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## Three essays on space, health and job quality

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**Three essays on space, health and job quality**

Soutenue par

**António MELO**

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# Résumé en Français



Les êtres humains sont naturellement limités par l'espace et le temps. La réduction de ces contraintes est un élément central du progrès et du développement social. À cet égard, les politiques publiques sont essentielles pour réduire les distances entre les citoyens, élargir l'accès à un plus large éventail d'informations, de services et d'opportunités et, en fin de compte, améliorer leur bien-être.

Ne pas tenir compte de l'importance de l'espace peut conduire à des résultats indésirables. Une partie de la population peut se sentir éloignée des décisions politiques, piégée économiquement et, conséquemment, négligée. Ce ressentiment peut pousser des régions dépeuplées et économiquement peu performantes vers des partis ou des mouvements populistes (Margalit 2019).

Pour concevoir des politiques qui répondent à ce mécontentement, il est essentiel de comprendre le rôle de l'espace. Dans cette thèse, j'analyse son interaction avec l'accès aux infrastructures de santé et aux opportunités d'emploi. Étant donné que la santé et le travail sont, selon l'indicateur du vivre mieux de l'OCDE, deux des déterminants les plus importants du bien-être subjectif, la réalisation d'évaluations d'impact dans ces domaines revêt une importance particulière pour les politiques.

Plus précisément, j'étudie comment la géographie peut influencer l'accès aux services d'interruption volontaire de la grossesse (IVG) et la qualité de l'emploi. Même si l'accès ne dépend pas uniquement de l'espace, l'accès géographique est, dans de nombreux cas, une condition nécessaire de l'accès. C'est particulièrement le cas dans le chapitre 1, où j'analyse comment les changements dans la proximité des services de santé qui pratiquent l'IVG au Portugal ont affecté la capacité des femmes à obtenir une IVG et les conditions dans lesquelles elles le font. L'accès géographique est également au cœur des chapitres 2 et 3, dans lesquels mes coauteurs et moi-même examinons comment la concentration du marché du travail affecte les salaires et la sécurité de l'emploi. Manning et Petrongolo (2017) montrent que la recherche d'emploi peut être confinée à des zones relativement petites. Si les travailleurs vivent à proximité de marchés du travail concentrés – et non compétitifs –, le manque d'opportunités d'emploi dans leur environnement proche peut les pousser vers des emplois de moins bonne qualité.

Tout au long de cette thèse, l'espace apparaît comme un déterminant pertinent de la qualité des soins et de l'emploi. Dans le chapitre 1, je constate que les régions les plus éloignées des services qui pratiquent l'IVG en effectuent moins. En outre, les femmes de

ces régions risquent davantage de connaître des conditions d'IVG moins favorables : elles avortent plus tard et sont plus susceptibles d'avoir recours à des avortements chirurgicaux, une procédure invasive et plus coûteuse que la méthode médicale. Les chapitres 2 et 3 montrent également que les travailleurs des marchés du travail plus concentrés ont des salaires et des niveaux de sécurité de l'emploi plus faibles ; ils sont moins susceptibles d'être embauchés sous contrat permanent et d'être convertis en contrat permanent lorsqu'ils sont embauchés sous contrat temporaire.

Dans la suite de cette introduction, je discuterai les trois chapitres selon la structure suivante. Je commencerai par expliquer pourquoi l'accès à l'IVG et le pouvoir de marché des employeurs méritent notre attention. Je présenterai ensuite de manière concise les méthodologies que j'utilise, les résultats que j'obtiens et les recommandations politiques correspondantes.

## **Motivation et objectifs de la thèse**

L'espace et le bien-être sont des éléments centraux de cette thèse. Cependant, les trois chapitres ont également un autre aspect en commun : ces derniers temps, leurs sujets ont gagné en attractivité dans le débat public.

### **Espace et l'IVG : un débat contemporain**

Si le débat sur l'accès à l'IVG est depuis longtemps une question controversée, les développements juridiques en cours aux États-Unis l'ont rendue encore plus pertinente. Le 20 juin 2022, la Cour suprême des États-Unis a annulé l'arrêt *Roe v. Wade*, qui légalisait l'IVG sur demande aux États-Unis. Cette décision a redonné à chaque État le pouvoir de maintenir ou d'interdire l'IVG légal, ce qui fait que certaines femmes vivant dans des États conservateurs se trouvent à des milliers de kilomètres des services de santé qui pratiquent l'IVG. En avril 2023, l'Iowa a été le premier État à adopter une loi limitant les déplacements entre États pour les femmes souhaitant avorter. Au contraire, certains États libéraux, comme Washington, ont promulgué des lois qui protègent les femmes arrivant sur leur territoire pour obtenir une IVG. Cela montre l'importance de l'accès géographique tant pour les groupes proavortement que pour les provie. Et ce n'est pas sans raison. Des études empiriques montrent que les lois américaines sur l'IVG

qui réduisent la proximité des services de santé qui pratiquent l'IVG réduisent les taux d'IVG – par exemple, Fischer et al. (2018) et Lindo et al. (2020).

Au Portugal, contrairement aux États-Unis, les lois sur l'IVG sont devenues de plus en plus libérales au cours des dernières décennies. Jusqu'en 1984, les femmes n'étaient en aucun cas autorisées à pratiquer une IVG. Cette année-là, l'IVG n'est devenu légale qu'en cas de viol, de malformation du fœtus ou lorsque la grossesse menaçait la vie de la femme. En 2007, après un référendum, le parlement portugais a approuvé l'IVG jusqu'à dix semaines de grossesse. En vertu de la nouvelle loi, comme les autres services de santé maternelle, les avortements dans le secteur public devaient être gratuits.

En outre, toutes les maternités publiques étaient tenues de fournir des services d'IVG, signe que les décideurs politiques entendaient profiter de la couverture géographique du Service national de santé (SNS) pour offrir un accès équitable à l'IVG. Toutefois, la loi protège également les médecins et les infirmières qui ont des objections éthiques ou morales à pratiquer l'IVG. Cette situation a posé des problèmes à certains établissements de santé, car la réticence de leurs professionnels à proposer ce service les a empêchés de remplir leurs obligations légales. Dans de nombreux cas, les hôpitaux du SNS ont dû se contenter d'orienter les femmes de leurs zones désignées vers d'autres prestataires. En conséquence, l'absence de planification dans la distribution des soins liés à l'IVG a entraîné des disparités dans l'accès géographique, qui ont récemment attiré l'attention des médias et suscité l'intérêt des législateurs.<sup>1</sup>

Néanmoins, les preuves de l'impact de ces disparités sur le bien-être des femmes font défaut. Tout d'abord, les résultats obtenus aux États-Unis peuvent ne pas être directement applicables à un pays dont l'échelle et les structures institutionnelles sont différentes. Deuxièmement, si les effets des grandes variations d'accès à la santé des femmes, comme la légalisation de l'IVG, sont bien documentés (Clarke et Mühlrad, 2021), les recherches sur l'influence spécifique de la proximité sur les conditions d'IVG des femmes sont limitées. Le premier chapitre de cette thèse vise à combler ces lacunes.

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<sup>1</sup>Depuis février 2023, le journal *Diário de Notícias* a publié une série d'articles sur l'accès à l'IVG au Portugal, y compris des rapports sur la disponibilité des services d'IVG dans le pays et sur l'objection morale (Câncio 2023a-2023c). Ces articles ont abouti à une audition parlementaire qui a eu lieu le 30 mars de la même année (Comissão de Saúde 2023).

## Espace et qualité de l'emploi : Concentration du marché du travail et pouvoir de marché des employeurs

Au-delà de l'accès aux soins de santé, l'espace influe également sur l'accès à d'autres déterminants du bien-être individuel, et en particulier aux possibilités d'emploi

Pullman était une ville de l'Illinois. Elle doit son nom à George Pullman, l'un des hommes d'affaires américains les plus influents du XIXe siècle, propriétaire de la Pullman Company, une entreprise dominante sur le marché des wagons de chemin de fer. La ville a été créée pour loger les travailleurs de la Pullman Company et, à l'exception de quelques magasins de détail (Lindsey 1939), toute la ville était sous le contrôle de l'entreprise. George Pullman est donc à la fois le patron et le propriétaire de cette population. Pour un habitant de Pullman, il est quasiment impossible de changer d'employeur sans quitter la ville, ce qui le rend plus vulnérable à des conditions de travail dégradées, comme l'illustrent les pratiques de "blacklistage, népotisme, favoritisme, licenciements arbitraires et tyrannie de la part des contremaîtres" (Lindsey 1939, p.284) de la Pullman Company. Après la dépression de 1893, la Pullman Company a décidé de réduire les salaires de 25 % (en moyenne), tout en maintenant leurs loyers élevés, même si les affaires de la Pullman Company n'étaient pas gravement affectées (Lindsey 1939). Cette décision a conduit à la célèbre "grève Pullman".<sup>2</sup>

S'il y avait eu plus d'employeurs dans la région au lieu d'un seul, la crainte de perdre une partie de sa main-d'œuvre au profit de concurrents potentiels aurait pu amener George Pullman à prendre une décision différente. Or, ce n'était pas le cas, ce qui signifie que l'offre de main-d'œuvre de Pullman (la ville) était contrôlée par la Pullman Company. Cette situation, dans laquelle un marché n'a qu'un seul acheteur, est définie comme un monopsonne. Néanmoins, la discussion sur le monopsonne sur le marché du travail va au-delà du cas d'un seul acheteur. Il s'agit plus généralement de la capacité des entreprises à payer des salaires inférieurs à la productivité marginale des travailleurs.

Comme l'illustre Manning (2021), le pouvoir de monopsonne sur le marché du travail est la norme plutôt que l'exception : si une entreprise baisse tous les salaires d'un centime, elle ne perdra pas la totalité de sa main-d'œuvre. Cela peut s'expliquer par différents facteurs. Premièrement, tous les travailleurs ne quittent pas l'entreprise parce

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<sup>2</sup>Pour plus de détails sur l'histoire de la "grève de Pullman", voir Lindsey (1939).

que la recherche d'un emploi est coûteuse. Deuxièmement, les préférences hétérogènes des travailleurs pour les caractéristiques des entreprises peuvent également être une source de monopsonie (Card et al. 2018). Par exemple, les travailleurs handicapés apprécieront une grande accessibilité du lieu de travail, ce qui réduira le nombre d'options extérieures appropriées, les rendant plus vulnérables au pouvoir de marché de l'employeur. Par ailleurs, les entreprises peuvent se coordonner pour réduire le nombre d'options extérieures disponibles pour les travailleurs. L'un des moyens d'y parvenir est la collusion. Pendant la pandémie de COVID-19, les clubs de football de la ligue professionnelle portugaise se sont mis d'accord pour ne pas embaucher des joueurs d'autres équipes s'ils avaient résilié unilatéralement leur contrat pour cause de salaires impayés. Cet accord de non-embauche a donné lieu à une décision historique de l'autorité anti-trust portugaise, qui a infligé sa première amende pour comportement anticoncurrentiel sur le marché du travail.<sup>3</sup> Par ailleurs, les employeurs peuvent également restreindre la mobilité des travailleurs entre les entreprises par le biais de clauses de non-concurrence, comme cela a été démontré aux États-Unis (Starr et al. 2021) et en Italie (Boeri et al. 2023).

Enfin, comme l'illustre l'affaire Pullman, le pouvoir de monopsonie peut résulter de la concentration du marché du travail – qui fait l'objet des chapitres 2 et 3. La littérature récente montre que les travailleurs sur des marchés plus concentrés ont des salaires plus bas – Azar et al. (2022), Marinescu et al. (2021), Bassanini et al. (2023), et Dodini et al. (2023) entre autres –, l'ampleur de ces effets variant d'un pays à l'autre. La question de savoir si ces différences sont spécifiques au contexte ou si elles sont dues aux diverses approches méthodologiques utilisées pour mesurer la concentration et son impact reste ouverte. Les chapitres 2 et 3 abordent cette question de deux manières distinctes. Le premier chapitre utilise des données administratives complètes pour comprendre comment chaque choix méthodologique affecte à la fois les niveaux et les effets de la concentration du marché du travail. Le second chapitre utilise des données provenant de six pays européens et une méthodologie commune pour fournir les premières estimations comparables entre pays de l'impact de la concentration sur la qualité de l'emploi.

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<sup>3</sup><https://www.concorrenca.pt/en/articles/adc-issues-sanctioning-decision-anticompetitive-agreement-labor-market-first-time>



Enfin, si de nombreux travaux ont été consacrés à l'influence de la concentration du marché du travail sur les salaires, les données sur les dimensions non salariales de la qualité de l'emploi sont rares (Qiu et Sojourner, 2023). À cet égard, le chapitre 3 apporte une contribution à la littérature en examinant un aspect de la qualité de l'emploi jusqu'ici inexploré : la sécurité de l'emploi.

## Méthodes et données

L'Académie royale suédoise des sciences a décerné le prix Nobel d'économie 2021 à David Card, Joshua Angrist et Guido Imbens pour avoir

(...) apporté de nouvelles idées sur le marché du travail et montré quelles conclusions peuvent être tirées d'expériences naturelles.

(Royal Swedish Academy of Sciences 2021)

Les expériences naturelles, comme leur nom l'indique, se rapprochent naturellement des conditions d'une expérience où les individus sont répartis de manière aléatoire entre un groupe de traitement et un groupe de contrôle. Elles permettent ainsi de comprendre les relations de cause à effet dans des situations où il serait difficile, voire impossible, de mener des expériences. Par exemple, il ne serait ni éthique ni pratique d'assigner aléatoirement des enfants à des femmes, afin de comprendre comment la fertilité affecte les résultats des femmes sur le marché du travail. Angrist et Evans (1998) contournent ingénieusement ce problème en utilisant un événement naturel qui imite le jeu de pile ou face : l'assignation du sexe à la naissance. Pour être plus précis, aux États-Unis, les couples ont une préférence pour la diversité des sexes parmi leurs enfants. Les familles qui ont deux garçons ou deux filles sont plus susceptibles d'essayer d'avoir un troisième enfant que celles qui ont un garçon et une fille. Angrist et Evans (1998) s'appuient sur cette quasi-expérience pour déterminer comment un enfant supplémentaire affecte l'offre de travail des mères. Ce n'est qu'un exemple de la manière dont, selon l'Académie royale suédoise des sciences, les lauréats du prix Nobel 2021 ont "révolutionné la recherche empirique".

Étant donné que les trois chapitres de cette thèse sont des recherches appliquées en microéconomie, ils utilisent naturellement certains des outils que les lauréats du

prix Nobel de 2021 emploient pour retrouver les relations causales, comme je détaille ci-dessous.

## Chapitre 1 : Différences dans les différences

Dans le chapitre 1, j'examine comment l'accès géographique affecte la capacité des femmes à pratiquer une IVG et leurs conditions, à savoir quand (nombre de semaines avant l'IVG), où (services publics ou privés) et comment (chirurgicalement ou médicalement). J'évalue l'accès géographique par le temps de trajet en voiture entre la municipalité de résidence des femmes et le service d'IVG le plus proche. Ensuite, pour identifier les effets de la proximité des soins en matière d'IVG, je m'appuie sur un modèle de différences en différences (DID).

L'estimateur DID classique calcule la différence de résultats entre deux groupes avant et après que l'un d'entre eux ait reçu un traitement. Il soustrait ensuite les différences avant et après pour calculer l'impact de ce traitement. L'hypothèse sous-jacente est la suivante : si le traitement n'avait pas eu lieu, les résultats des personnes traitées auraient suivi la même évolution que ceux des personnes non traitées – c'est ce que l'on appelle l'hypothèse de tendance parallèle.<sup>4</sup> Un exemple d'application de la méthode DID est l'article fondateur de Card et Krueger (1994), dans lequel ils exploitent l'augmentation du salaire minimum dans le New Jersey en 1992 pour examiner comment la croissance de l'emploi dans l'industrie de la restauration rapide a évolué par rapport à la Pennsylvanie (le groupe de contrôle).

Le modèle DID classique à deux groupes par deux périodes peut être complété par davantage de groupes, de périodes et plus d'un délai de traitement. Dans le cadre du chapitre 1, les services d'IVG ouvrent et ferment à des heures différentes dans des villes différentes, ce qui, pour certaines femmes, modifie le temps dont elles ont besoin pour accéder à un service ou, autrement dit, l'intensité de leur traitement. J'exploite cette expérience naturelle et compare les régions où la proximité d'un service qui pratique l'IVG a changé à celles où elle n'a pas changé. À cette fin, j'estime une régression de

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<sup>4</sup>Il est intéressant de noter que Callaway et al. (2021) montrent que lorsque le traitement n'est pas binaire – comme l'accès ou non à l'IVG légal – mais plutôt continu – comme le temps qu'il faut pour se rendre du domicile des femmes à un service de santé qui pratique l'IVG – les chercheurs doivent modifier cette hypothèse pour procéder à une inférence causale. Ils doivent supposer que toutes les régions réagissent de la même manière à chaque changement possible dans la proximité d'un service d'IVG.

Poisson avec des effets fixes à double sens – c’est-à-dire qui contrôle les caractéristiques invariantes dans le temps des municipalités et les chocs annuels qui les affectent toutes simultanément. L’un des problèmes que posent les régressions à effets fixes bidirectionnels dans des contextes où les traitements sont appliqués à des moments différents, comme c’est le cas ici, est qu’elles utilisent comme témoins des régions qui ont déjà été traitées, ce qui peut produire des estimations biaisées (de Chaisemartin et D’Haultfoeuille, 2020 ; Goodman-Bacon, 2021). Pour résoudre ce problème, j’utilise au chapitre 1 la méthode DID avec jalons proposée par Cengiz et al. (2019), ce qui signifie que pour chaque région traitée, je construis un ensemble de données comprenant cette région et, dans le groupe de contrôle, des régions qui n’ont pas encore connu de changement en matière de proximité des services d’IVG. Ensuite, je réunis tous les ensembles de données construits pour obtenir l’ensemble de données final.

L’analyse du chapitre 1 utilise les données des registres d’IVG qui contiennent tous les IVGs légaux pratiqués au Portugal entre 2008 et 2018.<sup>5</sup> Cet ensemble de données provient de la Direction générale de la santé et constitue un ensemble d’enquêtes obligatoires que les femmes doivent remplir lorsqu’elles demandent une IVG. Il contient des informations socio-économiques détaillées sur les femmes, à savoir leur âge, leur profession, leur niveau d’éducation et, surtout, leur commune de résidence. Il révèle également des aspects pertinents des soins reçus par les femmes, notamment la phase de la grossesse où elles ont contacté le service (en semaines de grossesse) et le moment où elles ont effectivement pratiqué l’IVG, le type de prestataire auquel elles ont eu recours et le lieu où elles ont avorté (public ou privé), ainsi que la méthode d’IVG utilisée (chirurgicale ou médicale). En outre, pour compléter l’analyse, ce chapitre intègre également des données sur les naissances, la population par sexe et par groupe d’âge, les mariages, le produit intérieur brut régional – cet ensemble de données provient de l’INE – Institut National de Statistiques – et les registres de chômage des municipalités – provenant de l’Institut de l’emploi et de la formation professionnelle.

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<sup>5</sup>Je ne peux pas utiliser les années 2017 et 2018 parce qu’il n’y a pas d’informations sur les services de santé pratiquant l’IVG au cours de ces années et qu’il n’est donc pas possible de calculer le temps de trajet jusqu’à le service le plus proche.

## **Chapitres 2 et 3 : Mesures de concentration du marché du travail et variables instrumentales**

Les chapitres 2 et 3 mesurent la concentration du marché du travail et estiment ses effets sur la qualité de l'emploi pour le Portugal seul et avec cinq autres pays européens : le Danemark, la France, l'Allemagne, l'Italie et l'Espagne.

### **Mesurer la concentration du marché du travail**

Pour évaluer les niveaux de concentration des marchés du travail locaux, la première étape consiste à déterminer ce qui constitue un marché du travail local. Idéalement, il devrait identifier l'ensemble des travailleurs et des entreprises qui peuvent offrir ou demander de la main-d'œuvre pour un emploi ou une tâche donnée. L'approche standard considère les marchés du travail locaux comme des zones géographiques par paires de caractéristiques d'emploi. L'utilisation de zones géographiques pour lier les marchés du travail locaux est cohérente avec la preuve que la recherche d'emploi est locale (Manning et Petrongolo 2017). Il est donc justifié de ne pas considérer les travailleurs qui vivent trop loin comme des employés potentiels, même si leur profil correspond bien à un poste vacant. Les caractéristiques des emplois constituent une autre limite raisonnable des marchés du travail locaux, car les inadéquations entre les tâches et les compétences des travailleurs devraient rendre ces personnes inadaptées à des postes vacants spécifiques.

Le chapitre 2 considère les paires district-profession portugaises comme la définition de référence des marchés du travail locaux, tandis que le chapitre 3 regroupe la profession avec une nouvelle unité géographique que nous construisons en combinant les régions NUTS III (ou les districts, dans le cas du Portugal) avec les zones urbaines fonctionnelles d'Eurostat.

Dans les chapitres 2 et 3, la définition préférée des caractéristiques de l'emploi est la profession et non le secteur, comme dans Rinz (2022) ou Benmelech et al. (2022), car cela séparerait probablement les travailleurs qui pourraient servir de substituts adéquats aux entreprises. Par exemple, un soudeur dans le secteur de la construction navale peut probablement entrer en concurrence pour un emploi avec un soudeur dans le secteur ferroviaire, même s'ils travaillent dans des secteurs différents. Néanmoins, au chapitre 2, nous testons la sensibilité des niveaux et des effets de concentration du marché du

travail à certaines des différentes définitions des marchés du travail locaux figurant dans la littérature, telles que la prise en compte de l'industrie au lieu de la profession ou l'utilisation de régions plus petites.

Après avoir défini les marchés du travail locaux, l'étape suivante consiste à mesurer leurs niveaux de concentration, ce que nous faisons en calculant l'indice Herfindahl-Hirschman (IHH) – la somme de toutes les entreprises d'un marché du travail local de leur part de l'emploi sur ce marché. Les stocks d'emploi (tous les travailleurs employés par l'entreprise) et les flux d'emploi (nouvelles embauches) peuvent être utilisés pour calculer cet indice. Le chapitre 2 calcule les IHH en utilisant les deux, tandis que le chapitre 3 n'utilise que le second, car les flux d'emploi peuvent mieux refléter le monopsonne que les stocks. En effet, les employeurs disposant d'un pouvoir de marché feront baisser les coûts liés à la main-d'œuvre (rémunération salariale ou non salariale) en rationnant leur demande de main-d'œuvre, ce qui se traduira probablement par l'ouverture de moins de postes vacants et l'embauche de moins de travailleurs.

### **Estimation des effets de la concentration du marché du travail**

Pour déterminer si les employeurs des marchés plus concentrés jouissent d'un plus grand pouvoir de marché et peuvent par conséquent offrir des salaires et une sécurité de l'emploi plus faibles,<sup>6</sup> il ne suffit pas de comparer la qualité de l'emploi entre les différents niveaux de concentration du marché du travail. Ce faisant, il ne serait pas possible de comprendre si la concentration du marché du travail affecte les coûts liés à la main-d'œuvre ou si c'est la situation inverse – un problème de causalité inversée. On peut en effet imaginer un scénario dans lequel une baisse des salaires dans une profession donnée dans une région donnée attire de nouvelles entreprises, diminuant ainsi la concentration sur ce marché du travail local. Par conséquent, la constatation d'une relation positive entre ces deux variables n'est pas significative d'un point de vue causal.

Une solution à ce problème consiste à utiliser une troisième variable qui affecte la concentration des marchés du travail locaux, mais qui n'est pas liée à la qualité de l'emploi – sauf par le biais de la concentration du marché du travail. En économétrie, cette troisième variable est appelée "instrument". Un exemple très influent de l'utilisation

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<sup>6</sup>Nous mesurons la sécurité de l'emploi comme la probabilité d'être embauché avec un contrat permanent ou d'être converti en contrat permanent si l'on est embauché avec un contrat temporaire.

de variables instrumentales (IV) est l'article susmentionné d'Angrist et Evans (1998). Dans l'expérience naturelle qu'ils exploitent, le fait d'avoir deux enfants du même sexe n'affecte l'offre de travail d'une mère que par le biais de l'impact sur la probabilité qu'elle ait un autre enfant.

Dans les chapitres 2 et 3, pour retrouver l'effet causal de la concentration du marché du travail sur la qualité de l'emploi, nous instrumentons l'indice de concentration des marchés du travail locaux par la moyenne (de l'inverse) du nombre d'employeurs qui opèrent sur ce marché du travail, à l'exclusion de la région du marché du travail local. Grâce à cet instrument, nous entendons saisir les changements de concentration à l'échelle nationale résultant, par exemple, de fusions et/ou de cessions. Ces changements affecteront la concentration du marché du travail local, mais ils n'auront pas d'impact direct sur les coûts de la main-d'œuvre dans ces marchés du travail locaux.

Les deux chapitres s'appuient sur des ensembles de données couplées employeur-employé – le chapitre 2 utilise les données portugaises de 1985 à 2019, tandis que le chapitre 3 utilise les données de la décennie 2010 pour six pays européens. Pour le Portugal, la source utilisée est le “Quadros de Pessoal”, un instantané annuel du marché du travail portugais au mois d'octobre (mars jusqu'en 1993). Cet ensemble de données couvre l'ensemble du secteur privé et comporte des identifiants uniques pour les entreprises, les établissements et les travailleurs. Il contient également des informations sur la localisation géographique et le secteur d'activité des entreprises et de leurs établissements, ainsi que des données sur l'âge, le sexe, le salaire, le type de contrat, le nombre d'heures travaillées et le niveau d'éducation des travailleurs, entre autres.

Outre le Portugal, les données disponibles dans les cinq autres pays européens – décrites dans la section F de l'annexe du chapitre 3 – nous permettent d'étudier l'effet de la concentration du marché du travail sur les salaires danois, français et allemands. En ce qui concerne les résultats liés à la sécurité de l'emploi, nous pouvons examiner comment la concentration affecte la probabilité d'être embauché sous contrat à durée indéterminée pour tous les pays, à l'exception du Danemark. En revanche, l'analyse de la probabilité de convertir un contrat temporaire en contrat permanent n'est possible que pour l'Italie et l'Espagne.

# Résultats et pistes pour la politique

## Chapitre 1 : Les effets de la proximité des services qui pratiquent l'IVG sur leur taux et conditions

Dans le chapitre 1, je montre que les régions situées à plus d'une heure d'un service qui pratiquent l'IVG enregistrent 22% d'avortements en moins que celles situées à moins de 30 minutes d'un service d'avortement. Ces régions affichent également des taux d'IVG plus faibles – c'est-à-dire le nombre d'IVG pour chaque grossesse dans la municipalité – ce qui est cohérent avec le fait que les femmes de ces régions arrivent plus tard dans les services d'IVG, et par conséquent le font plus tard. Ainsi, certaines femmes peuvent arriver trop tard pour avorter dans le délai légal de dix semaines. Ces femmes risquent également davantage de connaître des conditions plus difficiles, au-delà des délais. En effet, elles sont plus susceptibles d'être renvoyées des hôpitaux publics, qui ne sont pas en mesure de les accueillir, vers des prestataires privés et plus enclines à subir des avortements chirurgicaux, qui sont invasifs et plus coûteux que la méthode médicale.

Une question qui se pose naturellement est de savoir si la réduction des distances entre les femmes et les services pratiquant l'IVG par la création de nouveaux services entraînerait des économies pour le SNS. Toutefois, d'après mes conclusions, les économies potentielles seraient limitées, car elles ne permettraient d'embaucher qu'un seul médecin généraliste de niveau débutant sur trois ans. Malgré les économies limitées, une telle politique profiterait aux femmes qui, autrement, ne pourraient pas accéder aux services d'IVG en temps voulu.

Pour améliorer encore la proximité des soins en matière d'IVG, le SNS pourrait envisager d'utiliser le réseau de soins primaires en autorisant les médecins généralistes à pratiquer des IVGs médicamenteuses, étant donné que cette procédure n'est pas techniquement difficile, comme le reconnaît l'Organisation mondiale de la santé (2012).

Enfin, il convient de souligner qu'au Portugal, les informations sur la disponibilité des services ne sont pas facilement accessibles. Cela crée des obstacles inutiles aux femmes qui prétendent une IVG, ce qui est particulièrement pertinent, étant donné que je constate que les retards dans l'obtention d'une IVG causés par le manque de proximité sont principalement dus à des arrivées tardives dans les services. Ainsi, un simple outil

en ligne ou une liste de services d'IVG pourrait déjà améliorer la capacité des femmes à prendre leur décision d'IVG, ainsi que les conditions comme elles la pratiquent.

## **Chapitres 2 et 3 : Les niveaux de concentration du marché du travail et leurs effets sur la qualité de l'emploi**

Dans les chapitres 2 et 3, les mesures de référence de la concentration du marché du travail révèlent des indices de concentration moyens similaires pour le Portugal – environ 1 000. Ces niveaux sont comparativement plus élevés que ceux observés dans les cinq autres pays européens – environ 600. L'Allemagne présente notamment les niveaux de concentration les plus bas, avec un indice de 587.

Le chapitre 2 évalue également la sensibilité des indices de concentration aux choix méthodologiques. Le fait de considérer des régions administratives plus petites ou d'utiliser l'industrie plutôt que la profession pour caractériser les caractéristiques de l'emploi donne des indices de concentration près de deux fois plus élevés que notre mesure de référence. À l'inverse, l'utilisation de catégories professionnelles plus larges donne des estimations plus faibles de la concentration du marché du travail. Ensemble, ces résultats constituent un point de référence précieux pour les chercheurs lorsqu'ils définissent des marchés du travail locaux pour mesurer la concentration.

Dans l'ensemble, aux chapitres 2 et 3, la concentration du marché du travail diminue les salaires dans nos spécifications préférées – en contrôlant les caractéristiques temporelles invariantes des travailleurs, des entreprises et du marché du travail local, les chocs annuels à l'échelle du district dans le chapitre 2 et les chocs annuels par entreprise et par municipalité dans le chapitre 3. En fait, le chapitre 3 estime qu'une augmentation de 1 % de l'indice de concentration d'un marché du travail local entraîne une baisse des salaires comprise entre -0,019 % (Danemark) et -0,029 % (Allemagne). Ces estimations sont inférieures à celles documentées par des études américaines, où une augmentation de 1 % de la concentration réduit les salaires entre -0,05 % et -0,22 % (Benmelech et al. 2022, Qiu et Sojourner 2023, Arnold 2021). En outre, il convient de noter que la sensibilité aux choix méthodologiques va au-delà des seuls niveaux de concentration. Comme le montre le chapitre 2, les définitions des marchés du travail locaux peuvent également influencer sur l'ampleur des estimations des effets de la concentration du marché



du travail. Alors que l'élasticité salariale de la mesure de référence de la concentration est de -0,015 au Portugal,<sup>7</sup> d'autres mesures donnent des élasticités allant de -0,003 à -0,04 (chapitre 2).

Nous étendons également notre analyse pour étudier les effets non linéaires potentiels de la concentration du marché du travail sur les salaires, son impact sur l'inégalité salariale et le rôle de la taille de l'entreprise et des conventions collectives. Le chapitre 2 montre que les effets sur les salaires sont déterminés par la queue droite de la distribution de la concentration du marché du travail, que la concentration accroît l'inégalité salariale au sein de l'entreprise mais réduit les inégalités sur les marchés du travail locaux, et que les grandes entreprises sur les marchés concentrés peuvent jouir d'un plus grand pouvoir de marché. Dans le même temps, nos conclusions suggèrent que l'augmentation de la couverture des conventions collectives peut atténuer les démarques salariales (chapitres 2 et 3).

Les salaires ne sont toutefois pas le seul déterminant des coûts de la main-d'œuvre ou de la qualité de l'emploi. Le chapitre 3 contribue à la littérature en examinant les résultats de la sécurité de l'emploi, à savoir la probabilité d'avoir un contrat permanent ou d'être converti en contrat permanent dans l'année qui suit l'embauche. Nos résultats indiquent que les employeurs disposant d'un pouvoir de marché l'utilisent également pour réduire les coûts non salariaux. Les estimations sont considérables : les nouveaux embauchés sur les marchés où la concentration a augmenté d'un écart-type ont entre 5 % (France) et 24 % (Portugal) moins de chances d'obtenir un contrat permanent dans un premier temps et sont confrontés à des perspectives réduites de conversion en contrat permanent par la suite. Il est intéressant de noter que nous ne trouvons aucune preuve que la couverture sectorielle de la négociation collective atténue les effets de la concentration du marché du travail sur la sécurité de l'emploi. Étant donné que les conventions collectives sectorielles ne traitent généralement pas du type de contrat, nos résultats suggèrent que les entreprises peuvent exercer leur pouvoir de marché pour réduire les coûts non pécuniaires liés au travail plus librement que pour réduire les coûts pécuniaires.

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<sup>7</sup>La dimension des caractéristiques de l'emploi dans la définition de référence des marchés locaux du travail du chapitre 2 diffère légèrement de celle du chapitre 3 car elle utilise une classification plus désagrégée des professions. Lorsque l'on utilise le même niveau d'agrégation, les résultats sont identiques.

Dans l'ensemble, les éléments apportés par ces chapitres mettent l'accent sur des pistes politiques potentielles. En particulier, les mesures encourageant la syndicalisation et la négociation collective peuvent contribuer à contrecarrer le pouvoir de monopsonie. L'action préventive est également cruciale pour maintenir des marchés du travail compétitifs. Les autorités antitrust doivent intégrer des considérations relatives au marché du travail dans leur analyse des fusions potentielles et renforcer leur surveillance des comportements anticoncurrentiels préjudiciables aux travailleurs. À cette fin, l'amende infligée par les autorités antitrust portugaises aux clubs de football de la ligue professionnelle est un exemple d'un pas dans cette direction.

## Conclusion

L'espace est un facteur crucial pour déterminer l'accès aux soins de santé et aux opportunités d'emploi. Cette thèse montre comment il limite l'accès des travailleurs à des emplois de qualité et comment il affecte non seulement les conditions d'IVG des femmes, mais aussi leur capacité à en obtenir une. Sur ce dernier point, le chapitre 1 montre que plus les femmes vivent loin d'un service de santé qui pratique l'IVG, plus les conditions dans lesquelles elles font une IVG sont mauvaises. Il montre également que les régions qui s'éloignent le plus d'un service d'IVG connaissent moins d'IVGs, ce qui suggère que les longues distances qui séparent les femmes des services d'IVG les empêchent d'y avoir recours. Ces disparités régionales en matière d'accès peuvent donc nuire au bien-être des femmes dans de multiples domaines, en particulier à leurs droits génésiques et à leur santé. L'importance de l'espace s'étend également au marché du travail. Les chapitres 2 et 3 montrent que les travailleurs vivant dans des régions où les employeurs potentiels sont moins nombreux occupent des emplois de moins bonne qualité. En effet, les employeurs des marchés plus concentrés sont en mesure de leur verser des salaires plus bas et de leur proposer des contrats plus précaires.

À une époque de polarisation politique accrue, où certaines parties de la société, en particulier celles qui vivent dans les zones rurales, peuvent se sentir laissées pour compte, il est essentiel que les décideurs politiques inscrivent la lutte contre les disparités spatiales à leur ordre du jour. La promotion d'un accès régionalement équitable à l'IVG peut être réalisée par des solutions rentables telles que l'amélioration de la

transparence du fonctionnement du système de soins en matière d'IVG. Cela peut se faire en fournissant aux femmes des informations accessibles sur la manière et le lieu d'accès aux services d'IVG. De même, lors de l'évaluation des fusions et acquisitions, il est important de les évaluer non seulement sur la base de leur impact potentiel sur les marchés de produits, mais aussi sur les marchés du travail locaux. En outre, la mise en œuvre de politiques favorisant la syndicalisation est un autre moyen d'aider à égaliser les chances entre les travailleurs et les employeurs monopsonistiques. En conclusion, la négligence de l'espace est préjudiciable à la société. Si l'on veut que le bien-être des citoyens soit la priorité centrale des politiques publiques, l'espace doit lui aussi être au centre des décisions.

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# General Introduction

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Humans are naturally bounded by space and time. Curtailing these constraints is a central element of progress and social development. In that respect, public policy is essential to shorten distances between citizens, expanding access to a broader range of information, services, and opportunities, ultimately improving their well-being.

Disregarding the importance of space can lead to undesirable outcomes. Parts of the population may feel distant from political decisions, economically trapped, and, consequently, neglected. Such resentment may push depopulated and economically underperforming regions towards populist parties or movements (Margalit 2019).

To design policies addressing this discontent, it is crucial to understand the role of space. In this thesis, I analyze its interplay with access to health facilities and job opportunities. Given that health and work, are, according to the OECD's Better Life Index, two of the most important determinants of subjective well-being, conducting impact evaluations in these fields is of particular relevance for policy.

More specifically, I study how geography may influence access to abortion services and job quality. Even if access does not depend on space alone, geographical access is, in many cases, a necessary condition of access. This is particularly the case in Chapter 1, where I analyze how changes in proximity to abortion providers in Portugal affected women's ability to obtain an abortion and the conditions under which they abort. Geographical access is also at the core of chapters 2 and 3, where my co-authors and I examine how labour market concentration affects wages and job security. Manning and Petrongolo (2017) show that job search can be confined to relatively small areas. If workers live nearby concentrated – and uncompetitive – labour markets, the lack of job opportunities in their close environment may push them towards poorer quality jobs.

Throughout this thesis, space emerges as a relevant determinant of abortion care and job quality. In Chapter 1, I find that regions that become further away from an abortion provider experience fewer abortions. Moreover, women in those regions are at a higher risk of experiencing worse abortion conditions: they abort later and are more likely to have surgical abortions, an invasive and more costly procedure than the medical method. Chapters 2 and 3 also document that workers in more concentrated labour markets have lower wages and lower levels of job security; they are less likely to be hired on a permanent contract and converted to a permanent contract when hired on a temporary one.

In the remainder of this introduction, I will discuss the three chapters according to the following structure. I will first motivate why abortion access and employer market power warrant attention. I will then concisely present the methodologies that I use, the results I obtain and the corresponding policy recommendations.

## **1 Motivation and objectives of the thesis**

Space and well-being are central elements of this thesis. However, the three chapters also share another aspect in common: lately, their subjects have gained attraction in the public debate.

### **1.1 Space and abortion: a contemporaneous debate**

While the debate on abortion access has long been a contentious issue, ongoing legal developments in the United States have made it even more relevant. On June 2022, the US Supreme Court overturned *Roe v. Wade*, the case that legalized abortion on request in the United States. This decision gave back to each state the power to keep or ban legal abortion, making some women living in conservative states thousands of miles away from an abortion provider. On April 2023, Iowa was the first state to enable a legislation restricting inter-state traveling for women seeking an abortion. On the contrary, some liberal states, such as Washington, issued laws that protect women arriving within their borders to get an abortion. This points to the importance of geographical access for both pro-life and pro-choice advocates. And it is not without reason. Empirical studies document how US abortion laws that decreased proximity to abortion providers reduced abortion rates – e.g., Fischer et al. (2018) and Lindo et al. (2020).

In Portugal, unlike the United States, abortion laws have become increasingly liberal over the last decades. Until 1984, women were not allowed to abort under any circumstances. That year, abortion became legal only in cases of rape, fetal malformation, or when pregnancies threatened women’s lives. Then, in 2007, after a referendum, the Portuguese parliament approved abortion on request up to ten weeks of pregnancy. Under the new law, like other maternal health care services, abortions in the public sector were to be made free of charge.

Moreover, all public maternities were obliged to provide abortion services, a sign that policymakers intended to take advantage of the geographical coverage of the National Health Service (NHS) to provide equitable access to abortion. However, the law also protected doctors and nurses who had moral objections to performing abortions. This created challenges for some healthcare facilities as the unwillingness of their professionals to offer abortion services made it difficult to fulfill their legal obligations. In many cases, NHS hospitals had to rely on referring women in their designated areas to other providers. As a result, the absence of planning in the distribution of abortion care led to disparities in geographical access, which have recently gained significant media attention and attracted lawmakers' interest.<sup>8</sup>

Nonetheless, there is a lack of evidence on the impact of these disparities on women's well-being. First, findings from the United States may not be directly applicable to a country with different scale and institutional structures. Second, while the effects of large variations in access on women's health, such as the legalization of abortion, are well-documented (Clarke and Mühlrad 2021), there is limited research on the specific influence of proximity on women's abortion conditions. The first chapter of this thesis aims to fill these gaps.

## **1.2 Space and job quality: Labour market concentration and employer market power**

Beyond access to healthcare, space also affects access to other determinants of individual wellbeing, and in particular, to job opportunities.

Pullman was a town in Illinois. It was named after George Pullman, one of America's most influential businessmen in the XIX century and the owner of the Pullman Company, a dominant firm in the railway car market. The town was created to house Pullman Company's workers, and, except for some retail shops (Lindsey 1939), the entire city was under the firm's control. George Pullman was, thus, both the boss and the landlord of this population. For someone living in Pullman, it would be nearly impossible to change employer without leaving the city, making such a person more vulnerable

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<sup>8</sup>Since February 2023, the *Diário de Notícias* newspaper published a series of pieces on abortion access in Portugal, including reports on the availability of abortion provider across the country and on moral objection (Câncio 2023a-2023c). This culminated in a parliamentary hearing that took place on March the 30th in the same year (Comissão de Saúde 2023).

to worse job conditions, as the practices of “blacklisting, nepotism, favoritism, arbitrary dismissal, and tyranny on the part of foremen” (Lindsey 1939, p.284) of the Pullman Company illustrate. After the depression of 1893, the Pullman Company decided to cut down wages by 25% (on average), while keeping their high housing rents fixed, even if business at the Pullman Company was not severely affected (Lindsey 1939). This led to the historically famous “Pullman Strike”.<sup>9</sup>

Had there been more employers in the area instead of one, the fear of losing part of his labour force towards potential competitors could have led George Pullman to decide differently. However, there were not, meaning that the labour market supply of Pullman (the town) was controlled by the Pullman Company. This situation, in which a market has only one buyer, is defined as a monopsony. Nevertheless, the discussion of monopsony in the labour market extends beyond the one-buyer case. It refers more broadly to the ability of firms to pay wages below workers’ marginal productivity.

As Manning (2021) illustrates, monopsony power in the labour market is the norm rather than the exception: if a firm lowers all wages by one cent, it will not lose all of its labour force. This can be due to different factors. First, not all workers leave the firm because searching for jobs is costly. Second, workers’ heterogeneous preferences for firms’ characteristics can also be a source of monopsony (Card et al. 2018). For example, disabled workers will value high workplace accessibility, which reduces their number of suitable outside options, making them more vulnerable to employer’s market power. Also, firms may coordinate to reduce the number of outside options available to workers. One way to achieve this is through collusion. During the COVID-19 pandemic, Portuguese professional league football clubs agreed they would not hire players from other teams if they had unilaterally terminated their contracts due to unpaid wages. This no-poach agreement resulted in a landmark decision by the Portuguese antitrust authority, which issued its first fine for anti-competitive behaviour in the labour market.<sup>10</sup> Alternatively, employers can also restrict the mobility of workers across firms through non-compete clauses, as evidenced in the USA (Starr et al. 2021) and Italy (Boeri et al. 2023).

Finally, as the Pullman case illustrates, monopsony power can arise from labour

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<sup>9</sup>For further details on the history of the “Pullman Strike” see Lindsey (1939).

<sup>10</sup><https://www.concorrenca.pt/en/articles/adc-issues-sanctioning-decision-anticompetitive-agreement-labor-market-first-time>

market concentration – the scope of Chapters 2 and 3. Recent literature finds that workers in more concentrated markets have lower wages – Azar et al. (2022), Marinescu et al. (2021), Bassanini et al. (2023), and Dodini et al. (2023) among others –, with the magnitude of these effects varying across countries. Whether such differences are setting-specific or due to the diverse methodological approaches used to measure concentration and its impact is an open question. Chapters 2 and 3 address this issue in two distinct manners. The former chapter uses comprehensive administrative data to understand how each methodological choice affects both the levels and the effects of labour market concentration. The latter uses data from six European nations and a common methodology to provide the first cross-country comparable estimates of the impact of concentration on job quality.

Overall, while there has been extensive work on how labour market concentration affects wages, evidence on non-wage dimensions of job quality is scarce (Qiu and Sojourner 2023). In that regard, Chapter 3 contributes to the literature by examining a previously unexplored aspect of job quality: job security.

## 2 Methods and data

The Royal Swedish Academy of Sciences awarded the 2021 Nobel Prize in Economics Sciences to David Card, Joshua Angrist, and Guido Imbens for having

(...) provided us with new insights about the labor market and shown what conclusions about cause and effect can be drawn from natural experiments.  
(Royal Swedish Academy of Sciences 2021)

Natural experiments, as the name suggests, naturally approximate the conditions of an experiment where individuals are randomly assigned to treatment and control groups. In doing so, they allow for an understanding of cause and effect relationships in situations where experiments would be difficult, if not impossible, to conduct. For example, it would be neither ethical nor practical to randomly assign children to women, in order to understand how fertility affects female labour market outcomes. Angrist and Evans (1998) ingeniously circumvent this problem using a natural event that mimics a coin toss: the assignment of sex at birth. To be more precise, in the United States,



couples have a preference for gender diversity among their children. Families with either two boys or two girls are more likely to try to have a third child than those with one boy and one girl. Angrist and Evans (1998) leverage this quasi-experiment to identify how one more child affects mothers' labour supply. This is but one example of how, according to the Royal Swedish Academy of Sciences, the 2021 Nobel Prize winners "revolutionized empirical research".

Since the three chapters in this thesis are applied investigations in microeconomics, they naturally use some of the tools that the 2021 Nobel laureates employ to retrieve causal relationships, as I detail below.

