>Establishments</b>		
		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	

### 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.

## 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 A3.3 in appendix, job seekers just needed to click on a "Send an application" (in French "Poster") icon which appeared on the right hand side of the contact information page.

As can be seen in Table 3.2 below or Figure A3.4 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 3.2 below.

TABLE 3.2 – 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,

### 3.3.2 Randomization in practice

#### 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.<sup>7</sup> After

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7. While our experiment started on the 19/11/2019 we could only access administrative data which

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 months.<sup>8</sup>

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.<sup>9</sup> 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 3.3. 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 initially 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.

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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.

8. The 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.

9. We 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 through 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 and treatment groups (and consequently statistical power).

TABLE 3.3 – 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.

## 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.<sup>10</sup> 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 3.4. 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,<sup>11</sup> 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 3.4.2 and appendix A3.8.

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10. We 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.

11. We 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 3.4 – 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.

## 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 3.5, 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 3.5 – 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 3.6. 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 3.6 – 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.

### 3.4 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 see-

kers who were still unemployed when our experiment began (19/11/2019).<sup>12</sup> 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 3.3 above, the pre-intervention attrition rate is 34% and not significantly different in the treatment and control groups.

### 3.4.1 Job seekers

#### Take-up

Table 3.7 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 3.7 – TAKE-UP MEASURES

	mean	sd	count
Received email	0.96	0.19	533695
Opened email	0.64	0.48	533695
Clicked on link	0.25	0.43	533695
Click if opened email	0.36	0.48	340945
Total clicks if click	2.99	3.02	130946
Distinct clicks if click	1.95	1.09	130946
Application if click	0.27	0.44	10082

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

12. Because 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 3.3 we do not detect any significant unbalance in our treatment/control groups with respect to this particular dimension.

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 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 for the application rate of a highly selected set of workers.<sup>13</sup> 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.

## 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 :

$$y_{ic} = \alpha_1 + \beta_1 Z_{ic} + \epsilon_{ic} \quad (3.1)$$

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

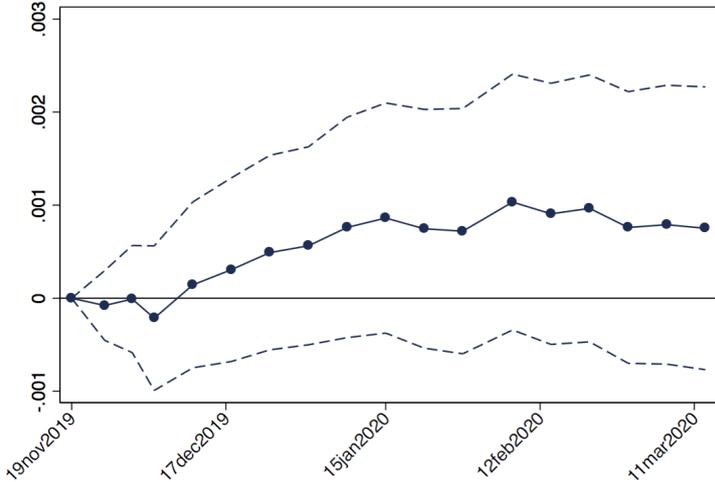
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13. Among treated workers who clicked the particular set of workers which were connected to PES' online application service while clicking on our links were 18.7% more likely to find a job within three months.

control individuals from treated commuting zones,  $\beta$  is therefore our "intention to treat" estimate from the job-seeker's side.

Figure 3.1 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 not statistically different from zero at the 5% confidence level.

FIGURE 3.1 – JOB-FINDING ITT ESTIMATES

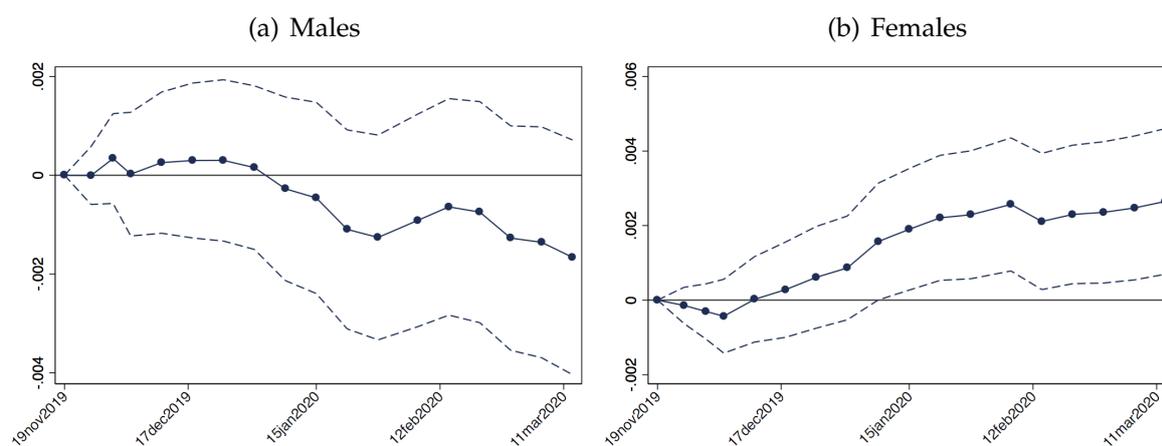


Note : This graph presents the ITT estimates for job finding at different time horizons. Sample restricted to job seekers who were still unemployed as of 19/11/2019. Standard errors are clustered at the labor market (Occ.\*CZ) level and associated 95% confidence intervals are displayed.

*Gender differences in job seekers' responses*

Hidden under the general picture given by Figure 3.1, the respective responses of males and females to our intervention differ markedly. As can be seen in Figure 3.2 which depicts the counterpart of Figure 3.1 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.