## 2.1 Chapter 1: Differences-in-Differences

In Chapter 1, I examine how geographical access affects the ability of women to abort and their abortion conditions, namely when (number of weeks with which they abort), where (public or private providers), and how (surgically or medically). I proxy geographical access by the travel time by car between women's municipalities of residence and their nearest abortion provider. Then, to identify the effects of proximity to abortion care, I rely on a Differences-in-Differences (DID) design.

The classical DID estimator calculates the difference in outcomes between two groups before and after one of them received a treatment. It then subtracts the before-and-after differences to compute the impact of that treatment. The underlying assumption is: had the treatment not occurred, the outcomes of treated individuals would have followed the same path as those of the non-treated – this is known as the parallel trend assumption.<sup>11</sup> An example of an application of the DID design is the seminal paper by Card and Krueger (1994), where they exploit the 1992 increase in New Jersey's minimum wage to examine how employment growth in the fast food industry changed relative to Pennsylvania (the control group).

The classic two-groups by two-periods DID design can be further augmented with more groups, periods, and more than one treatment timing. In the setting of Chapter

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<sup>11</sup>Interestingly, Callaway et al. (2021) show that when treatment is not binary – such as having access to legal abortion or not – but rather continuous – such as the time that it takes to go from women's residence to an abortion provider – researchers need to modify this assumption to make causal inference. They need to assume that all regions would react in the same manner to each possible change in proximity to an abortion provider.

1, abortion services open and close at different times in different cities, which, for some women, changes the amount of time they need to reach a provider or, said otherwise, their treatment intensity. I exploit this natural experiment and compare regions where proximity to an abortion provider changed to those where it did not. To that end, I estimate a Poisson regression with two-way fixed effects – i.e., that controls for time-invariant characteristics of municipalities and for yearly shocks that affect all of them simultaneously. One problem with two-way fixed effects regressions in settings with different treatment timings, such as this one, is their use of regions that were already treated as controls, which can produce biased estimates (de Chaisemartin and D’Haultfoeuille 2020, Goodman-Bacon 2021). To deal with this issue, in Chapter 1, I employ the stacked DID method proposed by Cengiz et al. (2019), meaning that for each treated region I build a dataset including that region and, in the control group, regions that had not yet seen any change in proximity to abortion services. Then, I stack all the constructed datasets together to obtain the final dataset.

The analysis in Chapter 1 uses abortion registries data which contain all legal abortions performed in Portugal between 2008 and 2018.<sup>12</sup> This dataset originates from the General Directorate of Health and is a collection of mandatory surveys that women must fill out when requesting an abortion. It contains detailed socioeconomic information on women, namely their age, occupation, educational level, and, most importantly, their municipality of residence. It also reveals relevant aspects of the abortion care that women received, including the number of weeks of pregnancy when they accessed abortion services and when they aborted, the type of provider they accessed and where they aborted (public or private), and the abortion method that was used (surgery or medical abortion). Additionally, to complement the analysis, this chapter also incorporates data on births, population by gender and age group, marriages, regional Gross Domestic Product – this dataset originates from Statistics Portugal – and unemployment records of municipalities – from the Institute of Employment and Vocational training.

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<sup>12</sup>I am unable to use the years of 2017 and 2018 because there is no information on which abortion providers operated in those years and, as such, it is not possible to compute the travel time to the nearest abortion provider.

## 2.2 Chapters 2 and 3: Labour market concentration measures and instrumental variables

Chapters 2 and 3 measure labour market concentration and estimate its effects on job quality for Portugal alone and with five other European countries – Denmark, France, Germany, Italy, and Spain.

### Measuring labour market concentration

To assess the levels of concentration of local labour markets, the first step is to determine what constitutes a local labour market. Ideally, it should identify the set of workers and firms that can supply or demand labour for a given job or task. The standard approach considers local labour markets as geographical areas by job characteristics pairs. Using geographical areas to bind local labour markets is consistent with evidence that job search is local (Manning and Petrongolo 2017). Thus, it is justifiable to disregard workers that live too far away as potential employees, even if their profile is a good fit for a vacancy. Job characteristics are another reasonable bound of local labour markets, as mismatches between workers' tasks and skills should make these individuals ill-suited for specific job openings.

Chapter 2 considers Portuguese district-by-occupation pairs as the benchmark definition of local labour markets, while Chapter 3 groups occupation with a new geographical unit that we construct by combining NUTS III regions (or districts, in the case of Portugal) with Eurostat's functional urban areas.

In Chapters 2 and 3, the preferred definition of job characteristics is occupation and not industry, like in Rinz (2022) or Benmelech et al. (2022), as this would probably separate workers who could serve as suitable substitutes to firms. For example, a welder in the naval construction industry can likely compete for a job with a welder in the railway industry, even if they work in different sectors. Nevertheless, in Chapter 2, we test the sensitivity of labour market concentration levels and effects across some of the various definitions of local labour markets present in the literature, such as considering industry instead of occupation or using smaller regions.

After defining local labour markets, the next step is to measure their concentration levels, which we do by computing the Herfindahl-Hirschman index (HHI) – the sum

across all firms in a local labour market of their share of employment in that market. Both employment stocks (all workers the firm employs) and employment flows (new hires) can be used to compute this index. Chapter 2 calculates HHIs using both, whereas Chapter 3 only uses the latter, since employment flows may capture monopsony better than stocks. This is because employers with market power will drive labour-related costs (wage or non-wage remuneration) down by rationing their demand for labour, which likely translates into opening fewer vacancies and hiring fewer workers.

### **Estimating the effects of labour market concentration**

Assessing if employers in more concentrated markets enjoy more market power and consequently can offer lower wages and job security<sup>13</sup> requires more than a simple comparison of job quality across different levels of labour market concentration. By doing so, it would not be possible to understand whether labour market concentration affects labour-related costs or whether it is the inverse situation – a problem of reverse causality. One can, indeed, imagine a scenario where a decrease in wages in a given occupation in a given region attracts new firms, hence decreasing concentration in that local labour market. Thus, finding a positive relationship between these two variables is not meaningful from a causal perspective.

One solution to this problem is to use a third variable that affects the concentration of local labour markets but is unrelated to job quality – except through labour market concentration. In econometrics, this third variable is known as an instrument. One highly influential example of the use of instrumental variables (IV) is the aforementioned paper of Angrist and Evans (1998). In the natural experiment they exploit, having two children of the same gender will arguably only affect a mother’s labour supply through the impact on the probability that she will have one more child.

In Chapters 2 and 3, to retrieve the causal effect of labour market concentration on job quality, we instrument the concentration index of local labour markets by the average (of the inverse) number of employers that operate in that labour market, excluding the local labour market’s region. With this instrument, we intend to capture nationwide changes in concentration resulting, for example, from mergers and/or divestitures. These will

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<sup>13</sup>We measure job security as the likelihood of being hired on a permanent contract or converted to a permanent contract if hired on a temporary one.

affect local labour market concentration, but they will not directly impact labour costs in those local labour markets.

Both Chapters rely on linked employer-employee datasets – Chapter 2 uses Portuguese data from 1985 to 2019, while Chapter 3 uses data for the 2010s decade for six European countries. For Portugal, the source we use is the “Quadros de Pessoal” records, a yearly snapshot of the Portuguese labour market as of October (March until 1993). This dataset covers the entire private sector and has unique identifiers for firms, establishments, and workers. It also contains information on the geographical location, industry of firms and their establishments, and data on workers’ age, gender, wage, contract type, hours worked, and educational level, among others.

Together with Portugal, the available data in the other five European countries – described in Appendix Section F of Chapter 3 – allow us to investigate the effect of labour market concentration on Danish, French, and German wages. Regarding the outcomes related to job security, we can examine how concentration affects the probability of being hired on a permanent contract for all countries except Denmark. In contrast, the analysis of the likelihood of converting a temporary contract into a permanent one is only possible for Italy and Spain.

## **3 Results and avenues for policy**

### **3.1 Chapter 1: The effects of proximity to abortion providers on abortion rates and conditions**

In Chapter 1, I show that regions over one hour away from an abortion provider experience 22% fewer abortions than those within 30 minutes of an abortion service. These regions also display lower abortion ratios – i.e., the number of abortions for every pregnancy in the municipality –, which is consistent with the fact that women in those regions arrive later at abortion services, consequently aborting later. Thus, some women may arrive at abortion services too late to get an abortion within the legal gestational limit of ten weeks. These women are also at a higher risk of experiencing worse abortion conditions across other dimensions beyond abortion timings. In fact, they are more likely to be referred from public hospitals, which are unable to accom-

moderate them, to private providers and more prone to have surgical abortions, which are invasive and more costly than the medical method.

One question that naturally arises is whether reducing distances between women and abortion providers through the creation of new services would result in savings for the NHS. However, based on my findings, the potential savings would be limited, as they would only allow for hiring one entry-level general practitioner over three years. Despite the limited cost savings, such a policy would still benefit women who would otherwise be unable to access abortion services in due time.

To further increase proximity to abortion care, the NHS could explore utilizing the primary care network by allowing general practitioners to perform medical abortions, since this procedure is not technically challenging, as recognized by the World Health Organization (2012).

Finally, it is worth highlighting that in Portugal information on provider availability is not readily accessible. This creates unnecessary barriers for women seeking abortion care, which is particularly relevant, considering that I find that the delays in obtaining abortion caused by lack of proximity are primarily due to late arrivals to abortion services. Thus, a simple online tool or listing of abortion providers could already improve women's ability to carry their decision to abort, as well as their abortion conditions.

### **3.2 Chapters 2 and 3: The levels of labour market concentration and its effects on job quality**

In both Chapters 2 and 3, the benchmark measures of labour market concentration reveal similar average concentration indices for Portugal – approximately 1,000. These levels are comparatively higher than those observed in the other five European countries – around 600. Notably, Germany exhibits the lowest levels of concentration, with an index of 587.

Chapter 2 also assesses the sensitivity of concentration indices to methodological choices. Considering smaller administrative regions or using industry rather than occupation to characterize job characteristics results in concentration indices nearly twice as high as our benchmark measure. Conversely, employing broader occupational categories yields lower estimates of labour market concentration. Together, these results

serve as a valuable point of reference for researchers when defining local labour markets to measure concentration.

Overall, in Chapters 2 and 3, labour market concentration decreases wages in our preferred specifications – controlling for worker, firm, and local labour market time-invariant characteristics, district-wide yearly shocks in Chapter 2 and firm-by-municipality-by year shocks in Chapter 3. In fact, Chapter 3 estimates that an increase by 1% in the concentration index of a local labour market leads to a decrease in wages between -0.019% (Denmark) and -0.029% (Germany). These estimates are lower than those documented by US studies, where a 1% increase in concentration reduces wages between -0.05% and -0.22% (Benmelech et al. 2022, Qiu and Sojourner 2023, Arnold 2021). Furthermore, it is worth noting that sensitivity to methodological choices extends beyond concentration levels alone. As Chapter 2 demonstrates, local labour market definitions can also impact the magnitude of the estimates of the effects of labour market concentration. While the wage elasticity of the benchmark measure of concentration is -0.015 in Portugal, other measures yield elasticities ranging from -0.003 to -0.04 (Chapter 2).

We also extend our analysis to investigate potential non-linear effects of labour market concentration on wages, its impact on wage inequality, and the role of firm size and collective bargaining agreements. Chapter 2 finds that wage effects are driven by the right tail of the labour market concentration distribution, that concentration increases within-firm wage inequality but reduces inequalities within local labour markets, and that larger firms in concentrated markets may enjoy more market power. At the same time, our findings suggest that increasing coverage of collective bargaining agreements can attenuate wage markdowns (Chapters 2 and 3).

Wages, however, are not the only determinant of labor costs or job quality. Chapter 3 contributes to the literature by examining job security outcomes, namely the probability of having a permanent contract or of conversion to one within one year after being hired. Our findings indicate that employers with market power also use it to lower non-wage costs. The estimates are sizeable: new hires in markets where concentration increased by one standard deviation are between 5% (France) and 24% (Portugal) less likely to have a permanent contract initially and face reduced prospects of conversion to a permanent contract later on. Interestingly, we do not find evidence that sector-level coverage of collective bargaining attenuates the effects of labour market concentration

on job security. As sector-level collective bargaining agreements do not typically address contract type, our results suggest that firms can exert their market power to reduce non-pecuniary labour-related costs more freely than to reduce pecuniary ones.

Overall, the evidence brought by these chapters emphasizes potential avenues for policy. In particular, measures fostering unionization and collective bargaining can help counteract monopsony power. Preventive action is also crucial in maintaining competitive labour markets. Antitrust authorities must incorporate labour market considerations in their analysis of potential mergers and increase their oversight over anti-competitive behaviour harming workers. To that end, the Portuguese antitrust authority fine of professional league football clubs is an example of a step in this direction.



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# Chapter I:

## Do all roads lead to the same destination? Proximity to abortion providers, abortions and their conditions in Portugal

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## 1. INTRODUCTION

Women undergoing unsafe abortion are at a higher risk of having health problems or even dying (Grimes et al. 2006). The World Health Organization (WHO) has defined the elimination of unsafe abortion as one of its top five priorities of its global reproductive health strategy (WHO 2004). Legalizing abortion was shown to be an effective way to decrease maternal mortality and morbidity (Clarke and Mühlrad 2021), and, indeed, since 2007, when Portugal made abortion legal and fully subsidized, there has been a decrease in the number of abortion-related deaths. Between 2001 and 2007, there were, on average, two deaths per year – out of an estimated number of 20,000 yearly illegal abortions – while, between 2008 and 2014, a total of two deaths were registered in seven years – for this period, the annual number of abortions ranged between 16,762 and 20,480 (Vicente 2020). However, the legal status of abortion is not the only dimension of access at stake. As Grimes et al. (2006) point out: "enabling abortion legislation is necessary but not sufficient: a new law might not translate into widespread access to safe services" (p. 6).

One important dimension is the actual distance to a provider, which can be seen as the physical, and logistical cost faced by women when traveling to an abortion facility (Kimport 2022). Increases in this cost render abortion more difficult to obtain, and thus, likely influence not only whether women abort (Kane and Staiger 1996), but also the conditions under which they abort, namely when (Bitler and Zavodny 2001; Lindo et al. 2020), where, and how. This is, of course, relevant for the well-being of women. In fact, not being able to abort can lead to worse present and future financial and economic conditions (Miller et al. 2023). Furthermore, when a woman is able to abort, "any delay increases the risk of complications" (Cates et al. 1977:268). Given that later abortions are more demanding in terms of logistics and training of healthcare professionals (Harris 2008), delays may also reduce the number of available abortion providers. In addition, as medically induced abortions lose efficiency after the ninth

week of pregnancy (Winikoff et al. 2008), delays may lead to the use of surgical abortion,<sup>1</sup> a less time-consuming method,<sup>2</sup> but an invasive and more costly procedure.

The role of proximity to abortion services in affecting whether women abort is a growing topic in applied economics. Quast et al. (2017), Fischer et al. (2018), Lindo et al. (2020), Venator and Fletcher (2021), and Myers (2021) document that the number of abortions in U.S. counties fell in response to increases in the travel distance to the nearest provider. However, there is not much research on how proximity affects when, where, or how women abort. To the best of our knowledge, only Lindo et al. (2020) address how being close to a provider affects when abortions occur.

In this paper, not only do we analyze if proximity to a provider affects whether and when women abort, but we also consider how proximity determines other characteristics of abortions, such as where they take place – with a public hospital or a private clinic – and how they are performed – using the medical or surgical method.

To measure proximity, we use the travel time from the women's municipality of residence to the nearest abortion provider. In Portugal, travel times to the nearest abortion provider have varied over time due to the shutdowns and openings of abortion services. To examine how changes in travel time affect abortion numbers and their conditions, we rely on an individual-level dataset of all abortions occurring in Portugal between 2008 and 2016. It contains information on the number of weeks of gestation at the time of abortion and women's socioeconomic characteristics, including their municipality of residence. Together with the information on the location of abortion providers throughout time, this allows us to compute

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<sup>1</sup> The Portuguese General Directorate of Health recommends the use of the surgical method to terminate pregnancies above nine weeks of gestation (DGS 2007a).

<sup>2</sup> For medical abortions, women need to make two to three visits to the doctor: one to take the mifepristone, and another, 48 hours later, to take Misoprostol. If abortion did not take place during the second visit, a third visit is required to check if abortion was successful (DGS 2007b). As for surgical abortions, women only need to make one trip to the doctor. However, as it is a surgical procedure, it demands more hospital resources, namely an operating room and an anesthesiologist (DGS 2007c).

the travel time from the populational center of each municipality to the nearest abortion provider. We find that living far away from a provider is associated with fewer abortions in the area of residence and also with aborting later. Moreover, these late abortions appear to be primarily caused by late arrivals to abortion care. For abortion services, it may prove challenging to accommodate women at later gestational ages, within the legal time limits, and to use the medical method since it is more time-consuming than the surgical procedure but non-invasive and less costly. The challenge is particularly difficult for public hospitals, which are chronically short of healthcare professionals who are not conscientious objectors (Oliveira da Silva 2009). As a consequence, they have the incentive to refer women to private clinics that predominantly use the surgical method. We provide evidence of this mechanism by showing that longer trips to a provider are associated with a higher likelihood of being referred from a public hospital to a private clinic and a greater risk of having a surgical abortion. Although abortion supply might be partially determined by demand, in the Results section, we show that our findings are unlikely to be driven by the endogeneity of the travel time to the nearest abortion provider. Finally, even though women living far away are at a higher risk of having the more costly surgical abortion, a back-of-the-envelope calculation suggests that providing proximity abortion services through the NHS would only save a limited amount of public money. However, this would reduce the individual burden of undergoing a late and invasive abortion.

This paper contributes to the economic literature studying how abortion access, in particular proximity, affects abortion rates. Theoretically, decreased access to abortion should reduce the number of abortions through two channels (Kane and Staiger 1996; Levine and Staiger 2002). On the one hand, pregnant women may decide to carry their pregnancy to term, as abortion becomes too costly. On the other hand, women may exert more effort to avoid pregnancy, leading to fewer unwanted pregnancies and, consequently, fewer abortions (Kane and Staiger



1996; Levine and Staiger 2002). Both theoretical predictions find support in empirical studies, most of which rely on natural experiments conducted in the USA as a source of exogenous variations in abortion access. Cook et al. (1999) take advantage of North Carolina's intermittent reimbursement of abortion procedures throughout the year – that randomly left some women uncovered – to show that unexpected funding restrictions reduce the number of abortions. Quast et al. (2017), Fischer et al. (2018), Lindo et al. (2020), and Venator and Fletcher (2021) exploit restrictive abortion laws that led to drastic reductions in the number of abortion clinics in Texas and Wisconsin and show that increasing the distance to an abortion provider reduces abortion rates. Recently, Myers (2021) used variation in distances to the nearest provider across the entire United States to find similar impacts to the ones found for Texas and Wisconsin. In our paper, we explore a setting where, unlike the United States, a universal health care system provides abortions free-of-charge and where traveling is the only tangible cost faced by women. In Portugal, the time constraint is particularly relevant since the legal gestational age limit for abortion is only ten weeks, the lowest among high-income countries (Popinchalk and Sedgh 2019). To our knowledge, together with Brooks and Zohar (2021),<sup>3</sup> this is also one of the few papers using non-US data to examine how abortion access affects the number of abortions. We also are the first in the literature to correct for the potential bias of two-way fixed effects (TWFE) regressions when measuring the impact of proximity on abortion rates (de Chaisemartin and D'Haultfoeuille 2020, Goodman-Bacon 2021). To that end, we implement a stacked difference-in-differences as in Cengiz et al. (2019).

Our paper also contributes to the literature on how barriers to abortion affect its timing. The intuition is that women with deprived access may need more time to reach abortion services, hence delaying abortions (Lindo et al. 2020). For the United States, a number of state-specific

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<sup>3</sup> Brooks and Zohar (2021) examine how expanding access to free abortion in Israel affected abortion rates. They find that abortions increased in response to this policy, while the probability to become pregnant was not affected.

studies find that women abort later when facing mandatory reflection periods (Joyce and Kaestner 2001; Lindo and Pineda-Torres 2021), or the need to obtain parental consent (Joyce and Kaestner 2001), or congested abortion services (Lindo et al. 2020; Kelly 2020). However, it is unclear how proximity affects abortion timing (Lindo et al. 2020). In the present paper, we find suggestive evidence that living far away from a provider leads women to abort later. As far as we know, we are the first to examine how access affects abortion timing in a European country. We also look at the consequences that these delays cause on other aspects of the abortion process, namely where – being referred to a private clinic by a public hospital – and how women abort – medical or surgical method – which has not been tackled so far in the literature.

The structure of the paper is the following: Section 2 provides institutional background on the legal framework and distribution of abortion services in Portugal; Section 3 presents the empirical strategy; Section 4 introduces and describes the data; Section 5 provides the results, Section 6 presents the discussion, and Section 7 the conclusions.

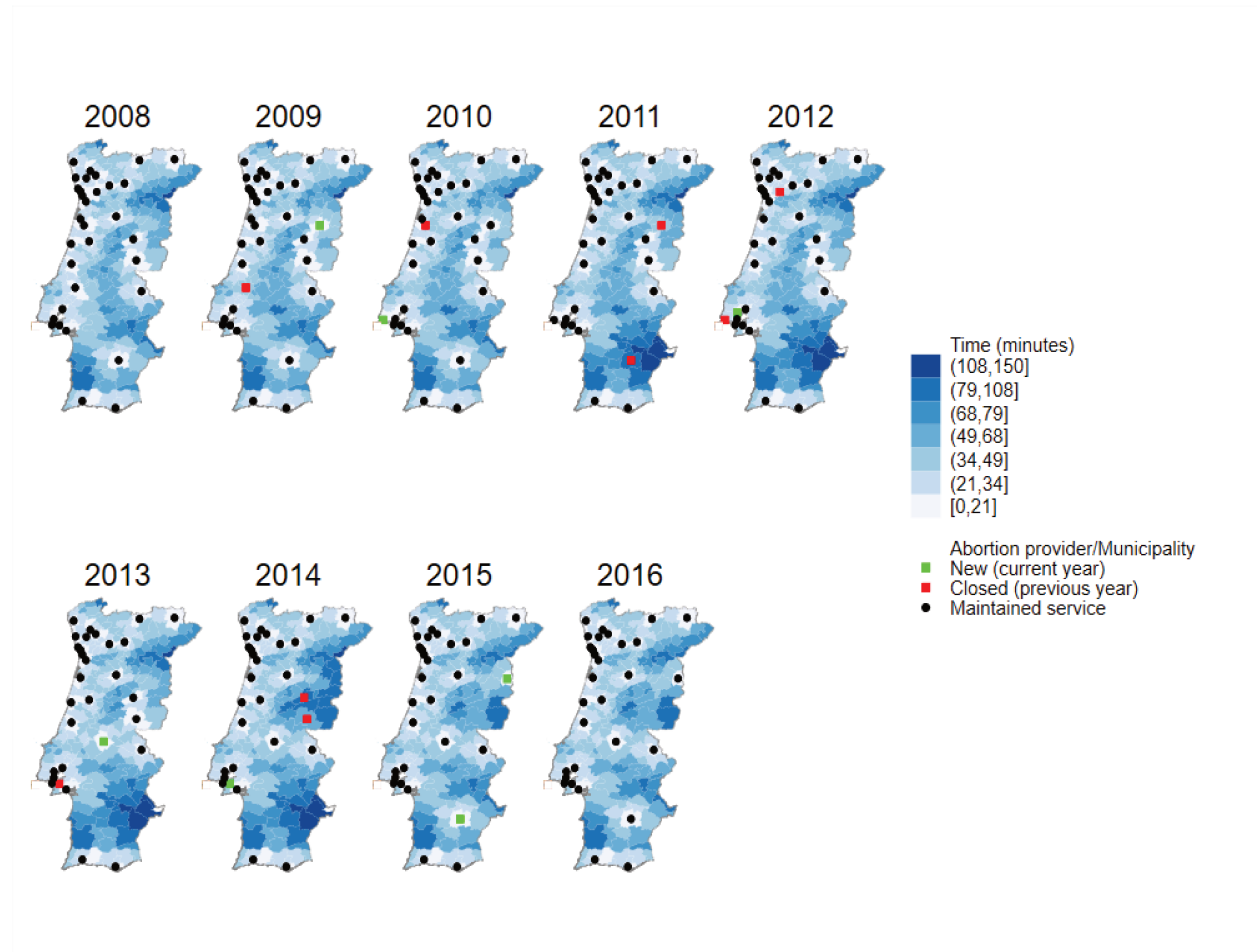
## **2. INSTITUTIONAL BACKGROUND**

Before dwelling on how proximity to a provider affects the number of abortions and the conditions under which they occur, we must point out that in Portugal, abortion on request only became legal in 2007 (Law 16/07). The Portuguese law is quite restrictive compared to its European counterparts: it has the lowest gestational age limit among high-income countries (Popinchalk and Sedgh 2019) – ten weeks of pregnancy – and it requires a mandatory reflection period of three days, as well as parental consent for minors below the age of 16.

Upon legalization, there was a swift effort to generalize access to abortion services, either public or private. A key factor for that rapid expansion was the provision of abortions free-of-

charge within the pre-existing public maternity network of the NHS, which is of universal access (Simões et al. 2017). To that end, the law determined that all public hospitals with a gynecology or obstetrics department should provide abortion services (Portaria741-A/2007). According to the standard procedure, to access public abortion, women should first contact primary care services, which should refer these women to the regional specialized public hospital. However, it is admissible that women bypass this system and access hospital services directly (Simões et al. 2017).

**Figure 1** Provision of abortion across municipalities and travel time to the nearest abortion provider



However, as 85% of gynecologists in Portugal were conscientious objectors (Oliveira da Silva 2009), some hospitals could not provide abortion services, undermining the local supply of

abortion and leading to discrepancies in the geographical distribution of providers, which, as we show in Figure 1, varied across time.

If public hospitals do not have abortion services or cannot promptly provide them within the legal limit of ten weeks of gestational age, hospitals must redirect women living in the area they cover toward another provider – either a public hospital or a certified private clinic. In that case, the cost of the abortion is directly paid by the hospital first contacted by the woman to that which performed the abortion – at a set price, defined by law, depending on the abortion method. Overall, in the NHS, the financing of abortion procedures, outsourced or not, is provided through each hospital's fixed budget. Hospital budgets are set according to the hospital's cost history, use, and complexity indicators (Simões et al. 2017). Women may also directly access private providers, but, in this case, they must pay for the abortion procedure themselves.

### **3. EMPIRICAL STRATEGY**

The first goal of this paper is to understand how proximity affects the probability that women abort. Given that we do not have individual-level data for all fertile females in Portugal to estimate how variations in travel time affects the probability to abort, we conduct an analysis on the number of yearly abortions at the municipality-level – the lowest geographical aggregation level at our disposal. Then, using individual-level data on all abortions, we examine how proximity affects the conditions under which abortion occurs, namely when, where, and how.

#### **3.1. Number of abortions in municipalities**

We start by examining how travel time affects the number of abortions among all women at the municipality-level. To estimate this effect, we follow the literature and use a difference-in-differences design where the treatment is continuous. In this setting, we want to assess the

causal response to treatment dosage – i.e., travel time to the nearest abortion provider, with longer distances representing higher treatment intensities or doses. One potential issue with previous studies is that they rely on TWFE regressions to retrieve causal responses. Recent literature shows that in some cases, TWFE regressions potentially estimate treatment effects poorly (de Chaisemartin and D’Haultfoeuille 2020, Goodman-Bacon 2021). This is particularly problematic in our setting where treatment is continuous (Callaway et al. 2021) and non-staggered, meaning that municipalities change treatment dose at different timings and, possibly, multiple times. First, TWFE regressions attribute greater weight to treatments occurring in the middle of the panel. Second, TWFE regressions make “forbidden comparisons” (Borusyak and Jaravel 2017), which, in our case, translates to comparing municipalities that change treatment at one point in time with those that had already changed dose. If treatment effects – or causal responses in the continuous treatment case – are heterogenous or dynamic, such comparisons can lead to a negative weighting of some of the estimated treatment effects, possibly yielding estimates bearing the wrong sign (Callaway et al. 2021).

The literature proposes different estimators to overcome the bias of TWFE regressions. The common element behind the new estimators is that they all try to avoid “forbidden comparisons” by taking out from the control group all units that have already been treated or, in the treatment intensity case, those which have already switched dose. We use the stacked difference-in-differences strategy suggested by Cengiz et al. (2019),<sup>4</sup> which is particularly

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<sup>4</sup> For recent papers in applied microeconomics that use stacked difference-in-differences when encountering similar issues see, for example, Abouk et al. (2023) or Mathur and Rhum (2023).

well-suited to the model we estimate – a non-linear regression model with a non-staggered and continuous treatment.<sup>5</sup>

We implement the stacked difference-in-differences as follows: for each municipality  $m$  changing treatment dose we construct a dataset – hereafter, we will refer to it as a stack – composed by that municipality and those where travel time never varied (never changers) or where it did not vary yet (not-yet-changers). This ensures we compare each municipality to a “clean” control group. We start with an initial number of 176 stacks. As our setting allows treatment dose to change multiple times, in the spirit of the solution proposed by de Chaisemartin and D’Haultfoeuille (2022a), we only keep stacks of municipalities that change dose for the first time so that we avoid contamination by past treatments, thereby keeping 109 stacks. Moreover, since we want outcomes at  $T+1$  to be caused by treatments at  $T$  rather than treatments at  $T+1$ , we also exclude stacks of municipalities that change dose in two consecutive years – thus doing we drop nine stacks. Then within each stack, we only keep observations corresponding to the two years before municipality  $m$ ’s change in treatment and the year immediately after.<sup>6</sup> To ensure that stacks are balanced and that all treatment changes occur in their mid-point – thus avoiding the weighting bias arising from different treatment timings – we restrict our analysis to municipalities changing treatment doses between 2010 and 2015. Hence, we drop 32 stacks from the analysis.<sup>7</sup> Finally, we append the remaining 68 stacks together and estimate the general model below:

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<sup>5</sup> Other strategies have been proposed but these are well-suited for linear models. See de Chaisemartin and D’Haultfoeuille (2022b) or Roth et al. (2023) for a survey of this recent literature, including a presentation of the alternative estimators to TWFE regressions.

<sup>6</sup> Since our time windows restricts the post-treatment period to  $T$  and  $T+1$ , we keep municipalities that change dose two or more times with at least a one year gap between switching dose as these will not pose a threat of contamination by future treatments.

<sup>7</sup> If we keep municipalities that were treated in 2009, we do not have data for the two years preceding the treatment. Similarly, for municipalities treated in 2016, we do not have data for 2017. Thus we drop those treatments from our analysis to ensure that our panel is balanced.

$$N^oAbortions_{m,t,s} = \beta Time_{m,t,s} + X_{m,t,s}\gamma + \varphi_{m,s} + \zeta_{t,s} \quad (1)$$

where  $N^oAbortions_{m,t,s}$  is the number of abortions among women living in municipality  $m$ , in year  $t$  in stack  $s$ .  $Time_{m,t,s}$  is the travel time between the nearest abortion provider and the town hall of municipality  $m$  in year  $t$  in stack  $s$ .  $X_{m,t,s}$  is a vector of time-varying municipality-level controls, namely the share of each age group – defined in 5-year intervals – in the population of fertile women, the insured unemployment rate of the municipality, the GDP per capita growth rate of the NUTS III region, and the number of marriages and catholic marriages per thousand inhabitants in the municipality. As control municipalities appear multiple times in different stacks, we follow the recommendation of Baker et al. (2022) and include municipality-by-stack ( $\varphi_{m,s}$ ) fixed effects. We cluster standard errors at this level. Finally, we also add year-by-stack fixed effects ( $\zeta_{t,s}$ ). The coefficient of interest is  $\beta$ , which we expect to carry a negative sign, as increases in travel time should make abortion more costly, leading women to either carry their pregnancies to term or to avoid becoming pregnant at all.

As some municipalities have zero abortions in a number of years – see Figure A1 (appendix) – we estimate the general model above by a Poisson pseudo-maximum likelihood estimator,<sup>8</sup> using the number of fertile women in municipality  $m$  in year  $t$  as the exposure variable.<sup>9</sup> To be more precise, we estimate:

$$E(N^oAbortions_{m,t,s} | Time_{m,t,s}, X_{m,t,s}, \varphi_{m,s}, \zeta_{t,s}) = e^{(\beta Time_{m,t,s} + X_{m,t,s}\gamma + \varphi_{m,s} + \zeta_{t,s})} \quad (2)$$

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<sup>8</sup> As Lindo et al. (2020), we also estimated the above model by OLS, using an Inverse Hyperbolic Sine Transformation of the dependent variable, which has the advantage of being defined at zero. As evidenced in Table B1 (online appendix) the results are robust to this change, both in magnitude and in statistical significance.

<sup>9</sup> Using the number of abortions as the dependent and the number of fertile women as the exposure variable is equivalent to analyzing the abortion rate, as the exposure variable has its coefficient restricted to one.

We do not consider alternative non-linear estimation methods such as the negative binomial regression because, unlike the Fixed Effects Poisson, they usually suffer from the incidental parameters problem (Cameron and Trivedi 2005).<sup>10</sup> Furthermore, Wooldridge (1999) shows that the Fixed Effects Poisson estimator with robust standard errors relies on minimal assumptions, namely that it is robust to distributional misspecifications and is consistent as long as the conditional mean is correctly specified.<sup>11</sup>

As a second step, to understand the potential channels in action, we analyze how travel time affects the number of abortions among pregnant women. To do so, we estimate the model described in equation (1), using the number of pregnant women as the exposure variable, which is equivalent to estimating the effect of travel time to the nearest provider on the abortion ratio – i.e., the number of abortions divided by the number of pregnancies.<sup>12</sup>  $\beta$  should be negative, as increases in travel time should make abortion harder to obtain, and thus, some pregnant women may no longer be able to abort. Finally, we examine whether municipalities becoming farther away from a provider experience fewer unwanted pregnancies. As we do not have information on unwanted pregnancies, we restrict our analysis to teenage pregnancies, which, in principle, should be unwanted. In this case, we use the population of adolescent women aged between 15 and 19 years old as the exposure variable. Once again,  $\beta$  should be either negative.

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<sup>10</sup>Moreover, the negative binomial regression with fixed effects may not even be able to account for time invariant variables (Guimarães 2008).

<sup>11</sup> Even when the outcome is continuous – or close to continuous, as in our case – the Fixed Effects Poisson regression is still a consistent estimator (Wooldridge 2010).

<sup>12</sup> In the abortion ratio analysis, besides examining the robustness of the results to estimating the model by OLS, we also use a fractional probit estimated by pooled Bernoulli quasi-maximum likelihood estimation (Papke and Wooldridge 2008). Again, the results are robust, both in magnitude and in statistical significance – see Table B2 in the online appendix.



### 3.2. Abortion conditions

We then analyze how travel time affects the conditions under which abortion takes place. Using individual-level data on women who aborted, we estimate the following general model:

$$Y_{i,m,t,s} = \lambda Time_{m,t,s} + X_{m,t,s} \alpha + V_{i,m,t,s} \sigma + \varphi_{m,s} + \zeta_{t,s} \quad (3)$$

where  $Y_{i,m,t,s}$  stands for the six different outcomes related to abortion conditions of woman  $i$  living in municipality  $m$  and conceiving her pregnancy at year  $t$  in stack  $s$ .  $Time_{m,t,s}$  is the travel time between the nearest abortion provider and the town hall of municipality  $m$  where woman  $i$  lives, at the year of the conception of her pregnancy  $t$ .  $X_{m,t,s}$  is a vector with the same time-varying municipality-level controls presented in equation 1.  $V_{i,m,t,s}$  is a vector of individual controls, namely: year of age, occupation, education, number of children, nationality, and cohabitation status. Again, standard errors are clustered at the municipality-by-stack level.

First, we examine three dependent variables related to the timing of abortion, namely: i) the gestational age at the moment when women make the first contact with abortion care, ii) the number of waiting days between that moment and the abortion intervention, and iii) the gestational age at the time of abortion. We also dichotomize the number of weeks of pregnancy to examine how travel time to the nearest abortion provider affects iv) the probability of having an abortion after nine weeks of pregnancy. This provides insight on a turning point in the quality of care, as, after nine weeks of pregnancy, the risk of complications and of using the surgical method increases. In fact, over nine weeks of pregnancy, the General Directorate of Health (DGS) recommends the use of the surgical method to abort (DGS, 2007a), as the efficiency of medical abortion decreases overtime (Winikoff et al. 2008).

Second, we analyze where abortions occur, namely, v) the probability of being referred by a public hospital to a private clinic. Third and last, we examine how travel time affects the abortion method, namely, vi) having a surgical abortion, which is invasive, unlike the medical

method. We expect the main coefficient of interest,  $\lambda$ , to be positive for the six different outcomes.

Regarding the estimation method, for variables i) to iii), given the count nature of these outcomes, we estimate a model in line with the one presented in equation 3 using a Poisson regression.<sup>13</sup> As for the binary variables – iv) to vi) –, we use a linear probability model for computational simplicity.<sup>14</sup>

## **4. DATA**

### **4.1. Data sources**

To carry out our investigation on the relationship between proximity and abortion numbers and conditions, we use administrative data on all women who aborted in Portugal between 2008 and 2016.<sup>15</sup> These data were provided by the DGS (2020) and contain detailed information on women, particularly their socioeconomic characteristics and municipality of residence, which is crucial to measure proximity to an abortion provider. This dataset also allows us to track women throughout important stages of the abortion process, namely the timing at which they request and obtain an abortion, the type of provider they go to, and the abortion method they use. Each observation in our data refers to an abortion. Data on births originate from birth records at Civil Registers, subsequently compiled by Statistics Portugal (2020a). The birth dataset has information on women’s municipality of residence, age, and the number of weeks

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<sup>13</sup> We also estimate our model by OLS using an inverse hyperbolic sine transformation of the dependent variable – see appendix Table B3.

<sup>14</sup> Using a probit or logit with year-by-stack and municipality-by-stack fixed effects requires introducing over 10,000 dummies. As the stacked difference-in-differences approach also increases our sample size by a factor of the number of stacks, this considerably increases computational time. In appendix Table B4 we run a probit on a less demanding specification where we only include year-by-stack and municipality fixed effects. These regressions suggest that our results are robust to the choice of estimation method.

<sup>15</sup> Miscarriages or involuntary abortions are not included in the abortion data.

of pregnancy. Together with the abortion data, we have information on the date of abortion or birth, and on the number of weeks of gestation, thus allowing us to estimate when each pregnancy was conceived.

To obtain the location of abortion providers for each year of the analysis, we construct a list of the abortion services operating in Portugal and the bordering regions of Spain – that women living in Portugal may potentially use. This information is provided by the DGS and the Spanish Ministry of Health, respectively, in annual reports on abortion registers (DGS 2010-2017; Sanidad 2009-2018).<sup>16</sup> For Portugal, these reports disclose the number of abortions that each provider performed.<sup>17</sup> We only consider a provider to have been in operation if it performed at least five abortions in a given year. Our results are robust to considering other thresholds, namely, having had at least one or ten abortions in a given year – see Tables B5 to B8 (appendix). Using the name of the hospital or clinic provided in the reports, we identify the coordinates where that abortion facility is based. Because of the 2018 General Data Protection Regulation, the DGS stopped publishing the number of abortions per provider in their annual reports. Since the report of 2017 was only published in 2018, we do not have information on the providers operating from 2017 onwards, and we can only conduct our analysis on the 2008-2016 period.

We also gather data to characterize the socioeconomic and demographic characteristics of municipalities. The data on the yearly number of marriages, population by age and gender, and the regional GDP per capita come from Statistics Portugal (2020b, 2020c, 2020d). In addition,

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<sup>16</sup> To compute travel time to the nearest Spanish provider we only consider Spanish private clinics as most abortions in Spain occur in the private sector – 91.9% in 2014 (Sanidad 2009-2018). Also, women living in Portugal are unlikely to follow the Spanish National Health System referral path since abortions abroad are not covered by the national health insurance neither in Portugal nor in Spain. For the period under study, the closest Spanish municipalities with a private abortion clinic were always Badajoz, Vigo, Huelva, Leon, Valladolid, or Salamanca, meaning that the travel time between each Portuguese municipality and its nearest Spanish municipality with an abortion provider remained constant between 2008 and 2016.

<sup>17</sup> Appendix Table C1 provides a detailed description of the number of abortions that each provider performs and the municipality where each provider locates.

data on the number of people receiving unemployment benefits originate from the Portuguese Employment Office (IEFP 2008-2017).

#### 4.2. Variables

Proximity to the nearest provider is at the core of our analysis and is measured by the travel time to the nearest abortion provider. Not knowing the exact coordinates where women live, we assume that they all live in the populational centroid of their municipality of residence, which is proxied by the coordinates of the town hall. We then compute travel times by car to the coordinates of the nearest abortion provider for each of the 278 municipalities in mainland Portugal between 2008 and 2016.<sup>18</sup> We also calculated the travel time by car between each municipality and the closest Spanish municipality with a private abortion clinic – see section A.1 in the appendix for more information on the data on travel time to Spanish providers. To compute these travel times, we use the Stata user-written GEOROUTE command (Weber and Péclat 2017).<sup>19</sup> As the literature provides evidence on the non-linear effects of travel time to the nearest abortion provider on abortion rates, we either conduct our analysis using a second-order polynomial of travel time or introducing travel time as a categorical variable with three bins, namely, living in a municipality *i*) within 30 minutes of an abortion provider; *ii*) between 30 minutes and one hour, and *iii*) more than one hour away from the nearest abortion provider – see Figure A2 in the appendix for the distribution of municipalities across time bins.

Each specification has its advantages and drawbacks. Using a second-order polynomial of travel time allows us to thoroughly explore the variation in our variable of interest, with the caveat that this functional form imposes some structure on the data. On the contrary, the

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<sup>18</sup> In Portugal, the travel time by car is particularly relevant, especially because according to the 2011 Census (Statistics Portugal 2013), in 262 out of 278 municipalities on the mainland more than 50% of the population commutes to work or school by car. Moreover, in 2014, 96% of all of the passenger transportation in Portugal was done either by car or bus (Eurostat 2016).

<sup>19</sup> GEOROUTE computes the travel time by car between two coordinates under normal traffic conditions.

categorical specification does not assume any particular functional form on the effects of travel time. However, this only allows us to take advantage of variations across bins of travel time, with the underlying assumption that travel time changes in 30-minute steps. As Lindo et al. (2020) note, this is not realistic. Thus, we see both specifications as complementary, and reassuringly, we show in section 5 that both specifications provide similar results.

When examining how travel time to the nearest provider affects the number of abortions at the municipality-level, we control for the age structure of the population, using the share of all age groups in the population of fertile-age women.<sup>20</sup> We also control for the economic conditions in the municipality of residence. These are proxied by the number of persons receiving unemployment benefits as a percentage of the active population in the municipality (aged 15 to 65) and by the GDP per capita growth rate of the NUTS III region. Since our analysis covers a nine-year period, we consider potential changes in social norms within municipalities. As a proxy of the level of conservatism, we control for the number of marriages and catholic marriages per thousand inhabitants in the municipality. Additionally, to estimate the effect of travel time on the abortion ratio, we use the number of pregnancies – by conception year – as the exposure variable of the Poisson Fixed Effects model, which we compute as the number of both abortions and births in a municipality.<sup>21</sup> Regarding the analysis of teenage pregnancies, we use the number of pregnancies – again, by conception year – among women aged between 15 and 19 years of age as the dependent variable and the population of teenage women of that age as the exposure variable.

To examine how proximity affects the conditions under which abortion occurs, we exploit a detailed individual-level dataset of all abortions in Portugal. This allows us to control for

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<sup>20</sup> Each age group is defined on the basis of a five-year interval.

<sup>21</sup> One caveat when measuring pregnancies is that we do not have data on miscarriages and, consequently, we are underestimating the number of pregnancies, also meaning that we are overestimating abortion ratios. In any case, changes in the number of miscarriages should be orthogonal to changes in travel time to the nearest provider, to the extent that hospital's gynecology wards keep working even when their abortion services close.

several woman-specific characteristics. The control variables we use are women's marital status (five categories), cohabitation status (two categories), occupation (ten categories), municipality of residence, age, educational level (seven categories), nationality (two categories), and number of previous children. Finally, we use the number of weeks of pregnancy at the time of abortion or birth to estimate the conception date of each pregnancy.

### **4.3. Descriptive statistics**

In our set-up, it is essential that travel time varies within municipalities across time for its effect to be identified, as our preferred specifications have municipality-by-stack fixed effects that capture all time-invariant features of municipalities. In Portugal, between 2008 and 2016, travel times from municipalities to their nearest abortion provider varied, as illustrated by Figure 1. These variations in travel time are primarily due to the shutdowns and openings of public abortion providers – that represent the vast majority of abortion providers (appendix Figure A3).<sup>22</sup> The most staggering variation occurred in the south of Portugal, where the shutdown of a provider in 2011 – later reversed in 2015 – led to increases by more than one hour in travel time for several municipalities (Figure 1).<sup>23</sup>

Tables C2 and C3 (appendix) present summary statistics of the variables we use in the municipality-level analysis,<sup>24</sup> while Tables C4 and C5 (appendix) provide the descriptive statistics of women who abort. It should be noted that, in our analysis of abortion conditions, we exclude outliers, namely, women who abort after age 60 (172 observations) – who should not be fertile anymore. We note that the 60-year-old cutoff is a conservative threshold since we

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<sup>22</sup> See also online appendix Figure A4 for a description of the yearly number of shutdowns and openings of abortion providers.

<sup>23</sup> During the period under analysis, Almost 80% of fertile women were living within 30 minutes of a provider, while only 3 to 6% were more than one hour away from one – see online appendix Figure A5.

<sup>24</sup> The size of Portuguese municipalities varies a lot. They range between 1721 square kilometers (Odemira) to eight (São Joao da Pesqueira). The average size is 315 square kilometers. For population statistics see Table D1 (online appendix).

only have one woman between 55 and 60 years old in our data. Regarding the number of previous children, we discard women with more than 15 children (four observations) and women who aborted more than 15 times (five observations). In addition, we drop women with a negative number of completed weeks of pregnancy at the time of access to abortion services. We also exclude observations with missing information on any control variable. Our final sample contains 152,124 abortions for the time period ranging from 2008 to 2016.

As we can see in Table C4 (appendix), between 2008 and 2016, 84% of abortions occurred among women living within 30 minutes of an abortion provider, 13% among those residing between 30 and 60 minutes, and 3% among women living over one hour away from an abortion provider. Upon arrival at an abortion provider, the average gestational age was 6.58 weeks, while it was 7.32 weeks at the time of the abortion. On average, women waited for 8.25 days between their first contact with abortion care and abortion, with 5% of them aborting above nine weeks of gestation. Most abortions occurred in an NHS hospital (69%) and were medically induced (68%). The proportion of abortions referred to private clinics increases from 21% – among women living within 30 minutes of a provider – to 37% – among women living over one hour away. Similarly, the proportion of surgical abortions increases from 31% among women living within 30 minutes of an abortion provider to 44% for those facing travel time over one hour. Finally, 97% of all abortions in the NHS were medically induced, whereas, in the private sector, surgical abortions represented 97%.

## 5. RESULTS

### 5.1. Validity of the empirical strategy

Our empirical strategy relies on the assumption that changes in proximity to the nearest abortion provider are as good as random. There are three main threats to identification: reverse causality, omitted variable bias, and residential sorting.

The first threat (reverse causality) is that variations in abortion demand may determine variations in abortion supply. If travel time to the nearest abortion provider has a negative effect on the number of abortions in municipalities, this would generate a downward bias in our estimates. We show, in what follows, that we do not find evidence of such mechanism in our data. If providers reacted to demand, past variations in the abortion rate should predict the opening or closure of a provider. To test for this, we construct a provider region-by-year panel by aggregating the number of abortions occurring within each provider's catchment area, which we define as all municipalities that, at some point in time, were closer to that provider than to any other. We then run event studies to assess how the abortion rate – the number of abortions per 10,000 fertile women – of the catchment area varied before an opening or a closure – see section D and Figures D1 and D2 in the appendix for a detailed discussion of the event studies. We find no evidence that variations in the abortion rate of catchment areas differ prior to the opening or closure of abortion providers. This suggests that the provision of abortion services does not change in reaction to demand.

The real limitation in providing abortion services is the availability of doctors and nurses since most gynecologists in Portugal are conscientious objectors (Oliveira da Silva 2009). This raises our second concern (omitted variable bias), as changes in norms may simultaneously affect abortion demand and supply. If conscientious objectors choose to work in growingly conservative areas where abortion demand is decreasing, then our estimates of the effect of



travel time on the number of abortions will again be downward biased. However, we find no evidence of this in our data. In the event studies that we conduct in section D of the appendix (Figures D3 and D4), we do not observe any changes in social norms – which we proxy by the number of marriages and catholic marriages per thousand inhabitant – before the closures or openings of abortion providers. Furthermore, to test whether these proxies of social norms are correlated with local abortion rates, we regress the number of abortions in municipalities on municipality fixed effects only. We then extract the residuals and regress them on all the control variables we use in the municipality-level analysis. As evidenced in Table B9, the number of abortions appears to be uncorrelated with our proxies of the local level of conservatism.

The third potential source of bias is residential sorting. If women who believe they will never abort choose to live further away from an abortion center, we will not be able to tell apart the effects of travel time to the nearest abortion provider from those of preferences. However, since most abortion centers are located within hospitals that provide a variety of health care services in addition to abortion, this is highly unlikely. As a matter of fact, women may still have to go to a hospital because of other health conditions even if they exclude that they will ever need an abortion. Also, the 2011 Census shows that from 2005 to 2011, only 8.5% of Portuguese families migrated across Portuguese municipalities (Gomes et al. 2019). Hence, between 2008 and 2016, most Portuguese women resided in the same municipality where they lived in 2007 when abortion was legalized by referendum. It is doubtful that women chose their home location based on the expectation of how far their house would become from a legal abortion provider in case the "Yes" vote would win in the referendum.

Finally, as Lindo et al. (2020), we assume that causal responses of municipalities nearby an abortion provider are identical to those of municipalities that are far from one. In their words: “changes in abortion rates for counties with small changes in access provide a good

counterfactual for the changes in abortion rates that would have been observed for counties with larger changes in access if their access had changed similarly” (Lindo et al., 2020:1148).<sup>25</sup>

## 5.2. Distance to an abortion provider and the number of abortions

We start by examining how travel times to the nearest provider affect the number of abortions at the municipality-level in Table 1. All regressions include municipality-level time-varying controls, year-by-stack, and municipality-by-stack fixed effects. Odd columns model travel time to the nearest provider as a second-order polynomial, while even columns use travel time as a categorical variable. In this case, the reference category is municipalities within 30 minutes of an abortion provider.

In the regressions displayed in columns 1 and 2 in Table 1, the dependent variable is the number of abortions in the municipality, while the exposure variable is the number of fertile women in the municipality. Thus, this is equivalent to estimating the effect of travel time on the abortion rate. In both columns 1 and 2 we find evidence of non-linear effects of travel time on abortion rates. In column 1, the joint test of significance of the linear and quadratic terms of travel time rejects the null hypothesis that the second-order polynomial equals zero (significant at the 1% level). To better grasp how travel time to the nearest provider affects abortion rates, in panel a) of Figure 2, we plot the effect of a 30-minute increase in travel time at different levels of proximity. We find that such increments in travel time are only important for municipalities that are already far from a provider. Our estimates in column 2 further corroborate this. Municipalities that are over one hour away from an abortion provider experience 22% fewer abortions (significant at the 1% level), suggesting that long travel times to the nearest provider are associated with a reduced probability of abortion.<sup>26</sup> To understand if the decrease in the

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<sup>25</sup> Therefore we are assuming “strong parallel trends” (Callaway et al. 2021), an assumption, which is not testable since it would require knowing the path of potential outcomes of both control and treated units at each dose.

<sup>26</sup> Percent effects are computed in the following way:  $(e^{\beta} - 1) \times 100$ .

number of abortions we observe is due to women living far away from a Portuguese provider aborting in Spain – a country for which we have no data on abortions –, we exclude municipalities closer to a Spanish abortion clinic than to a Portuguese one – see appendix Table B10. The results of this analysis are similar to the ones present in Table 1.

**Table 1** Abortions and access to abortion

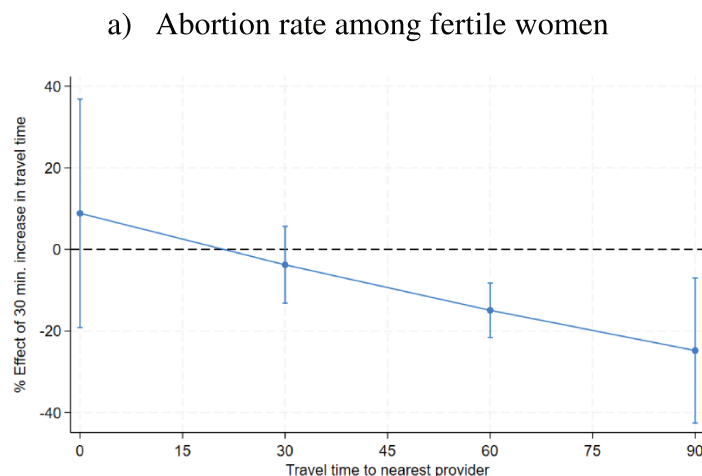
	Abortion rate among fertile women		Abortion to pregnancy ratio		Teen pregnancy rate	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Travel time (10 min.)</i>						
Linear travel time	0.0531 (0.0726)		0.0488 (0.0575)		0.0103 (0.0304)	
Quadratic travel time	-0.0007 (0.0006)		-0.0007 (0.0005)		-0.0001 (0.0002)	
P-value of joint test	0.00		0.00		0.83	
<i>Bins of travel time (Ref: within 30min.)</i>						
30 to 60min.		0.0981 (0.0980)		0.0306 (0.0848)		0.0137 (0.0857)
Over 60min.		-0.2490*** (0.0865)		-0.2644*** (0.0887)		0.0449 (0.0820)
Dep. Var.	Abortions		Abortions		Teen pregnancies	
Exposure Var.	Fertile women		Pregnancies		Teenage women	
Observations	51,304	51,304	51,304	51,304	51,304	51,304
Municipalities	246	246	246	246	245	245
Stacks	68	68	68	68	68	68

*Notes:* All regressions were estimated by Poisson quasi-maximum likelihood. All regressions have year-by-stack and municipality-by-stack fixed effects and control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

As mentioned above, women may not abort either because they carry their pregnancy to term or because they avoid pregnancy. To test the former possibility, we examine the relationship between travel time and the probability of getting an abortion if pregnant. Again, we regress the number of abortions in the municipality on our measures of travel time. However, instead

of using the number of fertile women as the exposure variable of the Fixed Effects Poisson model, we use the number of pregnancies, which is equivalent to estimating the impact of travel time on the abortion to pregnancy ratio. As columns 3 and 4 of Table 1 and panel b) of Figure 2 show, we find evidence of a non-linear association between travel time and the abortion ratio, with municipalities over one hour away from an abortion provider having abortion ratios that are, on average, 23% (3.7 percentage points)<sup>27</sup> lower than in municipalities within half an hour of a provider (statistically significant at least at the 1% level). Finally, we turn to the possibility that women living far from a provider exert more effort to avoid unwanted pregnancies. As we do not know whether a pregnancy is wanted, we restrict our analysis to pregnancies among 15 to 19-year-old teenagers. In principle, pregnancies at this age are unwanted. In columns 5 and 6 of Table 1, we use the number of teenage pregnancies as the dependent variable and the population of women in this age group as the exposure variable. We do not find any evidence that increases in travel time to a provider affect the teenage pregnancy rate. Hence, it does not seem these changes in abortion access affected the number of unwanted pregnancies.

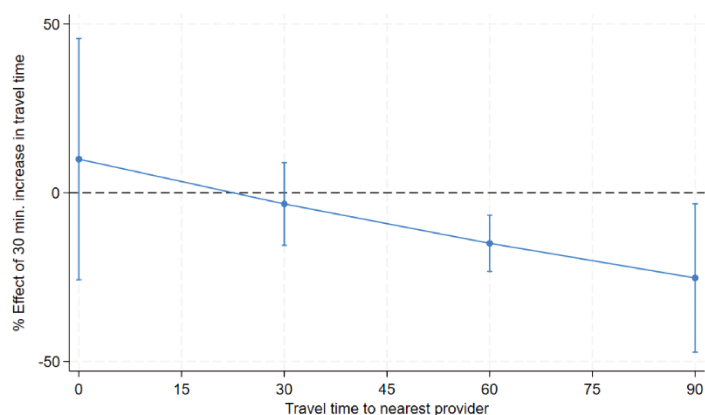
**Figure 2** Non-linear effects of travel time to the nearest provider on abortions



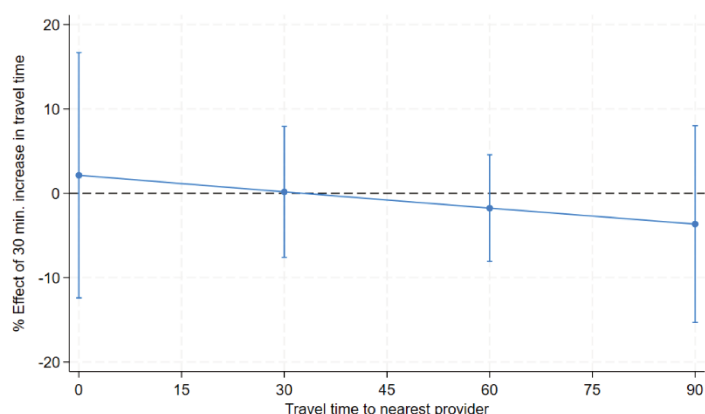
<sup>27</sup> Percentage points were computed in the following way: percent effect  $\times$  average abortion ratio of the reference bin = 23%  $\times$  14.2 = 3.7 percentage points.

**Figure 2** Non-linear effects of travel time to the nearest provider on abortions (continued)

b) Abortion to pregnancy ratio



c) Teenage pregnancy rates



*Notes:* Each dot represents the effect of an additional 30 minute increase in travel time from different starting points between 0 and 90 minutes of travel time and the respective 95% confidence interval. Graphs in panels a), b), and c) refer to the regressions in columns 1, 3 and 5 of Table 1, respectively.

Overall, these results suggest that municipalities located further away from an abortion provider experience fewer abortions because some pregnant women living in those municipalities are unable to abort.

### 5.3. Proximity and the conditions under which abortion takes place

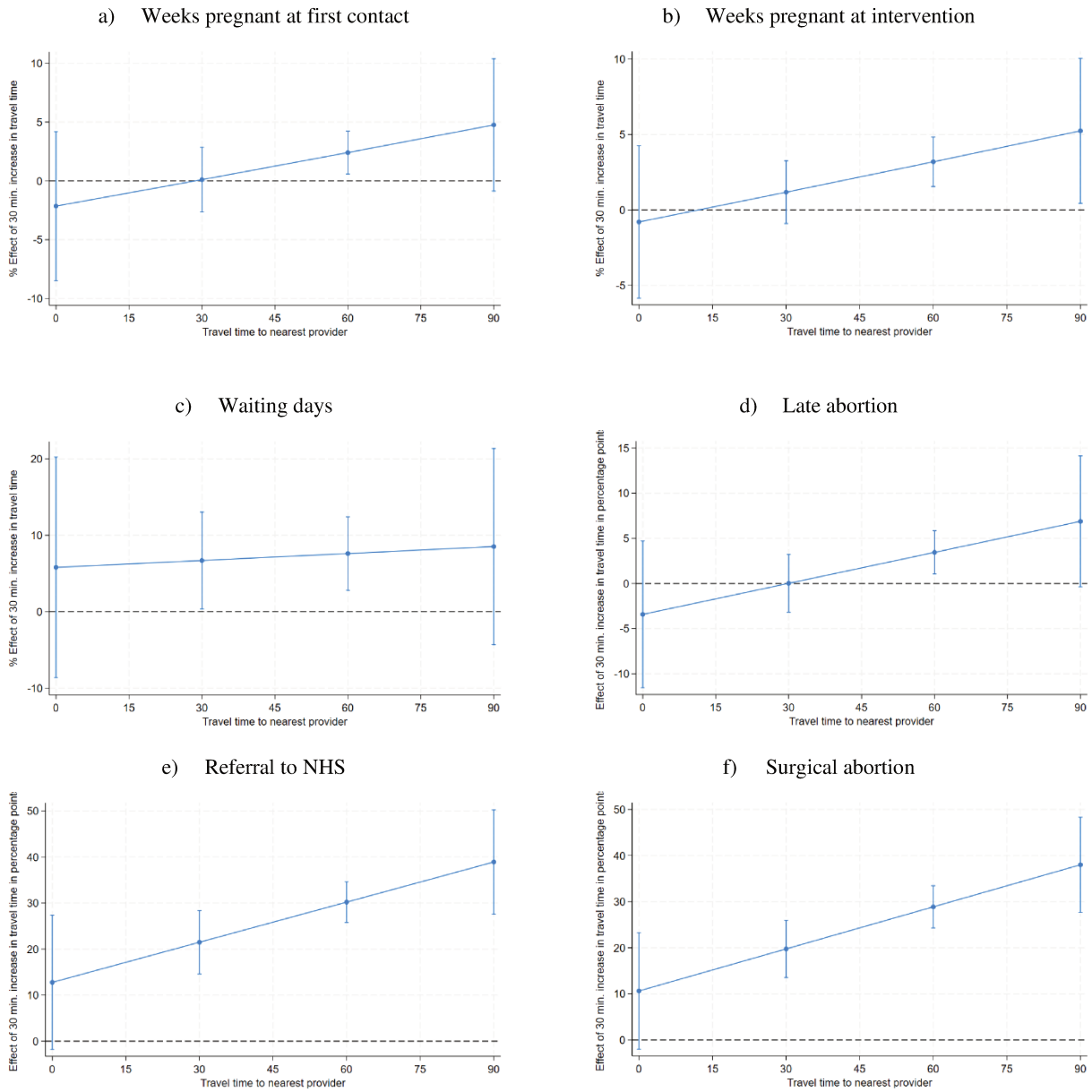
In the section above, we provided evidence that pregnant women living far away from a provider abort less. In this section, we will examine if proximity also affects the conditions under which abortion takes place.

**Table 2** Conditions under which abortion occurs and abortion access

	Number of weeks pregnant				No. of waiting days		Having abortion over 9 weeks		NHS referral to private clinic		Having surgical abortion	
	First contact with services		Abortion intervention		(5)	(6)	Yes		Yes		Yes	
	(1)	(2)	(3)	(4)			(7)	(8)	(9)	(10)	(11)	(12)
<i>Travel time (10 min.)</i>												
Linear travel time	-0.0110 (0.0143)		-0.0060 (0.0113)		0.0174 (0.0301)		-0.0171 (0.0181)		0.0280 (0.0317)		0.0203 (0.0273)	
Quadratic travel time	0.0001 (0.0001)		0.0001 (0.0001)		0.0001 (0.0002)		0.0002 (0.0001)		0.0004** (0.0002)		0.0005** (0.0002)	
P-value of joint test	0.01		0.00		0.00		0.00		0.00		0.00	
<i>Bins of travel time (Ref: within 30min.)</i>												
30 to 60min.		0.0362*** (0.0117)		0.0378*** (0.0112)		0.0327 (0.0599)		0.0699** (0.0303)		0.0943 (0.1032)		0.0781 (0.0970)
Over 60min.		0.0616*** (0.0117)		0.0705*** (0.0130)		0.1267 (0.0949)		0.0784*** (0.0211)		0.5893*** (0.1688)		0.5557*** (0.1626)
Observations	2706035	2706035	2706035	2706035	2706035	2706035	2706035	2706035	2569113	2569113	2706035	2706035
Municipalities	246	246	246	246	246	246	246	246	246	246	246	246
Stacks	68	68	68	68	68	68	68	68	68	68	68	68

*Notes:* Regressions in columns 1 to 6 were estimated by Poisson quasi-maximum likelihood while regression in columns 7 to 12 were estimated by OLS. All regressions include year of age, occupation, education, number of children, nationality, cohabitation, year-by-stack and municipality-by-stack fixed effects. All regressions also control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages, and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Figure 3** Non-linear effects of travel time to the nearest provider on abortion conditions



*Notes:* Each dot represents the effect of an additional 30 minute increase in travel time from different starting points between 0 and 90 minutes of travel time and the respective 95% confidence interval. Graphs in panels a), b), c), d), e), and f) refer to the regressions in columns 1, 3, 5, 7, 9, and 11 of Table 2, respectively.

We first analyze the timing of abortion. It arguably depends both on when women request an abortion and on the waiting time to get it. In columns 1 and 2 of Table 2, we estimate equation (2) for the number of weeks pregnant at the time of the first contact with abortion care. In both our specifications in columns 1, 2 and in panel a) of Figure 3, we find that women living over one hour away from the nearest provider arrive later to abortion services than women living within 30 minutes of a provider (statistically significant at the 1% level). Moreover, these women experience longer waiting periods between the first contact with abortion care and abortion (columns 5 and 6). Overall, the combined effect of travel time on both later arrivals to abortion care and waiting days leads women living over one hour away from a provider to abort almost four days later than the reference group (columns 3 and 4).<sup>1</sup>

In columns 7, 8, and panel d) of Figure 3, we analyze how travel time affects the probability of having an abortion beyond nine weeks of pregnancy – which we refer to as late abortions. Again, in both specifications, increases in travel time lead to increases in the probability of aborting later. We estimate that relative to living within 30 minutes away from a provider, women living between 30 minutes and one hour away have a 140%<sup>2</sup> higher probability of aborting after nine weeks of gestation, while women living over one hour away have a 157% higher likelihood of late abortion. Both estimates are significant at the 1% level.

With only ten weeks to abort legally, the delays we observe can explain why regions with low access to abortion have lower abortion ratios – women may be requesting abortions too late to be eligible to abort and, consequently, carry their pregnancies to term. These delays can also have repercussions at other stages of the abortion process, namely, where and how abortions occur. Public hospitals attending women near the legal gestational age limit may encounter

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<sup>1</sup> The number of days is computed as follows:  $(e^{\beta} - 1) \times \text{average number of weeks at the time of abortion among women residing within 30 min of a provider} \times 7 = 0.0731 \times 7.42 \times 7 = 3.74$  days.

<sup>2</sup> This percentage is computed as follows:  $(\text{point estimate} \times 100) / (\% \text{ Late abortion among women residing within 30 min of a provider}) = (0.0699 \times 100) / 5 = 140\%$



difficulties providing abortion in due time. We find evidence of this in columns 9 and 10, where we consider the probability of being referred to a private clinic by the NHS when one wants to abort. This probability increases with the travel time to an abortion provider, whatever the specification we use, as panel e) of Figure 3 illustrates.

As mentioned before, private clinics almost exclusively perform surgical abortions. The increased likelihood of referral by the NHS to private clinics, together with the higher probability of aborting after nine weeks of pregnancy, can arguably lead women who live further away from a provider to be more prone to surgical abortion. Our findings are consistent with this mechanism. The regression in column 11 and its respective graph of the marginal effects of travel time – panel f) of Figure 3 – shows that, for women living 30 minutes away from a provider, adding 30 minutes more of travel time would increase their likelihood of having a surgical abortion by 10 percentage points. For women living over 90 minutes away from an abortion service, that same increment in travel time yields a 38 percentage point increase in the probability of aborting through surgery – i.e., a 119% increase. Both these effects are significant at the 1% level. When modeling travel time as a categorical variable, we find that living over one hour away from a provider is associated with a 179% increase in the probability of having a surgical abortion.

These findings suggest that living far away from a provider makes it more difficult to obtain an abortion and may even prevent pregnant women from aborting. It takes more time for them to reach an abortion provider, making them more likely to have a late abortion. In a setting where abortions are only legal until ten weeks of pregnancy, these delays may impede women from accessing aborting services in due time. Moreover, aborting late leads women to be referred by the NHS to private clinics and eventually have a surgical rather than a less invasive medical abortion.

One potential concern about these results is selection. Specifically, one may worry that women who no longer abort because they became too far from a provider would have different abortion conditions than those who still abort. However, if selection is the mechanism driving our results, we should observe a change in the share of late or surgical abortions but no positive effect on the overall number of late and surgical abortions. To check for this, in line with Kelly (2020), we regress the number of abortions in each municipality according to referral type, gestational age, and method on our measures of travel time to an abortion provider. Our findings are coherent with an increase in late and surgical abortions in municipalities in response to longer travel times— see Table B11 (appendix). This suggests that the effect of proximity on abortion conditions is not an artifact of selection but that hindered access directly affects when, where, and how women abort.

## **6. DISCUSSION**

### **6.1. Comparing findings with existing literature**

In this paper, we provide evidence that proximity to abortion services is an important dimension of abortion access that affects the ability to abort and the conditions under which abortions occur. Papers exploiting decreases in abortion access in the USA (Quast et al. 2017, Fischer et al. 2018, Lindo et al. 2020, Venator and Fletcher 2021) also find that the abortion rate of counties decreases in response to longer distances to the nearest abortion provider. However, while all the previous papers find that the magnitude of this effect fades away the farther counties are from a provider, we see the opposite – the impact is even greater when municipalities are already far. Of course, our results are not directly comparable. The U.S. literature examines much larger geographical areas than us – Texas (Fischer et al. 2018, Lindo et al. 2020), Wisconsin (Venator and Fletcher 2021), and the USA. as a whole (Myers 2021).

For example, in 2014 in Texas, 23% of counties were more than 320 kilometers away from a provider (Quast et al. 2017), almost twice the maximum distance between any municipality in Portugal and its nearest abortion provider (175 kilometers). Thus, we might be only capturing the effects of travel time among municipalities that, in the Texas case, would be relatively close to a provider. Also, before 2022, women in the US could abort later than in Portugal, where abortion is only legal up to ten weeks of gestation. Regarding the magnitude of our effects, we use Lindo et al. (2020) as a reference point since they also measure access based on the travel time by car to the nearest provider. They find that counties over one hour away from a provider experience 17% fewer abortions than counties within one hour of one, which is in line with our findings.

We cannot exclude that the effects of travel time in the USA are even larger than ours. This is due to their use of TWFE regressions to estimate the impact of distance on abortion rates. As they allow for “forbidden comparisons”, this may lead to a negative weighting of some of the estimated causal responses, possibly attenuating the estimated effects of distance on abortion rates. In that regard, only Myers (2021) deals with this problem by estimating a regression with leads and lags of distance to an abortion provider, which is equivalent to running an event study. She argues that she finds no evidence of dynamic effects that can plague TWFE estimators. Yet, Sun and Abraham (2021) show that the coefficients of leads and lags of event studies estimated by TWFE regressions may also be biased when treatment timing varies across units.

Regarding abortion conditions, only Lindo et al. (2020) examine abortion timings. They find that longer distances decrease the number of abortions below seven weeks, between 7 and 12 weeks, and over 12 weeks of gestation, which is coherent with their finding that distance to a provider decreases abortion rates. However, these results do not allow them to conclude whether distance generates delays in getting an abortion since all abortion rates across

gestational age categories decrease. It also means that we cannot compare these results with our findings.

Even if the effects in Portugal and the USA do not align perfectly, one should consider the vastly different institutional settings. In particular, Portugal has a universal healthcare system where abortions are available free of charge; thus, the major obstacle to aborting is reaching an abortion provider in due time.

## **6.2. Medical vs. Surgical abortion costs: back of the envelope calculation**

We now discuss how our results may contribute to assess the efficiency of the current organization and financing of abortion care in Portugal.

Our findings suggest that women who live further away from a provider are more likely to abort late, be referred by the NHS to a private clinic, and have a surgical abortion. As mentioned in section 2, the NHS can outsource abortions to private clinics. Surgical abortions are more costly to provide and are reimbursed at a higher rate than medical abortions. As living further away from a provider increases the risk of having a surgical abortion, it is interesting to estimate how much the NHS would spend on abortion procedures in a hypothetical scenario where no woman would be more than 30 minutes away from an abortion service, as compared to what it currently spends.

Bringing women closer to an abortion provider would have increased the number of abortions in Portugal. According to our estimates, in that scenario, municipalities over one hour away from a provider would have had 7.8% more abortions between 2008 and 2016, i.e. 1156 additional abortions. Thus, the total number of abortions would have been 153,280 instead of 152,124. As 92% of women access abortion care through the public services, this would have resulted in 141,017 women accessing abortion through the NHS, instead of 140,207.

In parallel, bringing women closer to abortion providers would reduce the risk of aborting surgically among women who access abortion care through the NHS. As shown in Table D3 (appendix), 26% of women who accessed public abortion services and lived within 30 minutes of a provider had a surgical abortion. Transporting this proportion to our hypothetical scenario, there would have been a total of 36,664 surgical abortions instead of 38,603 (respectively, 104,353 medical abortions instead of 102,265). Therefore, in our scenario, the NHS would have paid for 1,938 fewer surgical abortions and 2,088 more medical abortions. Assuming an average cost of 400 euros per surgical procedure and 300 euros per medical abortion,<sup>3</sup> this would have translated into a reduction in abortion spending by 148,962 euros over nine years. If expanding abortion services required building new facilities or recruiting professionals in the NHS, the costs of making all women within 30 minutes of a provider likely exceeded the potential savings. The 148,962 euros saved on abortion costs indeed do not even allow employing one more entry-level general practitioner (GP) over four years.<sup>4</sup> Nevertheless, to promote equitable access to abortion, the NHS could take advantage of its already existing primary care network since, according to the WHO, medical abortions are not demanding in terms of expertise or equipment and can be provided by nurses and GPs (WHO 2012). In fact, this solution has already been implemented in three health care centers in Portugal.<sup>5</sup>

## 7. CONCLUSION

This paper shows that proximity to an abortion provider is likely to affect whether or not women are able to get an abortion in due time. We show that Portuguese municipalities that

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<sup>3</sup> Between 2008 and 2016 the reimbursement rates of surgical abortions ranged from 368 to 444 euros, while medical abortions were reimbursed at a rate ranging from 283 to 368 euros (Portaria n.8781-A/200; Portaria n.163/2013; Portaria n.20/2014; Portaria n.234/2015).

<sup>4</sup>As of 2012, entry-level specialized doctors in the public sector earn a gross annual income of 38,447.36 euros (Acordo Coletivo de Trabalho n.5/2012).

<sup>5</sup> In the health care centers of Amarante, Penafiel, and Viana do Castelo.

are more than one hour away from the nearest provider have 22% fewer abortions than municipalities located within 30 minutes.

We also document that proximity affects the conditions under which abortions occur. We show that women living further away from a provider abort later in their pregnancies and that this delay is primarily caused by their later arrivals at abortion services, possibly due to ignorance of how to access them. Late abortions make it difficult for public hospitals to provide abortions within the legal gestational age limit of 10 weeks, forcing them to outsource abortions to private clinics, where 97% of abortions are surgical. This likely explains why women living further away from providers are more likely to be referred to private clinics and have a surgical abortion.

Finally, a back-of-the-envelope calculation indicates that the NHS can only expect limited savings on abortion spending from providing proximity abortion care to all women in Portugal. However, this would enable more women to access abortion care within the legal gestational limit while allowing those who abort to escape invasive surgical abortions, thereby improving their health-related welfare.

Recently, the COVID-19 pandemic has disrupted healthcare provision globally, and abortion care was no exception. This led some countries to relax abortion legislation – such as gestational age limits, mandatory reflection periods, or even enabling women to abort from home – while others did not or even made abortion more difficult to access (Moreau et al., 2020). This natural experiment will provide a valuable opportunity to investigate how varying access to abortion impacts abortion decisions and conditions in the context of deprived access to healthcare facilities.

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# APPENDIX

## A Descriptive statistics: figures

Figure A1 Number of municipalities with zero abortions

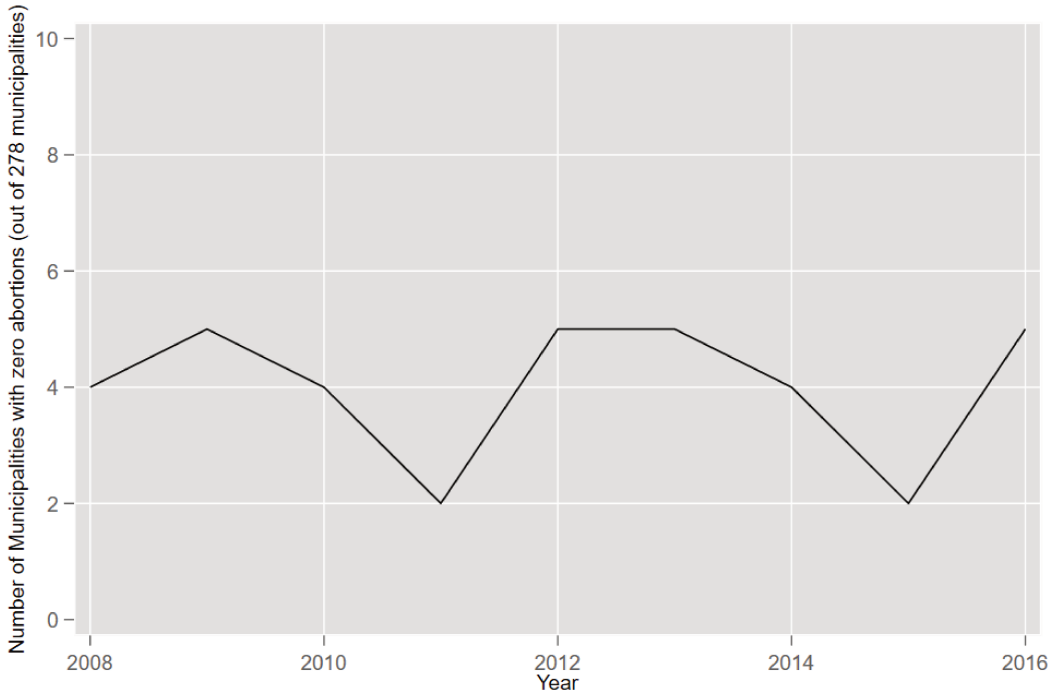
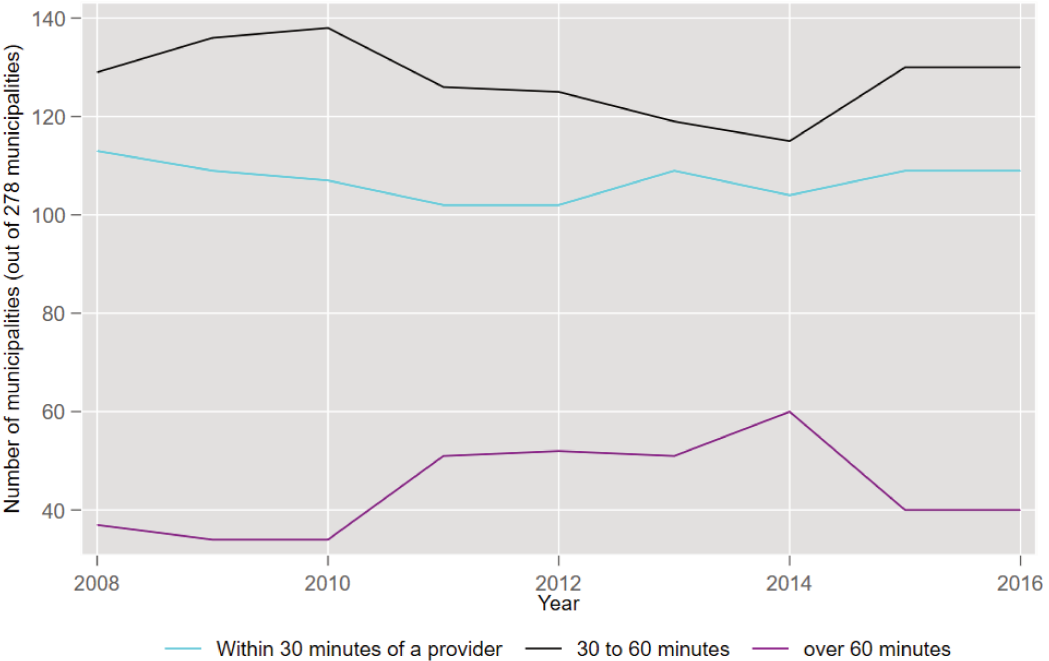
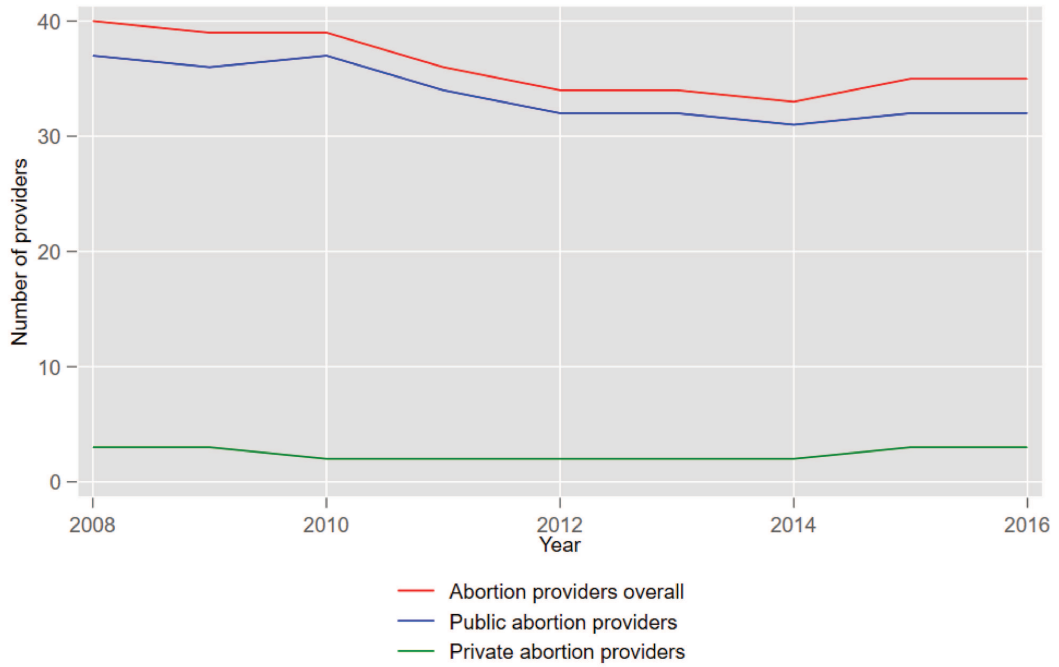


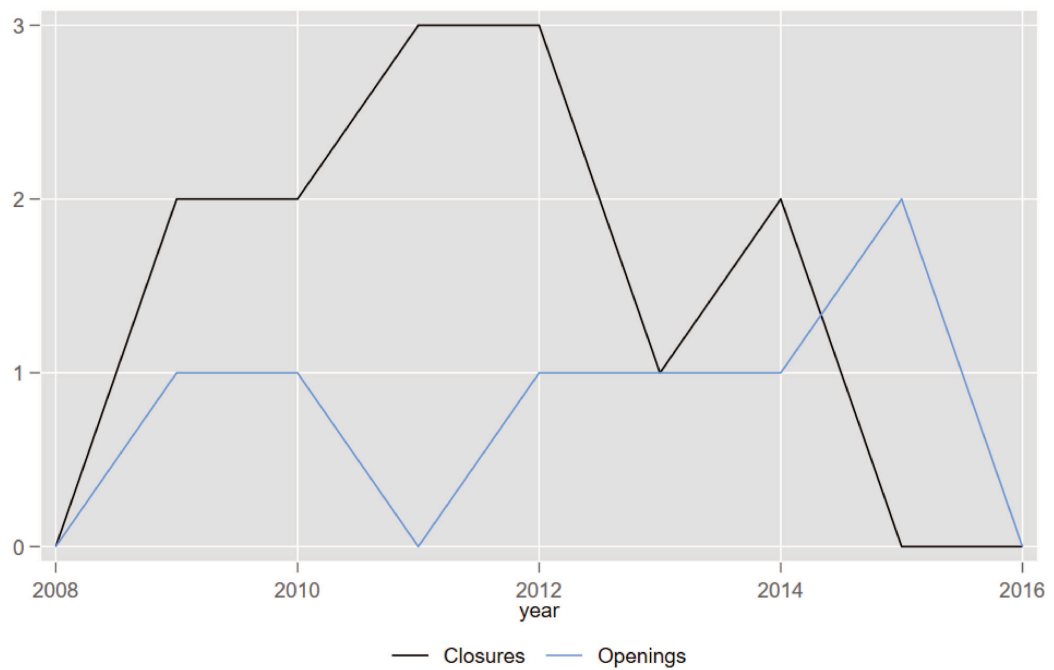
Figure A2 Number of municipalities per travel time bin



**Figure A3** Number of providers by type of abortion provider

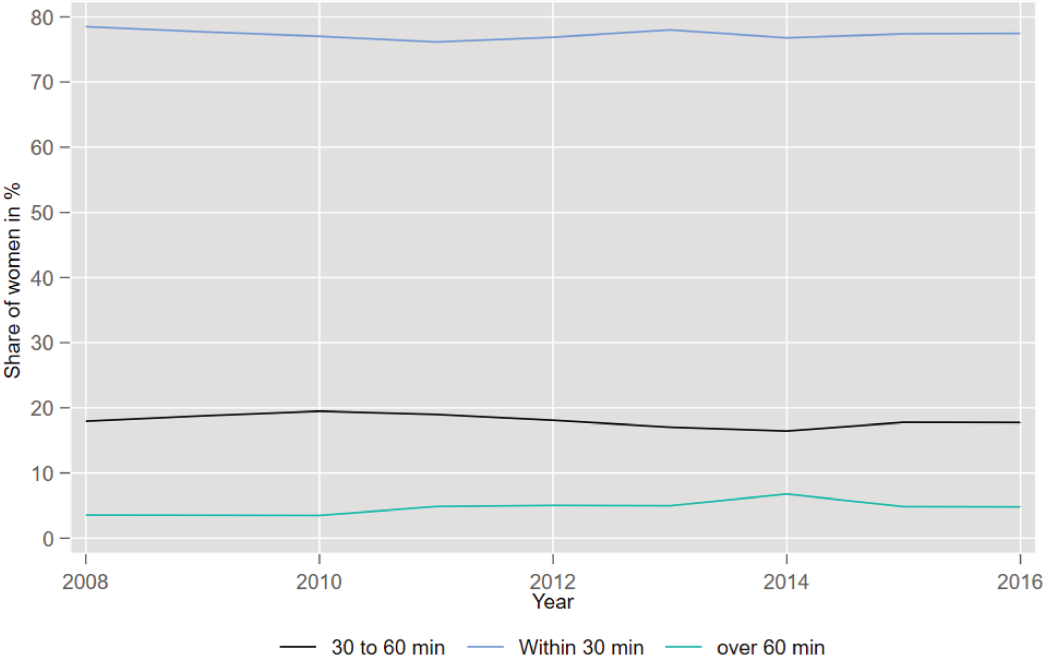


**Figure A4** Number of providers' openings and closures





**Figure A5** Share of fertile women per travel time bin



## B Robustness checks

**Table B1** Abortions and access to abortion: Inverse Hyperbolic Sine Transformation

	$\log\left(\frac{Var \times \sqrt{Var + 1}}{Denominator}\right)$					
	Abortion rate among fertile women		Abortion to pregnancy ratio		Teen pregnancy rate	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Travel time (10 min.)</i>						
Linear travel time	0.0344		0.0341		-0.0047	
	(0.0590)		(0.0532)		(0.0340)	
Quadratic travel time	-0.0006		-0.0006		-0.0000	
	(0.0005)		(0.0004)		(0.0002)	
P-value of joint test	0.00		0.00		0.90	
<i>Bins of travel time (Ref: within 30min.)</i>						
30 to 60min.		0.0593		-0.0096		-0.0051
		(0.0878)		(0.0775)		(0.1051)
Over 60min.		-0.2463***		-0.2640***		0.0400
		(0.0941)		(0.1004)		(0.0961)
Var	Abortions		Abortions		Teen pregnancies	
Denominator	Fertile women		Pregnancies		Teenage women	
Weighted by average number of:	Fertile women		Pregnancies		Teenage women	
Observations	51,304	51,304	51,304	51,304	51,304	51,304
Municipalities	246	246	246	246	245	245
Stacks	68	68	68	68	68	68

*Notes:* All regressions were estimated by OLS. All regressions have year-by-stack and municipality-by-stack fixed effects and control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B2** Abortion to pregnancy ratio and access to abortion: Fractional probit

	Poisson (benchmark from Table1)		Fractional Probit	
	(1)	(2)	(3)	(4)
	Coefficient	Coefficient	Coefficient	Marginal effects
<i>Travel time (10 min.)</i>				
Linear travel time	0.0488 (0.0575)		0.0025 (0.0278)	
Quadratic travel time	-0.0007 (0.0005)		-0.0022 (0.0021)	
P-value of joint test	0.00		0.00	
	in % i.e., $(e^{\beta} - 1) \times 100$		In pp	In %
Effect of increase in travel time by 30 min at initial distance of:				
0 minutes	8.837 (14.281)		0.180 (1.980)	1.353
30 minutes	-3.774 (4.809)		-0.772 (1.107)	-5.805
60 minutes	-14.924*** (3.406)		-1.631*** (0.353)	-12.263
90 minutes	-24.782*** (9.067)		-2.286*** (0.732)	-17.188
<i>Bins of travel time (Ref: within 30min.)</i>				
30 to 60min.		3.107 (8.488)		-1.087 (1.593) -7.655
Over 60min.		-23.233 *** (8.871)		-4.180* (2.401) -29.437
Observations	51,304	51,304	51,304	51,304
Municipalities	246	246	246	246
Stacks	68	68	68	68

*Notes:* Regressions in columns 1 and 2 are the same as those presented in Table 1 (columns 3 and 4). Regressions in columns 3 and 4 of this table were estimated using a fractional probit estimated by pooled Bernoulli quasi-maximum likelihood estimation. All regressions include year-by-stack dummies and control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages and catholic marriages per thousand inhabitants in the municipality. We also control for the average of every time varying variable within stack-by-geocode cell, which is equivalent to introducing municipality-by-stack fixed effects, while avoiding the incidental parameters problem. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Standard errors are clustered at the municipality-by-stack level. Marginal effects in percentage are computed by dividing the estimate in percentage points by the average abortion ratio (13.3%) or by the abortion ratio in the reference bin (14.2%). \* p <10% \*\* p <5% \*\*\* p <1%

**Table B3** Conditions under which abortion occurs and abortion access: Inverse Hyperbolic Sine Transformation

	$\log(\text{Var} \times \sqrt{\text{Var} + 1})$					
	Number of weeks pregnant				No. of waiting days	
	First contact with services		Abortion intervention		(5)	(6)
	(1)	(2)	(3)	(4)		
<i>Travel time (10 min.)</i>						
Linear travel time	-0.0089 (0.0143)		-0.0043 (0.0109)		-0.0061 (0.0269)	
Quadratic travel time	0.0001 (0.0001)		0.0001 (0.0001)		0.0003* (0.0001)	
P-value of joint test	0.02		0.00		0.00	
<i>Bins of travel time (Ref: within 30min.)</i>						
30 to 60min.		0.0421*** (0.0138)		0.0396*** (0.0117)		0.0171 (0.0741)
Over 60min.		0.0663*** (0.0114)		0.0714*** (0.0154)		0.1889 (0.1293)
Observations	2706035	2706035	2706035	2706035	2706035	2706035
Municipalities	246	246	246	246	246	246
Stacks	68	68	68	68	68	68