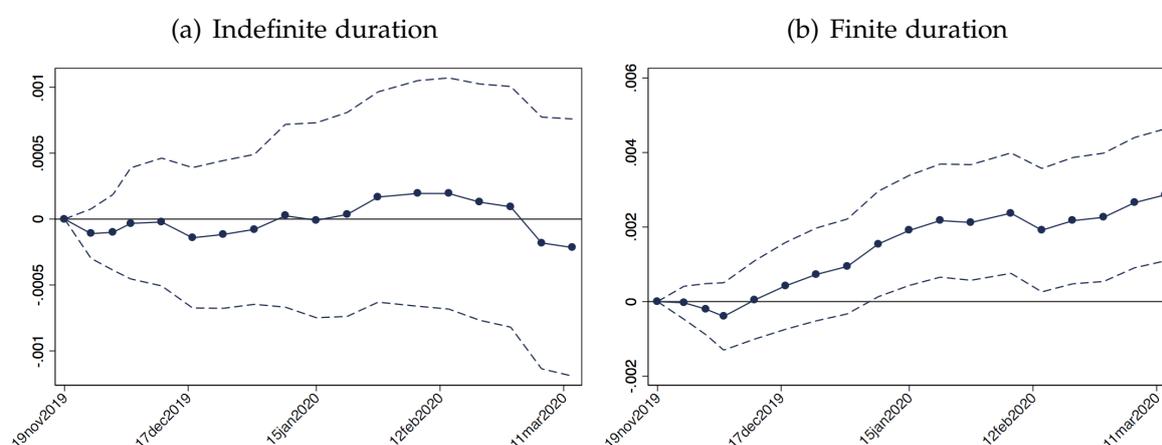
FIGURE 3.2 – JOB-FINDING ITT ESTIMATES BY GENDER



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 3.3), we find that the positive effect of our intervention is driven by a rise in treated women’s return to definite duration employment.<sup>14</sup>

FIGURE 3.3 – 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.

14. A 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.

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 3.8. 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.

TABLE 3.8 – 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.

**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 3.9 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 3.9 – 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 [A3.4](#) and [A3.5](#) in the appendix.

Outcomes are measured about two months after the emails were sent. Table [3.10](#) 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 [3.10](#) 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 3.10 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.<sup>15</sup> 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 A3.6 uses the bounding methods proposed by Lee (2009) and Behaghel et al. (2015) to correct for possible sample selectivity bias.<sup>16</sup> Overall, the bounding approaches provide evidence that the results of Table 3.10 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 3.10 is therefore a possible explanation for the differential effect of the intervention on job finding rates by gender.

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

16. Lee 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 A3.8, 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 3.10 – IMPACT OF EMAILING ON INTERMEDIARY OUTCOMES

	Women			Men			p-value different ITT
	Mean control	ITT (se)	p-value	N	Mean control	ITT (se)	
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
# Internet search channels used	2.43	-0.01 (0.09)	0.95	2447	2.53	0.04 (0.11)	0.74
Used LBB	0.20	0.06 (0.02)	0.02	2421	0.21	0.05 (0.03)	0.12
Responded to job ads	0.53	-0.01 (0.03)	0.69	2374	0.55	-0.02 (0.04)	0.63
# job ads responded	4.35	-0.31 (0.57)	0.59	2313	5.40	-0.40 (0.87)	0.65
Made spontaneous application	0.49	-0.01 (0.03)	0.65	2280	0.54	0.00 (0.04)	0.97
# spontaneous applications	3.54	0.33 (0.55)	0.55	2256	4.23	-0.09 (0.73)	0.90
Applied in other occupation	0.48	0.02 (0.03)	0.54	2213	0.52	-0.10 (0.04)	0.02
# hours searched per week	7.92	-0.04 (0.75)	0.96	2057	9.28	0.27 (0.98)	0.78
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
# calls for interviews	0.80	0.08 (0.13)	0.53	2191	1.15	-0.30 (0.15)	0.05
Received offer	0.24	0.00 (0.03)	0.99	2188	0.22	0.02 (0.04)	0.55
Accepted offer	0.15	0.01 (0.02)	0.62	2077	0.14	0.03 (0.03)	0.34
C. Response to survey							
Responded to survey	0.31	-0.06 (0.01)	0.00	6587	0.22	0.01 (0.02)	0.40
							0.00

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

Source : Survey see [A3.4](#)

### *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 3.11. Among males (Panel A in Table 3.11), 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.<sup>17</sup>

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17. One 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 3.11 – 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.

### 3.4.2 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.

#### 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 A3.7](#) and [Table 3.12](#). [Figure A3.7](#) 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 3.4.1](#)) 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.<sup>18</sup>

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 3.12 compares this overall number of regular clicks per establishment in the treated 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 A3.2 and A3.3 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.

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18. Note 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%.

TABLE 3.12 – 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.

### 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 identifier<sup>19</sup>, 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.

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

Because of the small but significant imbalances between treatment and control firms with respect to pre-treatment establishments' hirings demonstrated in Table A3.8, all the results we present include control dummies for hirings that occurred between May 1<sup>st</sup>, 2019, the earliest date for which we obtained data on individual level hirings, and November 19<sup>th</sup>, 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.<sup>20</sup> 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 A3.8. 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 :

$$N_{jc} = \alpha_2 + \beta_2 T_{jc} + \gamma \theta_{jc} + \kappa(\theta_{jc} * T_{jc}) + \omega_{jc} \quad (3.2)$$

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).  $\theta_{jc}$  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.  $\beta_2$  is therefore our "intention to treat" estimate at the establishment level. As shown 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 3.13 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).

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20. When 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.

When we consider indefinite duration contracts and definite duration contracts separately (Columns 2 and 3), however, the picture is quite different. Table 3.13 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 A3.10 presents these results).

TABLE 3.13 – ESTABLISHMENTS' ITT ESTIMATES FOR TOTAL HIRINGS BY CONTRACT TYPE

	(1)	(2)	(3)
	All	Indefinite	Definite
ITT	-0.0199	0.0616	-0.00507
	(0.556)	(0.0356)	(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 A3.9 in appendix).

### 3.4.3 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 3.14 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 3.14 – 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 (0.0356)	0.0483 (0.0615)	0.0816 (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 3.15 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 3.15 – 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 R2	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.

### 3.4.4 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 :

$$y_{ic} = \alpha_3 + \beta_3 Z_{ic} P_c + \delta_3 P_c + v_{ic} \quad (3.3)$$

Where  $P_c$  is a dummy for being in any treated commuting zone. In this specification,  $\beta_3$  is the difference between those assigned to treatment and those who are in treatment zones but are not themselves assigned to treatment and  $\delta_3$  is the effect of being untreated in a treated commuting zone compared to being untreated in an untreated zone.  $\delta_3$  is thus the estimate that informs us about the existence of crowding-out effects.

Table 3.16 runs this regression only for females, with  $y_{ic}$  corresponding to job finding on definite duration contracts. We observe that  $\delta_3$  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 3.16 – JOB-SEEKERS’ CROWDING OUT EFFECT FOR FEMALE IN DEFINITE DURATION CONTRACTS

	(1)
	Definite Duration Contract
Treated ( $\beta$ )	0.258 (0.108)
In a Treated Commuting Zone ( $\delta$ )	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' side<sup>21</sup>.

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 3.17 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

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21. Because of our intervention treated job seeker's are more likely to use LBB than control job seekers (see Table 3.10). 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 3.3).

target of LBB.

TABLE 3.17 – 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.

### 3.5 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.

## A3 Appendix

### A3.1 Context

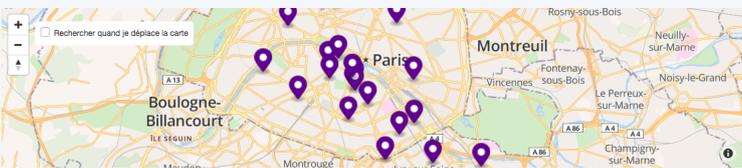
FIGURE A3.1 – LBB’S HOME PAGE



FIGURE A3.2 – LBB’S RESEARCH RESULTS PAGE

Enseignement supérieur Paris 75001

181 entreprises sont susceptibles de recruter en Enseignement supérieur autour de Paris



**UNIVERSITEPARIS1PANTHEON-SORBONNE - PARIS-05**  
 Enseignement supérieur  
 500 à 999 salariés  
 2.2 km de votre lieu de recherche  
 Potentiel d'embauche ★★★★★

**UNIVERSITE PARIS DIDEROT - PARIS 7 - PARIS-13**  
 Enseignement supérieur  
 250 à 499 salariés  
 Potentiel d'embauche ★★★★★

FIGURE A3.3 – 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

<b>Raison sociale</b> UNIVERSITE PARIS 1 PANTHEON SORBONNE	<b>Contact</b> <a href="mailto:racbiatss@univ-paris1.fr">racbiatss@univ-paris1.fr</a> 0144077918	<b>Mode de contact à privilégier</b> Envoyer un CV et une lettre de motivation
<b>Enseigne</b> UNIVERSITEPARIS1PANTHEON-SORBONNE	<b>C'est mon entreprise !</b> Modifier ces informations	<b>Informations supplémentaires</b> <a href="#">Google</a> <a href="#">Kompass</a> SIRET : 19751717000019
<b>Adresse</b> Service des ressources humaines 12 PLACE DU PANTHEON 75005 PARIS-05		



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

## FIGURE A3.4 – EMAIL SENT TO TREATED JOB SEEKERS

Bonjour M. Zuber,

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

**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.**

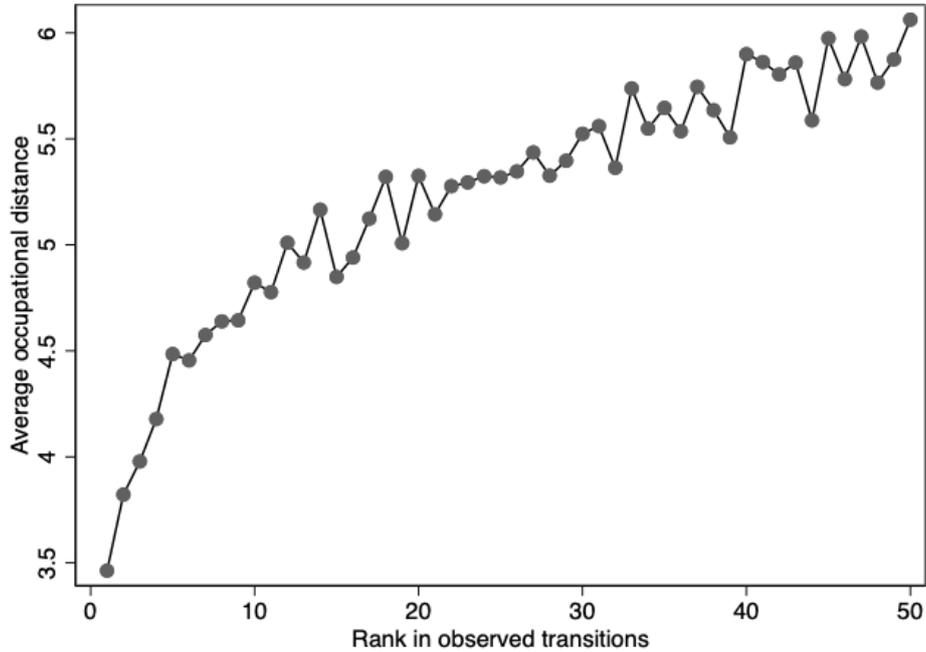
En cliquant sur ce lien, vous pourrez contacter l'entreprise grâce aux coordonnées qui s'affichent ou en utilisant l'outil de candidature en ligne « **postuler** » lorsque celui-ci est disponible.

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.

## A3.2 Occupational distance and observed transitions

FIGURE A3.5 – 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

### A3.3 Commuting zones and local labor markets

#### 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.<sup>22</sup> 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 A3.1 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.

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

TABLE A3.1 – 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.