*Notes:* Regressions in columns were estimated by OLS with an inverse hyperbolic sine transformation of the dependent variable. All regressions include year of age, occupation, education, number of children, nationality, cohabitation, year-by-stack and municipality-by-stack fixed effects. All regressions also control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages, and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B4** Conditions under which abortion occurs and abortion access: Linear Probability Model and Probit – no municipality-by-stack FE

	Having abortion over 9 weeks				NHS referral to private clinic				Having surgical abortion			
	LPM		Probit		LPM		Probit		LPM		Probit	
	(1) Coef.	(2) Coef.	(3) Coef.	(4) Marg.eff.	(5) Coef.	(6) Coef.	(7) Coef.	(8) Marg.eff.	(9) Coef.	(10) Coef.	(11) Coef.	(12) Marg.eff.
<i>Travel time (10 min.)</i>												
Linear travel time	-0.0111 (0.0170)		-0.0391 (0.0579)		0.0059 (0.0294)		0.1476 (0.0955)		0.0137 (0.0292)		0.0989 (0.1505)	
Quadratic travel time	0.0015 (0.0014)		0.0054 (0.0047)		0.0060*** (0.0022)		0.0151* (0.0083)		0.0054** (0.0021)		0.0178 (0.122)	
P-value of joint test	0.00		0.00		0.00		0.00		0.00		0.00	
Average Marginal Effect	-0.0059 (0.0122)		-0.0053 (0.0111)		0.0271 (0.0220)		0.0192*** (0.0054)		0.0329 (0.0220)		0.0216 (0.0144)	
<i>Bins of travel time (Ref: within 30min.)</i>												
30 to 60min.		0.0476* (0.0268)		0.0426* (0.0236)		0.0456 (0.0664)		-0.0125* (0.0065)		0.0974 (0.0870)		0.0614 (0.0486)
Over 60min.		0.0712*** (0.0226)		0.0807*** (0.0246)		0.4480** (0.1719)		0.4282*** (0.740)		0.4645** (0.1820)		0.4199*** (0.1381)
Observations	2706035	2706035	2702681	2702681	2569113	2569113	2107948	2107948	2706035	2706035	2669761	2669761

Notes: Regressions in columns 1, 2, 5, 6, 9, and 10 were estimated by OLS while regression in columns 3, 4, 7, 8, 11, and 12 were estimated by Probit. All regressions include year of age, occupation, education, number of children, nationality, cohabitation, year-by-stack and municipality fixed effects. All regressions also control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages, and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B5** Abortions and access to abortion: Criteria of open provider = at least 1 abortion

	Abortion rate among fertile women		Abortion to pregnancy ratio		Teen pregnancy rate	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Travel time (10 min.)</i>						
Linear travel time	0.0431		0.0394		0.0077	
	(0.0556)		(0.0443)		(0.0250)	
Quadratic travel time	-0.0006		-0.0006		-0.0001	
	(0.0005)		(0.0004)		(0.0002)	
P-value of joint test	0.00		0.00		0.75	
<i>Bins of travel time (Ref: within 30min.)</i>						
30 to 60min.		0.0606		0.0170		0.0408
		(0.0603)		(0.0521)		(0.0770)
Over 60min.		-0.2397***		-0.2378***		-0.0150
		(0.0656)		(0.0654)		(0.0814)
Dep. Var.	Abortions		Abortions		Teen pregnancies	
Exposure Var.	Fertile women		Pregnancies		Teenage women	
Observations	55,104	55,104	55,104	55,104	55,104	55,104
Municipalities	246	246	246	246	246	246
Stacks	74	74	74	74	74	74

*Notes:* All regressions were estimated by Poisson quasi-maximum likelihood. All regressions have year-by-stack and municipality-by-stack fixed effects and control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B6** Abortions and access to abortion: Criteria of open provider = at least 10 abortion

	Abortion rate among fertile women		Abortion to pregnancy ratio		Teen pregnancy rate	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Travel time (10 min.)</i>						
Linear travel time	0.0531		0.0488		0.0103	
	(0.0726)		(0.0575)		(0.0304)	
Quadratic travel time	-0.0007		-0.0007		-0.0001	
	(0.0006)		(0.0005)		(0.0002)	
P-value of joint test	0.00		0.00		0.83	
<i>Bins of travel time</i> (Ref: within 30min.)						
30 to 60min.		0.0981		0.0306		0.0137
		(0.0980)		(0.0848)		(0.0857)
Over 60min.		-0.2490***		-0.2644***		0.0451
		(0.0865)		(0.0887)		(0.0820)
Dep. Var.	Abortions		Abortions		Teen pregnancies	
Exposure Var.	Fertile women		Pregnancies		Teenage women	
Observations	51,304	51,304	51,304	51,304	51,304	51,304
Municipalities	246	246	246	246	245	245
Stacks	68	68	68	68	68	68

*Notes:* All regressions were estimated by Poisson quasi-maximum likelihood. All regressions have year-by-stack and municipality-by-stack fixed effects and control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B7** Conditions under which abortion occurs and abortion access: Criteria of open provider = at least 1 abortion

	Number of weeks pregnant		No. of waiting days		Having abortion over 9 weeks		NHS referral to private clinic		Having surgical abortion			
	First contact with services		Abortion intervention		Yes		Yes		Yes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Travel time (10 min.)</i>												
Linear travel time	-0.0065 (0.0105)		-0.0027 (0.0086)		0.0159 (0.0231)		-0.0071 (0.0154)		0.0084 (0.0248)		0.0035 (0.0022)	
Quadratic travel time	0.0001 (0.0001)		0.0001 (0.0001)		0.0000 (0.0002)		0.0001 (0.0001)		0.0006** (0.0002)		0.0006** (0.0002)	
P-value of joint test	0.00		0.00		0.00		0.00		0.00		0.00	
<i>Bins of travel time (Ref: within 30min.)</i>												
30 to 60min.		0.0272*** (0.0084)		0.0326*** (0.0087)		0.0515 (0.0340)		0.0759*** (0.0187)		0.0536 (0.0716)		0.0472 (0.0661)
Over 60min.		0.0515*** (0.0096)		0.0583*** (0.0108)		0.1020 (0.0758)		0.0648*** (0.0142)		0.4644*** (0.1467)		0.4429*** (0.1404)
Observations	2889026	2889026	2889026	2889026	2889026	2889026	2889026	2889026	2746298	2746298	2889026	2889026
Municipalities	246	246	246	246	246	246	246	246	246	246	246	246
Stacks	74	74	74	74	74	74	74	74	74	74	74	74

*Notes:* Regressions in columns 1 to 6 were estimated by Poisson quasi-maximum likelihood while regression in columns 7 to 12 were estimated by OLS. All regressions include year of age, occupation, education, number of children, nationality, cohabitation, year-by-stack and municipality-by-stack fixed effects. All regressions also control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages, and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%



**Table B8** Conditions under which abortion occurs and abortion access: Criteria of open provider = at least 10 abortions

	Number of weeks pregnant				No. of waiting days		Having abortion over 9 weeks		NHS referral to private clinic		Having surgical abortion	
	First contact with services		Abortion intervention		(5)	(6)	Yes		Yes		Yes	
	(1)	(2)	(3)	(4)			(7)	(8)	(9)	(10)	(11)	(12)
<i>Travel time (10 min.)</i>												
Linear travel time	-0.0110 (0.0143)		-0.0060 (0.0113)		0.0174 (0.0300)		-0.0171 (0.0181)		0.0280 (0.0317)		0.0203 (0.0273)	
Quadratic travel time	0.0001 (0.0001)		0.0001 (0.0001)		0.0001 (0.0002)		0.0002 (0.0001)		0.0004** (0.0002)		0.0005** (0.0002)	
P-value of joint test	0.01		0.00		0.00		0.00		0.00		0.00	
<i>Bins of travel time (Ref: within 30min.)</i>												
30 to 60min.		0.0362*** (0.0117)		0.0378*** (0.0112)		0.0327 (0.0599)		0.0699** (0.0303)		0.0943 (0.1032)		0.0781 (0.0970)
Over 60min.		0.0616*** (0.0117)		0.0704*** (0.0130)		0.1267 (0.0949)		0.0784*** (0.0211)		0.5894*** (0.1688)		0.5557*** (0.1626)
Observations	2705496	2705496	2705496	2705496	2705496	2705496	2705496	2705496	2568593	2568593	2705496	2705496
Municipalities	246	246	246	246	246	246	246	246	246	246	246	246
Stacks	68	68	68	68	68	68	68	68	68	68	68	68

*Notes:* Regressions in columns 1 to 6 were estimated by Poisson quasi-maximum likelihood while regression in columns 7 to 12 were estimated by OLS. All regressions include year of age, occupation, education, number of children, nationality, cohabitation, year-by-stack and municipality-by-stack fixed effects. All regressions also control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages, and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B9** Correlation between abortions and controls

Outcome:	Residuals of regression: $N^{\circ}Abortions_{mt} = Municipality\ FE + u_{mt}$	
	Coefficients	SD
<b>Demographic controls</b>		
<i>Share of age group in population of fertile women</i>		
15 to 19 y.o.	-96.826*	(51.408)
20 to 24 y.o.	(omitted)	(omitted)
25 to 29 y.o.	10.536	(49.620)
30 to 34 y.o.	2.156	(39.737)
35 to 39 y.o.	27.617	(38.158)
40 to 54 y.o.	-153.583***	(41.304)
45 to 49 y.o.	2.186	(40.593)
<b>Economic controls</b>		
Unemployment rate	29.636**	(14.649)
GDP per capita growth (NUTS III)	-24.851*	(9.997)
<b>Controls for social norms</b>		
Marriages ( <i>per</i> 1000 inhabitants)	-0.601	(0.668)
Catholic marriages ( <i>per</i> 1000 inhabitants)	0.858	(0.877)
Observations	2502	
Municipalities	278	
$R^2$	0.024	

*Notes:* The first regression was estimated by Poisson quasi-maximum likelihood, with the number of fertile women in the municipality as the exposure variable. The regression of the residuals on the controls was estimated by OLS. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B10** Abortions and access to abortion: Excluding municipalities closer to a Spanish provider

	Abortion rate among fertile women		Abortion to pregnancy ratio		Teen pregnancy rate	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Travel time (10 min.)</i>						
Linear travel time	0.0537		0.0494		0.0103	
	(0.0726)		(0.0575)		(0.0308)	
Quadratic travel time	-0.0072		-0.0069		-0.0012	
	(0.0058)		(0.0046)		(0.0022)	
P-value of joint test	0.00		0.00		0.77	
<i>Bins of travel time (Ref: within 30min.)</i>						
30 to 60min.		0.1025		0.0312		0.0247
		(0.1006)		(0.0859)		(0.0885)
Over 60min.		-0.2531***		-0.2649***		0.0369
		(0.0887)		(0.0901)		(0.0849)
Dep. Var.	Abortions		Abortions		Teen pregnancies	
Exposure Var.	Fertile women		Pregnancies		Teenage women	
Observations	49,172	49,172	49,172	49,172	49,172	49,172
Municipalities	245	245	245	245	245	244
Stacks	65	65	65	65	65	65

*Notes:* All regressions were estimated by Poisson quasi-maximum likelihood. All regressions have year-by-stack and municipality-by-stack fixed effects and control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

**Table B11** Conditions under which abortion occurs and abortion access – municipality-level analysis

	Number of abortions below 7 weeks		Number of abortions between 7 and 9 weeks		Number of abortions over 9 weeks		Number of NHS referrals to private clinic		Number of surgical abortions		Number of medical abortions	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Travel time (10 min.)</i>												
Linear travel time	0.0864 (0.0934)		0.0619 (0.0942)		-0.0318 (0.1330)		0.1436 (0.1405)		0.0227 (0.1456)		-0.0239 (0.1755)	
Quadratic travel time	-0.0126 (0.0077)		-0.0046 (0.0077)		0.0047 (0.0100)		0.0070 (0.0109)		0.0145 (0.0119)		-0.0210 (0.0143)	
P-value of joint test	0.00		0.77		0.26		0.00		0.00		0.00	
<i>Bins of travel time (Ref: within 30min.)</i>												
30 to 60min.		0.0653 (0.1395)		0.0410 (0.2012)		0.2793*** (0.0508)		-0.0109 (0.1586)		0.0380 (0.1181)		0.0278 (0.3432)
Over 60min.		-0.5126*** (0.1373)		0.0693 (0.1445)		0.3692*** (0.0809)		1.9132*** (0.2270)		1.6488*** (0.2261)		-1.2874** (0.6389)
Municipalities	246	246	244	244	243	243	136	136	216	216	244	244
Stacks	68	68	68	68	68	68	68	68	68	68	68	68
Observations	51304	51304	51304	51304	51304	51304	51304	51304	51304	51304	51304	51304

*Notes:* All regressions were estimated by Poisson quasi-maximum likelihood with the number of fertile women as the exposure variable. All regressions have year-by-stack and municipality-by-stack fixed effects and control for the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP, the number of marriages and catholic marriages per thousand inhabitants in the municipality. Odd columns present travel time as a second order polynomial, while even columns present it categorically. Robust standard errors in parenthesis. \* p <10% \*\* p <5% \*\*\* p <1%

## C Descriptive statistics: Tables

**Table C1** Distribution of abortions by providers and year

Municipality	Provider	Number of abortion in year:								
		2008	2009	2010	2011	2012	2013	2014	2015	2016
Aveiro	Hospital Infante D. Pedro	229	243	284	299	288	267	296	243	211
Santa Maria da Feira	Hospital de S. Sebastião	260	277	282	296	268	216	232	210	228
Oliveira do Bairro	Clínica de Oiã	185	113	0	0	0	0	0	0	0
Beja	H José Joaquim Fernandes	320	316	299	0	0	0	0	30	157
Braga	Hospital de Braga	220	277	241	273	293	247	254	280	269
Guimarães	C.H do Alto Ave: Unidade de Guimarães	278	298	384	406	233	301	265	236	251
Famalicão	Hospital S. João de Deus	123	147	134	131	175	254	213	136	133
Bragança	Hospital de Bragança	155	129	167	166	193	175	128	138	155
Castelo Branco	Hospital Amato Lusitano	88	106	101	105	107	36	0	0	0
Covilhã	Centro Hospitalar Cova da Beira	148	142	164	160	141	102	0	0	0
Coimbra	Maternidade Daniel de Matos	300	348	144	384	362	300	294	265	192
Bragança	Maternidade Bissaya Barreto	477	584	565	583	481	455	403	370	362
Figueira da Foz	Hospital Distrital da Figueira da Foz	91	81	107	104	77	70	72	55	53
Faro	Hospital de Faro	752	790	810	680	745	724	653	610	490
Portimão	Hospital de Portimão	519	539	491	589	478	456	447	463	546
Almeida	Clinica Multimédica	0	0	0	0	0	0	2	37	44
Guarda	Unidade Local de Saúde da Guarda	1	43	86	0	0	0	0	0	0

**Table C1** Distribution of abortions by providers and year (continued)

Municipality	Provider	Number of abortion in year:								
		2008	2009	2010	2011	2012	2013	2014	2015	2016
Leiria	Hospital Santo André	328	361	384	406	411	351	361	355	299
Cascais	Hospital de Cascais Dr. José D'Almeida	0	0	180	49	0	0	0	0	0
Lisboa	Hospital Santa Maria	499	566	502	493	438	526	473	454	467
Lisboa	Hospital Dona Estefânia	415	426	245	0	0	0	0	0	0
Lisboa	Hospital dos Lusíadas	0	0	0	0	0	0	0	0	0
Lisboa	Maternidade Dr. Alfredo da Costa	1406	1497	1476	1555	1132	875	1143	1133	1159
Lisboa	Clínica dos Arcos	5072	5780	5871	6460	5607	5456	4644	4449	4115
Lisboa	Hospital SAMS	80	80	104	89	104	84	131	103	134
Loures	Hospital Beatriz Ângelo	0	0	0	0	533	705	697	875	806
Torres Vedras	SOERAD	0	0	0	0	4	2	0	0	0
Vila Franca de Xira	Hospital Reynaldo dos Santos	456	198	534	412	520	443	285	445	519
Amadora	Hospital Dr. Fernando Fonseca	1	1	0	0	0	0	0	0	0
Portalegre	Hospital Dr. José Maria Grande	156	130	153	203	177	161	161	160	8
Amarante	Centro de Saúde de Amarante	77	67	142	163	226	172	131	141	110
Matosinhos	Hospital Pedro Hispano	274	239	219	231	192	158	169	149	131
Penafiel	Centro de Saúde de Penafiel: T. São Vicente	89	197	202	168	3	0	0	0	0
Penafiel	Hospital Padre Américo	174	94	1	0	0	0	0	0	0
Porto	Maternidade Júlio Diniz	755	862	1006	1026	1111	1065	1007	1011	1007
Porto	Hospital Santo António	30	1	0	0	0	0	0	0	0

**Table C1** Distribution of abortions by providers and year (continued)

Municipality	Provider	Number of abortion in year:								
		2008	2009	2010	2011	2012	2013	2014	2015	2016
Porto	Hospital de S. João	346	477	501	597	554	563	494	432	465
Póvoa de Varzim	Hospital S. Pedro Pescador	142	157	138	131	107	109	88	100	78
Vila Nova de Gaia	Centro Hospitalar de Vila Nova de Gaia	499	483	478	530	484	474	400	399	424
Abrantes	Hospital de Abrantes	0	0	0	0	0	112	153	160	150
Santarém	Hospital Distrital de Santarém	231	0	0	0	0	0	0	0	0
Almada	Hospital Garcia de Orta	683	856	756	759	762	754	674	638	618
Barreiro	Hospital Nossa Senhora do Rosário	415	487	509	546	496	.	461	429	403
Setúbal	Hospital de S. Bernardo	535	628	626	697	658	632	512	500	441
Viana do Castelo	H. Viana do Castelo, St. Luzia	139	90	88	136	167	198	199	197	162
Viana do Castelo	Centro de Saúde de Viana do Castelo	117	164	131	92	0	0	0	0	0
Chaves	Hospital de Chaves	50	39	56	73	54	39	36	32	33
Vila Real	Hospital de Vila Real	159	183	219	213	174	190	154	143	171
Viseu	Hospital São Teotónio	453	425	442	432	395	358	354	310	295

**Table C2** Descriptive statistics of municipalities

Variable	Panel	Mean	SD	Min	P1	P25	P50	P75	P99	Max
<i>Abortion access</i>										
Travel time (minutes)	Overall	39.25	23.61	1.38	1.75	24.12	36.07	51.17	110.90	159.68
	Between Municipalities		21.51							
	Within Municipalities		9.81							
<i>Demographics</i>										
Number of abortions	Overall	63.08	183.39	0	0	6	14	45	661	2,700
	Between Municipalities		182.16							
	Within Municipalities		23.58							
Number of women in fertile-age	Overall	8,353.50	13,548.56	317	528	1433	3239	9145	73559	117,277
	Between Municipalities		13,552.08							
	Within Municipalities		701.57							
Number of teenager (15 to 19 y.o.)	Overall	929.21	1,435.71	31	58	169	383	1020	8061	11,641
	Between Municipalities		1,436.03							
	Within Municipalities		75.40							
Number of pregnancies	Overall	372.83	759.57	6	13	49	122	361	2986	8,859
	Between Municipalities		758.33							
	Within Municipalities		68.62							

Notes: There are 2,502 observations in total, corresponding to a balanced panel of 278 municipalities over a nine year period.



**Table C2** (continued)

Variable	Panel	Mean	SD	Min	P1	P25	P50	P75	P99	Max
Number of teenage pregnancies	Overall	16.96	40.60	0	0	2	5	15	188	552
	Between Municipalities		39.62							
	Within Municipalities		9.13							
<i>Economy</i>										
Insured unemployment rate	Overall	7.93	2.57	2.01	3.27	6.03	7.68	9.50	15.38	18.81
	Between Municipalities		2.09							
	Within Municipalities		1.49							
NUTSIII GDP per capita (thousand euros)	Overall	18.24	3.97	11.75	12.11	15.46	17.67	19.73	30.02	32.89
	Between Municipalities		3.83							
	Within Municipalities		1.06							
<i>Proxies for social norms</i>										
Marriages ( <i>per</i> 1000 inhabitants)	Overall	2.70	0.91	0	0.58	2.9	2.70	3.28	4.90	7.12
	Between Municipalities		0.64							
	Within Municipalities		0.65							
Catholic marriages ( <i>per</i> 1000 inhabitants)	Overall	1.19	0.66	0	0	0.70	1.11	1.60	3.12	3.93
	Between Municipalities		0.50							
	Within Municipalities		0.43							

Notes: There are 2,502 observations in total, corresponding to a balanced panel of 278 municipalities over a nine year period.

**Table C3** Abortion rates per travel time bin

	Travel time to nearest provider							
	<i>Within 30 minutes</i>		<i>30 to 60 minutes</i>		<i>Over 60 minutes</i>		<i>Total</i>	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
<i>Abortion rates (per 10000 fertile women)</i>								
Overall	6.228	3.992	4.659	2.642	4.785	2.611	5.283	3.310
Below 7 weeks of gestation	3.735	2.446	2.922	1.880	2.953	1.811	3.240	2.142
Between 7 and 9 weeks	2.256	1.919	1.584	1.308	1.667	1.392	1.856	1.614
Between 9 and 10 weeks	0.237	0.389	0.153	0.318	0.166	0.382	0.187	0.359
Referred from NHS to Private	0.611	2.500	0.913	1.917	1.368	2.603	0.861	2.152
Surgical abortions	0.968	2.504	1.159	2.097	1.600	2.702	1.154	2.368
Medical abortions	5.241	3.347	3.472	2.716	3.158	2.515	4.105	3.082
<i>Abortions to pregnancies ratio</i>								
Overall	0.142	0.061	0.126	0.057	0.129	0.061	0.133	0.060
<i>Teenage Pregnancy rate</i>								
Overall	0.016	0.011	0.014	0.010	0.017	0.014	0.015	0.011

**Table C4** Descriptive statistics of women who aborted – outcome variables

	Travel time to nearest provider											
	Within 30 minutes			30 to 60 minutes			Over 60 minutes			Total		
	No.	Col (%)	Row (%)	No.	Col (%)	Row (%)	No.	Col (%)	Row (%)	No.	Col (%)	Row (%)
<i>Abortions</i>	127,783	100	84	19,088	100	13	5,253	100	3	152,124	100	100
<i>Weeks pregnant</i>												
Less than seven	71,014	56	84	10,915	57	13	3,078	59	4	85,007	56	100
From seven to nine	50,319	39	84	7,317	38	12	1,930	37	3	59,566	39	100
From nine to ten	6,450	5	85	856	4	11	245	5	3	7,551	5	100
Total	127,783	100	84	19,088	100	13	5,253	100	3	152,124	100	100
<i>Type of provider</i>												
Private clinic - own initiative	10,334	8	87	1,252	7	11	331	6	3	11,917	8	100
Private clinic - NHS referral	27,318	21	77	6,172	32	17	1,950	37	6	35,440	23	100
NHS Hospital	90,131	71	86	11,664	61	11	2,972	57	3	104,767	69	100
Total	127,783	100	84	19,088	100	13	5,253	100	3	152,124	100	100
<i>Method</i>												
Medical abortion	88,346	69	86	11,612	61	11	2,965	56	3	102,923	68	100
Surgical abortion	39,437	31	80	7,476	39	15	2,288	44	5	49,201	32	100
Total	127,783	100	84	19,088	100	13	5,253	100	3	152,124	100	100
<i>Method (Among women accessing to services through the NHS)</i>												
Medical abortion	87,749	74	86	11,560	65	11	2,956	60	3	102,265	73	100
Surgical abortion	30,294	26	78	6,332	35	16	1,977	40	5	38,603	27	100
Total	118,043	100	84	17,892	100	13	4,933	100	4	140,868	100	100
<i>Method (Abortions performed in the NHS)</i>												
Medical abortion	87,163	97	86	11,496	99	11	2,934	99	3	101,593	97	100
Surgical abortion	2,968	3	94	168	1	5	38	1	1	3,174	3	100
Total	90,131	100	86	11,664	100	11	2,972	100	3	104,767	100	100
<i>Method (Abortions performed in private clinics)</i>												
Medical abortion	1,183	3	89	116	2	9	31	1	2	1,330	3	100
Surgical abortion	36,469	97	79	7,308	98	16	2,250	99	5	46,027	97	100
Total	37,652	100	80	7,424	100	16	2,281	100	5	47,357	100	100
		Mean	SD		Mean	SD		Mean	SD		Mean	SD
<i>Weeks pregnant at:</i>												
Time of access		6.57	1.53		6.59	1.50		6.67	1.46		6.58	1.52
Time of abortion		7.32	1.37		7.30	1.32		7.29	1.29		7.32	1.36
Waiting days		8.31	4.69		8.07	4.62		7.53	4.32		8.25	4.67
Travel time (minutes)		13.57	8.77		40.5	7.72		75.35	16.25		20.31	17.53

**Table C5** Descriptive statistics of women who aborted – control variables

	Mean	SD	No.	Travel time to nearest provider								
				Within 30 minutes			30 to 60 minutes			Over 60 minutes		
				No.	Col (%)	Row (%)	No.	Col (%)	Row (%)	No.	Col (%)	Row (%)
<i>Age</i>	28.5	7.3										
Less than 15			640	536	0	84	79	0	12	25	0	4
15 to 19			16,499	13,957	11	85	1,998	10	12	544	10	3
20 to 24			34,559	29,502	23	85	3,926	21	11	1,131	22	3
25 to 29			32,908	27,930	22	85	3,881	20	12	1,097	21	3
30 to 34			30,956	25,851	20	84	4,042	21	13	1,063	20	3
35 to 39			25,134	20,767	16	83	3,415	18	14	952	18	3
Over 40			11,422	9,234	7	81	1,747	9	15	441	8	4
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3
<i>Education</i>												
Illiterate			418	344	0	82	52	0	12	22	0	5
Reads/writes			307	248	0	81	44	0	14	15	0	5
Primary school			6,147	5,123	4	83	807	4	13	217	4	4
Lower middle school			18,751	15,404	12	82	2,645	14	14	702	13	4
Higher middle school			41,032	34,205	27	83	5,372	28	13	1,455	28	4
High school			54,342	45,691	36	84	6,750	35	12	1,901	36	3
College			31,127	26,768	21	86	3,418	18	11	941	18	3
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3
<i>Occupation</i>												
Agriculture (ISCO08:6-8)			23,926	19,679	15	82	3,454	18	14	793	17	3
Armed Forces (ISCO08:0)			827	705	1	85	110	1	13	12	0	2
Services (ISCO-08:4-5)			16,621	14,154	11	85	2,013	10	12	454	10	3
Unskilled (ISCO-08:9)			27,892	23,558	18	84	3,605	19	13	729	16	3
Managers (ISCO-08:1)			1,520	1,357	1	89	134	1	9	29	1	2
Professionals (ISCO-08:2)			10,628	9,183	7	86	1,152	6	11	293	6	3
Technicians (ISCO-08:3)			11,664	10,036	8	86	1,312	7	11	316	7	3
Domestic worker			3,445	2,662	2	77	634	3	18	149	3	4
Student			25,554	21,768	17	85	2,996	15	12	790	17	3
Unemployed			30,047	24,921	20	83	4,039	21	13	1,087	23	4
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3

**Table C5** (continued)

	Mean	SD	No.	Travel time to nearest provider								
				Within 30 minutes			30 to 60 minutes			Over 60 minutes		
				No.	Col (%)	Row (%)	No.	Col (%)	Row (%)	No.	Col (%)	Row (%)
<i>Nationality</i>												
Portuguese			125,483	104,010	81	83	16,802	88	13	4,671	89	4
Other			26,641	23,773	19	89	2,286	12	9	582	11	2
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3
<i>Civil Status</i>												
Married			37,375	30,118	24	81	5,727	30	15	1,530	29	4
Divorced			11,317	9,168	7	81	1,743	9	15	406	8	4
Separated			2,263	1,890	1	84	295	2	13	78	1	3
Single			100,518	86,098	67	86	11,208	59	11	3,212	61	3
Widow			651	509	0	78	115	1	18	27	1	4
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3
<i>Cohabitation</i>												
Lives with partner			74,954	61,528	48	82	10,513	55	14	2,913	55	4
Lives without partner			77,170	66,255	52	86	8,575	45	11	2,340	45	3
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3
<i>Previous children</i>												
0	1.0	1.0	61,676	52,725	41	85	7,006	37	11	1,945	37	3
1			44,694	37,767	30	85	5,386	28	12	1,541	29	3
2			33,630	27,365	21	81	4,962	26	15	1,303	25	4
3			8,959	7,271	6	81	1,330	7	15	358	7	4
4			2,247	1,892	1	84	281	1	13	74	1	3
More than 4			918	763	0	83	123	0	13	32	0	4
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3
<i>Previous abortions</i>												
0	0.4	0.7	112,079	93,180	73	83	14,845	78	13	4,054	77	4
1			30,607	26,306	21	86	3,354	18	11	947	18	3
2			6,887	6,050	5	88	651	3	9	186	4	3
3			1,676	1,482	1	88	153	1	9	41	1	2
4			503	454	0	90	39	0	8	10	0	2
More than 4			176	142	0	81	23	0	13	11	0	6
Total			152,124	127,783	100	84	19,088	100	13	5,253	100	3

## D Event studies

To better understand if openings and closures of abortion providers are driven by abortion demand or broader social forces, we conduct event studies which are displayed in Figures D1 to D4 below. In this analysis, we use the abortion rate and the number of marriages and catholic marriages of the catchment area of a provider – which is defined as the set of municipalities to which that provider was, at some point in time, the closest abortion provider – as an outcome and estimate the model described below:

$$\text{Log}(Y_{rt}) = \sum_{i=-Pre}^{-2} \alpha_i T_{rti} + \sum_{i=0}^{Post} \alpha_i T_{rti} + X_{rt}\beta + \varphi_r + \zeta_t + u_{rt}$$

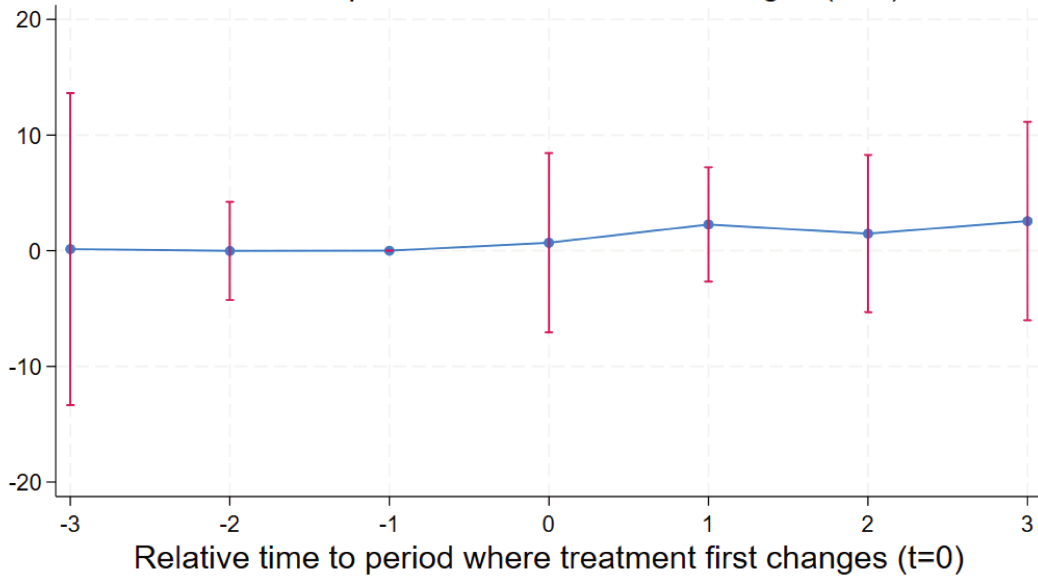
Where  $Y_{rt}$  is either the abortion rate or one of our two proxies for social norms (number of marriages and catholic marriages per thousand inhabitants) and  $T_{rti}$  is a dummy equal to one for the catchment area of provider  $r$ , in year  $t$ , which is  $i$  years away from the event (opening or closure of provider).  $X_{rt}$  is a vector of time-varying characteristics of the provider's catchment area, namely: the share of all age groups in the population of fertile-age women, the insured unemployment rate, the growth rate of the NUTS III region's GDP. If the outcome variable is the abortion rate, we also include as controls the number of marriages and catholic marriages per thousand inhabitants.  $\varphi_r$  and  $\zeta_t$  are provider's catchment area and year fixed effects, respectively.  $u_{rt}$  is the error term.

However, estimating this model by a standard TWFE regression is problematic, as regions where an abortion provider closed in year T-1 will be used as controls for regions experiencing a shutdown in year T (Sun and Abraham 2021). To correctly estimate the value of the coefficient of both leads and lags, we implement the DIDM estimator (de Chaisemartin and D'Haultfoeuille 2020), where only never treated or not-yet-treated observations are used in the control group. The advantage of this procedure is that unlike other solutions – e.g. Callaway and Sant'Anna (2021) – it allows us to control for time varying variables. This was done using

the Stata `did_multipligt` command (de Chaisemartin et al. 2019). As shown in Figures D1 to D4, we find no evidence that there were systematic differences in abortion rates or changes in social norms prior to the closures or openings of abortion providers.

**Figure D1** Abortion rate before and after clinic closure

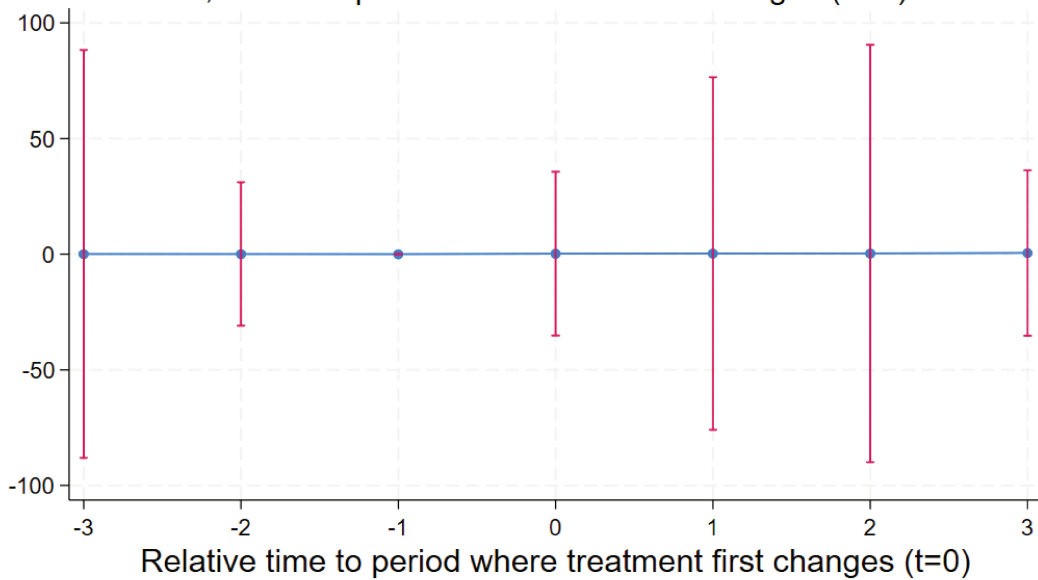
DID, from last period before treatment changes ( $t=-1$ ) to  $t$



Notes: Each dot presents the coefficient of lags/leads and is represented along with the respective 95% confidence interval.

**Figure D2** Abortion rate before and after clinic opening

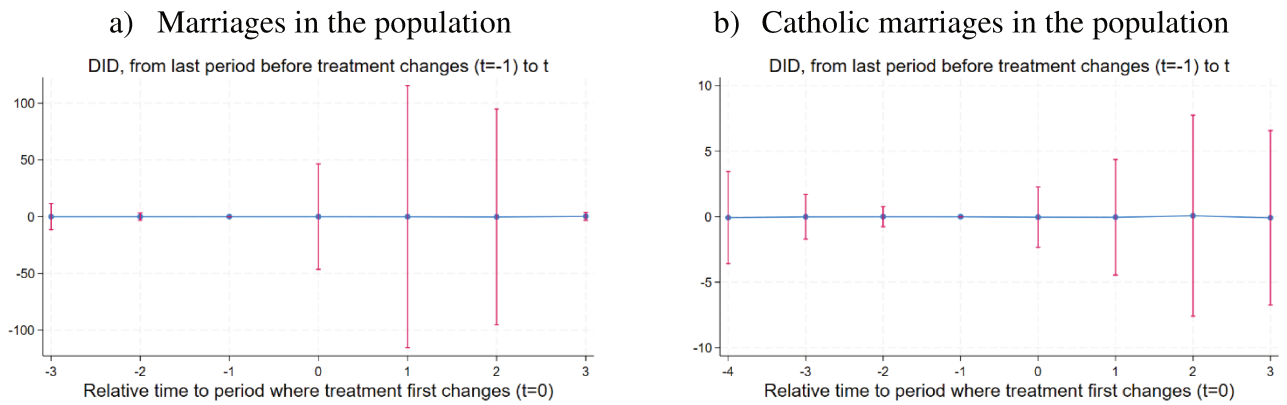
DID, from last period before treatment changes ( $t=-1$ ) to  $t$



Notes: Each dot presents the coefficient of lags/leads and is represented along with the respective 95% confidence interval.

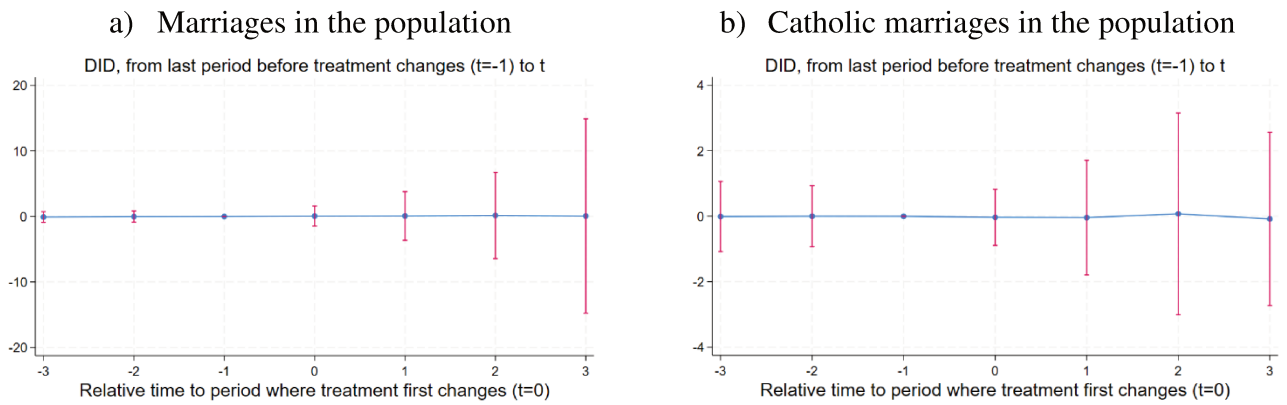


**Figure D3** Marriages before and after clinic closure



Notes: Each dot presents the coefficient of lags/leads and is represented along with the respective 95% confidence interval.

**Figure D4** Marriages before and after clinic opening



Notes: Each dot presents the coefficient of lags/leads and is represented along with the respective 95% confidence interval.

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## Chapter II:

# Making their own weather? Estimating employer labour-market power and its wage effects\*

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This Chapter has been co-written with Pedro S. Martins

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# 1 Introduction

The limited wage growth observed in many countries in the recovery following the 2008 financial crisis and during the recent inflationary period since 2021 has prompted important questions about the degree of wage setting power enjoyed by employers. These questions have received additional attention following the recent evidence of declining labour shares, the rise of 'superstar firms' (Autor et al., 2020), the relevance of 'non-compete' and 'no-poaching' arrangements (Krueger and Ashenfelter, 2022), and the limited disemployment effects of minimum wages. While many of these findings may still be reconciled with largely competitive labour markets, the relevance of evidence on the extent of employer labour-market power and its effects is clear. For instance, competition agencies may need to pay more attention to labour markets - while labour policy may also need to pay more attention to competition issues.

This paper contributes to the literature on labour monopsony (Staiger et al., 2010; Manning, 2010, 2011; Falch, 2010; Matsudaira, 2014; Webber, 2015; Card et al., 2018) by providing empirical evidence on two major questions: first, how concentrated are local labour markets? and, second, what is the impact of labour market concentration on wages? We address these questions by exploiting rich matched employer-employee data: our data covers the full population of workers (and occupations) in a European country, Portugal. Furthermore, we exploit the availability in our data of information on the occupation, industry, region, and wages of each worker. For instance, in contrast to some other papers, we consider not only manufacturing but also services. All variables are particularly detailed and comparable not only across firms but also over time, during the very long (34-year) period that we cover.<sup>1</sup>

Two of the data dimensions above - coverage of both all occupations and of the entire labour market - are critical for a comprehensive analysis of employer market power. The majority of workers in developed countries have been for many decades employed in the services sector (which is however not covered in a number of data sets used in this research) and concentration levels may be very different (much higher) in manufacturing than services. Moreover, less than comprehensive information on the location of comparable occupations across firms and regions, as well as an analysis based exclusively either on employment flows or stocks may also lead to overestimated measures of concentration.

The literature on labour market concentration consistently documents its negative effect on wages – Azar et al. (2022), Benmelech et al. (2022) and Rinz (2022) on the U.S, Bassanini et al. (2023a) on six European countries (including Portugal)<sup>2</sup>, Dodini et al. (2023) and Dodini et al. (2021) for Norway, Bassanini et al. (2023b) and Marinescu et al. (2021) on France, and Duan and Martins (2022) on China. Our results are consistent with this literature in that we also find that employer market power is a significant phenomenon in terms of both its magnitude and time constancy. However, our results

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<sup>1</sup>To our knowledge, only Rinz (2022) and Benmelech et al. (2022) use a longer period than us.

<sup>2</sup>We note that Bassanini et al. (2023a) use a different period in their analysis of Portugal (2010-2019).

indicate that employer market power potential, as measured by concentration indices<sup>3</sup>, is smaller than in earlier evidence that could not draw on data with the same level of detail as ours.<sup>4</sup> This finding of smaller employer market power applies both in terms of the percentage of the workforce employed in high-concentration local labour markets and in the size of the wage effects of concentration. Specifically, we find that less than 9% of workers are exposed to concentration levels thought to raise (product) market power concerns (DoJ/FTC, 2010).

One of our contributions is the finding that methodological choices can have a significant impact on the levels of labor market concentration and the magnitude of its wage effects. Leveraging our comprehensive data, which encompasses variables not concurrently available in datasets used in other studies, we are able to explore a variety of methodological choices. As a result, this paper provides a benchmark for comparing findings from research that adopts only specific methodologies. We document that defining job characteristics using workers' industry – such as Rinz (2022) – rather than occupation – such as Azar et al. (2022) – and deciding the degree of occupational specificity can significantly impact both the measured levels of concentration and its wages effects. For instance, using industry-based definitions of local labor markets can lead to twice as high wage markdowns than occupation-based ones. Also, we find that labour-market concentration can more than double when considering more granular occupation codes. Altogether, our results highlight the considerable quantitative (even if not qualitative) sensitivity of estimated effects to methodological choices.

Another contribution of this paper is to examine the importance of controlling for worker and firm time-invariant heterogeneity when estimating wage effects of labour-market concentration. This is in contrast to some research which, due to data limitations, does not control simultaneously for these factors (Azar et al., 2022; Benmelech et al., 2022; Rinz, 2022). However, when not controlling for both worker and firm fixed effects, we find a positive association between employer concentration and wages. This may reflect the fact that concentrated labour markets tend to be characterised by large firms, which can be more productive and or enjoy higher product market power. These two dimensions will in turn create scope both for stronger selection in hires (recruiting and retaining workers of higher productivity) as well as rent sharing, through bilateral bargaining, particularly when labour market policies are supportive of collective bargaining as is the case of Portugal and much of continental Europe. On the other hand, Portugal and other European countries are characterised by relatively strong employers' associations (Martins, 2020), which can facilitate collusion in wage setting across firms operating in the same industry (Martins and Thomas, 2023).

We assess the potential relevance of these multiple factors by again exploiting the richness of our data and estimating the relationship between wages and concentration while controlling both for worker and firm fixed effects. We follow Azar et al. (2022) and employ instrumental variables based on the number of firms in the same occupations but other local labour markets. In these models, we find a negative

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<sup>3</sup>There are other measures that also measure market power, including the elasticity of labour supply to the firm (Manning, 2021).

<sup>4</sup>Some of these papers only examine manufacturing (Benmelech et al., 2022) or cannot consider the roles of occupations (Rinz, 2022) or of employment stocks (Azar et al., 2022).

effect of local labour market concentration on wages, with elasticities of -0.015. These results indicate that workers that would move from low- to high-concentration local labour markets (percentiles 25th and 75th, respectively) would experience a drop in wages of approximately 3%, a figure significantly smaller than in Azar et al. (2022). Interestingly, the effect of concentration does not appear to be linear: we estimate larger elasticities when restricting our analysis to markets in the top 25% of the concentration distribution, although these estimates are still below the ones that Azar et al. (2022) finds.

Our paper also contributes to the literature examining the role of labour market institutions in possibly counteracting monopsonitic power (Benmelech et al., 2022; Bassanini et al., 2023a; Dodini et al., 2021). Indeed, the smaller effect that we find, when compared to studies using U.S data, may reflect institutional differences, in particular sectoral collective bargaining, which is much stronger in Continental Europe (and Portugal) than the U.S, and which can weaken the negative effect of employer market power on wages. To test for this, we interact labour-market concentration with a measure of collective bargaining coverage (including sectoral or other agreements). As Bassanini et al. (2023a), we find evidence that higher coverage rates by collective bargaining agreements attenuate wage markdowns.

Our findings also shed new light on the role of larger firms in concentrated labour-markets. According to Card et al. (2018), worker heterogeneity in preferences for firm amenities may allow smaller firms in larger markets to enjoy more wage setting power, as they are more likely to employ workers with strong preferences for their specific amenities. Thus, if there are fewer employers in a market, large firms may be able to attract employees more readily, since it is less likely that these could find a job that aligns particularly well with their preferences. Moreover, an asymmetric Cournot model (Boal and Ransom, 1997) also predicts that, in more concentrated markets, large firms are able to impose stronger wage markdowns than smaller employers. We contribute to this literature by investigating the interaction between labour market concentration and firm size from an empirical perspective.<sup>5</sup> In line with the aforementioned theoretical predictions, we find that the top five local employers in each local labour market exhibit larger wage effects than smaller firms.

The structure of the remaining of the paper is as follows: Section 2 briefly presents the literature on labour market concentration, Section 3 introduces the matched employer-employee data used. Sections 4 describe our main measures of employer concentration and a comprehensive sensitivity analysis. The wage effects results and their robustness as well as some extensions are presented in Section 5. Section 6 concludes.

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<sup>5</sup>Rinz (2022), also suggests that top employers may be important drivers of labour market-concentration.



## 2 Literature

The literature on monopsony power in the labour market and, notably, on the effects of labour market concentration on wages has grown rapidly in the last decade.

As Boal and Ransom (1997) demonstrate, using a Cournot model of the labour market, concentrated labour markets may raise employer market power. It can also be shown that the exploitation rate – i.e. the deviation from the perfect competition wage – is positively correlated with the Herfindhal-Hirschman Index (HHI), which, after the work of Azar et al. (2022), became the standard measure of concentration used in the literature.

Azar et al. (2022), is also a key reference because they propose the standard instrument employed by most papers examining the wage effects of labour market concentration. In short, they instrument local labour market concentration by the average of the inverse of the number of firms in a that labour market, excluding the local labour market's region. Using data on the top occupations from a leading U.S. employment website – see also Azar et al. (2020)<sup>6</sup> –, they find that the average labour market is highly concentrated – HHI of 3,157 or 2,970 when using quarterly or yearly data, respectively – and that concentration is associated with large declines in earnings: a wage elasticity of -0.1 or, alternatively, cuts of 17% from moving from the 25th to the 75th percentile of the Herfindhal distribution.

Leveraging on administrative U.S. establishment-level data, a long time period, and defining local labour markets as county or commuting zone (CZ) by industry pairs rather than CZ-by-occupation as Azar et al. (2022), Rinz (2022) (all industries) and Benmelech et al. (2022) (manufacturing sector only) also find a negative effect of concentration on wages – wage elasticities of around -0.1.

Furthermore, Rinz (2022) contributes to the literature by documenting trends in labour market concentration over a large time window (1976 to 2015). He shows that while national industrial concentration followed a U-shaped path, local concentration constantly decreased between 1976 and 2015. This is also the first paper to examine how labour market concentration affects inequality – more precisely the inequality within local labour markets – showing that the decrease in local concentration between 1976 and 2015 reduced the 90<sup>th</sup> to 10<sup>th</sup> percentile wage ratio by 6%.

Overall, the U.S. literature provides evidence on the wage-setting power of firms in concentrated markets. It is therefore important to understand if this finding holds for countries with different labour market institutions. Using French administrative data, Marinescu et al. (2021) and Bassanini et al. (2023a) also find that concentration results in wage markdowns, although these effects are lower in magnitude than the ones found for the USA – they find wage elasticities between -0.0244 and -0.048. Unlike the U.S. papers, these studies are able to account for worker time-invariant heterogeneity.

Without making an exhaustive survey of all European papers in the literature, Dodini et al. (2021) and Dodini et al. (2023) are two other references worth noting due to their novel definition of labour markets. They argue that using industry or occupational categories to define job characteristics is

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<sup>6</sup>Azar et al. (2022) use a subset of the data of Azar et al. (2020) for which posted wage information is available,

too restrictive as a representation of workers' outside options. Thus, they map occupations to a classification of skills, which they then cluster into 40 different groups – their local labour markets are skill cluster by region pairs. Using this definition and Norwegian data, Dodini et al. (2023) find that increases in HHI by 0.1 lead to a decrease in wages by 1%.

One question that naturally arises is whether or not more unionized labour markets are able to counteract employer wage setting power. Both Benmelech et al. (2022) and Dodini et al. (2021) examine this and find that, indeed, unionization attenuates the effect of labour market concentration on wages.

In the remainder of the paper, we will discuss how some of these different methodological approaches may influence concentration measures and wage effects. This is only possible due to the detailed data that we use, which we describe in the next section.

### 3 Data

Our study is based on the 'Quadros de Pessoal' (Personnel Records) data set, a comprehensive matched employer-employee panel. This data set provides detailed annual information on all private sector firms based in Portugal that employ at least one worker, including information on each of their employees, their establishments, and time-invariant firm, establishment, and worker identifiers. The data set follows from an annual mandatory survey collected by the Ministry of Employment for the purposes of enforcing compliance with employment law and collective bargaining.

Worker individual information concerns the month of October of each year (March up to 1993) and features a number of variables, the most important for us being the monthly wage (base and total) and the occupation. The latter is defined using the national occupations classification, which features about 1,400 different entries, defined at a fifth-digit level in most of the period. Other worker-level variables that we also consider include the year and month of hiring by the firm, the applicable collective agreement, and the job title code of the worker under the collective agreement. At the establishment level, we use its geographical location (at the 'concelho' level - over 300 different locations - and the more aggregated 'distrito' level - 30 different locations).<sup>7</sup> Regarding the construction of our sample, we use all worker observations in the data set except the small number with missing information on the occupation, date of hiring, and region variables.

Our benchmark definition of local labour markets is based on the occupation definition composed of 1,400 different values and the region definition composed of 30 'distritos' (including islands). These regions have an average size of about 3,000 square kilometres and 340,000 inhabitants, including multiple cities and villages in most cases. 18 regions are located in the continental part of Portugal; the remaining 12 correspond to the islands of Azores and Madeira. With the exception of some areas

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<sup>7</sup>At the establishment-level, the data also provides information on industry (five-digit variable) - see Cahuc et al. (2023) for a recent analysis of the establishment dimension of this data. At the firm-level, available information includes total sales, legal type of firm, capital equity, and type of ownership.

in the least populated and least densely populated regions, typically located in the East and South of the country, all urban or industrial areas can be reached from virtually all residential areas in each district in less than one hour by car and in many cases in less than one hour by public transport as well. With the exception of the district of the capital city, Lisbon, the largest city in each region is located closed to the geographical centre of the region. This ensures limited overlap between commuting zones of neighbouring regions. This benchmark definition also leads to an average of 14,300 local labour markets (occupation-region pairs) per year over the 33-year period covered, as we discuss below. We also consider all years between 1985 and 2019 (except 1990 and 2001, which are not available).

## 4 Employer concentration

### 4.1 Benchmark measure

Following the literature, we measure employer concentration using the Herfindhal-Hirschman index:

$$HHI_{l(o,d),t} = \sum_{j=1}^{N(l,t)} share_{j,t}^2, \quad (1)$$

in which  $HHI_{l(o,d),t}$  is the HHI measure for local labour market  $l$  (correspond to both occupation  $o$  and district  $d$ ), in in year  $t$ ;  $N(l,t)$  is the number of firms employing (or having hired) workers in that occupation, district and year; and  $share_j$  is 100 times the ratio between the employment (or new hires) of firm  $i$  in year  $t$  and the total employment (or new hires) in the local labour market of that firm (occupation  $o$  and district  $t$  in year  $t$ ). Given this definition, the HHI can range between 0 - no concentration - and 10,000 - maximum concentration (a single firm).

In Table 1, we present the HHI indices for each year, considering both the cases of employment ('stocks') and new hires ('flows'). The latter are defined as workers employed in the firm as of the census month - October of each year since 1994 (March of each year until 1993) - and hired over the previous 12 months.<sup>8</sup> The results indicate that the mean annual HHIs in the case of stocks range between 625 and 912, while in the case of flows they range between 700 and 1,167. These figures are significantly below the concentration levels thought to raise market power concerns, at least in the case of product markets, at HHIs of 2,500.<sup>9</sup> We acknowledge that the relevance of this threshold

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<sup>8</sup>Strictly speaking, we do not observe new hires over the eleven months before the census month whose jobs come to an end before such census month. However, these new hires will have short durations by construction, of not more than eleven months.

<sup>9</sup>Figure B2 presents an illustration of this approach, considering one particular occupation, construction and public works technician (code 31120), in a particular district (Leiria), and in a particular year (2006), all chosen randomly. Each observation in the histogram corresponds to one of 240 firms, located in the district, that employ at least one worker of this occupation in that year. The Herfindhal index for this case is 51.7, highlighting the significantly dispersed nature of the employment of this occupation in this district, as over 200 of the 240 firms employ only one worker registered in this occupation.

is debatable in the context of labour markets, which have different dynamics from those of product markets.<sup>10</sup> Nonetheless, for comparative purposes with Azar et al. (2022), we use it as a reference.

As can be seen from Figure 1, which presents graphically the HHI time series, there is no particular trend that can be discerned from our results, including an increase in labour market concentration over time. If anything, we find a possible downward trend, particularly over the period 1998-2010, in the stocks measure. The data may also be consistent with moderate counter-cyclicality, particularly in the hirings measure: years such as 2004, 2009 and 2012, when unemployment increased significantly, are also periods of above-average HHIs. These high HHIs will reflect not only the steep reductions in hires during downturns but also the particular distribution of hires across firms that arises then, in such a way that the HHI concentration measure increases significantly.

How do the stock and flow series compare? Table 1 indicates that the HHIs in stocks are almost always lower than the corresponding (same year) HHI's in flows. For instance, in 2006, the former is 696 and the latter 981. Overall, stock HHIs are, on average, about 30% higher than flows HHIs. This gap reflects the fact that not all firms employing workers in a given year also hire in that same year.

Table 1 also presents information about the number of occupation-district cells used to compute the HHIs as well as the total number of workers and hirings in each year. The first series ranges between 11,000 and 21,000 cells, reflecting the growth of the formal sector since the late 1980s and a change in occupation codes in 1995 and in 2010,<sup>11</sup> as well as the labour effects of the financial crisis and debt crises from 2008. The numbers of workers and new hires follow a similar pattern to that of local labour market cells, increasing from 1.6 million employees (200,000 new hires) to 3.2 million employees (808,000 new hires), over the period.

We also describe our data in terms of the nearly 470,000 local labour market cells considered, when pooling over the entire period of our analysis, 1985-2019 - Table 2. The mean number of workers per cell is 174, while the mean number of new hires is 35 (both statistics are not weighted). The mean HHI measures are, when considering employment stocks, 726 (weighted) and 4,263 (unweighted). When considering employment flows (new hires), these figures are 998 and 3,201, respectively. The latter figure is the most comparable with those reported in Azar et al. (2022), based on 16 of the most frequent occupations posted on a large online jobs board in the US over 2010-2013, which present a remarkably similar mean HHI figure of 3,157. While Azar et al. (2022) focus their analysis on average HHIs across local labour markets, we focus on employment-weighted HHIs, i.e. we adopt the perspective of the labour market faced by the average worker (and not that of the average labour market), to take into account the possibility - supported in our analysis - that concentrated labour markets tend to be

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<sup>10</sup>As Azar et al. (2022) note, the two-sided matching process between buyers (firms) and sellers (workers) may render labour markets less active compared to one-sided product markets. Thus, any given level of concentration may affect these markets differently.

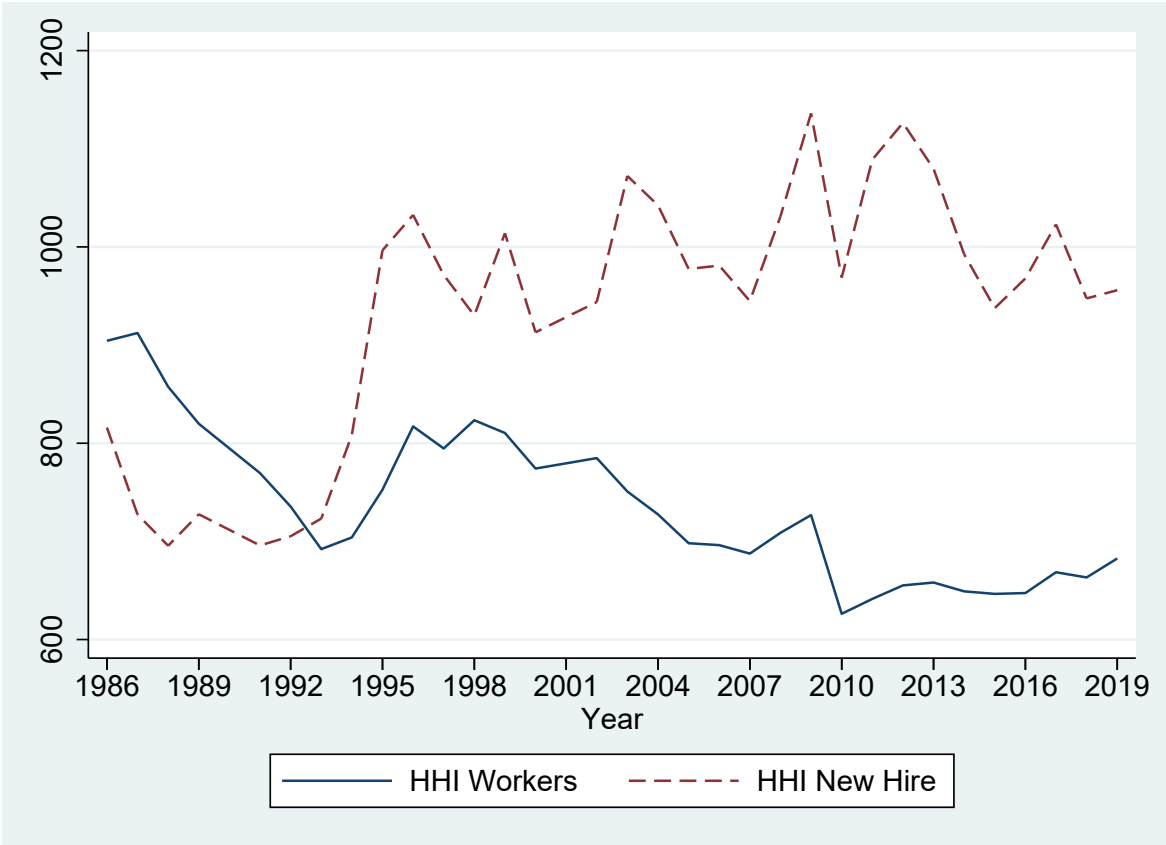
<sup>11</sup>The classification of occupations we use are the following: *i*) the CNP80 (equivalent to ISCO68) between 1985 and 1994, *ii*) the CNP94 (equivalent to ISCO88) between 1995 and 2009, and *iii*) the CPP 2010 (equivalent to ISCO08) from 2010 onwards. To deal with these changes in occupational codes we include occupation by region by occupational classification fixed effects when estimating how labour market concentration affects wages.

Table 1: HHI values (weighted), 1985-2019

Year	HHI (workers)	HHI (hirings)	N. cells	N. workers	N. hirings
1985	1023.7	1166.7	6219	876559	80589
1986	904.4	816.0	11133	1677977	206893
1987	912.3	727.4	11249	1706841	246613
1988	857.5	695.6	11288	1751868	303258
1989	819.9	727.7	11584	1902171	364735
1991	769.6	695.9	11704	1963350	374348
1992	735.5	705.2	11666	1986647	359127
1993	692.1	723.3	11690	1980966	327438
1994	704.0	808.4	11722	1927972	321427
1995	752.6	996.5	13323	1993603	339957
1996	817.1	1032.6	13327	1968477	346704
1997	794.7	971.5	13696	2150737	438352
1998	823.4	930.2	14889	2185320	454425
1999	810.5	1014.0	16224	2283315	449316
2000	774.2	913.1	16941	2494350	552783
2002	784.9	943.8	18701	2695196	584535
2003	750.8	1072.4	19690	2800003	540625
2004	727.5	1042.4	20252	2891960	569950
2005	698.2	977.6	20656	3065839	630755
2006	696.2	981.0	20753	3111190	661384
2007	687.6	944.9	20927	3220102	742339
2008	708.8	1031.2	21062	3267603	763912
2009	726.8	1136.1	20867	3125383	623430
2010	626.3	968.8	12214	2897224	569937
2011	641.4	1089.0	12185	2849773	532959
2012	655.2	1126.3	12148	2665448	429924
2013	658.1	1079.7	12013	2654089	482447
2014	649.1	992.6	12006	2732976	563518
2015	646.5	937.7	12069	2813684	619577
2016	647.4	967.9	12090	2921037	663948
2017	668.6	1022.9	12107	3053565	738881
2018	663.3	947.5	12190	3173048	792708
2019	682.6	955.9	12197	3227059	808045

**Notes:** See the Herfindhal index formula in equation 1. HHI (workers) denotes the mean Herfindhal index in each year considering the distributions of employees across firms in each district-occupation-year cells ('stocks'). HHI (hirings) denotes the mean Herfindhal index in each year considering the distributions of new hires (workers hired over the previous 12 months, as of October of each year (March in 1991 up to 1993)), across firms in each district-occupation-year cells ('flows'). 'N. cells' indicates the number of district-occupation pairs in each year. 'N. workers' ('N. hirings') indicates the total number of workers (hirings) in each year. Figures for 1990 and 2001 are not presented as worker-level data for this year is not available. Occupation codes change in 1995 and 2010. The weights of the two series are the employment count of each occupation-district-year cell.

Figure 1: HHI by number of employees and of new hires (weighted), 1985-2019



**Notes:** Own calculations based on the 'Quadros de Pessoal' data set. See the Herfindhal index formula in equation 1 and Table 1 for more details on each time series. Figures for 1990 and 2001 are interpolated as worker-level data for these years are not available. Occupation codes change in 1995 and 2010. The weights of the two series are the employment count of each occupation-district-year cell. 'HHI works' corresponds to employment stocks; 'HHI new hires' corresponds to employment flows (workers employed in October of each year (March, up to 1993) and hired over the previous 12 months).

Table 2: **Descriptive statistics, district-occupation-year cells, 1985-2019**

Variable	Obs	Unweighted		Weighted		Min	Max
		Mean	Std. Dev.	Mean	Std. Dev.		
Year	463,964	2003.0	9.2	2004.3	9.9	1985	2019
Number of workers	463,964	172.7	1,064.9	6,737.7	13,623.1	1	105,322
Number of new hires	463,964	35.0	251.4	1,441.6	2,772.5	0	18,044
HHI (n. of workers)	463,964	4,289.3	3,721.3	736.7	1,679.1	1	10,000.0
HHI (n. of new hires)	463,964	3,207.4	3,850.8	1,007.4	1,923.7	0	10,000.0
Mean base pay	463,964	602.0	592.6	566.9	606.0	0	35,160.5
Mean total pay	463,964	705.6	706.7	649.6	681.9	0	35,160.5
Mean Col. Barg. coverage	463,964	0.82	0.38	0.84	0.37	0	1

**Notes:** Each observation corresponds to a district-occupation combination observed in a year. 'Unweighted' ('Weighted') denotes statistics in which all cells carry the same weight (each cell carries the weight proportional to its employment). 'Mean base pay' is the average nominal base pay of the workers employed in the district-occupation-year cell. 'HHI (n. of workers)' denotes the Herfindhal index (under its stock-base measure) for each district-occupation-year cell. 'HHI (n. of new hires)' denotes the Herfindhal index (under its flow-base measure) for each district-occupation-year cell. 'Mean Col. Barg. coverage' is the average coverage of workers by any type of collective bargaining agreement for each district-occupation-year cell.

characterised by small numbers of workers. We also find an interquartile range of 510 HHI points (556 - 46) when weighting cells by employment but much higher, 7,216 (8,144 - 928), when not weighting.<sup>12</sup> Moreover, as Table 2 shows, between 1985 and 2019, the base wage in the average local labour market was of approximately 602 euros, while the average private sector worker earned 40 euros less (561 euros). When including other pecuniary benefits workers received, total remuneration increases to 706 euros in the average local labour market, and to 646 euros for the average worker. We also note the high share of workers covered by a collective bargaining agreement during this period – 81%.

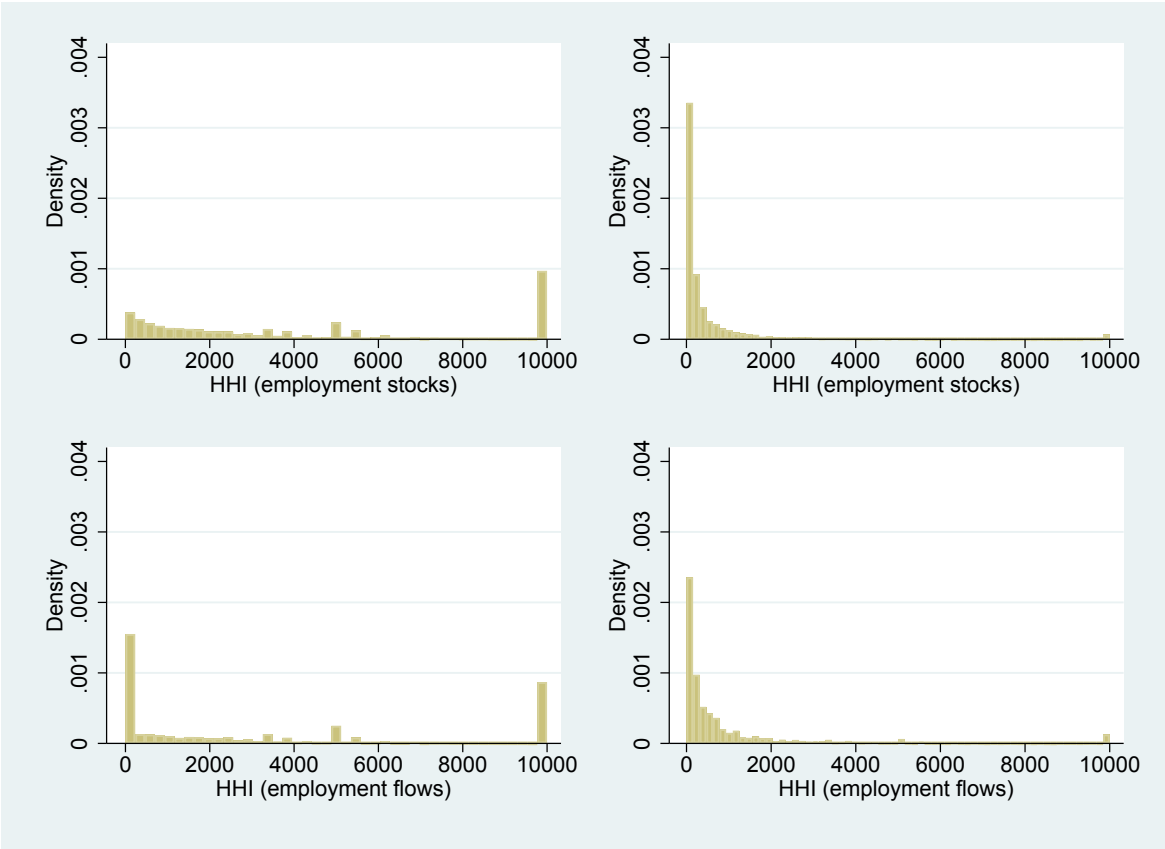
To illustrate our findings, Figure 2 presents the distributions of HHIs across local labour markets – defined as district-occupation pairs – in a particular year, 2006, not weighting or weighting each cell by its employment (left- and right-hand-side histograms, respectively), considering stocks or flows (top and bottom graphs, respectively). As in the case of the pooled cells, we find that this weighting makes a significant difference, as the most concentrated local labour markets tend to be those employing fewer workers, in many cases a single worker. Very similar results emerge when considering different years than 2006. The range of HHI concentration levels between 0 and 2,000 is the one that applies to most workers, as in the case study of a particular occupation (construction/public works technician) in a particular district (Leiria) presented above and in Figure B2.

To provide greater detail on our results, we also describe here a subset of occupations with the highest and lowest HHI values.

As an illustration, we present some occupations – employing at least 1,000 workers – according to

<sup>12</sup>Standard deviations of HHI measures range between around 1,600 (weighted analysis) and 3,800 (unweighted case), regardless of the type of measure (stock or flow). Furthermore, mean salaries range between 561 and 706 euros, depending on whether one is considering base or total wages and weighted or unweighted means. We consider these figures later, when computing the wage implications of more concentrated markets, together with the elasticities presented in Section 5.

Figure 2: Distribution of HHI by number of employees across local labour markets (left: unweighted; right: employment-weighted; top: stocks; bottom: flows), 2006



**Notes:** Own calculations based on the 'Quadros de Pessoal' data set. See the Herfindhal formula in equation 1. Left-hand-side distributions: All local labour markets (occupation-district pairs) carry the same weight, regardless of the number of workers. Right-hand-side distributions: Each local labour markets (occupation-district pairs) carries a weight in the histogram that is proportional to its employment level. Top (bottom): measures based on employment stocks (flows).



their average level of concentration. First, between 1985 and 1994 – when the ISCO68 occupation classification was in place – ‘Retail trade salesman’ was the occupation with the lowest concentration index (average HHI of about 30), while the occupation with the highest levels of concentration was ‘Telephone and telegram wireman’ (10,000). Between 1995 and 2009 – ISCO88 – the occupations with the lowest and highest average HHIs were, ‘Managers of small enterprises in wholesale and retail trade’ (15) and ‘Postman’ (9,918), respectively. Similarly, from 2010 onwards – ISCO08 – ‘Other services managers’ (8) and ‘Postman’ (9,198), were at the opposite ends of the HHI distribution. The last case reflects the fact that the post distribution operates as a (product market) monopoly in Portugal, as in many other countries.

## 4.2 Sensitivity

The results above follow from choices we made regarding the definition of a local labour market. Thus, before presenting the effects of labour-market concentration on wages, we investigate the robustness of our findings to different choices. Departing from our benchmark model described above (based on 1,400 occupations and 30 districts), we exploit the richness of our data set to measure local labour markets in up to seven alternative ways, so that we can better understand the sensitivity of labour-market concentration to different measures and choices.

In the first alternative approach, we consider a narrower classification of occupations, namely, collective bargaining job titles. There are typically over 30,000 job titles established in collective agreements in any one year, each one of which typically subject to a different and time-varying minimum wage and applying to nearly the full employee population (Martins, 2021). However, these alternative occupation measures are also very likely to double-count occupations and thus overestimate the number of local labour markets, leading to spuriously high concentration figures.<sup>13</sup> Table 3, second row, presents the results (the first row presents the benchmark results, weighted by employment, to facilitate the comparison with alternative measurement approaches). This definition of occupations leads to a significant increase in the average number of labour-market cells, from 14,266 to 81,539, as occupation codes increase by more than a factor of 20 when moving from the benchmark case to collective bargaining job titles. We find that average HHI measures more than double in both cases (employment stocks and flows), reaching 1,785 and 2,033, respectively. The percentage of workers in local labour markets with HHIs above the 2,500 also more than doubles, reaching nearly 20%, a figure considerably above the benchmark case (8.7%) and more in line to the one found for the U.S. in Azar et al. (2020).

Another approach is to consider occupations at a more aggregate level. Consistent with our results using collective bargaining job titles, when we define labour markets using a 4-digit classification instead of a 5-digit one, we document lower levels of labour-market concentration using both measures of HHI.

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<sup>13</sup>For instance, a secretary in the metalwork industry will have a different collective bargaining job title code than a secretary in the retail sector even if their jobs are highly substitutable and if both employees are located in the same district.

Table 3: **HHI mean values, different measurement approaches**

Measurement type	HHI		Average number of			% workers
	Workers	Hirings	cells	workers	hirings	HHI $\geq$ 2,500
Benchmark case	742.8	943.7	14266	2485313	499541	8.7
Collective barg job titles	1784.9	2032.8	81539	2485313	499541	19.7
Occupation (agg at 4 digits)	513.6	670.0	6948	2485313	499541	4.7
Less aggregated regions	1460.8	1907.5	61094	2485313	499541	17.8
Hirings (prev 6 months only)	742.8	996.8	14266	2485313	435746	11.5
Only firms 10+ employees	1017.9	1334.1	12682	1880514	366342	10.7
Manufacturing sector only	1137.3	1815.0	8218	694827	90138	13.5
Industry	1667.5	1515.1	9628	2485313	499541	18.8

**Notes:** 'Benchmark' measurement type is the main one adopted in the paper, based on approximately 1,400 occupations and 30 district-equivalent regions (18 districts plus 12 islands). 'Hirings in previous 6 months' is the same approach except that the HHI hirings measure is based on workers hired over the previous six months only (and not the previous 12 months, as in the benchmark and all other measurement approaches). 'Collective bargaining titles' is the same approach as the benchmark model except that, instead of considering occupations, we differentiate occupations by using the collective bargaining job titles (about 30,000). 'Occupation (agg at 4 digits)' is the same approach as the benchmark model except that, instead of considering 5-digit occupations, we use the 4-digit classification. 'Smaller districts' is the same approach as the benchmark model except that, instead of considering the 30 district codes above, we consider instead a finer classification, amounting to 452 different codes. 'Only firms 10+ employees' is the same approach as the benchmark model except that we disregard from the analysis firms that, in each year of the analysis, employ with fewer than 10 employees. 'Manufacturing sector only' is based on manufacturing firms only. 'Industry' defines labour-markets using the 5-digit industry classification rather than occupation. HHI and number of cells, workers and hirings are unweighted averages of the weighted HHI yearly data, over the 1986-2019 period. The last column indicates the weighted percentage of cells across all years that have HHI levels above 2,500 (workers HHI except the second row, which denotes new hirings HHI).

A third alternative measure involves considering more disaggregated regional definitions ('concelhos' instead of 'distritos'). This option is influenced by the findings in Manning and Petrongolo (2017), which present evidence that labour markets can be very local, as jobseekers reduce their search efforts considerably at longer commuting distances. In our case, the 'concelho' level has 10 times more units in total when compared to 'distritos' and are roughly equivalent to U.S. counties.<sup>14</sup> We find, first, that the number of occupation-region-year cells increases by more than four times from the benchmark case to over 60,000, reflecting the far more disaggregated nature of the local labour market - Table 3, fourth row. On the other hand, the HHI measurements also increase, to 1,461 and 1,908, for employment stocks and flows, respectively. While this change is significant, these averages fall short of the levels measured in the case of collective bargaining titles and also correspond to a smaller percentage of workers in local labour markets above the critical thresholds of 2,500 (17.8% in this case).

Another alternative from our benchmark measure is based on restricting the time window under which a worker is considered a new hire, from the 12-month period used before to six months only. Our interest in this analysis stems from the vacancy perspective adopted in other papers (Azar et al., 2022, 2020). We find that the HHI for new hires increases by only 6%, from 944 to 997 (the employment stocks results are unchanged as they are not affected by this criterion). The number of new hires is naturally reduced, from about 500,000 to 435,000, as in some local labour market hirings only take place in the first half of the year. Overall, despite halving the time window adopted for the consideration of new hires, the degree of concentration increases by a much smaller magnitude, indicating that these measures are not very sensitive in this dimension.

We also examine the role of the large percentage of small firms in the labour market of Portugal as potential driver of low HHI levels found so far.<sup>15</sup> Removing firms with fewer than 10 employees from our analysis leads to a significant decrease in the average number of workers and hires, and a corresponding increase in the HHI measurements. However, this increase is not as high as in other measurement approaches. Hence, our findings suggest that firm size is not the main factor behind the low concentration measures found in our benchmark analysis.

Finally, we consider the sectoral dimension. As indicated before, some studies of employer concentration define local labour-markets using the firm's industry rather than the worker's occupations or only examine the manufacturing sector because of data limitations (Benmelech et al., 2022; Duan and Martins, 2022; Rinz, 2022). In order to shed light on the potential impact of such methodological approaches, we focus on the industry of each establishment indicated in our data to make a couple of additional analyses. First, we restrict our attention to manufacturing sector firms only. Row seven of Table 3 presents the results, which indicate significantly higher levels of concentration, with stock and

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<sup>14</sup>As this level will be too small to be considered a distinctive commuting zone in the cases of the two largest metropolitan areas, Lisbon and Porto, which are served by good public transport links, we still consider units based on 'distritos' (not 'concelhos') in those two particular cases.

<sup>15</sup>As documented in Cabral and Mata (2003) and Braguinsky et al. (2011), Portugal tends to exhibit a different distribution of firm sizes than some other developed countries, with a larger mass at smaller sizes. This may be due to factors such as limited access to credit, poor management, strict regulations and employment laws, and a preference for service providers over formal employees.

flows HHI's of 1,173 and 1,815, respectively, and an employment share above 2,500 of 13.5%. Second, we define local labour-markets as industry-by-region cells, using 5-digit industry classifications. Here we find that the HHI stock measure increases by 125% compared to the benchmark case, while the flows measure increases by 60%. Under this case, about 19% of workers are exposed to a level of concentration greater than 2,500.

In conclusion, by exploiting the richness of our matched employer-employee data, we find that the measurement of employer concentration is sensitive to different methodological choices. Specifically, most - but not all - departures from our benchmark case increase concentration estimates. In any case, the direction and magnitude of the variation of our estimates across methods may be particularly useful when considering findings from studies that draw on less comprehensive data. In our next section, we complement this analysis by presenting evidence on the implications of some of these alternative measurements on the magnitude of the wage effects of concentration.

## 5 Wage effects

### 5.1 Empirical strategy and results

If workers have limited choice in alternative employers in their occupation and commuting zone, firms may exploit the resulting wage setting power by reducing the pay offered to, and eventually accepted by workers. In this context, after establishing above the levels (and sensitivity) of employer concentration across labour markets, we now estimate the impact of such concentration on wages. Specifically, in our benchmark specifications, we consider the following equation:

$$\log Y_{i,l(o,d),t} = \alpha + \beta \log HHI_{l(o,d),t} + \delta_{l(o,d)} + \phi_{d,t} + \tau_i + \epsilon_{l(o,d),t}, \quad (2)$$

in which  $Y_{i,l(o,d),t}$  is the (monthly) wage of worker  $i$  in local labour market  $l(o,d)$ , corresponding to occupation  $o$  and district  $d$ , in year  $t$ ; and  $HHI_{l(o,d),t}$  is the Herfindhal-Hirschman index of the same local labour market  $l(o,d)$  and year  $t$ .  $\delta_{l(o,d)}$  are fixed effects for each local labour market,  $\phi_{d,t}$  are district-by-year fixed effects, and  $\tau_i$  are worker fixed effects. While we do not control specifically for local labour market tightness (Azar et al., 2022), all specifications include both district-by-year (district-year pairs,  $\phi_{d,t}$ ) and local labour market (occupation-district pairs,  $\delta_{l(o,d)}$ ) fixed effects. Given our log-log specification, the estimate of  $\beta$  will indicate the elasticity of the individual wage with respect to the Herfindhal index of the market where the worker is based in each year.

We consider three specifications: the first without controlling neither for worker nor firm fixed effects, the second controlling only for worker fixed effects, and the third controlling for both worker and firm fixed effects. Highly-concentrated labour markets may derive their concentration from the presence of large firms that will tend to have both higher productivity and stronger product market power. These dual advantages may then be shared with workers via collective bargaining, for instance.

For each specification, in order to provide a comprehensive analysis of the effects of labour market concentration on wages, we consider two different measures of HHI: one based on employment stocks (odd columns), and another based on employment flows (even columns). While the former measure may provide a more stable measure of HHI, the latter may be more directly linked to the dynamics in the labour market in each year. The flows measure may thus better reflect the opportunities for wage growth of incumbent workers, for instance through mobility to other firms.

Table 4 presents the first set of results, covering the full 1986-2019 period (and about 70 million worker-year observations).<sup>16</sup> In columns 1 and 2, we exclude worker fixed effects to obtain estimates equivalent to those derived from pooled worker data. In the first column, we find a significantly positive elasticity of 0.018 when using the stock measure of HHI. These results are partially at odds with the view that employer concentration erodes wages but are consistent with the perspective that employer concentration can be a proxy for rent sharing, for instance through (collective) wage bargaining. When controlling for worker fixed effects the magnitude of the coefficient of the stock measure drops considerably to -0.002 (column 3). This drop suggests that high-wage workers tend to be employed in high-concentration local labour markets and that rent sharing can again be relevant in shaping the wage distribution. The point above is less evident when using the flow measure, since including worker fixed effects leads to a much smaller drop, from an elasticity of -0.001 (not statistically significant) to -0.004 (respectively, columns 2 and 4).

Finally, we examine the role of firms in wages and their potential confounding effect. In what is our main specification, we add firm fixed effects to our equation. Joint identification of firm and worker effects is based on mobility of workers across firms and regions over time. In this case, when we include firm fixed effects (columns 5 and 6), both coefficients remain relatively stable. Thus, firm-level unobserved heterogeneity does not appear to be relevant for now.

One may argue that concentration measures are not exogenous with respect to wages. For instance, larger local labour markets (typically with lower concentration levels) may attract more productive firms that pay higher wages and want to be able to hire workers more quickly. While we control for time-invariant differences across local labour markets and for economy-wide year effects at the district level, our results could be influenced by labour demand or labour supply shocks at the level of the local labour market. An increase in the number of university graduates in particular occupations may lead to both lower levels of concentration (if they create new firms) and lower wages in the resulting jobs (due to an increase in labour supply), generating a spurious downward bias in our estimates. Declines in the international demand for specific products may lead to both lower labour demand and lower wages, as in the context of a 'China shock' (Cabral et al., 2021), and more concentrated employment in specific occupations, as some firms exit the market, generating an upward bias in this case.

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<sup>16</sup>We drop 1985 due its high share of missing values, including in the date of hiring, which is crucial to compute the flow measure of HHI. Also, given that the QP dataset before 1990 failed to fully cover the private sector (Braguinsky et al., 2011), we test the robustness of our results by restricting the sample to the 1991 to 2019 period – see Appendix Table E3. In another robustness check we also exclude the islands from the analysis, as their labour markets may not be strictly comparable with those of the mainland – see Appendix Table A2. In both cases, results are robust to these restrictions.

Table 4: Wage results, OLS, 1986-2019

	(1)	(2)	(3)	(4)	(5)	(6)
Log Herfindhal Index (Stocks)	0.018 (0.002)***		-0.002 (0.001)*		-0.005 (0.001)***	
Log Herfindhal Index (Flows)		-0.001 (0.002)		-0.004 (0.001)***		-0.002 (0.000)***
<i>N</i>	69348046	68191301	69348046	68191301	69348046	68191301
adj. <i>R</i> <sup>2</sup>	0.588	0.585	0.785	0.783	0.818	0.817
F	55.93	.1045	3.179	13.99	53.45	23.35
(District x Year) FEs	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by OLS using the estimator proposed by Correia (2017). The columns present different specifications of wage equation 2. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). <sup>†</sup>(District x Year) FEs denote fixed effect for each district-year pair. <sup>‡</sup>(Occupation x District) FEs denote fixed effect for each district-occupation pair. <sup>§</sup>1 denotes that the set of fixed effects in the row is included in the specification. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.

We address this potential endogeneity issue by instrumenting  $HHI_{l(o,d),t}$ , following Azar et al. (2022) and earlier research in industrial organisation and labour economics. Our instrument corresponds to the average of the number of employers in the same occupation and year but different region. Formally, it corresponds to  $\sum_e \log(1/N_e)/29$ , where  $N_e$  indicates the number of firms in each one of the other (29) regions ('distritos'), except  $d$ , that employ workers of the same occupation  $o$ , in the same year  $t$ . This instrumental variable does not depend directly on market shares as it is based on the number of firms and not their employment. Moreover, it provides variation in local labour market concentration driven by changes across the country except for the specific local labour market that is being instrumented. In this way, we can obtain estimates that are not biased by the local labour demand or labour supply shocks mentioned above.

Table 5 presents our first set of IV results using the variable above. In the first stage regression, we find the positive (and highly significant) effects predicted. The higher the average number of firms employing workers of the same occupation in other regions, the lower the value of the instrument, and the lower the level of concentration in the local labour market.<sup>17</sup> In the second stage, when not controlling for worker fixed effects (columns 1 and 2), we find again significant positive effects when using the stock measure (elasticity of 0.76) and the new hires measure (elasticity of 0.75). When we introduce worker fixed effects, the elasticity becomes negative, ranging between -0.006 (column 3) and -0.007 (column 4). Both are statistically significant at the 10% level.

Finally, we add firm fixed effects to our equation to control for the forces above. Thus, with this set of fixed effects, to identify how labour market concentration affects wages we rely on variations in HHI within local labour markets over time. The source of such variation is worker mobility across local labour markets – i.e., changes in occupation within districts or movements across districts. For the HHI stock measure, when workers move, the measure of concentration of both the labour markets of departure and of arrival change, since, all else equal, the share of employment of the worker's old (respectively, new) firm in a given local labour market reduces (increases). For the HHI flow measure, worker mobility only affects the level of concentration of the local labour market of arrival, since, all else equal, the share of new hires in the local labour market of a worker's old firm is not affected by employment outflows.

Columns 5 and 6 in Table 5 present the results of this more comprehensive specification, in which we find negative elasticities of -0.014 for both measures of HHI. This increase in (the absolute value of) the elasticities is consistent with our discussion above that relatively high-wage firms tend to be more common in high-concentration local labour markets. Such correlation can introduce an upward bias in the wage-concentration elasticities that may make one underestimate the overall negative effect of concentration on wages. Given the same interquartile range of 510 HHI points presented in Section 4 and its resulting difference of 2.49 log points, these results indicate that workers that would move from low- to high-concentration local labour markets (defined here as the 25th and 75th percentiles,

<sup>17</sup>Moreover, we can exclude that the first stage is weak, as the Kleibergen and Paap (2007) Wald statistic is well above the Stock and Yogo (2005) critical threshold.

Table 5: Wage results, IV, 1986-2019

	(1)	(2)	(3)	(4)	(5)	(6)
Log Herfindhal Index (Stocks)	0.076 (0.006)***		-0.006 (0.003)*		-0.014 (0.002)***	
Log Herfindhal Index (Flows)		0.075 (0.006)***		-0.007 (0.003)*		-0.014 (0.002)***
<i>N</i>	69328824	68185800	69328824	68185800	69328824	68185800
adj. <i>R</i> <sup>2</sup>	0.587	0.583	0.785	0.783	0.818	0.817
F	178.58	152.3	3.449	3.55	40.9	38.21
(District x Year) FEs	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1
<i>Auxiliary regression (First-stage)</i>						
Log inverse of number of firms	0.64970 (0.030)***	0.63685 (0.029)***	0.62552 (0.025)***	0.61747 (0.025)***	0.60023 (0.021)***	0.57648 (0.021)***
<i>N</i>	69328824	68185800	69328824	68185800	69328824	68185800
adj. <i>R</i> <sup>2</sup>	0.959	0.923	0.962	0.924	0.966	0.928
F	460.5	476.3	604.9	611.2	785.2	766
Kleibergen-Paap rk Wald F-stat	460.5	476.3	604.9	611.2	785.2	766
P-value	0	0	0	0	0	0
Shea's R2	.1225	.07359	.1148	.06599	.1012	.05273

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). '(District x Year) FEs' denote fixed effect for each district-year pair. '(Occupation x District) FEs' denote fixed effect for each district-occupation pair. '1' denotes that the set of fixed effects in the row is included in the specification. The instrument is the average of the log of the inverse of the number of firms in the same occupation and year but other districts. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.



respectively) would experience a drop in wages of approximately 3.5%. Also, it is worth noting that our coefficients are about ten times lower than the benchmark results in Azar et al. (2022), who find elasticities between -0.116 and -0.2.

## 5.2 Robustness

We now test whether the wage effects that we find are robust to considering each of the seven alternative measures of labour market concentration presented in section 4.2. Again, this robustness exercise seeks to exploit the richness of our data and provide comparisons that are not feasible with other data sets.

These results are presented in Table 6 and are based on our main specification including worker and firm fixed effects and instrumenting for labour market concentration. In columns 1 to 4, we study the impact of aggregating occupations at different levels. Interestingly, we find wage effects increase as we move towards more aggregated classifications of occupations. When we define labor markets using collective bargaining job titles (columns 1 and 2), elasticities range from -0.003 to -0.005 (both not statistically significant), and considerably lower than the benchmark case (-0.014). However, if we consider 4-digit occupations (instead of the 5-digit level used in the benchmark case), the estimated results nearly double (columns 3 and 4). These results point to the importance of defining precisely the local labour market. Considering extremely narrow classifications, such as collective bargaining job titles, overestimates the levels of labour market concentration as it separates occupations which are close substitutes to firms. This overestimation of concentration, which may be regarded as a form of measurement error, leads to lower wage effects, even in IV models.

Next, in columns 5 and 6, we examine how using less aggregated regions influence our results. Unlike in the previous case, considering 'concelhos' instead of 'distritos' does not alter our results significantly. Similarly, considering a shorter time window to define a new hire leads to the same elasticity found before (column 7). As for the alternative approach, where we exclude from the analysis firms with fewer than ten employees, we find a modest increase in the absolute value of the HHI coefficients, to about -0.017.