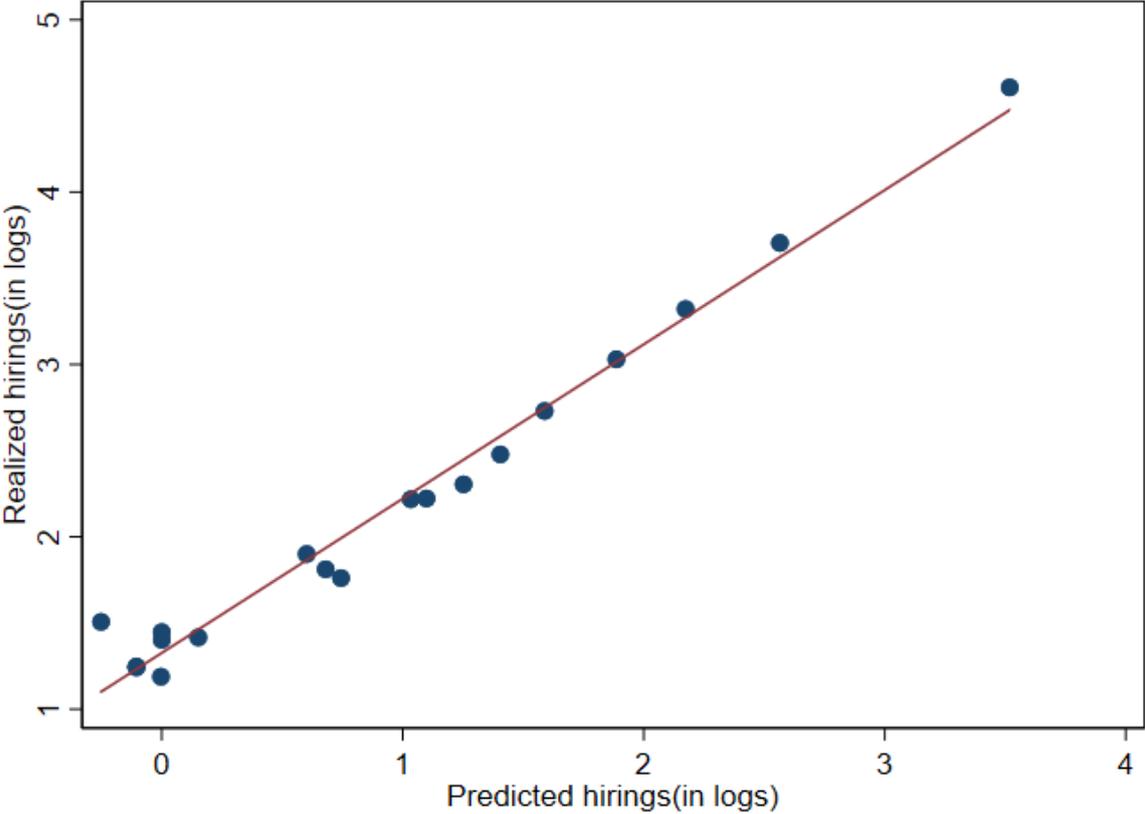
### Local Labor Markets

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 services<sup>23</sup>). 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.

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

### A3.4 Correlating predicted and realized hirings

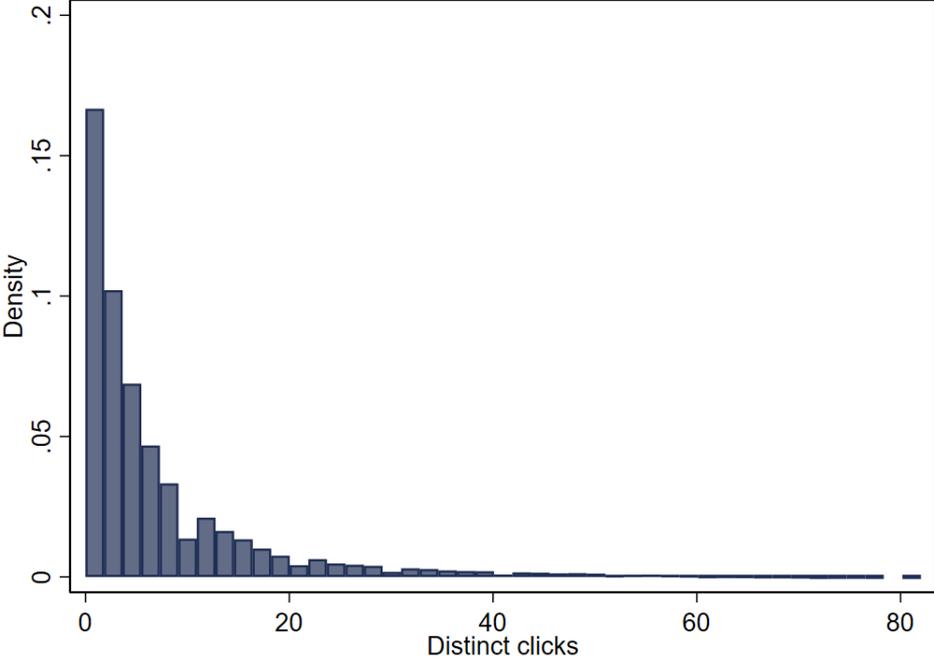
FIGURE A3.6 – REALIZED HIRINGS AMONG UNEMPLOYED JOB SEEKERS OVER THE 30/09/2019-13/03/2020 PERIOD VS LBB’S PREDICTED HIRINGS AS OF 11/08/2019 (IN LOGS)



Note : Correlation of the number of predicted hirings per establishment and the number of realized hirings.  $\text{LOG}(\text{REALIZED HIRINGS}) = 1.33(0.0053) + 0.89(0.0039) \times \text{LOG}(\text{PREDICTED HIRINGS}), R^2 = 0.37$

### A3.5 Ex-post treatment

FIGURE A3.7 – 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 A3.2 – 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 A3.3 – 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.

## A3.6 Survey Design

TABLE A3.4 – 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*<sup>24</sup>)
- Temporary employment agency websites
- Sites specialized in job offers (monster, keljob, apec...)
- "Leboncoin.fr" website<sup>25</sup>
- Professional social networks (Viadeo, LinkedIn...)