Finally, we consider how restricting our analysis to the manufacturing sector or using industry instead of occupation to define a local labour market (Benmelech et al., 2022; Rinz, 2022) affects our estimates. We find that the coefficients increase considerably (in absolute value) in both instances (columns 10 to 13), especially the latter. These elasticities range between 2.3% and 3.7%.<sup>18</sup> These results are important, given that, as mentioned above some firms, like post distribution services, operate as monopolies that are possibly government-granted. One concern that may arise is that such firms may not be profit maximizers, and thus may not exert their monopsony power the same way as true private firms would. By restricting our analysis to the manufacturing sector, we are discarding quasi-governmental firms. The larger magnitude of the wage effects of labour market concentration

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<sup>18</sup>When comparing the current findings for 1986-2019 to those of the period 1991-2013 (Martins, 2018), we find that they are very similar. This indicates that the effects of concentration on wages have not changed significantly over the last four decades.

Table 6: Wage results, IV, different measurement approaches, 1986-2019

	Coll. Barg. titles		Occupation 4 digit		Less agg regions		Hiring (6 months)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(7)
Log Herfindhal Index (Stocks)	-0.003 (0.002)		-0.024 (0.003)***		-0.015 (0.003)***			
Log Herfindhal Index (Flows)		-0.005 (0.003)		-0.025 (0.004)***		-0.014 (0.003)***		-0.014 (0.002)***
<i>N</i>	66147032	59496992	69345193	68924363	69329930	67105344		67953166
adj. <i>R</i> <sup>2</sup>	0.820	0.811	0.817	0.816	0.820	0.816		0.817
<i>F</i>	1.549	2.932	54.96	51.06	23.65	16.65		38.19
Kleibergen-Paap rk Wald F-stat	2212	1364	293.8	958.7	464.1	322.0		750.7
Shea's R2	.1008	.05153	.07499	.03357	.09423	.0487		0.5151
	Firms 10+ employees		Manufacturing sector		Industry			
	(8)	(9)	(10)	(11)	(12)	(13)		
Log Herfindhal Index (Stocks)	-0.017 (0.003)***		-0.023 (0.003)***		-0.035 (0.007)***			
Log Herfindhal Index (Flows)		-0.018 (0.004)***		-0.024 (0.004)***		-0.037 (0.008)***		
<i>N</i>	55072549	53806618	20093288	19339698	67105344			66508564
adj. <i>R</i> <sup>2</sup>	0.826	0.824	0.872	0.852	0.814			0.812
<i>F</i>	27.16	25.13	45.30	36.38	25.52			23.63
Kleibergen-Paap rk Wald F-stat	658.8	450.6	471.0	357.0	600			253.8
Shea's R2	.0821	.0395	.09369	.04022	0.4045			.0161

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the each different measurement presented in Table 3 based on either employment stocks (odd columns) or flows (even columns). All regressions include district-by-year, district-by-occupation, worker, and firm fixed effects. The instrument is the average of the log of the inverse of the number of firms in the same occupation and year but other districts. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.

we obtain indicates that, indeed, it is possible that including such firms in the analysis attenuates our estimates.

### 5.3 Extensions

In the analysis above we have established that employer concentration has a negative impact on wages, even when using different approaches to measure it. Here, we extend the analysis to examine four additional issues: *i*) the potential attenuation of market power through collective bargaining; *ii*) the role of large firms in concentrated markets; *iii*) potential non-linear effects of labour-market concentration on wages; and *iv*) the impact of market power on wage inequality.

#### *Collective bargaining*

In our main analyses above, the introduction of firm fixed effects led to a substantial increase in the (negative) effect of labour market concentration on wages, highlighting the relevance of firm heterogeneity. One point potential factor that may be captured by these firm fixed effects is the role of unionisation or collective bargaining in wage determination. Indeed, in the same way that workers faced with a dominant employer in their local labour market will not have many job alternatives, a dominant employer faced with a powerful union may find it difficult to replace their workers. Such employer will then have to share at least some of its rents with its workers, particularly when the firm has not only labour market power but also product market power.

Such countervailing, trade-union-based mechanism would lead to a more balanced relationship between the employer and the employee sides. In this case, the wage setting power of the employer is attenuated and the rent sharing dimension may even possibly gain greater predominance (see Card et al. (2018) and Garin and Silverio (2023) for recent evidence of rent sharing in Portugal, also based on the QP data set.) Note that, as in other European countries, collective bargaining plays an important role in the labor market of Portugal (Martins, 2021; Martins and Saraiva, 2020).<sup>19</sup> Local labour markets may also be associated to industrial clusters, generating external scale economies, leading to higher wages (see Figueiredo et al. (2014), which uses the same data set as in this paper).

To test whether collective bargaining is indeed an important force in counter-acting employer market power, we interact our measures of concentration with the level of coverage of collective bargaining of each local labour market – in the first year we observe it.<sup>20</sup> Similarly, we instrument this variable using the interaction between collective bargaining coverage and the instrument. We use the first period and

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<sup>19</sup>Despite low unionisation rates, collective agreements are frequently extended by the government, requiring most firms and employees in a given industry to follow sectoral collective agreements. As to other labour market institutions that may also affect wage determination, the most important include minimum wages and unemployment benefits. While the former were relatively low in Portugal during the period under analysis (particularly until 2006), unemployment benefits were relatively generous, in terms of replacement rates and maximum duration.

<sup>20</sup>From collective bargaining job title codes we are able to observe which workers were covered by a collective bargaining agreement.

not the contemporaneous level of coverage to avoid any correlation between our instrument and wages, except, of course, through the instrumented variable. Our motivation is that the wage markdown resulting from increasing employer concentration (and the resulting fewer employer alternatives for workers) may lead to the emergence or strengthening of unionisation and collective bargaining. Such emergence may be particularly important when employers also coordinate, namely through employers' associations (Martins, 2020).

Table 7 presents the results. Interestingly, when we account for collective bargaining, the elasticities that we obtain without firm fixed effects (columns 3 and 4) are actually higher (in absolute terms) than those we get when including them (columns 5 and 6). This suggests that greater levels of coordination between employees can indeed attenuate wage markdowns. This finding is in line with Benmelech et al. (2022) and Dodini et al. (2021), who also find that unionisation counteracts the wage effect of labour-market concentration. There is also evidence for Germany and Portugal that higher levels of industry-level collective bargaining coverage also attenuates markdowns (Bassanini et al., 2023a).

### ***Local labour-market leaders***

We also examine the impact of firm size on wage effects. As Boal and Ransom (1997) note, in an asymmetric Cournot model, larger firms may be able to impose larger wage reductions. However, this may not be the case if we consider models with random search or worker heterogeneity in preferences for firm amenities Card et al. (2018). In fact, according to the latter model, large firms operating in smaller markets might have less market power than small firms in large markets due to the smaller pool of workers they have access to, making it difficult to employ those who value their specific amenities.

To examine this, we interact our measures of labour-market concentration with a dummy for when a firm is a local market leader, which we define as being a top five employer in a local labour-market in a year. In our preferred specifications in Table 8 (columns 5 and 6), the coefficient of the interaction term ranges between -0.002 and -0.005 (both are statistically significant at the 1% level). This means that markdowns are 15% to 30% higher for local leaders compared to their smaller counterparts.

### ***Non-linear effects***

The effects of labor-market concentration are not necessarily linear. This idea is supported by the findings of Azar et al. (2019) and Munguía Corella (2020), who find that the impact of minimum wage policies on employment varies depending on the level of labour-market concentration. Here, we examine potential non-linearities in wage effects instead of employment. For instance, the relatively small wage effects that we find may reflect the fact that most of our HHI distribution lies at relatively low concentration levels. Wage effects may be considerably higher at higher HHI levels.

We approach this question by restricting our analysis to the top and bottom 25% of the HHI distribution in our sample. The results, presented in Tables 9 and 10, support the case of non-linearities. We find large and significant elasticities between -0.030 and -0.032 when only considering

Table 7: Wage results, IV, Collective Bargaining, 1986-2019

	(1)	(2)	(3)	(4)	(5)	(6)
	Spec 1	Spec 2	Spec 3	Spec 4	Spec 5	Spec 6
Log Herfindhal Index (Stock)	0.070 (0.017)***		-0.019 (0.009)**		-0.010 (0.007)	
Log Herfindhal Index (Flow)		0.085 (0.020)***		-0.023 (0.011)**		-0.012 (0.008)
Log Her indhal Index (Stock) x CBA	0.006		0.015 (0.011)		-0.005 (0.008)	
Log Her indhal Index (Flow) x CBA		-0.012 (0.021)		0.018 (0.012)		-0.003 (0.008)
<i>N</i>	27737840	27281537	26928936	26473229	26767439	26310938
adj. <i>R</i> <sup>2</sup>	0.587	0.583	0.785	0.783	0.822	0.821
F	60.85	50.65	2.162	2.265	13.42	12.73
Kleibergen-Paap rk Wald F-stat	210.1	216.9	285.1	294.5	361.8	357.3
Shea's R2						
(District x Year FEs)	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns).  $\gamma$ (District x Year) FEs' denote fixed effect for each district-year pair.  $\gamma$ (Occupation x District) FEs' denote fixed effect for each district-occupation pair.  $\gamma$ 1' denotes that the set of fixed effects in the row is included in the specification. The instruments are the average of the log of the inverse of the number of firms in the same occupation and year but other districts and its interaction with the local labour-market's average coverage of collective bargaining. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table 8: Wage results, IV, Local labour market leader, 1986-2019

	(1)	(2)	(3)	(4)	(5)	(6)
Log Herfindhal Index (Stock)	0.073 (0.006)***		-0.006 (0.003)*		-0.014 (0.002)***	
Log Herfindhal Index (Flow)		0.073 (0.006)***		-0.007 (0.004)*		-0.015 (0.002)***
LLM top 5 employer x Log Herfindhal Index (Stock)	-0.044 (0.003)***		-0.012 (0.001)***		-0.002 (0.001)***	
LLM top 5 employer x Log Herfindhal Index (Flow)		-0.063 (0.004)***		-0.017 (0.002)***		-0.005 (0.001)***
<i>N</i>	27741901	27285652	27741901	27285652	27741901	27285652
adj. <i>R</i> <sup>2</sup>	0.588	0.583	0.785	0.783	0.822	0.821
F	936.3	868	200.2	196.2	21.02	24.51
Kleibergen-Paap rk Wald F-stat	226.5	232.2	297.8	297.1	389.7	367.5
(District x Year) FEs	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). '(District x Year) FEs' denote fixed effect for each district-year pair. '(Occupation x District) FEs' denote fixed effect for each district-occupation pair. '1' denotes that the set of fixed effects in the row is included in the specification. The instruments are the average of the log of the inverse of the number of firms in the same occupation and year but other districts and its interaction with the a dummy for working for a top 5 employer in the local labour-market. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.

high levels of concentration (Table 9). When we restrict the sample to the bottom 25% of the HHI distribution, results using HHI stocks are no longer statistically significant, while elasticities for HHI flows are similar in magnitude but less precisely estimated (Table 10). These results indicate that concentration effects might be stronger at higher concentration levels.

### *Inequality*

Finally, we analyse whether labour-market concentration may influence wage inequality both within firms and within local labour markets. There are potentially many mechanisms in this relationship. For example, if workers are paid the (statutory or collective bargaining) minimum wage, firms may have limited power to influence their wages (assuming no disemployment effects). This means that it would be workers who earn higher wages that may be more influenced by labor concentration. In this case, increases in concentration would reduce wage inequality. Different workers may also be distributed differently within firms and local labour markets, leading to potentially different effects of concentration on the two types of inequality.

To analyse these effects, even if only tentatively, we follow Rinz (2022) and Dodini et al. (2021) and examine the effects of concentration on within-firm and within labour-market inequality. We consider the ratios between the 90<sup>th</sup> and the 10<sup>th</sup> percentiles (P90/P10), the 90<sup>th</sup> and the 50<sup>th</sup> percentiles (P90/P50), and the 50<sup>th</sup> and the 10<sup>th</sup> percentiles (P50/P10).<sup>21</sup>

Our results - Table 11 - indicate that within-firm inequality increases with labour-market concentration. In contrast, inequality within local labour-markets decreases with labour-market concentration. Specifically, the coefficients in columns 1 and 2 of panel A indicate that an increase in HHI by 1% increases the P90/P10 ratio from 7% to 8%. These coefficients are twice the magnitude of the effects of labour-market concentration on the P90/P50 and the P50/P10 ratios, at around 4%. This larger increase in inequality between the top and bottom percentiles can have different explanations. It could be due to firms sharing their monopsonistic rents with top managers in the firm, as suggested by Rinz (2022), or, for instance, because wages stagnate at the bottom of the wage distribution (within the firm).

Regarding inequality within local labour-markets, increases in concentration by 1% lead to a decrease of between 5 to 10% in the P90/P10 ratio, and between 7 to 11% in the P90/P50 ratio. We find no effect of labour-market concentration on the P50/P10 ratio. One possible explanation for this is that, as mentioned above, there is not much room to adjust wages at the bottom tail of the wage distribution, possibly due to the minimum wage or collective bargaining agreements. Overall, we regard the analysis of the relationship between labour market concentration and wage inequality to be a promising topic for further research.

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<sup>21</sup>In the within-firm analysis, we consider the average level of labour-market concentration in the firm (across all its workers). That is, for every firm  $f$  with  $N$  workers we compute:  $HHI_{f,t} = [\sum_{i=1}^{N_f} HHI_{i,l,f,t}]/N_f$ . In the local labour-market analysis we use the same concentration measures as in our main analyses above.

Table 9: Wage results, IV, subset of high HHI levels, 1986-2019

	(1)	(2)	(3)	(4)	(5)	(6)
Log Herfindhal Index (Stocks)	0.115 (0.017)***		-0.001 (0.009)		-0.030 (0.007)***	
Log Herfindhal Index (Flows)		0.099 (0.014)***		-0.011 (0.007)*		-0.032 (0.005)***
$N$	18098138	17575051	16751952	16027600	16665083	15907819
adj. $R^2$	0.603	0.599	0.813	0.812	0.836	0.838
F	45.53	51.83	.003743	2.891	16.38	37.91
Kleibergen-Paap rk Wald F-stat	492.7	926	615.6	958.7	1,097	755.2
Shea's R2	.09785	.04734	.08383	.0356	.06502	.02513
(District x Year) FEs	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. Only observations corresponding to the top 25% HHI (employees) levels. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). '(District x Year) FEs' denote fixed effect for each district-year pair. '(Occupation x District) FEs' denote fixed effect for each district-occupation pair. '1' denotes that the set of instruments in the row is included in the specification. The instrument is the average of the log of the number of firms in the same occupation and year but other districts. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.



Table 10: Wage results, IV, subset of low HHI levels, 1986-2019

	(1)	(2)	(3)	(4)	(5)	(6)
	Spec 1	Spec 2	Spec 3	Spec 4	Spec 5	Spec 6
Log Herfindhal Index (Stocks)	0.074 (0.020)***		-0.020 (0.023)		-0.020 (0.014)	
Log Herfindhal Index (Flows)		0.069 (0.022)***		-0.038 (0.023)		-0.035 (0.015)**
<i>N</i>	15822974	16232508	14463389	14744956	14310448	14583125
adj. <i>R</i> <sup>2</sup>	0.572	0.585	0.828	0.829	0.862	0.863
F	13.39	10	.7784	2.674	2.171	5.526
Kleibergen-Paap rk Wald F-stat	442.7	458.5	581.8	589.4	757.2	738.7
Shea's R2	.1251	.0751	.1171	.06731	.1031	.05372
(District x Year FEs)	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. Only observations corresponding to the bottom 25% HHI (employees) levels. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). '(District x Year) FEs' denote fixed effect for each district-year pair. '(Occupation x District) FEs' denote fixed effect for each district-occupation pair. '1' denotes that the set of instruments in the row is included in the specification. The instrument is the average of the log of the inverse of the number of firms in the same occupation and year but other districts. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table 11: Wage results, IV, Inequality, 1986-2019

<i>Panel A</i>	Within firm					
	P90/P10		P90/P50		P50/P10	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Herfindhal Index (Stocks)	0.141 (0.023)*		0.0527 (0.001)***		0.0529 (0.020)***	
Log Herfindhal Index (Flows)		0.153 (0.024)**		0.0565 (0.001)***		0.0578 (0.021)***
<i>N</i>	7256237	7231318	7256237	7231318	7256237	7231318
adj. <i>R</i> <sup>2</sup>	0.126	0.126	0.264	0.263	0.1130	0.1130
F	38.59	40.05	3847	3950	7.14	7.54
Kleibergen-Paap rk Wald F-stat	766202	809680	766202	809680	766202	809680
Shea's R2	.4802	.4394	.4802	.4394	.4802	.4394

<i>Panel B</i>	Within local labour markets					
	P90/P10		P90/P50		P50/P10	
	(1)	(2)	(3)	(4)	(5)	(6)
Log Herfindhal Index (Stocks)	-0.261 (0.143)*		-0.170 (0.006)***		0.024 (0.130)	
Log Herfindhal Index (Flows)		-0.166 (0.037)***		-0.106 (0.008)***		-0.003 (0.018)
<i>N</i>	449881	301189	449881	301189	449881	301189
adj. <i>R</i> <sup>2</sup>	0.0819	0.3116	0.4346	0.4609	0.0791	0.2548
F	3.33	20.34	847.3	170.4	0.033	0.0258
Kleibergen-Paap rk Wald F-stat	14935	10534	14935	10534	14935	10534
Shea's R2	.2278	.1516	.2278	.1516	.2278	.1516

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panels present different specifications of wage equation 2. The dependent variables are the ratio between the 90<sup>th</sup> and the 10<sup>th</sup> wage percentiles (columns 1 and 2), the ratio between the 90<sup>th</sup> and the 50<sup>th</sup> wage percentiles (columns 3 and 4), and the ratio between the 50<sup>th</sup> and the 10<sup>th</sup> wage percentiles (columns 5 and 6). Panel A uses wage percentiles to describe how wages are distributed within firms, while in Panel B they describe how wages are distributed within local labour-markets. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). In panel A the Herfindhal is the average concentration level across all workers in the firm, instrumented by the average value of the instrument within the firm. In panel B the the Herfindhal is the same as the one used in the main analysis. The instrument is the average of the log of the inverse of the number of firms in the same occupation and year but in other districts. All regressions in panel A include firm and district-by-year fixed effects. In panel B, regressions control for district-by-occupation and district-by-year fixed effects. Standard errors clustering at the firm level in panel A and at the local labour market level in panel B. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.

## 6 Conclusions

Exploiting rich matched data for Portugal, we made a number of contributions to the empirical literature on local labour market power. In particular, we present evidence of the significance of employer labour-market concentration, in terms of its magnitude and wage effects. First, we find that at least 9% of workers can be regarded to be exposed to concentration levels assumed to raise (product) market power concerns. We also find that this percentage has remained relatively stable over the 34-year period covered but can vary significantly depending on methodological choices (or data availability). Second, we estimate wage-concentration elasticities that are significantly negative, at around -0.015. This estimate implies that workers that would move from the bottom to the top quartiles of the concentration distribution could see their wages fall by about 3%.

All in all, our results suggest that employer market power may be an important but not necessarily major driver of wage inequality. In fact, while we find labour-market concentration to increase wage inequality within firms, the relationship changes when we analyse inequality within local labour-markets. Also, the smaller elasticities that we document here, when compared to the case of the U.S., may follow from the different labour market institutions in the two countries. In particular, sectoral collective bargaining in Portugal sets common industry wage floors across multiple regions and jobs, regardless of the specific conditions of the local labour market, including its concentration level. These practices are common in many other European countries but largely not applicable in the U.S. On this point, we find suggestive evidence that collective bargaining counteracts the negative effect of employer concentration.

From a methodological perspective, our findings highlight the different sensitivity of concentration measures with respect to different choices or data constraints, including the sector under analysis, the focus on employment flows or stocks, and the level of aggregation of occupations and geographical areas. For instance, we find that concentration measures can more than double depending on the approach adopted. Given the relative novelty of measuring concentration in local labour markets, these estimates may also inform policy makers that wish to establish critical thresholds that may be more appropriate to address monopsony.

Our results also underline the importance of taking into account both the worker and the firm heterogeneity dimensions when estimating wage-concentration elasticities. Several high-concentration labour markets are characterised by larger and more productive firms, which tend to pay higher wages and employ more skilled workers. As we show in our results, these factors can potentially bias towards zero or even positive relationships any existing negative effects of concentration on wages.

Finally, from a policy perspective, our results underline the relevance of labour markets for competition agencies - and that of competition for labour institutions. While possibly not immediately clear, higher labour market concentration can have a detrimental effect on wages, as employers may take advantage of their market power. Even in a generally highly regulated labour market as Portugal, wages are depressed by diminished competition. This implies that, for instance, firm mergers may have

competition implications that extend beyond product markets. This is particularly important since we find that concentration and its interaction with firm size lead to higher wage markdowns. Moreover, employers' associations may also deserve greater attention from both competition and labour authorities. Employers' associations may promote collusion across firms operating in the same industry and be an important driver of the negative wage effects documented here.

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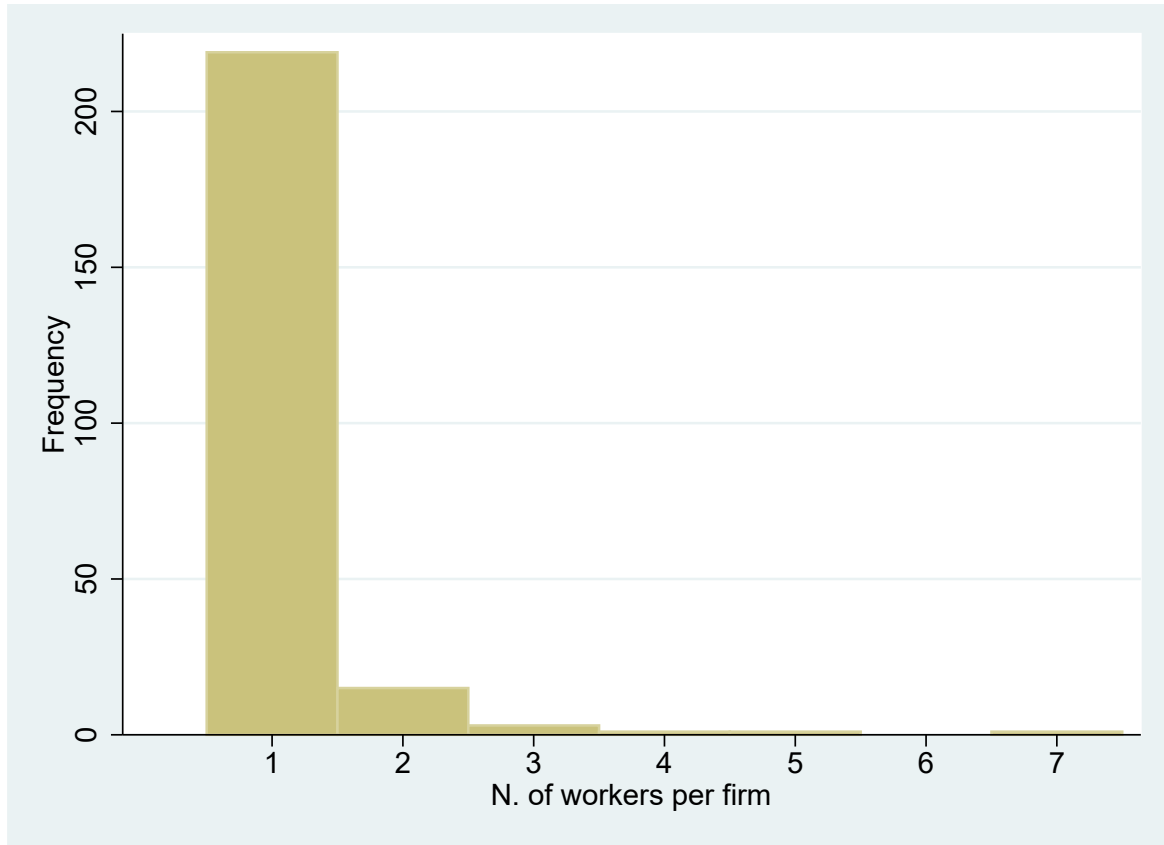
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## A Appendix: Additional figures and tables

Figure A1: Case study: Distribution of workers per firm, Construction/public works technician, Leiria district, 2006



**Notes:** Own calculations based on the 'Quadros de Pessoal' data set. Analysis of a particular occupation (Construction and public works technician, code 31120) in a particular district (Leiria) in a particular year (2006). Each observation in the histogram corresponds to one of 240 firms located in the district that employ at least one worker with this occupation in that year. The Herfindhal index (which ranges between 0 and 10,000) for this local labour market in this year is 52.7.



Table A1: Wage results, IV, 1991-2019 – excluding islands

	(1)	(2)	(3)	(4)	(5)	(6)
	Spec 1	Spec 2	Spec 3	Spec 4	Spec 5	Spec 6
Log Herfindhal Index (Stocks)	0.077 (0.006)***	-0.008 (0.003)**	-0.013 (0.002)***			
Log Herfindhal Index (Flows)		0.076 (0.006)***	-0.008 (0.004)**			-0.014 (0.002)***
Constant						
<i>N</i>	63798977	62878795	61985410	61067505	61839086	60920361
adj. <i>R</i> <sup>2</sup>	0.500	0.496	0.746	0.745	0.785	0.784
F	175	150.1	5.768	5.859	37.25	36.1
Kleibergen-Paap rk Wald F-stat	434.2	463.5	584.9	605.8	766.2	776.9
Shea's R2	.1313	.07969	.1214	.07038	.1066	.0561
(District x Year FEs)	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). '(District x Year) FEs' denote fixed effect for each district-year pair. '(Occupation x District) FEs' denote fixed effect for each district-occupation pair. '1' denotes that the set of fixed effects in the row is included in the specification. The instrument is the average of the log of the inverse of the number of firms in the same occupation and year but other districts. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.

Table A2: Wage results, IV, 1986-2019 – excluding islands

	(1)	(2)	(3)	(4)	(5)	(6)
	Spec 1	Spec 2	Spec 3	Spec 4	Spec 5	Spec 6
Log Herfindhal Index (Stocks)	0.075 (0.006)***		-0.007 (0.003)**		-0.014 (0.002)***	
Log Herfindhal Index (Flows)		0.074 (0.006)***		-0.007 (0.004)**		-0.014 (0.002)***
Constant						
<i>N</i>	66652340	65671964	64706813	63728092	64561344	63581908
adj. <i>R</i> <sup>2</sup>	0.587	0.583	0.785	0.784	0.819	0.818
F	168	143.7	4.204	4.387	40.08	37.86
Kleibergen-Paap rk Wald F-stat	442.7	458.5	581.8	589.4	757.2	738.7
Shea's R2	.1251	.0751	.1171	.06731	.1031	.05372
(District x Year FEs)	1	1	1	1	1	1
(Occupation x District) FEs	1	1	1	1	1	1
Firm FEs	0	0	0	0	1	1
Worker FEs	0	0	1	1	1	1

Standard errors in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Notes:** All regressions were estimated by two-stage least-squares using the estimator proposed by Correia (2017). The columns in the top panel present different specifications of wage equation 2. The dependent variable is the log of individual total wage. The Herfindhal index corresponds to the benchmark model presented above – see Table 2 – based on either employment stocks (odd columns) or flows (even columns). '(District x Year) FEs' denote fixed effect for each district-year pair. '(Occupation x District) FEs' denote fixed effect for each district-occupation pair. '1' denotes that the set of fixed effects in the row is included in the specification. The instrument is the average of the log of the inverse of the number of firms in the same occupation and year but other districts. Standard errors clustering at the local labour market level. Significance levels: \* 0.10, \*\* 0.05, \*\*\* 0.01.



## Chapter III:

# Labour Market Concentration, Wages and Job Security in Europe\*

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This Chapter has been co-written with Andrea Bassanini, Giulia Bovini, Eve Caroli, Jorge Casanova Ferrando, Federico Cingano, Paolo Falco, Florentino Felgueroso, Marcel Jansen, Pedro S. Martins, Michael Ober ichtner, and Martin Popp

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## Introduction

Recent years have seen a renewed interest in the issue of monopsonistic competition in the labour market, both among academics (see e.g. Manning, 2003, 2021, and the special issues of the *Journal of Labor Economics*, 2010, and the *Journal of Human Resources*, 2022)<sup>1</sup> and among policy-makers.<sup>2</sup> An important source of monopsony power is labour market concentration since, when there are fewer employers in a market, it is more difficult for workers to find suitable outside options (Jarosch et al., 2019).

A recent literature has estimated the impact of local labour market concentration on wages, both in the United States (e.g. Arnold, 2021; Schubert et al., 2021; Azar et al., 2022; Rinz, 2022; Benmelech et al., 2022) and in other countries (Martins, 2018; Dodini et al., 2020, 2022; Marinescu et al., 2021; Bassanini et al., 2021; OECD, 2021; Popp, 2021). However, due to heterogeneity in the definition of local labour markets and in the resulting measures of concentration, and to differences in specifications, the estimated elasticities are hardly comparable across studies.

Moreover, this strand of research only considers the impact of labour market concentration on wages.<sup>3</sup> However, there is broad evidence in the literature that workers also value non-wage job attributes and that they may be willing to trade off wages for other dimensions of job quality (Mas and Pallais, 2017; Taber and Vejlín, 2020; Kesternich et al., 2021). If offering high-quality jobs is costly, employers enjoying monopsony power are likely to offer poorer non-wage attributes – see e.g. Manning (2003). Hence, considering only the wage effects of labour market concentration is likely to underestimate its true cost for workers.

This paper addresses these two limitations by providing comparable evidence of the effects of labour market concentration in six European countries and by considering how such concentration affects not only wages, but also one key dimension of job quality, namely job security. To do so, we leverage rich, administrative linked employer-employee data from Denmark, France, Germany, Italy, Portugal and Spain in the 2010s. We build comparable measures of concentration, by computing Herfindahl-Hirschman indexes for new hires in local

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<sup>1</sup> *Journal of Labor Economics*, 28(2), April 2010 and *Journal of Human Resources*, 57(S), April 2022.

<sup>2</sup> See the US Horizontal Merger Guidelines, 2010 (<https://www.justice.gov/atr/horizontal-merger-guidelines-08192010>), and its revised version in preparation (<https://www.justice.gov/opa/press-release/file/1463566/download>), the US Antitrust Guidance for Human Resource Professionals, 2016 (<https://www.justice.gov/atr/file/903511/download>), and M. Vestager's speech on *A new era of cartel enforcement* in October 2021 ([https://ec.europa.eu/commission/commissioners/2019-2024/vestager/announcements/speech-evp-m-vestager-italian-antitrust-association-annual-conference-new-era-cartel-enforcement\\_en](https://ec.europa.eu/commission/commissioners/2019-2024/vestager/announcements/speech-evp-m-vestager-italian-antitrust-association-annual-conference-new-era-cartel-enforcement_en)).

<sup>3</sup> A notable exception is Qiu and Sojourner (2022) – see below.

labour markets, defined as a combination of 4-digit occupations and functional areas. The latter are characterised as the set of all Eurostat functional urban areas (FUAs) – corresponding to a city and its catchment area – and all NUTS-3 regions<sup>4</sup> in which at least 70% of the municipalities are not part of a FUA. As a robustness check, we also use FUAs only and, alternatively, NUTS-3 regions to build two other measures of local labour market concentration.

We have information on wages in Denmark, France, Germany and Portugal. We first investigate the impact of labour market concentration on daily wages of full-time workers. Our specification includes individual fixed effects along with time-varying individual characteristics. Since it is crucial to properly control for product market concentration and establishment-level productivity, we follow Bassanini et al. (2021) and include firm-by-municipality-by-year fixed effects in our regressions. These fixed effects capture productivity at a very disaggregate level, as well as concentration in local product markets, two key confounders of labour market concentration. We can include this very rich set of fixed effects and still identify the coefficient of interest thanks to the fact that, in our data, there are many occupations within each firm-by-municipality in any given year, which in turn leads to variation in the concentration indexes even within firm-by-municipality-by-year cells. To address the endogeneity of concentration, we use the standard quasi-leave-one-out instrument employed, among others, by Azar et al. (2022), Rinz (2022), Qiu and Sojourner (2022), and Marinescu et al. (2021). We discuss the identifying assumptions, highlight their limitations and show that our results are robust to large violations of these assumptions. Despite the heterogeneity of most labour market institutions across the countries we study, the wage elasticities we estimate are strikingly similar, ranging from -0.019 in Germany to -0.022 in France, -0.025 in Portugal and -0.029 in Denmark. They imply that increasing labour market concentration by one standard deviation from the mean reduces daily wages by 3% in Denmark, 2.4% in France, 2.1% in Germany and 2.5% in Portugal. Using hourly wages of either full-timers or all employees, in countries where they are available, yields very similar results. Interestingly, when considering separately new hires and incumbent workers, we find a negative effect of labour market concentration on daily wages of full-timers for both groups. This indicates that reduced outside options not only affect the bargaining power of workers at the time of hiring, but also

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<sup>4</sup> The NUTS classification (Nomenclature of Territorial Units for Statistics) is a hierarchical system for dividing up the economic territory of the EU and the UK for the purpose of collection, development and harmonisation of European regional statistics. NUTS-3 is the most disaggregate level of this classification.

that of incumbents (or their representatives, e.g. trade unions) when negotiating pay raises and/or promotions.

As a second step, we consider the impact of labour market concentration on job security, as proxied by employment contract type (permanent vs. temporary).<sup>5</sup> Information on the type of contract at the time of hiring is available – or can be reconstructed – in all our countries except Denmark. We therefore estimate the effect of concentration in a local labour market on the probability of being hired on a permanent rather than a temporary contract. In Italy and Spain, we also know whether individuals hired on a temporary contract in a given year have been converted to a permanent contract by the end of the following calendar year. This allows us to estimate how labour market concentration affects conversions from temporary to permanent contracts. We find that higher labour market concentration reduces the probability of being hired on a permanent contract in France, Germany and Portugal. The corresponding elasticities are as large as -0.046, -0.051 and -0.234,<sup>6</sup> respectively, i.e. more than twice as large as those estimated for wages. This implies that increasing the HHI by one standard deviation from the mean reduces the probability of being hired on a permanent contract by 5% in France, 6% in Germany and 24% in Portugal. We do not find the same effect in Italy and Spain where the impact of labour market concentration on the type of contract of new hires is not significant at conventional levels. However, in both countries, labour market concentration strongly affects the probability of being converted from a temporary to a permanent contract by the end of the first year following that of hiring: the elasticity of conversions with respect to labour market concentration is as large as -0.241 in Italy and -0.068 in Spain. Increasing labour market concentration by one standard deviation from the mean therefore reduces the probability of conversion by 28% in Italy and 8% in Spain. Overall, this suggests that when firms have some monopsony power, the cost for workers materialises in various dimensions of job quality: not only in the form of lower wages, but also in terms of poorer job security.

We conjecture that the greater effect of concentration on contract type as compared to wages, and the similarity of wage elasticities across countries, could be explained by the fact that the countries we study have high coverage of collective bargaining which takes place predominantly at the industry level. As a consequence, wages are largely set by industry-level collective agreements, which likely leaves little room for single employers to adjust pay

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<sup>5</sup> This measure of job security is consistent with evidence provided by OECD (2014) suggesting that, in European countries, people employed on temporary contracts perceive a much higher risk of losing their job within the next six months than people employed on permanent contracts.

<sup>6</sup> Elasticities are computed at sample average, unless otherwise indicated.



downward even when they gain market power. By contrast, the type of contract on which employees are hired is largely unregulated by collective agreements. This should make it easier for firms to use contract type, rather than wages, as a margin of adjustment when concentration increases. We provide evidence supporting this conjecture using data for Germany and Portugal, the two countries for which we have information on collective bargaining. We show that the effect of concentration on wages is indeed significantly smaller in industries where coverage of sector-level collective bargaining is larger, while sector-level collective bargaining does not affect the relationship between concentration and contract type.

Our paper contributes to the literature on labour market concentration and wages by providing estimates for four European countries. To the best of our knowledge, this paper is the first providing comparable estimates across countries.<sup>7</sup> The existing literature has found different wage elasticities across EU countries, ranging from -0.014 in Portugal to -0.048 in France – see Martins (2018), Marinescu et al. (2021), Bassanini et al. (2021) and Popp (2021). However, it is hard to disentangle whether these differences are due to truly different effects of concentration on wages across countries or to differences in specifications and variable definitions. The fact that the wage elasticities estimated for the United States also vary substantially from one paper to another<sup>8</sup> suggests that differences in methods play an important role. As a matter of fact, we show that, when using the same methodology for Denmark, France, Germany and Portugal, we find very similar wage elasticities, which are all quite small.

We also contribute to the surprisingly small literature focusing on the impact of labour market concentration on non-wage job attributes. To our knowledge, the only two papers doing so are Qiu and Sojourner (2022) and Meiselbach et al. (2022), who find a negative effect of concentration on employer-provided health insurance in the United States. We consider another dimension of job quality, namely job security, a very important non-wage job attribute in dual labour markets where permanent contracts providing a high degree of employment protection coexist with temporary ones. To our knowledge, we are the first to show that higher labour market concentration reduces the probability of being hired on a permanent contract or converted to such a contract during the first year of employment. This finding suggests that

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<sup>7</sup> OECD (2021) estimates wage elasticities for six countries. However, as acknowledged in the study itself, the confidence intervals are so large that they do not allow country-by-country comparisons. Country-specific estimates are therefore only used to derive an average cross-country elasticity.

<sup>8</sup> When using micro data – as we do – the wage elasticities estimated in the USA vary between -0.05 and -0.22 – see Benmelech et al. (2022), Qiu and Sojourner (2022) and Arnold (2021).

considering only the effect of labour market concentration on wages underestimates its overall impact on job quality and hence the resulting welfare loss for workers.

Finally, our paper speaks to the literature investigating the role of the countervailing power of organised labour in shaping the effect of employers' market power on labour market outcomes. Early work shows that bilateral monopolies in the labour market can yield efficient bargaining outcomes since monopolistic unions offset the market power of monopsonists – see MaCurdy and Pencavel (1986) and Espinosa and Rhee (1989). More recently, Marinescu et al. (2021), Benmelech et al. (2022) and Dodini et al. (2022) provide evidence that in industries where the unionisation rate is greater, the wage elasticity to labour market concentration is smaller. We complement this literature by showing that, in countries with prevalence of sectoral collective bargaining – such as many European countries –, wage elasticities to labour market concentration are significantly smaller where coverage by sectoral agreements is larger. This might explain why previous research has tended to find smaller elasticities in many European countries than in the United States, where collective bargaining coverage is low and sectoral bargaining does not exist. By contrast, we find that the effect of labour market concentration on the probability of being hired on permanent contract does not vary with sector-level collective bargaining coverage, which is consistent with the fact that the choice of contract type is rarely regulated by sectoral agreements.

The remainder of the paper is organised as follows. Section 1 presents the data and some descriptive statistics. Section 2 lays out our empirical specification. Section 3 presents the results and Section 4 concludes.

## **1. The Data**

### *1.1 Data sources*

We use near-universe national administrative data for Denmark, France, Germany, Italy, Portugal and Spain – see Appendix F for a detailed description of each dataset. We drop agriculture and industries where the public administration is dominant.<sup>9</sup> We exclude self-employed and household employees and only keep workers with at least 1 month of tenure with their current employer.

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<sup>9</sup> In practice, we keep NACE Rev.2 industries ranging from 05 to 82 as well as 90 and 92 to 96.

The structure of the data varies across countries. In Germany and France, we observe all job matches (i.e. an employee matched with an employer) during the year, but not contract changes within a job match.<sup>10</sup> In Denmark and Portugal, we only observe job matches at one month of the year, namely October for Portugal and November for Denmark. In Italy and Spain, our observations are contracts rather than job matches. In Italy, we have information on the start and end dates of each contract during the year, while in Spain we only have information on the start date. Sample periods are also slightly different across countries, although they all span the 2010s: 2010-2018 for Denmark, 2009-2017 for France, 2012-2018 for Germany and Italy, 2010-2019 for Portugal and 2010-2017 for Spain.

### *1.2 Labour Market Concentration*

A local labour market  $l = (o, z)$  is defined as the intersection between an occupation  $o$  and a geographical area  $z$ . Denoting with  $E$  the total number of employers hiring in each market, we measure concentration by a Herfindahl-Hirschman index (HHI):

$$HHI_{l,t} = \sum_{e=1}^E s_{e,l,t}^2$$

where  $s_{e,l,t}$  is the share of employer  $e$  in the total number of hirings ( $H$ ) in local labour market  $l$  at time  $t$ :<sup>11</sup>

$$s_{e,l,t} = \frac{H_{e,l,t}}{\sum_{m=1}^E H_{m,l,t}}$$

In Italy and Germany, all establishments of a given firm located in a given municipality are reported in the data as a single establishment. Moreover, in Germany, the data do not allow to identify firms, but only establishments, i.e. firm-by-municipality couples. In order to harmonise our units of observation across countries, we define an employer as being composed of all the establishments belonging to a given firm and located in a given municipality. Using this firm-by-municipality concept, by definition we only have single-establishment employers in Italy and Germany, while in other countries employers may be composed of several establishments,

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<sup>10</sup> In particular, we do not observe conversions from temporary to permanent contracts.

<sup>11</sup> We use an HHI based on hirings rather than on employment. Jarosh et al. (2021) show that in a stationary search and matching model with granular search where concentration affects wages by changing workers' outside options, HHIs based on either employment or hirings can be used interchangeably to obtain a measure of labour market concentration that is relevant for wage determination. However, an HHI based on hirings is more adapted to our context since, if the environment is not stationary, firms may have a positive share in employment while not contributing to create outside options for workers (see Marinescu et al., 2021).

all located in the same municipality. We show in Appendix Figure B.1 that, in countries where we can also use a more standard definition of employers based on firm rather than firm-by-municipality, the resulting HHIs are strongly correlated with one another.<sup>12</sup> Moreover, in each country, regressing one HHI on the other yields point estimates close to 1, which suggests that they can be used interchangeably and that the estimation error we make by using a firm-by-municipality concept is small.<sup>13</sup> As an additional proof of this point, in countries where this is feasible, we run a robustness check using the firm – rather than firm-by-municipality – concept to construct the HHI. In the remainder of the paper,  $f$  will index a firm-by-municipality couple.<sup>14</sup>

New hires are defined as individuals who are in a firm-by-municipality couple at time  $t$  and were not there at  $t - 1$ . Since the precise definition varies slightly across countries,<sup>15</sup> we show in Appendix Figure B.2 that, in countries where more than one definition of new hires can be used, the resulting HHIs are, here again, strongly correlated with one another<sup>16</sup> and that

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<sup>12</sup> When an employer is defined as the firm instead of the firm-by-municipality couple, the hiring shares are defined as the ratio of the number of new hires of the firm in the local labour market to the total number of new hires in that market. However, all transfers are excluded when computing the number of new hires, including those coming from other establishments of the same firm located in other geographical areas. Correlation coefficients between  $\log(HHI)$  based on firm-by-municipality and  $\log(HHI)$  based on firm identifiers are 0.87 for Denmark, 0.95 for France, 0.98 for Italy, 0.96 for Portugal and 0.95 for Spain.

<sup>13</sup> The point estimates of the regressions of  $\log(HHI)$  based on firm-by-municipality on  $\log(HHI)$  based on firm identifiers are 0.880 for Denmark, 1.003 for France, 1.017 for Italy, 1.008 for Portugal and 0.981 for Spain.

<sup>14</sup> Municipalities are of different sizes across countries. France has the smallest ones (with an average population of 1,751 individuals per municipality) while Denmark and Portugal have the largest ones (59,402 and 32,968 individuals per municipality, respectively). One could therefore worry that HHIs based on firm-by-municipality identifiers will not be comparable across countries. In French data, we also know in which *cantons* and in which *intercommunalités* firms are located. *Cantons* and *intercommunalités* are geographical units larger than French municipalities but of comparable size to Portuguese and Danish municipalities (with an average population of 33,579 individuals in *cantons* and 53,497 individuals in *intercommunalités*). We show in Appendix Figure A.1 that HHIs based on firm-by-municipality and on firm-by-*canton* or firm-by-*intercommunalité* identifiers are approximatively superposed. Moreover, we run robustness checks using firm-by-*canton* and firm-by-*intercommunalité* (rather than firm-by-municipality) identifiers and show that our results are unchanged.

<sup>15</sup> In France, Germany and Italy a new hire is defined as an individual who is employed in a firm-by-municipality couple (with at least 1 month of tenure) at year  $t$  and was not employed in the same firm-by-municipality couple at  $t - 1$ . In Denmark, new hires are defined as employees who are employed in a firm-by-municipality in November of year  $t$  (with at least 1 month of tenure by the end of the month) and were hired between December of year  $t - 1$  and October of year  $t$ . In Portugal, we only know the month in which individuals were hired (but not the day) and we do not know whether they worked during the whole month of October. As a consequence we define new hires as employees who are employed in a firm-by-municipality in October of year  $t$  and were hired between November of year  $t - 1$  and August of year  $t$  since we want them to have at least one month of tenure. In Spain, new hires are employees who started a contract (whose expected duration was at least 1 month) at year  $t$  and did not start a contract in the previous 12 months with the same employer.

<sup>16</sup> In France, Germany and Italy, in the main specification new hires are defined using a year concept, i.e. as workers who are employed for at least 1 month at the firm-by-municipality couple in year  $t$  and were not employed there in year  $t - 1$ . As a robustness check, in these countries we can alternatively define new hires using a month-year concept, i.e. as workers who are employed at the firm-by-municipality couple in month  $m$  of year  $t$  (with tenure being at least 1 month) and were not employed by the same employer in the same month of year  $t - 1$ . The reference month  $m$  chosen for this exercise is December in Germany and November in France and Italy. Correlation coefficients between  $\log(HHI)$  computed for new hires defined on the basis of a month-year concept

regressing one on the other yields point estimates close to 1.<sup>17</sup>

We build three alternative HHIs, based on three types of geographical areas  $z$ . The first one is the Functional Urban Areas (FUAs) defined by Eurostat. A FUA consists of a city and its catchment area, i.e. a commuting zone whose labour market is highly integrated with the city (OECD, 2012; Dijkstra et al., 2019). FUAs are constructed using the same algorithm in all countries and hence provide a harmonised definition of cities and their areas of influence in international perspective.<sup>18</sup> The second type of geographical areas we consider are NUTS-3 regions.<sup>19</sup> Their main advantage over FUAs is that they cover the entire territory of each country and not only urban areas. In contrast, an important drawback of this concept is that it does not take into account the fact that catchment areas of cities often go beyond the administrative borders of NUTS-3 regions. To overcome this limitation but still include rural areas in our analysis, we consider a third type of geographical area, based on a mix of FUAs and rural NUTS-3 – to which we refer as Functional Areas (FAs), hereafter. More specifically, we consider the set of all FUAs and all NUTS-3 regions (excluding the municipalities that are part of a FUA) in which at least 70% of the municipalities are not part of a FUA. Using these three types of geographical areas  $z$ , we build three different HHIs for each local labour market  $l = (o, z)$  where occupations  $o$  are 4-digit categories.<sup>20</sup> Our preferred results are obtained using measures of labour market concentration based on FAs, but we also run robustness tests using measures based alternatively on FUAs and NUTS-3 regions.

Table 1 reports the distribution of country-specific HHIs based on FAs and weighted by the number of new hires in each local labour market. The level and distribution of labour market concentration appear to be rather similar across the six countries we consider despite their different industrial structures and labour market institutions. More than 75% of the new hires are employed in local labour markets with an HHI below 0.15, the threshold for moderate concentration defined by the US Antitrust Authorities. Moreover, the 90<sup>th</sup> percentile of the HHI

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and  $\log(HHI)$  computed for new hires defined on the basis of a year concept are 0.99 for France and Germany and 0.97 for Italy.

<sup>17</sup> The point estimates obtained when regressing  $\log(HHI)$  computed for new hires defined on the basis of a month-year concept on  $\log(HHI)$  computed for new hires defined on the basis of a year concept are 1.005 for France, 0.992 for Germany and 0.945 for Italy.

<sup>18</sup> FUAs have been used by Ascheri et al. (2021) to characterise labour market concentration in European urban areas.

<sup>19</sup> In Portugal, we use districts (*distritos*) instead of NUTS-3 regions since the latter are smaller than in other countries while the former are of comparable size. In Germany, a few NUTS-3 regions are composed of single urban municipalities which are enclaves of other NUTS-3 regions. When this is the case, we merge the enclave with the surrounding region to increase cross-country comparability.

<sup>20</sup> The choice of relying on very granular occupations is in line with that prevailing in the literature (see e.g. Azar et al., 2022, among others).

distribution is higher than 0.25 – the threshold for high concentration – in only one country, i.e. Portugal. These measures of concentration could be affected by our definition of employers which relies on a firm-by-municipality concept. As mentioned above, in all countries except Germany, we can recompute the HHI distribution based on the classic firm concept which aggregates all establishments of the same firm, whatever the municipality they are located in. When doing so, labour market concentration does not increase substantially: it is still the case that in all countries less than 25% of the new hires are employed in a local labour market with an HHI higher than 0.15, and that the 90<sup>th</sup> percentile of the HHI distribution is below 0.25 everywhere except in Portugal.<sup>21</sup>

**Table 1 – Labour Market Concentration**

HHI	Mean	Std Deviation	P25	P50	P75	P90
Denmark	.0587	.1093	.0096	.0216	.0544	.1437
France	.0657	.1297	.0055	.0194	.0604	.1710
Germany	.0591	.1213	.0057	.0171	.0511	.1508
Italy	.0635	.1370	.0046	.0144	.0528	.1642
Portugal	.0957	.1686	.0108	.0311	.0921	.2654
Spain	.0693	.1477	.0051	.0167	.0591	.1776

Note: Herfindahl-Hirschman index based on hiring, computed using firm-by-municipality identifiers. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). FAs are defined as FUAs and NUTS-3 regions (excluding the municipalities that are part of a FUA) for which at least 70% of the municipalities are not part of a FUA. Districts instead of NUTS-3 regions are used for Portugal. Statistics are weighted by the number of new hires. Labour market concentration is computed over the following time periods: 2011-2018 for Denmark, 2010-2017 for France, 2013-2018 for Germany and Italy, 2011-2019 for Portugal and 2011-2017 for Spain.

### *1.3 Dependent and control variables*

We have information on wages for Denmark, France, Germany and Portugal. Monthly wages are available for Denmark and Portugal, while annual wages are available for France and Germany. In Denmark and France, we have information on the number of days of employment with each employer and the number of hours worked. German data only report the number of days of employment and Portuguese data only have the number of hours worked. So, we construct monthly wages for Portugal, daily wages for Denmark, France and Germany, and

<sup>21</sup> In this case, the 75<sup>th</sup> (resp. 90<sup>th</sup>) percentiles of the HHI distribution are 0.0718 (resp. 0.1667) in Denmark, 0.0750 (resp. 0.2088) in France, 0.0597 (resp. 0.1822) in Italy, 0.1024 (resp. 0.2802) in Portugal and 0.0795 (resp. 0.2222) in Spain.

hourly wages for Denmark, France and Portugal.<sup>22</sup> To ensure that our results on monthly and daily wages are not affected by the incidence of short part-time employment, we restrict our sample to full-time workers. For the sake of comparability, we also do so when using hourly wages, although in this case we also run additional estimates on the whole population of full- and part-timers. Whatever our measure of wages (either monthly, daily or hourly) we trim the top and bottom 1% of the distribution.

Information on the type of contract upon hiring (permanent vs temporary) is available in all countries except Denmark and Portugal. In Portugal, however, we observe the type of contract of workers employed in October of each year and we know the month in which they were hired. We therefore approximate the contract type at the time of hiring by the contract type observed in October of each year, for the subsample of employees hired in June, July and August of that year. These indeed have at least 1 month of tenure – see above – and have been hired sufficiently recently to have a high probability of being still employed on the type of contract on which they were hired.<sup>23</sup> For France, Germany, Italy, Spain and Portugal, we then define a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract.<sup>24</sup> In Italy and Spain, the nature of the data also allows to identify conversions from temporary to permanent contracts. We define a dummy variable for conversion that is equal to 1 if the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year, and 0 otherwise.

Our data also contain information on individuals' age, gender and education – grouped in four categories: less than upper secondary education, upper secondary, more than upper secondary education and a category for missing values since in several countries the information on education is missing for a substantial number of employees.<sup>25</sup> We also know whether

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<sup>22</sup> Since daily wages are defined as wages per day of employment and since we only consider individuals with at least one month of job tenure, monthly wages are equivalent to daily wages multiplied by a constant.

<sup>23</sup> See Cahuc et al. (2022) for a recent analysis of temporary contracts in Portugal using the same dataset.

<sup>24</sup> We do so rather than the opposite – i.e. define a dummy variable equal to 1 if the individual is hired on a temporary contract and 0 otherwise – since permanent contracts are very similar in all countries – they are essentially open-ended contracts – while there exists a large variety of temporary contracts, both within and across countries.

<sup>25</sup> The information on education is missing for 21% of the observations in Italy and 35% in Germany in the original data. In Germany, IAB provides an imputed education variable following a procedure described in Thomsen et al. (2018), which reduces the proportion of missing values to less than 5% of all observations. The proportion of missing values is particularly large in France (58%) since the administrative data on which we run our estimates (DADS) do not contain information on education. To retrieve it, we match these data with a reduced random sample of individuals (*Echantillon Démographique Permanent*). The resulting missing variables are therefore independent from any individual characteristic.

individuals work part time or full time and in which industry they are employed.<sup>26</sup> For new hires, we also know whether they were in employment the year before.

In Portugal, we have information on the type of collective agreements each employer is subject to. This information is not available for other countries. However, for Germany we retrieve information on collective bargaining from the IAB Establishment Panel and match it with our data at the industry-level.<sup>27</sup> These data are averaged over time for each industry to improve representativeness. For comparability, information on collective bargaining coverage is aggregated at the industry level and over time also in the case of Portugal. In each country, we define four alternative indicators of collective bargaining coverage. First, in each industry, we compute the proportion of employees covered by any type of collective agreement, i.e. either at the sector or at the firm level. Second, we define a dummy variable equal to 1 if an industry has a proportion of employees covered by any type of collective agreement larger than the median and 0 otherwise. Finally, we define two similar indicators based on the proportion of employees covered by sectoral collective agreements only.

Although our measures of labour market concentration are constructed using the entire population, the regressions are conducted on random subsamples of the population in three out of six countries.<sup>28</sup> Descriptive statistics of our data are provided in Appendix Table C.1. Unsurprisingly, (monthly, daily and hourly) wages of full-timers are lower for new hires than in the whole working population, in all countries. Women represent about 40% of new hires and this share is quite homogeneous across countries – 40% in Denmark, 42% in France, 44% in Germany, 38% in Italy, 45% in Portugal, and 41% in Spain. Workers in our full samples are about 40 years old on average. New hires are somewhat younger with a mean age ranging from 33 in Denmark and France, to 34 in Spain and 36 in Germany, Italy and Portugal. Full-timers represent a vast majority of the workforce, ranging from 93% in Portugal to 79% in Denmark, 72% in France and 64% in Germany. The distribution of educational levels (computed excluding missing values) varies substantially across countries. In Germany, 70% of the workers have upper secondary education while only 16% have attended higher education. In

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<sup>26</sup> We do not consider full-time vs part-time work as a dependent variable in our analysis since we have no information as to whether working full time or part time is voluntary or not on the part of employees. As a consequence, the results would be hard to interpret.

<sup>27</sup> The IAB Establishment Panel is an annual survey of about 15,500 establishments. The industry partition for which the IAB Establishment Panel is representative contains 19 industries – see Ellguth et al. (2014). We use this level of aggregation to match collective bargaining data with our main German dataset.

<sup>28</sup> The full population is used in Denmark, Italy and Portugal. To save computational time, random subsamples of the population are used in Germany (10% of the population) and Spain (15%). In France, panel data are available only for a random subsample covering one twelfth of the population (see Appendix F.2).



contrast, in France 45% of the working population has not attained upper secondary education while 33% has some higher education. Denmark lies in between with only 23% of the employees without upper secondary education and 28% with some higher education. In Portugal, the educational level of the workforce is overall lower with 53% of the workers having less than upper secondary education and 28% having no more than high-school education. The proportion of new hires on permanent contracts also varies a lot across countries from 67% in Germany to 44% in France, 22% in Portugal, 32% in Italy and 16% in Spain. The dual nature of the Spanish labour market is confirmed by the small rate of conversion from temporary to permanent contracts: only 5.7% in the course of the first year following hiring, as compared to 16% in Italy. Finally, the vast majority of employees are covered by sector-level collective agreements in our data – 51% in Germany and 74% in Portugal – while firm-level agreements only cover 10% of employees in Germany and 7% in Portugal – see Appendix Table C.2.

## 2. Empirical specification

### 2.1 Baseline model

We first estimate the impact of labour market concentration on wages using the following specification:

$$\log(w_{i,j,f,l,s,t}) = \beta \log(HHI_{l,t}) + \gamma X_{i,j,f,l,s,t} + \mu_i + \mu_{ft} + \mu_l + \varepsilon_{i,j,f,l,s,t} \quad (1)$$

where  $i$  indexes the worker,  $j$  the establishment,  $f$  the firm-by-municipality couple,  $l$  the local labour market,  $s$  the industry and  $t$  is the year.  $w$  alternatively denotes the daily or hourly wage.  $HHI$  is the concentration index computed using firm-by-municipality identifiers.  $X$  is a vector of individual controls including yearly dummies for the worker's age, whether or not the individual is a new hire in the firm and if so, whether or not he/she was in employment the year before.  $X$  also includes establishment and/or industry fixed effects when different from firm-by-municipality fixed effects, as well as a dummy variable for working part time vs full time, whenever our regression sample is not restricted to full-timers. Our specification also includes individual and local-labour-market fixed effects ( $\mu_i$  and  $\mu_l$ , respectively). Finally firm-by-municipality-by-year fixed effects ( $\mu_{ft}$ ) allow to control for firm productivity at a very disaggregate level.<sup>29</sup> They also control for both national and local product market competition.

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<sup>29</sup> As, in the countries of our sample, establishments are not accounting centres, no linked employer-employee dataset could be used to measure productivity at a more disaggregate level.

The latter is a likely confounder of labour market concentration if firms sell their products mainly on the local market rather than on national or international markets.

With this large set of fixed effects, the model is identified only if there exist several occupations in each firm-by-municipality couple. This is the case in our data since the average number of occupations per firm-by-municipality (weighted by the number of observations) ranges from 2.9 in Spain to 17.9 in Denmark.<sup>30</sup> Identification then stems from three different sources of variations. The main one lies in the different changes over time in the HHIs of the various occupations within each firm-by-municipality. Moreover, two other sources of variation are provided by individuals changing occupation within a firm-by-municipality or changing firm-by-municipality. This indeed modifies the average HHI in the firm-by-municipality since it is computed as the weighted average of the various occupational HHIs where the weights are given by the number of workers employed in each occupation in this firm-by-municipality. When this number changes, the average HHI in the firm-by-municipality changes too. Since we control for a firm-by-municipality-by-time fixed effect, at any point in time, the HHI attached to an individual is, de facto, defined in deviation from the firm-by-municipality mean which varies because of individuals changing occupation or firm-by-municipality.

We also estimate the impact of labour market concentration on job security. To do so, we restrict our sample to new hires and estimate the following specification:

$$S_{i,j,f,l,s,t} = \beta \log(HHI_{l,t}) + \gamma X_{i,j,f,l,s,t} + \mu_{ft} + \mu_l + \varepsilon_{i,j,f,l,s,t} \quad (2)$$

where  $S$  alternatively denotes a dummy variable equal to 1 when the individual is hired on a permanent contract – 0 otherwise –, or a dummy variable equal to one when the individual was hired on a temporary contract and is converted to a permanent one by the end of the following year – 0 otherwise. In the latter case, the regression sample is further restricted to those initially hired on a temporary contract. We do not include individual fixed effects in equation (2) to avoid confining the analysis to individuals who changed job several times over the period under study. Individual heterogeneity is accounted for by augmenting the vector of individual controls  $X$  with invariant or quasi-invariant characteristics (gender and education), in addition to the individual's age, a dummy variable for being part-time and another one for whether he/she was

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<sup>30</sup> The average number of occupations per firm-by-municipality couple is 6.4 in Italy, 7.5 in France, 9.1 in Germany and 11.2 in Portugal.

in employment the year before. Standard errors are clustered at the local-labour-market-by-year level for all estimates.

## 2.2 IV strategy and violation of exogeneity of the instrument

In this set-up, identification may be under threat if an omitted variable varies across local labour markets and over time, and is correlated with both the HHI and our dependent variables. This may occur if positive or negative shocks to local labour supply or demand affect both the wage (and/or the contract type) workers are willing to accept, and the number of firms that find it attractive to operate in the local labour market. For instance, if a school specialised in training students in skills that are particularly useful in certain occupations opens in a given geographical area. This will increase the supply of labour with those specific skills and likely reduce the corresponding wage and non-wage job attributes that firms are willing to offer. At the same time, it may increase the number of firms that find it attractive to operate in this area. If this reduces labor market concentration, it will generate a positive spurious correlation between concentration on the one hand and wages and contract type on the other hand.

As standard in the literature – see e.g. Marinescu et al. (2021), Azar et al. (2022), and Rinz (2022) – we tackle this issue by using a quasi-leave-one-out instrumental variable strategy. We instrument  $\log(HHI)$  in local labor market  $l = (o, z)$  at time  $t$  with the average of  $\log(1/N_{o,z',t})$ , where  $N_{o,z',t}$  is the number of firms with a positive number of hires in all other geographical areas  $z'$  for the same occupation  $o$  and time period  $t$ .<sup>31</sup> This instrument aims at capturing changes in labour market concentration taking place at the national level as a result, for example, of mergers or divestitures of large national companies. When such firms merge or split, this modifies the number of companies operating in all local labour markets where they are present without being correlated with idiosyncratic shocks in these markets. As a consequence, it generates an exogenous shock that allows to identify the causal impact of labour market concentration on wages and/or contract type.<sup>32</sup>

One worry with this instrument is that its variations could also capture national trends in occupational labour supply and demand, which could also affect our dependent variables. To

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<sup>31</sup> Instrumenting a variable in one zone using the average of this variable in other zones (i.e. a Hausman or leave-one-out instrument) is standard in international economics and industrial organisation – see e.g. Hausman et al. (1994), Autor et al. (2013), Bai et al. (2017) and Azar et al. (2019).

<sup>32</sup> Alternatively, one could consider using the leave-one-out HHI as an instrument. However, we want to capture changes in HHI that are due to mergers or divestitures, and not to changes in efficiency. Now, when a large firm becomes more efficient (for example because it discovers a new product), its share in the local labour market will increase and so will the HHI even if competition does not decrease. By contrast  $1/N$  will not change since it is

alleviate this concern, we proceed in the following way. First, we augment equations (1) and (2) by including the share of each 4-digit occupation in new hires at the national level to capture occupation-specific national trends in labour supply and demand. One caveat is that the potential endogeneity of this variable could bias the estimates of the effect of  $\log(HHI_{l,t})$ , even in the IV specification. However, if our instrument substantially captured national occupational trends in labour supply and demand, we would expect that the introduction of the share of each 4-digit occupation in new hires should significantly modify our estimates. We will show that this is not the case in our data so that, if any, the violations of exogeneity of the instrument are likely to be small.

As a second step, we quantify the exogeneity violation that our models may tolerate using the plausibly exogenous instrument regression method proposed by Conley et al. (2012). We consider the following model, in which the instrument ( $Z$ ) is not fully exogenous and therefore may have a direct effect on the dependent variable:

$$Y_{i,j,f,l,s,t} = \beta \log(HHI_{l,t}) + \gamma Z_{l,t} + \delta C_{i,j,f,l,s,t} + \varepsilon_{i,j,f,l,s,t} \quad (3)$$

where  $Y$  is the outcome variable and  $C$  is the vector of controls including fixed effects.

If the true value of  $\gamma$  – denoted by  $\gamma^*$  – were known,  $Z$  would be a valid instrument in the following equation (i.e. it would no longer be correlated with the error term):

$$Y_{i,j,f,l,s,t} - \gamma^* Z_{l,t} = \beta \log(HHI_{l,t}) + \delta C_{i,j,f,l,s,t} + \varepsilon_{i,j,f,l,s,t} \quad (4)$$

In practice, the direct effect of  $Z$  is unknown, but we can still determine how large it should be to make the coefficient of interest,  $\beta$ , insignificant. This is done in three steps. First, we show that  $Z$  is positively correlated to  $\log(HHI)$ . As a consequence, if  $\gamma^*$  were positive, incorrectly using  $Z$  as an instrument for  $\log(HHI)$  would generate a bias towards positive values when estimating  $\beta$  by 2SLS in equation (1). The risk of overestimating the magnitude of the (negative) wage elasticity hence only exists if  $\gamma^*$  is negative. As a consequence, we can take 0 as the upper bound of the support of  $\gamma$  - which corresponds to the situation in which  $Z$  is exogenous. We then consider decreasing potential values of  $\gamma^*$  one-by-one, take them as given and estimate equation (4) by 2SLS, instrumenting  $\log(HHI)$  with  $Z$  and storing confidence intervals for  $\hat{\beta}$  at every step. This allows us to determine the lowest value of  $\gamma^*$  that would still

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influenced only by the number of firms on the market. This suggests that the leave-one-out  $1/N$  is a better instrument than the leave-one-out HHI. This is why we use the former rather than the latter.

make  $\hat{\beta}$  significant at the 10% level. Finally, we estimate the following reduced form equation by OLS:

$$Y_{i,j,f,l,s,t} = \alpha Z_{l,t} + \delta C_{i,j,f,l,s,t} + u_{i,j,f,l,s,t} \quad (5)$$

where  $u$  is a standard disturbance term.  $\alpha$  captures the overall effect of  $Z$  on the dependent variable  $Y$ , i.e. both the direct effect – independent of  $\log(HHI)$  – and the indirect effect going through  $\log(HHI)$ . We can then express the lowest value of  $\gamma^*$  that would still make  $\hat{\beta}$  significant at the 10% level in percentage of the reduced-form effect  $\hat{\alpha}$ , estimated in equation (5). This ratio provides an order of magnitude of the violations of instrument exogeneity that our model may tolerate, i.e. that still yield a point estimate  $\hat{\beta}$  significant at the 10% level.

### 3. Results

#### 3.1 Labour market concentration and wages

We first estimate the impact of labour market concentration on daily wages of full-timers in Denmark, France, Germany and Portugal. IV estimates are presented in Table 2 while OLS estimates are provided in Appendix Table C.3.<sup>33</sup> As shown by the values of the F-statistics and first stage estimates – see Appendix Table C.4 –, the instrument is strongly positively correlated with labour market concentration in all countries. The F-statistic is lower in Portugal and Denmark than in Germany and France<sup>34</sup> but remains higher than standard critical thresholds in all countries. The point estimates of the coefficient on labour market concentration are strikingly similar across the four countries, ranging from -0.019 in Germany to -0.022 in France, -0.025 in Portugal and -0.029 in Denmark, all significant at the 1% level. Since we found similar HHI distributions across countries – see Table 1 –, this implies that the effective range of wage variation induced by labour market concentration is also very similar. In fact, increasing labour market concentration by one standard deviation from the mean reduces daily wages by 3% in Denmark, 2.4% in France, 2.1% in Germany and 2.5% in Portugal.

This similarity is all the more striking that these countries have different labour market institutions, for example as regards employment protection legislation, the minimum wage and

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<sup>33</sup> OLS point estimates are negative, but much smaller than those obtained when instrumenting labour market concentration. This suggests that they are possibly upward biased.

<sup>34</sup> This is likely due to the smaller country size of Denmark and Portugal which implies a smaller number of functional areas in these countries.

the importance of active and passive labour market policies.<sup>35</sup> However, Denmark, Germany, France and Portugal have in common strong collective bargaining at the industry level – see OECD (2019) – with large coverage among employees.<sup>36</sup> As a consequence, wages are largely set by collective agreements signed at the industry level, which leaves little room for single firms to adjust pay downward even when they gain market power. This may explain not only why the wage elasticity with respect to labour market concentration is similar across the four countries we consider, but also why it is small when compared to what has been found in the United States<sup>37</sup> where sectoral collective bargaining does not exist and firm-level bargaining covers only 12% of employees.

**Table 2 – Labour Market Concentration and Daily Wages of Full-Timers – IV estimates.**

Dep. Var Daily Wages	Denmark	France	Germany	Portugal
Log HHI	-.029*** (.007)	-.022*** (.002)	-.019*** (.002)	-.025*** (.009)
KP F Test Observations	32.1 5,486,000	708.9 8,269,375	271.4 11,050,435	14.6 15,086,998

Note: 2SLS estimates. The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

In the case of Germany and Portugal, we have access to industry-level data on collective bargaining that we can leverage to further explore the role of sector-level collective bargaining. We re-estimate equation (1) adding an interaction term between  $\log(HHI)$  and each of our

<sup>35</sup> In 2015, the OECD index of employment protection legislation on regular contracts was 1.53 in Denmark, 2.50 in France, 2.60 in Germany and 3.14 in Portugal on a scale ranging from 0 to 6. The Kaitz index (ratio of the minimum to the median wage) was 0.62 in France, 0.48 in Germany and 0.55 in Portugal while there was no national minimum wage in Denmark. The share of spending on active labour market programmes in GDP amounted to 1.65% in Denmark, 0.68% in France, 0.48% in Portugal and 0.28% in Germany. The share of passive labour market policy spending (including unemployment benefits) was 1.27% of GDP in Denmark, 2.05% in France, 0.88% in Germany and 1.36% in Portugal. Source: <https://www.oecd.org/employment/emp/employmentdatabase-labourmarketpoliciesandinstitutions.htm>.

<sup>36</sup> In 2015, collective agreements covered 83% of the wage and salary employees in Denmark, 98% in France, 57% in Germany, and 74% in Portugal, while the OECD average was 33% – Source: OECD/AIAS Collective Bargaining Database (<https://www.oecd.org/employment/ictwss-database.htm>).

<sup>37</sup> See footnote 9.

indicators of collective bargaining coverage, alternatively.<sup>38</sup> The results regarding coverage by any type of collective agreement – either firm or sector level – are presented in columns (1) and (2) of Table 3. As evidenced in Panel A, in Germany, the point estimate on the interaction between  $\log(HHI)$  and coverage by any type of collective agreement is positive and significant at conventional levels, whatever the indicator we use. It is also positive in Portugal, although not significant when using the dummy variable for high coverage. Consistent with the literature,<sup>39</sup> this suggests that the negative effect of labour market concentration on wages decreases as the proportion of employees covered by any type of collective agreement increases. Interestingly, results in columns (3) and (4) of Table 3 show that in both countries this dampening effect is driven by sector-level, rather than firm-level collective bargaining. The point estimates on the interaction terms between  $\log(HHI)$  and sector-level coverage indeed tend to be larger than those on the interactions between concentration and coverage by any type of agreement.<sup>40</sup> As a matter of fact, when including both firm and sector-level collective bargaining separately – which stretches our data to its limits – the only point estimates that turn out positive are those on the interaction terms between  $\log(HHI)$  and sectoral collective bargaining coverage.<sup>41</sup> The key role of sector-level collective bargaining in countries where it is dominant – like Germany and Portugal, but also Denmark and France – may help explain why we find similar, but also rather low, wage elasticities in the four countries. To the extent that wages are one of the main job attributes negotiated at the industry level and subsequently set by sector-level collective agreements, it is unlikely that individual firms be able to exert a strong downward pressure on wages even when concentration increases in their local labour market.

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<sup>38</sup> In Germany, we also control for the share of employees covered by a works council in each industry interacted with  $\log(HHI)$ . We do so because works councils cover a large proportion of employees in this country – 39% in 2017 (see Ellguth and Kohaut, 2018) – and decide, among other things, how bonuses are attributed to workers – see Fulton (2021). In contrast, this is not the case in Portugal. According to the Ministry of Labour – see Ministério do Trabalho (2016) – there were only 191 works councils in the entire country in 2015 and they were essentially responsible for health and safety matters.

<sup>39</sup> See Marinescu et al. (2021), Benmelech et al. (2022) and Dodini et al. (2022).

<sup>40</sup> The differences in point estimates on the interaction terms in columns (1) and (3) – resp. (2) and (4) – may not seem very large. However, in interpreting these differences, one has to take into account the fact that few workers are covered by firm-level collective agreements in both countries (see Appendix Table C.2), which implies that the indicators for sector-level coverage and coverage by any type of agreement are not much different one from the other.

<sup>41</sup> The point estimate on  $\log(HHI)$  interacted with the share of employees covered by a sector-level collective agreement is 0.0077 in Germany (resp. 0.0074 in Portugal) with standard error 0.0037 (resp. 0.0024 in Portugal). The point estimate on  $\log(HHI)$  interacted with the dummy variable indicating whether the share of employees covered by sector-level collective agreements is higher than the median is 0.0024 in Germany (resp. 0.0011 in Portugal) with standard error 0.0010 (resp. 0.0009 in Portugal).

**Table 3 – Labour Market Concentration and Daily Wages of Full-Timers according to Collective Bargaining Coverage – IV estimates**

Dep. Var Daily Wages	(1)	(2)	(3)	(4)
Panel A. Germany				
Log HHI	-.0224*** (.0029)	-.0164*** (.0026)	-.0229*** (.0029)	-.0161*** (.0026)
LogHHI*Coverage share, Any CBA	.0112*** (.0036)			
LogHHI*High coverage dummy, Any CBA		.0037*** (.0011)		
LogHHI* Coverage share, Sector CBA			.0137*** (.0033)	
LogHHI* High cover. dummy, Sector CBA				.0044*** (.0011)
KP F Test	90.5	90.5	90.5	90.5
Observations	11,050,435	11,050,435	11,050,435	11,050,435
Panel B. Portugal				
Log HHI	-.0310*** (.0094)	-.0257*** (.0090)	-.0308*** (.0092)	-.0263*** (.0090)
LogHHI*Coverage share, Any CBA	.0070*** (.0024)			
LogHHI*High coverage dummy, Any CBA		.0009 (.0008)		
LogHHI* Coverage share, Sector CBA			.0083*** (.0016)	
LogHHI* High cover. dummy, Sector CBA				.0020** (.0008)
KP F Test	7.29	7.31	7.43	7.33
Observations	15,086,998	15,086,998	15,086,998	15,086,998

Note: 2SLS estimates. The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). "Coverage share, Any (resp. Sector) CBA" is the proportion of employees covered by any (resp. a sector-level) collective agreement in the industry where the individual is employed. "High coverage dummy, Any (resp. Sector) CBA" is a dummy variable equal to 1 if the individual is employed in an industry with a proportion of employees covered by any (resp. a sector-level) collective agreement above the median, 0 otherwise. Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. In Germany, control variables also include the share of employees covered by a works council in the industry interacted with  $\log(HHI)$ .  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation.  $\log(HHI)$  interacted with a collective bargaining indicator (resp. the works council variable) is instrumented by the average of the log inverse number of firms in other FAs for the same occupation interacted with the collective bargaining indicator (resp. the works council variable). Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



Our instrument for  $\log(HHI_{l,t})$  being the average of  $\log(1/N_{o,z',t})$  in all other geographical areas  $z'$  for the same occupation  $o$  and time period  $t$ , it provides a source of variation in labour market concentration based on national rather than local changes in the occupation we consider. If changes in an occupation result, for example, from mergers and divestitures of large national companies, they will likely affect concentration in the local labour markets where these companies are present, without being correlated to idiosyncratic shocks in these markets. However, variations in the instrument could, in principle, also capture national trends in supply and demand which could also affect the dependent variables. To alleviate this concern, we first augment equation (1) by including the share of each 4-digit occupation in new hires at the national level to capture occupation-specific national trends in labour supply and demand. If our instrument were in practice capturing these trends, the introduction of this control should substantially modify our estimates. As shown in Appendix Table D.1, this is not the case in France, Germany and Portugal where the wage elasticities are very similar to those presented in Table 1, ranging from -0.021 in France to -0.022 in Portugal and -0.025 in Germany. In Denmark, controlling for 4-digit occupational shares in new hires at the national level lowers the magnitude of the point estimate from -0.029 to -0.013, although the coefficient remains statistically significant. This suggests that, in this country, the instrument may partly capture national trends in labour supply and demand. In contrast, in France, Germany and Portugal, the violation of exogeneity of the instrument, if any, is likely to be small.

As a second step, we quantify the exogeneity violation that our models may tolerate using the method presented in Section 2.2. As evidenced in Appendix Table E.1, our IV estimates are robust to large violations of exogeneity of the instrument:  $\hat{\beta}$  would still be significant at the 10% level if the direct effect of the instrument on wages were as large as 87% of the reduced-form estimate in France, 80% in Germany and 57% in Portugal. We consider such large violations as unlikely given the stability of our estimates when controlling for the 4-digit national occupational shares in new hires. In Denmark, our estimate is robust to violations of exogeneity as large as 74% of the effect obtained with the reduced form, which still allows us to draw conclusions – although cautiously – from our findings.

For the sake of cross-country comparability, we measure labour market concentration using a firm-by-municipality concept, i.e. considering that an employer is composed of all its

establishments located in a given municipality – see Section 1 above.<sup>42</sup> Since this definition is not quite standard in the literature, for countries in which we can identify establishments and aggregate them nationwide into firms (i.e. all but Germany), we re-run our estimates using this more standard definition of employers. The results are presented in Appendix Table C.5. The elasticity of wages with respect to labour market concentration is similar to that estimated in Table 1 for France (-0.024) and Portugal (-0.021). It is even larger for Denmark (-0.051), although less precisely estimated. This suggests that our main results are not driven by the specificity of the employer concept that we use.

**Table 4 – Labour Market Concentration and Hourly Wages – IV estimates.**

Dep. Var Hourly Wages	Denmark	France	Portugal
Panel A. Full-Timers			
Log HHI	-.033*** (.008)	-.016*** (.002)	-.023*** (.008)
KP F Test Observations	29.1 5,989,967	721.3 8,233,169	13.9 14,678,273
Panel B. All employees			
Log HHI	-.026*** (.005)	-.014*** (.002)	-.024*** (.008)
KP F Test Observations	46.7 8,552,216	891.5 11,435,329	15.3 16,227,766

Note: 2SLS estimates. The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. In Panel B, controls also include a dummy variable for whether the individual works full time or not.  $\text{Log}(\text{HHI})$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Our estimates could also be sensitive to the definition of local labour markets based on functional areas. Appendix Tables C.6 and C.7 provide robustness checks using alternatively

<sup>42</sup> We show in Appendix Table A.1 that, for the only country where we can do so, i.e. France, our results are unchanged when measuring labour market concentration using an HHI based on firm-by-*canton* or firm-by-*intercommunalité* (i.e. larger administrative areas) rather than on firm-by-municipality identifiers.

FUAs and NUTS-3 regions to define local labour markets. The results are stable, thus suggesting that the choice of functional areas does not affect our findings in a major way.<sup>43</sup>

For a subset of countries, we can compute hourly wages, in addition to daily wages. Estimating the impact of labour market concentration on the former yields similar results to those obtained with daily wages. When the sample is restricted to full-timers only – see Table 4 - Panel A – point estimates range from -0.016 in France to -0.023 in Portugal and -0.033 in Denmark. Using hourly wages also allows to consider the entire population of salaried workers and not only full-timers, since the results are immune from any bias due to short part-time work. The corresponding results are shown in Panel B of Table 4. The elasticities we find are extremely close to those estimated for full-timers – -0.014 for France, -0.024 for Portugal and -0.026 for Denmark – thereby suggesting that restricting our sample to full-timers when considering daily wages does not substantially affect our results.

The impact of labour market concentration on wages that we have estimated so far is the aggregation of the impact on two different groups of employees: those who have been hired over the past year (the new hires) and those who were already employed in the firm the year before (the incumbents). The literature conjectures that the depressing effect of labour market concentration on wages could be larger for new hires since they are more sensitive to market conditions – Haefke et al. (2013); Kudlyak (2014); Marinescu et al. (2021). To investigate this potential source of heterogeneity in the effect of labour market concentration, we re-run our regressions interacting  $\log(HHI)$  with two dummy variables, for new hires and incumbents, separately. Results are reported in Table 5. In all countries, we do find that labour market concentration negatively affects daily wages of newly hired full-timers. But we also find a negative effect on incumbents' wages with elasticities ranging from -0.020 in Germany to -0.022 in France, -0.025 in Portugal and -0.028 in Denmark – all significant at the 1% level.<sup>44</sup> This finding is consistent with results from Arnold (2021), Thoresson (2021) and Bassanini et al. (2021), which suggest that labour market concentration generates downward pressure on incumbents' earnings, too. The estimates reported in Table 5 suggest that the negative effect of labour market concentration on wages is stronger for new hires than for incumbents in Denmark

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<sup>43</sup> When using *districts* instead of functional areas in Portugal – Appendix Table C.7 –, the first stage becomes relatively weak and the point estimate is lower and noisier. It still remains within one standard deviation off the estimate presented in Table 2 but it is no longer significant at conventional levels.

<sup>44</sup> One could worry that the effect we find on incumbents could actually be due to the downward pressure exerted by labour market concentration on wages at the time of hiring which would persist over time. Yet, using a specification very similar to ours, Bassanini et al. (2021) show that the effect they estimate on French incumbents remains remarkably stable when controlling for a job spell fixed effect, thereby netting out the effect of labour market concentration at the time of hiring.

and, marginally, in France, but not in Germany and Portugal, pointing to the absence of a universally applicable pattern. This finding is also consistent with a recent literature showing that the wages of new hires are actually no more flexible than those of job stayers (see Grigsby et al., 2021). Overall, these findings suggest that labour market concentration not only affects the bargaining power of workers at the time of hiring, but also that of incumbents (or their representatives, e.g. trade unions) when negotiating pay raises and/or promotions.

**Table 5 – Labour Market Concentration and Daily Wages of Full-Timers – New Hires vs Incumbents – IV estimates.**

Dep. Var Daily Wages	Denmark	France	Germany	Portugal
Log HHI*New hire	-.037*** (.007)	-.024*** (.002)	-.016*** (.002)	-.024*** (.009)
Log HHI*Incumbent	-.028*** (.007)	-.022*** (.002)	-.020*** (.002)	-.025*** (.008)
KP F Test	16.1	354.5	135.5	7.3
Observations	5,486,000	8,269,375	11,050,435	15,086,998

Note: 2SLS estimates. The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Interactions between  $\log(HHI)$  and another variable are instrumented by this variable interacted with the instrument for  $\log(HHI)$ . Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

### 3.2 Labour market concentration and job security

As a second step, we investigate the impact of labour market concentration on a second job attribute, namely job security. First, we capture job security by the probability that an individual be hired on a permanent rather than temporary contract. To do so, we restrict our samples to new hires.<sup>45</sup> To avoid identifying the effect only on individuals who changed job several times over our relatively short time period, we do not include individual fixed effects. We control for individual heterogeneity in the best way we can, by adding gender and education to our vector

<sup>45</sup> We do not investigate the effect of labour market concentration on the probability of being *employed* on a permanent contract separately for new hires and incumbents. The reason for not doing so is that, for incumbents, being on a permanent contract is generally an absorbing state, so that our model – see equation (2) – would be misspecified.

of covariates. As shown in Table 6, our IV estimates<sup>46</sup> suggest that a higher level of concentration in the local labour market significantly reduces the probability of new workers to be hired on a permanent rather than a temporary contract, both in France and Germany.<sup>47</sup> When computed at sample average, the corresponding elasticities<sup>48</sup> are large: -0.046 (resp. -0.051) for France (resp. Germany). In Portugal, the effect of labour market concentration is significant only at the 11% level.<sup>49</sup> However, the elasticity of the probability of being hired on a permanent contract is very large: -0.234, at sample average. Taking into account the distributions of HHIs, these estimates imply that increasing the HHI by one standard deviation from the mean reduces the probability of being hired on a permanent contract by 5% in France, 6% in Germany and 24% in Portugal.

**Table 6 – Labour Market Concentration and Probability of being hired on a Permanent Contract – IV estimates – New hires only.**

Dep. Var	France	Germany	Italy	Portugal	Spain
Perm. contract					
Log HHI	-.0206*** (.0046)	-.0344*** (.0119)	-.0037 (.0066)	-.0503+ (.0313)	.0022 (.0020)
KP F Test	592.6	255.4	99.3	13.4	1126.1
Observations	3,530,660	4,167,918	16,645,917	1,039,792	4,875,973

Note: 2SLS estimates. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ , +  $p < 0.11$ .

<sup>46</sup> The corresponding OLS estimates are provided in Appendix Table C.8. The first stage estimates are presented in Appendix Table C.9.

<sup>47</sup> We show in Appendix Table A.2 that, for the only country where we can do so, i.e. France, our results are unchanged when measuring labour market concentration using an HHI based on firm-by-*canton* or firm-by-*intercommunalité* (i.e. larger administrative areas) rather than on firm-by-municipality identifiers.

<sup>48</sup> Elasticities are computed by dividing the point estimate by the average incidence of permanent contracts among new hires – see Appendix Table C.1.

<sup>49</sup> Robustness checks on the instrument are provided in Appendix Tables D.2 and E.2 for the countries where the point estimates on  $\log(HHI)$  are statistically significant in Table 6. When controlling for the share of 4-digit occupations in new hires at the national level, the point estimates are of smaller magnitude in France and Portugal, but they are still within the confidence intervals of the baseline estimates. Moreover, as shown in Appendix Table E.2, in France – and Germany – the results are robust to large violations of exogeneity of the instrument. This test would not be meaningful for Portugal since the baseline results are significant only at the 11% level. Appendix Tables C.10, C.11 and C.12 provide robustness checks conducted using firm (instead of firm-by-municipality) identifiers and FUs and NUTS-3 respectively (instead of functional areas) when computing HHIs. The robustness checks confirm the negative estimates for France and Germany, but yield somewhat noisy results for Portugal.

In contrast, labour market concentration does not seem to affect the probability of being hired on a permanent contract in a noticeable way either in Italy or in Spain. This is likely due to the fact that, in these countries, most workers are hired on temporary contracts even in local labour markets where concentration is low. In fact, in markets which belong to the first decile of the concentration distribution, the proportion of new hires on temporary contracts is already very high in Italy (62.2%) and Spain (80.4%).<sup>50</sup> Therefore, there is not much room for further increasing the probability of being hired on a temporary contract when local labour markets become more concentrated. To the extent that Portugal also has a large proportion of new hires on temporary contracts it is particularly remarkable that we do find a large and borderline significant effect in this country.

Our findings for Italy and Spain do not imply, however, that concentration in the labour market does not affect job security in these countries. In Table 7, we provide IV estimates of the effect of  $\log(HHI)$  on the probability that an employee hired on a temporary contract at year  $t$  be converted to a permanent contract by the end of the following calendar year.<sup>51</sup> Our findings suggest that in local labour markets where concentration is higher, the probability of conversion to a permanent contract is indeed significantly lower with an elasticity of -0.068 in Spain and -0.241 in Italy, when computed at sample average.<sup>52</sup> Increasing labour market concentration by one standard deviation from the mean therefore reduces the probability of conversion by 28% in Italy and 8% in Spain. The estimate for Italy looks large. However, it should be considered with some caution since it is sensitive to the specification we use. When we control for 4-digit occupational shares in new hires at the national level, the point estimate on  $\log(HHI)$  is three times lower – see Appendix Table D.3. This suggests that the instrument could be partly endogenous. Yet, as shown in Appendix Table E.3, our estimate is robust to large violations of exogeneity (66% of the effect estimated in the reduced form), which makes it possible to draw clear, albeit cautious, conclusions from our findings. In Spain, the instrument does not seem to capture national trends in labour supply and demand since the results in Appendix Table D.3 are similar to those presented in Table 7. They are also robust to violations of exogeneity of the instrument as large as 50% of the effect estimated in the reduced form – see Appendix Table E.3. Taken together, the above findings suggest that when most workers are anyway hired on temporary contracts (as in Italy and Spain), the negative effect of labour

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<sup>50</sup> By contrast, this proportion is only 40.8% in France and 30.9% in Germany.

<sup>51</sup> OLS estimates are provided in Appendix Table C.13.

<sup>52</sup> Appendix Tables C.14, C.15 and C.16 provide robustness checks conducted using firm (instead of firm-by-municipality) identifiers and FUAs and NUTS-3 respectively (instead of functional areas) when computing HHI. These robustness checks confirm our results.

market concentration on job security materialises through a lower chance of subsequent conversion to a permanent contract, rather than through more precarious conditions at the time of hiring.

**Table 7 – Labour Market Concentration and Conversions from Temporary to Permanent Contracts – IV estimates – New hires on temporary contracts only.**

Dep. Var Conversion	Italy	Spain
Log HHI	-.0384*** (.0091)	-.0039*** (.0011)
KP F Test	59.1	993.3
Observations	8,927,725	4,089,355

Note: 2SLS estimates. The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year; it is equal to 0 otherwise. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $Log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Overall, our results provide evidence of a negative effect of labour market concentration on both wages and job security in all the countries we study. The magnitude of the effect on job security appears to be larger than the effect on wages in all countries where we can estimate the two of them – i.e. France, Germany and Portugal.<sup>53</sup> A possible explanation for this difference may be that, in contrast to what occurs for wages, the type of contract on which employees are hired is largely unregulated by sectoral collective agreements. This makes it easier for firms to use contract type, rather than wages, as a margin of adjustment when concentration increases. Consistent with this explanation, when we re-estimate our baseline specification including an interaction between concentration and industry-level coverage of sectoral collective bargaining,

<sup>53</sup> This is not due to the fact that, in contrast with equation (1), equation (2) is estimated on the subsample of new hires only and does not include individual fixed effects. When re-estimating equation (1) on new hires only and without individual fixed effects, we find that the magnitude of the negative impact of concentration on wages decreases in all countries.

the point estimate on this interaction turns out statistically insignificant both in Germany and Portugal – see Table 8. This suggests that sectoral collective bargaining plays little role in mitigating the effect of concentration on job security.