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

**[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]

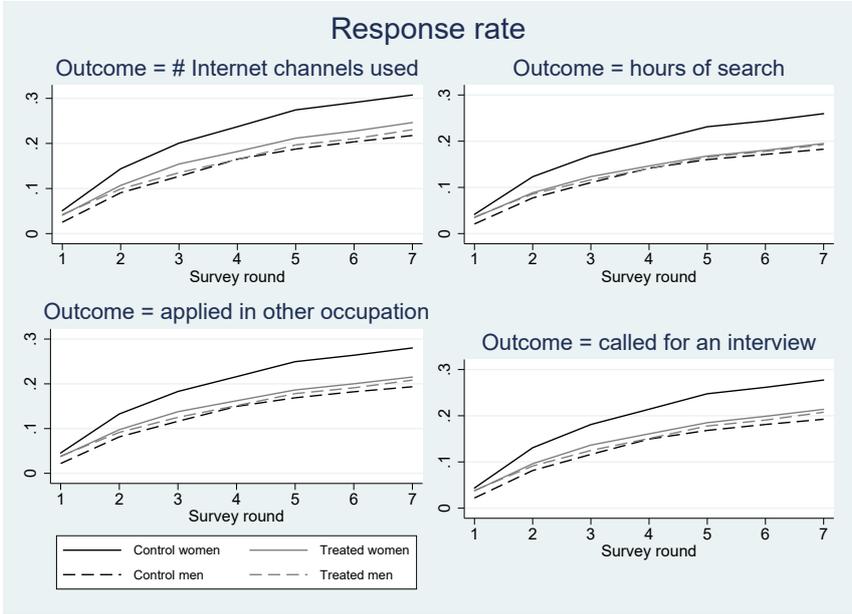
TABLE A3.5 – COMPARISON OF OBSERVABLE CHARACTERISTICS BY JOB SEEKER’S SURVEY STATUS.

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.

### A3.7 Survey evidence on job seekers’ response

FIGURE A3.8 – 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 A3.6 – IMPACT OF EMAILING ON INTERMEDIARY OUTCOMES : ROBUSTNESS 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 [Behaghel et al. \(2015\)](#) (call again) ; following [Lee \(2009\)](#) (Lee).  
Source : Survey of job seekers.

### A3.8 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 A3.7. In Table A3.8, 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 A3.7 – 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 A3.8 – 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 <sup>2</sup>	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.

### A3.9 Results on firms' effects on indefinite duration contracts by treatment arm

TABLE A3.9 – Establishments' ITT estimates for Indefinite Duration Contracts by treatment arm

	(1)	(2)	(3)
	All	20%	60%
ITT	0.0616	0.0761	0.0493
	(0.0356)	(0.0528)	(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 Contracts according to the different treatment arms since 19/11/2019 controlling for quantiles of pre-19/11/2019 indefinite durations hirings on Indefinite Duration Contracts. Standard errors are clustered at the labor market (Sector\*CZ) level.

### A3.10 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 "debiases" 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 A3.10 reports the result for the three main dependent variables considered in the paper (hirings in all, indefinite and definite contracts).

TABLE A3.10 – DOUBLY ROBUST ESTIMATION OF ITT ON FIRMS' HIRINGS

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

### A3.11 Directing job search?

Despite the fact that the specific recommendations contained in the emails do not appear to have affected the overall job finding rate of treated job seekers, it is still possible that they contributed to shift worker/firm matches away from what would otherwise have occurred.

To test this hypothesis we compare the match rate of treated versus control worker/firm among treated workers and firms. Indeed, the nature of experiment allows to compare the match level outcome of worker/firm pair which we randomly included in a recommendation email to the match level outcome of another worker/firm pair which we randomly did not include in our recommendations.

In the space if potential worker/firm recommendations among treated workers and firms denote :

- $R_{i,j} \in \{0,1\}$  the fact that we recommended firm  $j$  to job seeker  $i$
- $H_{i,j} \in \{0,1\}$  the fact that firm  $j$  hired job seeker  $i$

We are interested in the effect on  $H$  of recommending firm  $j$  to job seeker  $i$  rather than to job seeker  $i'$ .

$$R_{i,j} = 1 \text{ but } R_{i',j} = 0$$

In order to ensure independence of the recommendation choice  $R$  and match level potential outcomes, we restrict our reduced form analysis to job seeker/firm pairs to lie within the same occupation. This restriction ensures that by design :

$$P(R_{i,j} = 1) = P(R_{i',j} = 1)$$

for any two workers  $i$  and  $i'$  in firm  $j$ 's hiring occupations. Importantly, we should note that in this setting SUTVA is likely to fail. Our results should thus be interpreted conditional on the level of local labor market congestion that was created by our intervention.

The results of this exercise are reported in Table [A3.11](#) below. Column (1) shows that among pairs of treated job seekers and firms, matches which we randomly recommended were more than twice as likely to materialize in the following 3 months than matches which we randomly chose not to recommend. When we decompose this

effect by gender (second and third columns) we find that the reallocation effect of our intervention was relatively stronger for women than for men.

TABLE A3.11 – TREATED VS CONTROL WORKER/FIRM PAIRS

	$H_{i,j}$	$H_{i,j}$	$H_{i,j}$
$R_{i,j}$	0.0170 (0.00241)	0.0123 (0.00294)	0.0194 (0.00308)
Constant	0.0108 (0.000108)	0.0106 (0.000163)	0.0110 (0.000122)
Sample	All	Males	Females
$N$	29,330,163	10,035,723	19,293,546

Standard errors in parentheses.

Note : Effect of recommendations on hirings at the dyadic level and including firm fixed effects. Restricted to job seekers/firms pairs in the same occupation. Standard errors clustered at the CZ level. Reported results are in percentage points.

### A3.12 Occupational distance

How did occupational distance affect the success rate of our recommendation? To answer this question we look at the space of realized recommendations  $R_{i,j} = 1$ . Among these recommendations occupational distance  $d_{i,j}$  is randomly influenced by job seeker  $i$ 's and firm  $j$ 's treatment arms. Table A3.12 reports the effect of occupational distance on clicks, applications and hirings among treated job seekers/firm pairs and instruments the realized occupational distance  $d_{i,j}$  by each agent's treatment status ("far" or "close")

TABLE A3.12 – THE EFFECT OF OCCUPATIONAL DISTANCE ON TREATED JOB SEEKERS’ SEARCH BEHAVIOR

	(1)	(2)	(3)
	Clic	App	Hired
Distance	-0.00456 (0.000496)	-0.00503 (0.00394)	-0.0000839 (0.0000391)
Constant	0.111 (0.000364)	0.0805 (0.00247)	0.000654 (0.0000293)
F	103028.1	1285.4	103028.1
Mean	0.108	0.0779	0.000603
% effect	-4.23	-6.45	-14.14
N	2320124	36596	2320124

Standard errors in parentheses

Note : Effect of recommendations on clicks, applications and hirings at the dyadic level. Instrumented by each job seeker and firm treatment status. Restricted to realized job seekers/firms recommendations clustered at the CZ level.

The results displayed in Table A3.12 show that being one occupation away from one’s own occupation decrease the click rate by 4.24%, the application rate by 6.45% and the final hiring rate in the recommended match by 14.4%. Whereas the click and hiring effects are precisely estimated the effect on application is not because of smaller sample size (we can only observe the application behavior of small selected subset of job seekers who were connected to Pôle emploi’s website and chose to apply through our recommendation link). The fact that the negative effect of occupational distance increases markedly between the application and hiring columns shows that firms may be reluctant to hire workers from another occupation.

### A3.13 Controlling for occupational gender differences

TABLE A3.13 – THE EFFECT OF OCCUPATIONAL DISTANCE ON TREATED JOB SEEKERS' SEARCH BEHAVIOR

	(1)	(2)	(3)
Treated	0.00287 (0.00108)	0.00375 (0.00155)	0.00373 (0.00162)
Male	0.0347 (0.00244)	0.00843 (0.00270)	-0.00242 (0.00239)
Treated*Male	-0.00328 (0.00172)	-0.00503 (0.00250)	-0.00451 (0.00258)
Sample	All	Identified	Identified
Controls	None	None	3dgt occupations
N	800297	398538	398538
F	75.88	4.013	2970.7
Mean	0.154	0.159	0.159
Adjusted R2	0.00201	0.0000539	0.182

Standard errors in parentheses

Note : The effect of treatment on definite duration employment interacted with gender. Column (1) reports our baseline result. Column (2) restrict the samples to 3dgt occupation for which both the male and female treatment effect is identified on a  $[0.2, 0.8]$  support. Column (3) use this identified sample to interact the full heterogeneous model with a set of demeaned 3dgt occupational dummies (coefficients not reported). Standard errors are clustered at the CZ\*Occ level.

## A3.14 Forming pairwise recommendations

### Introducing random treatments into a matching problem

This appendix describes the exact design we used to form pairwise recommendations linking each treated worker to some specific treated firm. After the randomization step described in subsection 3.3.1 we are left with some treated job seekers and treated firms in each one of the four treatment arms : **few/close**, **many/close**, **few/far** and **many/far**.

In practice all treated job seekers who were assigned the **few** treatment status received 4 recommendations while job seekers who were assigned the **many** treatment status received 8. Given these numbers we needed to find as many firms to recommend as was required for each job seeker. One solution could have been to randomly as many recommendations as were required (4 or 8) among treated firms. The drawback of proceeding in such a way would have been that close/far distinction and, on the firms' side, the few/many one, would have disappeared from our ex-post treatment altogether. A second drawback of using a completely random allocation of job seekers to firms would have been to create even more noise than existed already on the labor market, hence contributing to increase rather than decrease search frictions. Rather than proceed in a completely random fashion we thus needed a way to randomly draw pairwise recommendations among a set of *acceptable* worker/firm pairs. Here *acceptable* meant first that our recommendations should not have distorted "too much" the "natural" matching of job seekers and firms absent the intervention — we did not want to systematically recommend bakers to banks while sending bankers to bakeries — and second that our recommendations should have reproduced to the greatest possible extent each agent's random treatment status. This meant recommending would be bakers of the *close* treatment arm to the bakeries which happened to belong to the close treatment arm themselves, while sending bakers of the *far* treatment arm to banks belonging to *far* treatment arm.

Given the occupational distribution of treated job seekers and establishments, our initial problem was to match one side of the market to the other in each treated commuting zone without making pairwise recommendations that are too far apart or flooding some agents with recommendations while leaving others empty-handed. By adding different treatment arms in our design we add a further level complexity : pairwise recommendations should now reflect the empirical distribution of job seekers and predicted vacancies across the occupational space (reallocating some agents when necessary), but also each treated agent's particular treatment status.

The problem of forming acceptable pairwise recommendations while abiding by each agent's treatment status could be seen as a complex matching problem in which

any treated worker could potentially be matched to any treated firm. To solve this random allocation problem in a simple and tractable way we proceeded in three steps.

1. We used a simple model of the labor market in order to predict whether or not job seeker  $i$  would be hired by firm  $j$  conditional on :
  - (a) job seeker  $i$ 's search occupation,
  - (b) firm  $j$ 's predicted number of hirings in each occupation ,
  - (c) whether or not we recommended a job seeker  $i$  to firm  $j$ .

*If we knew the parameters of the model, maximizing the predicted number of hires over the set of possible recommendations gave us the "optimal" set of recommendations that we would have to make.*
2. We assigned to each treated agent of our economy individual parameters reflecting their randomly drawn treatment status (i.e. whether they should be sent close or far, and receive many or few recommendations).
3. We used these randomly drawn individual parameters to draw the "optimal" set of recommendations maximizing the total number of predicted hires.

A probabilistic model of worker/firm matches

Heuristically we thought of our intervention in along the following lines :

1. We recommend firms to workers.
2. Workers choose or not to apply to these firms according to occupational distance.
3. Firms skim through the applications they receive and randomly decide to look more deeply into some of them. Firms are more or less efficient at screening applications and the screening rate is increasing in a firm's predicted hirings.
4. Firm review each selected application and decide whether or not to hire each reviewed applicant according to occupational distance.

To describe more formally the actual model of the labor market used to solve our matching problem, we begin with some notation. Let :

- $w$  and  $f$  index individual workers and firms,
- $i, j, \dots$  index occupations (each worker has a single occupation while each firm operates in several),
- $d_{i,j}$  index the occupational distance between two occupations  $i, j$ ,
- $V_{f,j}$  denote the "vacancies" posted by firm  $f$  in occupation  $j$ ,
- $m_f$  the efficiency of firm  $f$ 's screening technology,
- $\rho_f \in (0, 1)$  the occupational discount factor of firm  $f$

- $\rho_w \in (0, 1)$  the occupational discount factor of worker  $w$
- Conditional on recommending worker  $w$  to some firm,  $\alpha_{f,j}^w \in (0, 1)$  the probability to recommend worker  $w$  to the branch  $j$  of firm  $f$ . Hence :

$$\sum_{f,j} \alpha_{f,j}^w = 1$$

- $T(w)$  the treatment type of worker  $w$  (i.e. the number of recommendations that will be sent to worker  $w$ , to fix ideas  $T(w) \in (0, 3, 6)$ ).

Let us consider a worker  $w$  whose occupation is  $i$ . The hiring process of this economy unfolds as follows :

1. The central planner draws  $T(w)$  recommendations for worker  $w$  according to the generalized Bernoulli distribution  $\alpha^w = \{\alpha_{f,j}^w\}_{f,j}$ . Hence the probability that worker  $w$  is recommended to branch  $j$  of firm  $f$  is exactly :

$$P(w \in R_{f,j}) = 1 - (1 - \alpha_{f,j}^w)^{T(w)}$$

Where  $R_{f,j}$  denotes the set of workers who have been recommended to  $(f, j)$ .

2. Given that worker  $w$  has been recommended to  $(f, j)$ , he actually sends a CV to  $(f, j)$  with probability :

$$P(w \in C_{f,j} | w \in R_{f,j}) = \rho_w^{d_{i,j}}$$

Where  $C_{f,j}$  is the set of workers who sent a CV to  $(f, j)$ .

3. Hence, the number of workers who sent a CV to  $(f, j)$  is :

$$W_{f,j} = \sum_w \rho_w^{d_{i,j}} (w \in R_{f,j})$$

Whose unconditional expectation is given by :

$$E[W_{f,j}] = \sum_w \rho_w^{d_{i,j}} [1 - (1 - \alpha_{f,j}^w)^{T(w)}]$$

And whose variance is :

$$V[W_{f,j}] = \sum_w \rho_w^{d_{i,j}} [1 - (1 - \alpha_{f,j}^w)^{T(w)}] [1 - \rho_w^{d_{i,j}} [1 - (1 - \alpha_{f,j}^w)^{T(w)}]]$$

4. Given that  $(f, j)$  receives  $W_{f,j}$  CVs, it randomly selects among them the ones that will be considered for employment. This selection occurs through the firm specific screening technology  $q_f$  which takes as its only argument the branch specific slackness ratio  $\theta_{f,j} = W_{f,j}/V_{f,j}$ . We assume that  $q_f$  is non-increasing,

that  $q_f(0) = 1$ , and that  $q_f(+\infty) = 0$ . Conditional on applying to  $(f, j)$  a worker has probability  $\pi_{f,j}$  to be interviewed :

$$\pi_{f,j} = E[q_f(\theta_{f,j})]$$

This expectation can be approximated by :

$$\pi_{f,j} = q_f(E[\theta_{f,j}]) + \frac{V[\theta_{f,j}]}{2} \frac{\partial^2 q_f}{\partial \theta^2}(E[\theta_{f,j}]) + o(E[(\theta_{f,j} - E[\theta_{f,j}])^3])$$

5. Once  $(f, j)$  has selected the  $\pi_{f,j} W_{f,j}$  workers it wil interview, the probability of each of them to be hired is simply  $\rho_f^{d_{i,j}}$ .
6. Coming back to worker  $w$ , its unconditional probability of being hired by  $(f, j)$  is :

$$P(w \in H_{f,j}) = \rho_f^{d_{i,j}} \times \pi_{f,j} \times \rho_w^{d_{i,j}} \times [1 - (1 - \alpha_{f,j}^w)^{T(w)}]$$

7. Hence, ignoring the possibility of a worker being hired by two firms the probability that worker  $w$  will be hired by some firm is :

$$P(w \in H) = 1 - \prod_{f,j} [1 - P(w \in H_{f,j})]$$

Which can be approximated by :

$$P(w \in H) \sim \sum_{f,j} P(w \in H_{f,j})$$

i.e.

$$P(w \in H) \sim \sum_{f,j} \rho_f^{d_{i,j}} \times \pi_{f,j} \times \rho_w^{d_{i,j}} [1 - (1 - \alpha_{f,j}^w)^{T(w)}]$$

If we follow these steps for each worker we find that the total number of hires in the economy can be written as :

$$M = \sum_{w,f,j} \rho_f^{d_{i,j}} \times \pi_{f,j} \times \rho_w^{d_{i,j}} \times [1 - (1 - \alpha_{f,j}^w)^{T(w)}]$$

The problem of the central planner is to maximize  $M$  subject to :

$$\forall w, \sum_{f,j} \alpha_{f,j}^w = 1$$

$$\forall (w, f, j), 0 \leq \alpha_{f,j}^w \leq 1$$

This problem has dimensionality  $\#(\text{Workers}) \times \#(\text{Firms}) \times \#(\text{Occupations})$ , which is too large. To reduce the dimensionality we parametrize  $\alpha_{f,j}^w$  using the information we

have on workers and firms. Denote  $X_{w,f,j}$  the vector of worker/firm/branch characteristics that will be used to predict  $\alpha_{f,j}^w$ . We assume that :

$$\alpha_{f,j}^w = \frac{\exp(X'_{w,f,j}\beta)}{\sum_{f,j} \exp(X'_{w,f,j}\beta)}$$

Hence the dimensionality of the problem is reduced to #(worker/firm characteristics) so that, in the end, the maximization problem reduces to :

$$\max_{\beta} \sum_{w,f,j} \rho_f^{d_{i,j}} \times \pi_{f,j} \times \rho_w^{d_{i,j}} \times [1 - (1 - \frac{\exp(X'_{w,f,j}\beta)}{\sum_{f,j} \exp(X'_{w,f,j}\beta)})^{T(w)}]$$

### Randomization :

To create random variations in the overall number of recommendations sent to firms and in the relative occupational distance of recommendations sent to firms and workers we randomize :

1. firms' distaste for occupational distance  $\rho_f \in [\rho_f^H, \rho_f^L]$
2. workers' distaste for occupational distance  $\rho_w \in [\rho_w^H, \rho_w^L]$
3. firms' relative efficiency of screening with respect to applications  $m_f \in [m_f^H, m_f^L]$ .

### A3.15 Choice of the screening technology :

More specifically we choose to parametrize our screening function  $q_{f,j}$  as :

$$q_{f,j}(\theta_{f,j}) = \frac{1}{[1 + (\frac{\theta_{f,j}}{\Gamma m_f \bar{\theta}_j})^\gamma]^{1/\gamma}}$$

Where  $\gamma > 1$  and  $\Gamma$  are constants verifying :

$$\Gamma = (\frac{\gamma - 1}{2})^{-1/\gamma}$$

And where  $\bar{\theta}_j$  denotes the local slackness ratio in occupation  $j$ . This local slackness ratio is defined as the ratio of possible recommendations present in the vicinity of occupation  $j$  to the total number of hirings predicted in occupation  $j$ . Formally :

$$\bar{\theta}_j = \frac{\sum_w \rho_w^{d_{i(w),j}} T(w)}{\sum_f V_{f,j}}$$

For  $\gamma > 1$  this function is monotonous in  $\theta_{f,j} = W_{f,j}/V_{f,j} > 0$  and verifies :

$$q_{f,j}(0) = 1$$

$$q_{f,j}(+\infty) = 0$$

What's more  $q_{f,j}$  has an inflection point at  $m_f\theta_j$  so that according to the value of  $m_f$ , firm's  $f$  congestion effect will start to quick in either before ( $m_f = m_f^L < 1$ ) or after ( $m_f = m_f^H > 1$ ) the number of recommendations sent to  $(f,j)$  relative to its predicted hirings (i.e  $W_{f,j}/V_{f,j}$ ) reaches the local slackness ratio  $\theta_j$ .

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## RÉSUMÉ

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Sous le nom de changement structurel, les déplacements de l'offre et de la demande de travail provoqués par la concurrence internationale, le progrès technologique asymétrique ou les déplacements de la demande de biens liées à la non-homothéticité des préférences des consommateurs, affectent en permanence l'équilibre du marché de l'emploi des économies développées. Ce travail regroupe trois études portant sur l'accélération du changement structurel liée à la concurrence internationale ; sur la dynamique microéconomique que le changement structurel impose aux travailleurs des firmes en déclin ; et sur les politiques actives de re-direction de l'offre de travail qui permettraient d'atténuer les effets négatifs du changement structurel sur l'emploi. La première de ces études se concentre sur l'accélération du changement structurel causé par la concurrence internationale dans le secteur manufacturier français. En utilisant des données de douane au niveau produit nous identifions la structure de production des firmes françaises et distinguons les biens produits par chaque firme (produits exportés) des biens utilisés comme consommation intermédiaire (produits importés). Nous utilisons les différences de structures de production entre firmes pour évaluer l'effet de la forte croissance des exportations en provenance de Chine entre 2000 et 2007. Ce chapitre conclut à un effet négatif de l'accroissement des importations en provenance de Chine sur l'emploi et l'innovation des entreprises françaises lorsque ces importations concernent le marché des produits par opposition aux biens intermédiaires. La deuxième étude porte sur la dynamique d'emploi des firmes en situation de difficulté financière. En utilisant les données d'ouverture de redressement judiciaire appariées aux trajectoires individuelles de leur salariés, ce chapitre met en évidence le caractère stratégique des décisions de départ et de licenciement dans les firmes en faillite. Enfin le dernier chapitre présente une évaluation d'un service d'aide à la recherche d'emploi (« La Bonne Boîte ») visant à inciter les demandeurs d'emploi à effectuer des candidatures spontanées. Cette étude met en évidence d'une part l'importance des frictions contraignant les firmes en croissance dans leur processus de recrutement, et d'autre part le potentiel que présentent les dispositifs de re-direction ciblée de la recherche d'emploi (doublement de la probabilité de recrutement dans une entreprise donnée).

## MOTS CLÉS

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Changement structurel, Compétition internationale, Dynamique des firmes, Marché de l'emploi frictionnel, Politiques actives du marché du travail

## ABSTRACT

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Through trade competition, asymmetric technological change or demand non-homotheticities, an ongoing structural change affects the way firms and worker match in the labor market.

This work brings together three studies on the consequences of structural change on employment. The first study focuses on the acceleration of structural change caused by international competition in the French manufacturing sector. Using detailed product level customs data we are able to identify trading firms' output and inputs through their exports and imports. We then leverage differences in firm level production structures within narrow industries to identify the effect to increased trade competition with China between 2000 and 2007. This first chapter concludes that horizontal trade competition on outputs has a negative effect on firm level employment and innovation. The second study focuses on the employment dynamics of financially distressed. Using data on firms' bankruptcy filings matched to the individual trajectories of their employees, this chapter highlights the strategic nature of quit and layoff decisions in bankrupt firms. The quit behavior of employees and the layoff strategies of firms appear consistent with a simple dynamic game of worker/firm separations where firms are unable to retain their high productivity employees. Finally, the last chapter presents an evaluation of a job search assistance service ("La Bonne Boîte") aimed at encouraging job seekers to make spontaneous applications. Using a two sided random allocation of workers to firms we find a positive effect on women's job finding rate in short duration contracts as well as a positive effect on indefinite duration hirings of treated firms.

## KEYWORDS

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Structural Change, Trade Competition, Firm dynamics, Frictional Labor Markets, Active Labor Market Policies