**Table 8 – Labour Market Concentration and Probability of being hired on a Permanent Contract according to Collective Bargaining Coverage – IV estimates**

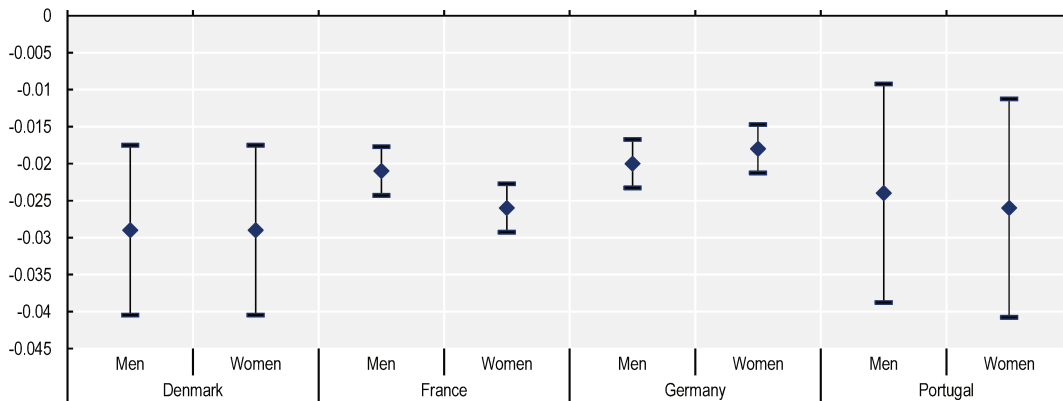
Dep. Var Permanent contract	(1)	(2)
Panel A. Germany		
Log HHI	-.0672*** (.0123)	-.0653*** (.0122)
LogHHI* Coverage share, Sector CBA	.0078 (.0057)	
LogHHI* High coverage dummy, Sector CBA		-.0015 (.0018)
KP F Test	85.1	85.1
Observations	4,167,918	4,167,918
Panel B. Portugal		
Log HHI	-.0514* (.0303)	-.0523* (.0309)
LogHHI* Coverage share, Sector CBA	.0014 (.0061)	
LogHHI* High coverage dummy, Sector CBA		.0030 (.0023)
KP F Test	6.73	6.77
Observations	1,039,792	1,039,792

Note: 2SLS estimates. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). "Coverage share, Sector CBA" is the proportion of employees covered by a sector-level collective agreement in the industry where the individual is employed. "High coverage dummy, Sector CBA" is a dummy variable equal to 1 if the individual is employed in an industry with a proportion of employees covered by a sector-level collective agreement above the median, and 0 otherwise. Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. In Germany, control variables also include the share of employees covered by a works council in the industry interacted with  $\log(HHI)$ .  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation.  $\log(HHI)$  interacted with a collective bargaining indicator (resp. the works council variable) is instrumented by the average of the log inverse number of firms in other FAs for the same occupation interacted with the collective bargaining indicator (resp. the works council variable). Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



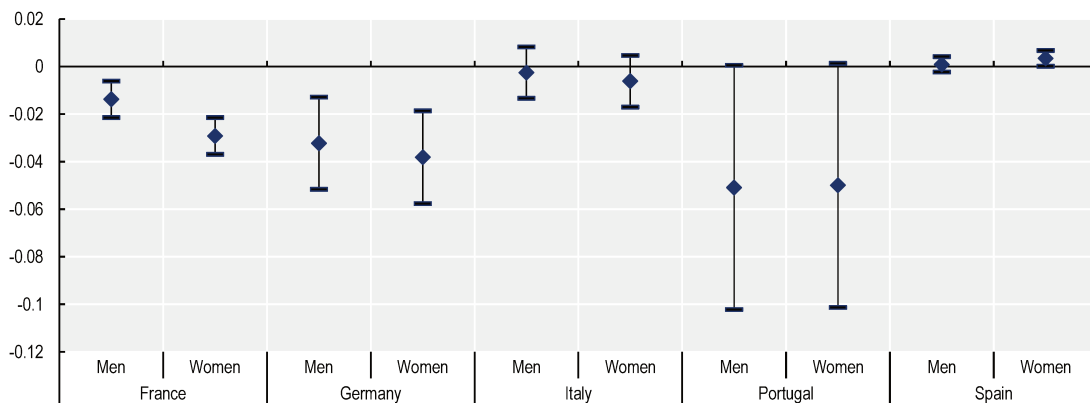
**Figure 1. Effect of labour market concentration by gender**

Panel A: Log of daily wages of full-timers



Note: 2SLS estimates. The figure reports point estimates and 90% confidence intervals of the interactions of  $\log(HHI)$  with gender dummies. The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. The interaction between  $\log(HHI)$  and gender is instrumented by gender interacted with the instrument for  $\log(HHI)$ . Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level.

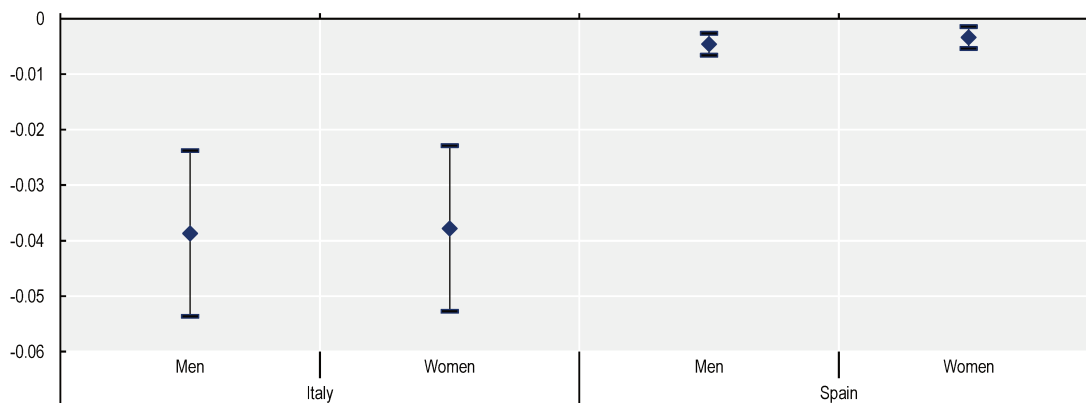
Panel B: Probability of being hired on a permanent contract (new hires only)



Note: 2SLS estimates. The figure reports point estimates and 90% confidence intervals of the interactions of  $\log(HHI)$  with gender dummies. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. The interaction between  $\log(HHI)$  and gender is instrumented by gender interacted with the instrument for  $\log(HHI)$ . Standard errors are clustered at the labour-market-by-year level.

**Figure 1. Effect of labour market concentration by gender (*cont.*)**

Panel C: Probability of being converted to a permanent contract (new hires on temporary contracts only)



Note: 2SLS estimates. The figure reports point estimates and 90% confidence intervals of the interactions of  $\log(HHI)$  with gender dummies. The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. The interaction between  $\log(HHI)$  and gender is instrumented by gender interacted with the instrument for  $\log(HHI)$ . Standard errors are clustered at the labour-market-by-year level.

### 3.3 Heterogeneity by gender and age

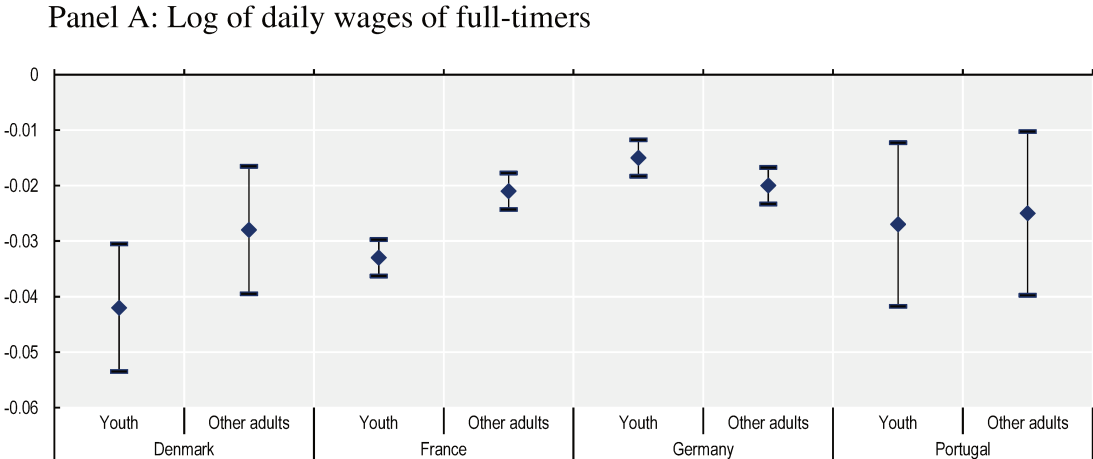
The evidence presented so far suggests a strong effect of labour market concentration on job quality. This is consistent with the idea that employers who have market power take advantage of it to reduce labour demand in order to lower the cost associated with wages and non-wage attributes. Employers depressing their labour demand may also be expected to become more selective in hiring in a context of asymmetric information on the labour market. In particular, they may prefer job candidates with long work experience since their resume provides a more accurate signal of their productivity. If this is the case, we may expect labour market concentration to be particularly harmful to the wages and job security of workers with shorter work experience and, in particular, youth and women.<sup>54</sup>

<sup>54</sup> Labour market concentration could also be particularly harmful to workers with low levels of education since they have been shown to have fewer outside options – Caldwell and Danieli (2022) – and lower bargaining power

To test this hypothesis, we interact  $\log(HHI)$  separately with dummies for being a man or a woman on the one hand, and with dummies for being younger or older than 25 on the other hand, in equations (1) and (2). The results are presented in Figures 1 and 2.

As evidenced on Figure 1 - Panel A, the effect of labour market concentration on wages appears to be very similar for men and women in Denmark. It is slightly larger for women in France and Portugal, but slightly smaller in Germany, and in both cases confidence intervals at the 90% level largely overlap. The same holds when considering the probability of being hired on a permanent contract: the point estimate on labour market concentration is slightly more negative for women in France, Germany and Italy, but this is not the case in Portugal and Spain, and here again confidence intervals overlap – see Panel B of Figure 1. Finally, in the case of conversions too, no systematic gender difference is uncovered by our analysis – see Figure 1 - Panel C.

**Figure 2. Effect of labour market concentration by age group**

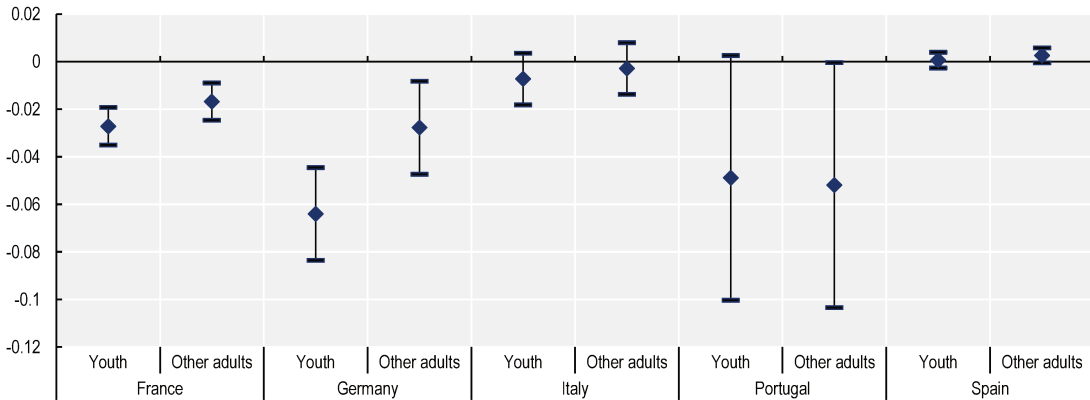


Note: 2SLS estimates. The figure reports point estimates and 90% confidence intervals of the interactions of  $\log(HHI)$  with dummy variables for being younger (resp. older) than 25. The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. The interaction between  $\log(HHI)$  and age is instrumented by age interacted with the instrument for  $\log(HHI)$ . Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level.

– Cahuc et al. (2006). Unfortunately, we cannot test this assumption with our data since the information on education is missing for a substantial number of observations in Denmark, France and Italy and since the education variable is imputed for a large proportion of our sample in Germany – see footnote 26. This allows us to use education as a control in our regressions – adding a category for "missing" –, but the scarce quality of the variable prevents us from conducting a meaningful heterogeneity analysis based on the level of education.

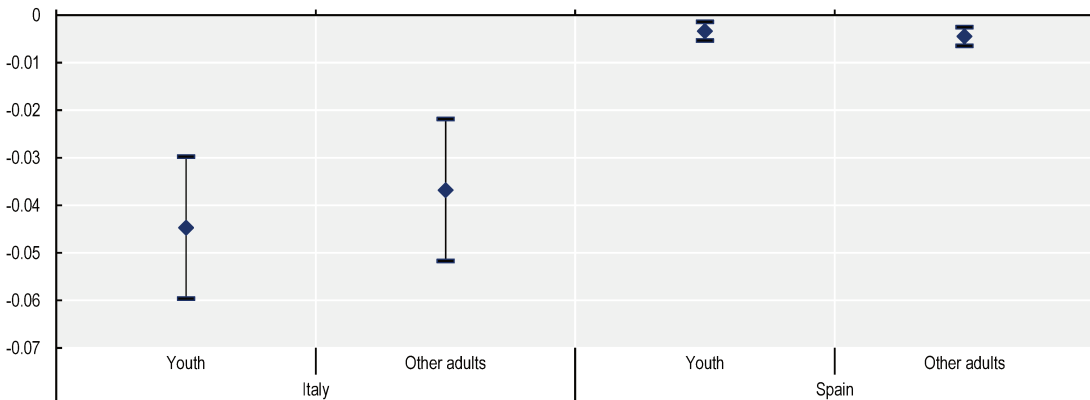
**Figure 2. Effect of labour market concentration by age group (cont.)**

**Panel B: Probability of being hired on a permanent contract (new hires only)**



Note: 2SLS estimates. The figure reports point estimates and 90% confidence intervals of the interactions of  $\log(HHI)$  with dummy variables for being younger (resp. older) than 25. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. The interaction between  $\log(HHI)$  and age is instrumented by age interacted with the instrument for  $\log(HHI)$ . Standard errors are clustered at the labour-market-by-year level.

**Panel C: Probability of being converted to a permanent contract (new hires on temporary contracts only)**



Note: 2SLS estimates. The figure reports point estimates and 90% confidence intervals of the interactions of  $\log(HHI)$  with dummy variables for being younger (resp. older) than 25. The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. The interaction between  $\log(HHI)$  and age is instrumented by age interacted with the instrument for  $\log(HHI)$ . Standard errors are clustered at the labour-market-by-year level.

Regarding youth, results are very similar. Panel A of Figure 2 points to a more negative effect of labour market concentration on youth's wages in Denmark and France and, to a lower extent in Portugal, but the opposite holds in Germany, and confidence intervals largely overlap in most countries. The same goes for the probability of being hired on a permanent contract – see Panel B: the point estimates on labour market concentration tend to be more negative for youth in France, Germany, Italy and Spain, but this is not the case in Portugal and confidence intervals overlap in all countries. For conversions, differences in the effect by age are small and go in opposite directions for Spain and Italy – see Panel C.

Overall, our findings suggest that there is no systematic difference in the effect of labour market concentration on wages and job security across gender or age. However, this does not mean that women and youth are exposed to the same degree of monopsony power as men and older adults. It has been shown that women tend to search for jobs closer to their home and are ready to accept a significant wage penalty for a closer job (Le Barbanchon et al., 2020; Jacob et al., 2019). For the same reason, they may also be willing to accept lower job security. Youth may also search closer to their home if they live with their parents and have not obtained yet their driving license. Thus, a given level of concentration will imply fewer acceptable outside options for women and young workers, and therefore lower wages and possibly lower job security.<sup>55</sup> Despite this difference, an increase in labour market concentration by, say, 10% may still have a similar percentage effect on the rarefaction of available alternatives for both men and women on the one hand, and older workers and youth on the other hand, consistent with the pattern of results shown in Figures 1 and 2.

#### **4. Conclusion**

This paper contributes to the debate on the effects of labour market concentration on job quality. We leverage rich administrative linked employer-employee data from Denmark, France, Germany, Italy, Portugal and Spain in the 2010s to provide the first comparable cross-country evidence in the literature. First, we show that the distribution of labour market concentration is similar across these six countries. Second, controlling for productivity and local product market concentration, we show that, despite different labour market institutions, the elasticities of wages with respect to labour market concentration are very similar across countries, ranging

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<sup>55</sup> In fact, the literature on own-firm-labour-supply elasticity systematically finds lower elasticities for women than for men, thereby suggesting that the former are more exposed to monopsony power than the latter – see Manning (2021).

from -0.019 in Germany to -0.029 in Denmark. We then consider a second dimension of job quality, i.e. job security. Our results suggest that higher labour market concentration reduces the probability of being hired on a permanent contract in France, Germany and Portugal, with elasticities as large as -0.046, -0.051 and -0.234, respectively. In Italy and Spain, where most workers are anyway predominantly hired on temporary contracts, we detect no significant effect of labour market concentration on such probability. However, we find that higher concentration significantly reduces the probability of being converted to a permanent contract once hired on a temporary one.

These results suggest that firm monopsony power not only negatively affects wages but also degrades other non-wage attributes and, in particular, job security. Policy interventions limiting employer concentration and/or its effects are therefore likely to improve labour market outcomes along such dimensions for incumbent workers and new hires. Potential interventions may include enforcement actions by antitrust authorities, such as taking systematically into account labour market outcomes in merger reviews and cracking down on labour market collusion, including on no-poaching and wage-fixing agreements – see e.g. Hovenkamp and Marinescu (2019). On the one hand, evidence suggests that mergers increasing concentration do not need to create dominant employers to have a strong negative effect in the labour market – see e.g. Arnold (2021); Prager and Schmitt (2021). On the other hand, collusion is more likely to occur in concentrated markets, since coordination among fewer actors is typically easier to sustain – see e.g. Asker and Nocke (2021). More generally, the promotion of entrepreneurship and firm creation, leading to the emergence of new employers, will also reduce the labour market power of incumbent firms.

Other interventions to counteract the effect of labour market concentration are rather in the realm of labour policy. These notably include direct interventions to facilitate collective bargaining.<sup>56</sup> Labour unions and collective bargaining have been shown to help counterbalance the effect of firm market power in case of monopsonistic competition. In line with this literature, we indeed find small and strikingly similar wage elasticities in Denmark, France, Germany and Portugal and show that the effect of labour market concentration on contract type is much larger than on wages. We provide suggestive evidence that this pattern of results could be explained by the fact that the countries we study have high coverage of collective bargaining which takes place predominantly at the industry level. Since wages are largely set up by sectoral collective

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<sup>56</sup> Raising the minimum wage and improving geographical mobility and training are other labour policies that have been shown to reduce the effect of monopsony power – see e.g. Manning (2003) and OECD (2022).

agreements whereas contract types are not, firms likely have more margin of manoeuvre to reduce job security rather than adjust wages downward when labour market concentration increases. This suggests that enlarging the scope of collective bargaining to all non-wage attributes, including contract types, may help limit the effect of monopsony power on workers' welfare.

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## Appendices

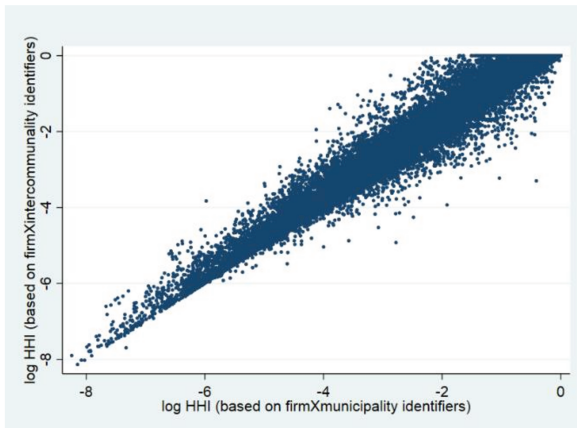
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### Appendix A – HHI based on firm-by-municipality vs HHI based on firm-by-*intercommunalité* and firm-by-*canton* identifiers in France.

*Cantons* and *intercommunalités* are geographical units larger than French municipalities but of comparable size to Portuguese and Danish municipalities, respectively.

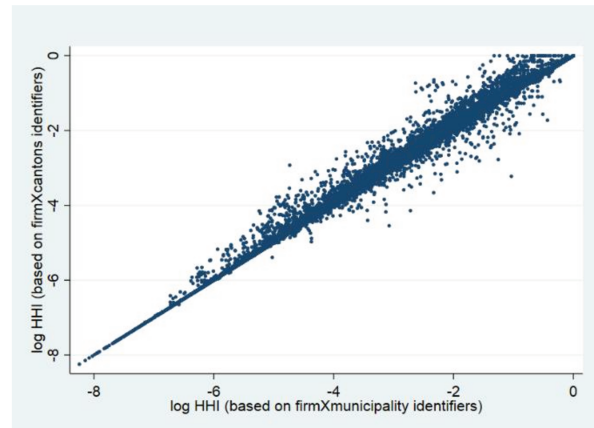
### Appendix Figure A.1. Correlations between $\log(HHI)$ constructed using identifiers based on firm-by-municipality and firm-by-larger geographical area (France only).

Panel A: Firm-by-*Intercommunalité* identifiers



Regression coefficient: 0.9303887; R-squared: 0.9702

Panel B: Firm-by-*Canton* identifiers



Regression coefficient: 0.9970262; R-squared: 0.9970

Note: Local labour markets are defined based on Functional Areas (FAs).

**Table A.1 – Labour Market Concentration and Daily Wages of Full-Timers in France – IV estimates – other identifiers.**

Dep. Var	Firm-by- <i>intercommunalité</i> identifiers	Firm-by- <i>canton</i> identifiers
Daily Wages		
Log HHI	-.0236*** (.0020)	-.0227*** (.0019)
KP F Test	599.8	696.2
Observations	8,269,106	8,269,343

Note: The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Employers are defined as the aggregation of all their establishments in an *intercommunalité* (resp. *canton*). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-*intercommunalité*-by-year (resp. firm-by-*canton*-by-year) fixed effects and establishment fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

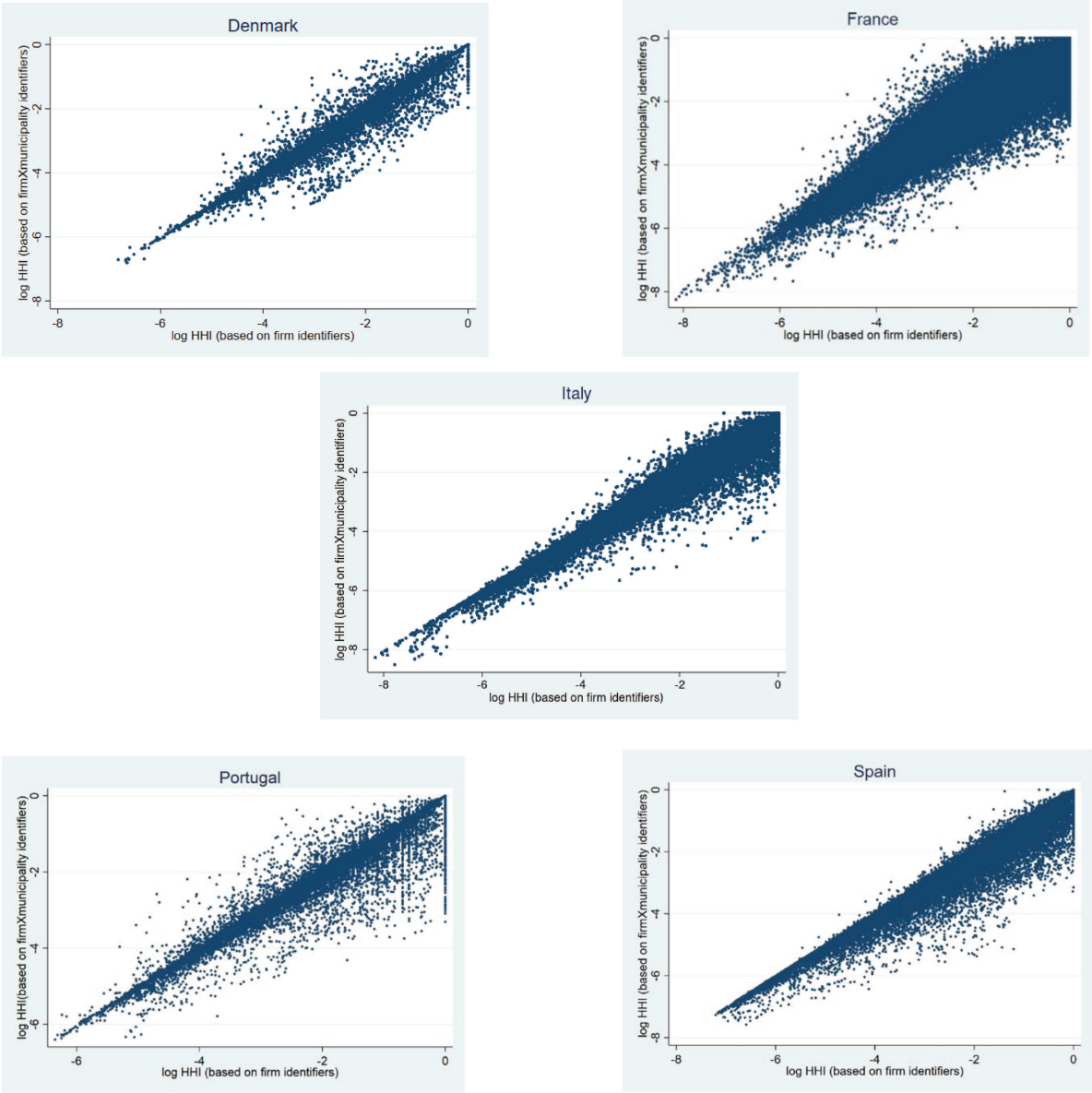
**Table A.2 – Labour Market Concentration and Probability of being hired on a Permanent Contract in France – IV estimates – New hires only – other identifiers.**

Dep. Var Perm contract	Firm-by- <i>intercommunalité</i> identifiers	Firm-by- <i>canton</i> identifiers
Log HHI	-.0199*** (.0048)	-.0204*** (.0047)
KP F Test	637.9	580.7
Observations	3,376,061	3,503,243

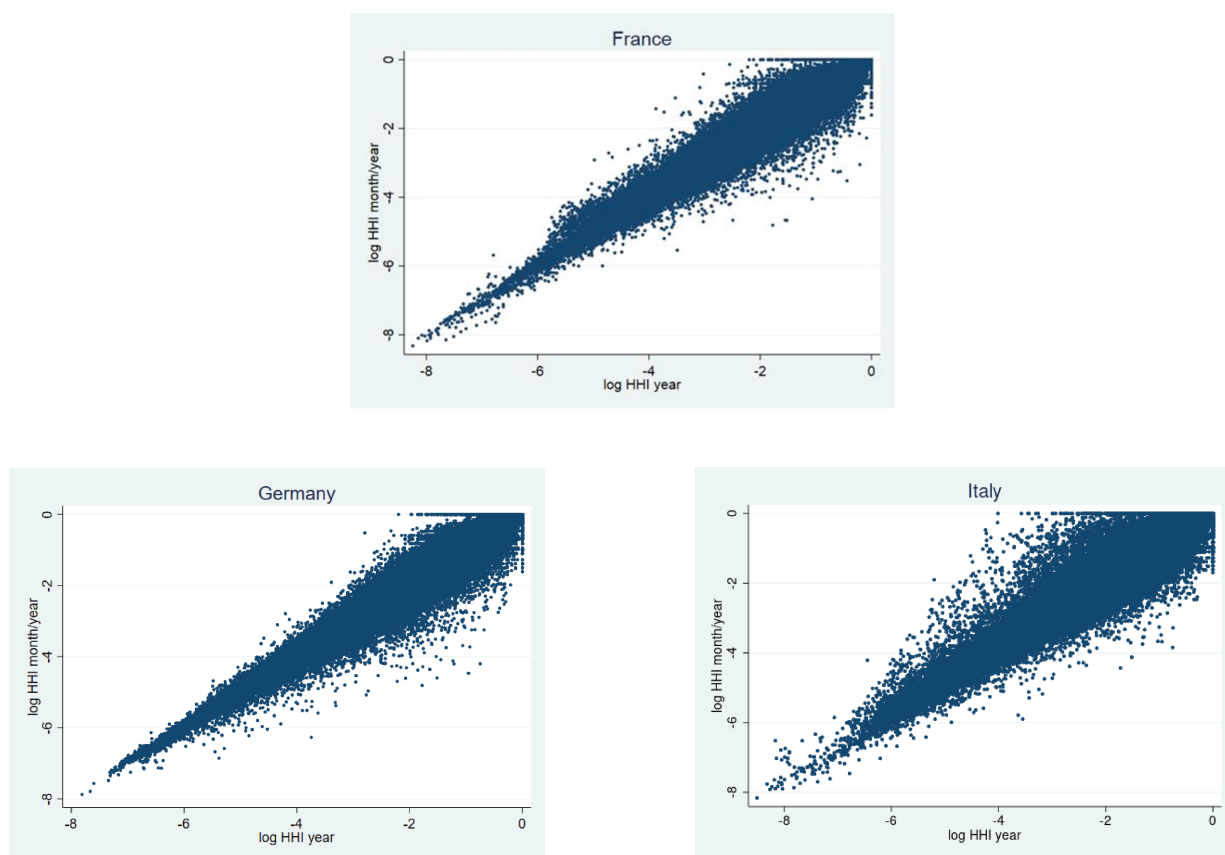
Note: 2SLS estimates. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Employers are defined as the aggregation of all their establishments in an *intercommunalité* (resp. *canton*). Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-*intercommunalité*-by-year (resp. firm-by-*canton*-by-year) fixed effects and establishment fixed effects.  $\text{Log}(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Appendix B – Appendix Figures**

**Appendix Figure B.1 - Correlations between *log (HHI)* constructed using firm-by-municipality and firm identifiers**



**Appendix Figure B.2 – Correlations between  $\log(HHI)$  computed with new hires defined based on a month-year concept and  $\log(HHI)$  computed with new hires defined based on a year concept**



Note: when using a year concept, new hires are workers who are employed for at least 1 month at the firm-by-municipality couple in year  $t$  and were not employed there in year  $t - 1$ . When using a month-year concept, new hires are workers who are employed at the firm-by-municipality couple in month  $m$  of year  $t$  (with tenure being at least 1 month) and were not employed by the same employer in the same month of year  $t - 1$ . The reference month  $m$  chosen for this exercise is December in Germany and November in France and Italy.

**Appendix C – Appendix Tables**

**Table C.1 – Descriptive Statistics**

Variable	Denmark		France		Germany		Italy		Portugal			Spain	
	Full sample	New hires	Full sample	New hires	Full sample	New hires	Full sample	New hires	Full sample	New hires	New hires (June to August)	Full sample	New hires
<b>HHI</b>													
Mean	.0682	.0594	.0691	.0640	.0630	.0599	-	.0635	.0957	.0956	.0950	-	.0543
Standard deviation	.1174	.1086	.1298	.1270	.1236	.1229	-	.1370	.1649	.1685	.1689	-	.1277
<b>Daily wage (full-timers)</b>													
Mean	157.16	139.96	68.51	57.48	116.3	95.17	-	-	-	-	-	-	-
Standard deviation	62.78	58.63	36.58	30.69	60.46	53.63	-	-	-	-	-	-	-
<b>Monthly wage (full-timers)</b>													
Mean	-	-	-	-	-	-	-	-	1059.5	914.7	818.2	-	-
Standard deviation	-	-	-	-	-	-	-	-	680.1	565.3	448.2	-	-
<b>Hourly wage (full-timers)</b>													
Mean	30.35	27.0	13.30	11.32	-	-	-	-	6.44	5.60	5.02	-	-
Standard deviation	12.06	11.28	6.41	5.24	-	-	-	-	4.18	3.46	2.68	-	-
<b>Hourly wage (all)</b>													
Mean	28.58	24.39	12.78	10.97	-	-	-	-	6.20	5.34	4.82	-	-
Standard deviation	12.09	10.89	6.20	4.98	-	-	-	-	4.06	3.29	2.53	-	-
<b>Women</b>													
Mean	.3720	.3988	.4079	.4209	.4162	.4399	-	.3760	.4301	.4532	.4346	-	.4118
Standard deviation	.4833	.4896	.4914	.4937	.4929	.4964	-	.4844	.4951	.4978	.4957	-	.4921
<b>Age</b>													
Mean	39.71	32.96	38.00	32.93	42.64	36.05	-	35.65	40.23	36.33	33.93	-	34.36
Standard deviation	13.46	13.04	12.30	11.70	13.52	13.44	-	11.50	11.05	11.36	11.17	-	10.52
<b>Full-time work</b>													
Mean	.7923	.6464	.7233	.6424	.6354	.5357	-	.5787	.9345	.8807	.8543	-	.5358
Standard deviation	.4056	.4781	.4474	.4793	.4813	.4987	-	.4938	.2475	.3241	.3528	-	.4987

Notes: Wages are in nominal euros.

**Table C.1 – Descriptive Statistics (cont.)**

Variable	Denmark		France		Germany		Italy		Portugal			Spain	
	Full sample	New hires	Full sample	New hires	Full sample	New hires	Full sample	New hires	Full sample	New hires	New hires (June to Aug.)	Full sample	New hires
<b>Education</b>													
<b>Below upper secondary</b>													
Mean	.2262	.2996	.1888	.1778	.1307	.2119	-	.3747	.5322	.4871	.4904	-	.6536
Standard deviation	.4183	.4581	.3913	.3824	.3371	.4086	-	.4841	.4990	.4998	.4999	-	.4758
<b>Upper secondary</b>													
Mean	.4835	.4435	.0934	.0906	.6714	.5495	-	.3314	.2841	.3186	.3400	-	.2543
Standard deviation	.4997	.4968	.2909	.2870	.4697	.4975	-	.4707	.4510	.4659	.4737	-	.4355
<b>Above upper secondary</b>													
Mean	.2818	.2466	.1367	.1128	.1505	.1419	-	.0838	.1816	.1906	.1648	-	.0919
Standard deviation	.4499	.4310	.3435	.3164	.3537	.3490	-	.2771	.3855	.3928	.3710	-	.2889
<b>Missing</b>													
Mean	.0085	.0103	.5812	.6188	.0474	.0967	-	.2101	.0022	.0038	.0048	-	0
Standard deviation	.0917	.1009	.4934	.4860	.2124	.2956	-	.4074	.0463	.0614	.0691	-	0
<b>New hires</b>													
Mean	.2682	-	.3038	-	.2145	-	-	-	.2330	-	-	-	-
Standard deviation	.4430	-	.4599	-	.4105	-	-	-	.4227	-	-	-	-
<b>Employed year before if new hire</b>													
Mean	-	.6789	-	.7273	-	.7171	-	.6405	-	.4035	.3819	-	.2809
Standard deviation	-	.4669	-	.4453	-	.4504	-	.4799	-	.4906	.4859	-	.4494
<b>New hires on perm. contract</b>													
Mean	-	-	-	.4444	-	.6693	-	.3161	-	.4062	.2157	-	.1614
Standard deviation	-	-	-	.4969	-	.4705	-	.4650	-	.4911	.4113	-	.3679
<b>Conversion to permanent contract</b>													
Mean	-	-	-	-	-	-	-	.1596	-	-	-	-	.0570
Standard deviation	-	-	-	-	-	-	-	.3662	-	-	-	-	.2318

Notes: Conversions are computed on the samples of new hires on temporary contracts. For Spain, “Employed year before if new hire” means “Started a contract the year before if new hire”.



**Table C.2 – Proportion of workers covered by collective bargaining, by type of agreement**

	Germany	Portugal
Firm-level collective agreement	10%	7%
Sector-level collective agreement	51%	74%
No agreement	39%	19%

Notes: In Germany the table indicates the proportion of overall employment accounted by establishments subject to a collective agreement. In Portugal, the table indicates the proportion of workers whose wage is regulated by a collective agreement. In this country, agreements between one association of employers and one or more unions are considered sector-level agreements, while agreements between one or more firms and one or more unions are considered firm-level agreements.

**Table C.3 – Labour Market Concentration and Daily Wages of Full-Timers – OLS estimates.**

Dep. Var	Denmark	France	Germany	Portugal
Daily Wages				
Log HHI	-.0020 (.0006)	-.0007** (.0003)	-.0003 (.0002)	-.0007*** (.0002)
Observations	6,740,546	8,269,430	11,050,435	15,087,543

Note: The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table C.4 – Labour Market Concentration and Daily Wages of Full-Timers – First Stage Equation.**

Dep. Var Log (HHI)	Denmark	France	Germany	Portugal
Avg. of log ( $1/N_{o,z,t}$ )	.168*** (.030)	.766*** (.0288)	.726*** (.044)	.163*** (.043)
R <sup>2</sup>	0.947	0.971	0.966	0.955
Observations	4,986,191	8,269,375	11,050,435	15,086,998

Note: The dependent variable is  $Log(HHI)$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and plant fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $N_{o,z,t}$  is the number of firms with positive hirings in all other FAs  $z'$  for the same occupation  $o$  and time period. Standard errors are clustered at labour-market-by-year level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table C.5 – Labour Market Concentration and Daily Wages of Full-Timers – IV estimates – Nationwide firms.**

Dep. Var Daily Wages	Denmark	France	Portugal
Log HHI	-.051** (.020)	-.024*** (.002)	-.021*** (.008)
KP F Test	8.0	558.7	15.6
Observations	5,483,456	8,266,589	15,074,086

Note: 2SLS estimates. The dependent variable is  $\log(\text{wage})$ . Employers are defined as the aggregation of all their establishments nationwide, rather than the aggregation of their establishments at the municipality level, as done in our baseline regressions. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $Log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table C.6 – Labour Market Concentration and Daily Wages of Full-Timers – IV estimates – Local Labour Markets based on FUAs.**

Dep. Var Daily Wages	Denmark	France	Germany	Portugal
Log HHI	-.034*** (.008)	-.022*** (.002)	-.017*** (.003)	-.028*** (.009)
KP F Test	35.5	540.9	214.9	17.5
Observations	3,640,305	6,973,615	8,472,227	9,902,973

Note: 2SLS estimates. The dependent variable is log(wage). Local labour markets are defined based on 4-digit occupations and Functional Urban Areas (FUAs), rather than Functional Areas, as in our baseline regressions. Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. *Log(HHI)* is instrumented by the average of the log inverse number of firms in other FUAs for the same occupation. Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C.7 – Labour Market Concentration and Daily Wages of Full-Timers – IV estimates – Local Labour Markets based on NUTS-3 regions.**

Dep. Var Daily Wages	Denmark	France	Germany	Portugal
Log HHI	-.030*** (.004)	-.023*** (.001)	-.021*** (.002)	-.017 (.011)
KP F Test	86.3	2695.1	547.5	6.5
Observations	7,279,543	9,564,667	11,037,434	17,362,165

Note: 2SLS estimates. The dependent variable is log(wage). Local labour markets are defined based on 4-digit occupations and NUTS-3 regions (districts in Portugal), rather than Functional Areas, as in our baseline regressions. Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. *Log(HHI)* is instrumented by the average of the log inverse number of firms in other NUTS-3 regions for the same occupation. Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C.8 – Labour Market Concentration and Probability of being hired on a Permanent Contract – OLS estimates – New hires only.**

Dep. Var	France	Germany	Italy	Portugal	Spain
Perm contract					
Log HHI	.0012 (.0010)	-.0024** (.0012)	.0006 (.0007)	-.0022* (.0012)	-.0055*** (.0008)
Observations	3,530,688	4,167,918	16,645,917	1,039,822	4,895,950

Note: The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. Standard errors are clustered at the labour-market-by-year level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C.9 – Labour Market Concentration and Probability of being hired on a Permanent Contract – New hires only – First Stage Equation.**

Dep. Var	France	Germany	Italy	Portugal	Spain
Log (HHI)					
Avg. of log (1/ $N_{o,z',t}$ )	.769*** (.032)	.789*** (.049)	.513*** (.051)	.182*** (.050)	.656*** (.020)
R <sup>2</sup>	.976	.979	.979	.9721	.986
Observations	3,530,660	4,167,918	16,645,917	1,039,792	4,875,973

Note: The dependent variable is  $Log(HHI)$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $N_{o,z',t}$  is the number of firms with positive hirings in all other FAs  $z'$  for the same occupation  $o$  and time period. Standard errors are clustered at the labour-market-by-year level. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C.10 – Labour Market Concentration and Probability of being hired on a Permanent Contract – IV estimates – New hires only – Nationwide firms.**

Dep. Var Perm contract	France	Italy	Portugal	Spain
Log HHI	-.0222*** (.0050)	.0013 (.0094)	-.0569* (.0329)	.0024 (.0022)
KP F Test	596.5	61.05	13.7	357.76
Observations	3,195,832	16,281,601	1,035,808	4,895,897

Note: 2SLS estimates. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Employers are defined as the aggregation of all their establishments nationwide, rather than the aggregation of their establishments at the municipality level, as done in our baseline regressions. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\text{Log}(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C.11 – Labour Market Concentration and Probability of being hired on a Permanent Contract – IV estimates – New hires only – Local Labour Markets based on FUs.**

Dep. Var Perm contract	France	Germany	Italy	Portugal	Spain
Log HHI	-.0225*** (.0051)	-.0332*** (.0116)	-.0005 (.0105)	-.0129 (.0324)	.0013 (.0022)
KP F Test	473.7	256.1	33.61	12.7	913.6
Observations	3,031,859	3,274,302	11,027,960	712,828	3,947,159

Note: 2SLS estimates. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Urban Areas (FUs), rather than Functional Areas, as in our baseline regressions. Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\text{Log}(HHI)$  is instrumented by the average of the log inverse number of firms in other FUs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table C.12 – Labour Market Concentration and Probability of being hired on a Permanent Contract – IV estimates – New hires only – Local Labour Markets based on NUTS-3.**

Dep. Var Perm contract	France	Germany	Italy	Portugal	Spain
Log HHI	-.0180*** (.0032)	-.0391*** (.0116)	-.0061 (.0061)	-.0934 (.0667)	.0018 (.0017)
KP F Test	1569.6	444.3	146.2	4.3	1578.2
Observations	4,021,376	4,167,909	18,198,802	1,159,438	5,230,111

Note: 2SLS estimates. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and NUTS-3 regions (districts in Portugal), rather than Functional Areas, as in our baseline regressions. Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\log(HHI)$  is instrumented by the average of the log inverse number of firms in other NUTS-3 regions for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table C.13 – Labour Market Concentration and Conversions from Temporary to Permanent Contracts – OLS estimates – New hires on temporary contracts only.**

Dep. Var Conversion	Italy	Spain
Log HHI	-.0002 (.0006)	-.0025*** (.0005)
Observations	8,927,725	4,105,318

Note: The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. Standard errors are clustered at the labour-market-by-year level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table C.14 – Labour Market Concentration and Conversions from Temporary to Permanent Contracts – IV estimates – New hires on temporary contracts only – Nationwide firms.**

Dep. Var Conversion	Italy	Spain
Log HHI	-.0768*** (.0218)	-.0042*** (.0012)
KP F Test	20.4	317.5
Observations	8,714,750	4,089,334

Note: 2SLS estimates. The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Employers are defined as the aggregation of all their establishments nationwide, rather than the aggregation of their establishments at the municipality level, as done in our baseline regressions. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $Log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table C.15 – Labour Market Concentration and Conversions from Temporary to Permanent Contracts – IV estimates – New hires on temporary contracts only – Local Labour markets based on FUAs.**

Dep. Var Conversion	Italy	Spain
Log HHI	-.0404*** (.0143)	-.0037*** (.0013)
KP F Test	22.4	799.6
Observations	5,686,754	3,256,943

Note: 2SLS estimates. The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Local labour markets are defined based on 4-digit occupations and Functional Urban Areas (FUAs), rather than Functional Areas, as in our baseline regressions. Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\text{Log}(HHI)$  is instrumented by the average of the log inverse number of firms in other FUAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table C.16 – Labour Market Concentration and Conversions from Temporary to Permanent Contracts – IV estimates – New hires on temporary contracts only – Local Labour markets based on NUTS-3 regions.**

Dep. Var Conversion	Italy	Spain
Log HHI	-.0401*** (.0079)	-.0040*** (.0010)
KP F Test	87.4	1344.9
Observations	9,819,974	4,403,598

Note: 2SLS estimates. The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Local labour markets are defined based on 4-digit occupations and NUTS-3 regions (districts in Portugal), rather than Functional Areas, as in our baseline regressions. Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\text{Log}(HHI)$  is instrumented by the average of the log inverse number of firms in other NUTS-3 regions for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .



## Appendix D – Controlling for the share of 4-digit occupations in new hires at the national level

**Table D.1 – Labour Market Concentration and Daily Wages of Full-Timers – IV estimates – controlling for the share of 4-digit occupations in new hires at the national level .**

Dep. Var Daily Wages	Denmark	France	Germany	Portugal
Log HHI	-.013*** (.003)	-.021*** (.002)	-.025*** (.002)	-.022*** (.007)
Share of occupation $o$ in hiring at the national level	yes	yes	yes	yes
KP F Test	85.5	921.9	338.3	24.0
Observations	5,486,000	8,269,375	11,050,435	15,086,998

Note: 2SLS estimates. The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\text{Log}(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table D.2 – Labour Market Concentration and Probability of being hired on a Permanent Contract – IV estimates – New hires only – controlling for the share of 4-digit occupations in new hires at the national level.**

Dep. Var Permanent contract	France	Germany	Portugal
Log HHI	-.0139*** (.0040)	-.0331*** (.0109)	-.0341* (.0191)
Share of occupation $o$ in hiring at the national level	yes	yes	yes
KP F Test	876.2	317.1	34.3
Observations	3,530,660	4,167,918	1,039,792

Note: 2SLS estimates. The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $\text{Log}(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table D.3 – Labour Market Concentration and Conversions from Temporary to Permanent Contracts – IV estimates – New hires on temporary contracts only – controlling for the share of 4-digit occupations in new hires at the national level.**

Dep. Var Conversion	Italy	Spain
Log HHI	-.0116* (.0063)	-.0028** (0.0010)
Share of occupation <i>o</i> in hiring at the national level	yes	yes
KP F Test	117.2	2247.0
Observations	8,927,725	4,089,355

Note: 2SLS estimates. The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects.  $Log(HHI)$  is instrumented by the average of the log inverse number of firms in other FAs for the same occupation. Standard errors are clustered at the labour-market-by-year level. KP F Test: Kleibergen-Paap Wald F Test. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## Appendix E – Plausibly exogenous instrument regressions

**Table E.1 – Labour Market Concentration and Daily Wages of Full-Timers – Plausibly exogenous instrument regressions.**

Dep. Var Daily Wages	Denmark	France	Germany	Portugal
(1) Reduced-form estimate of $\alpha$ from eq. (5)	-.0073*** (.0012)	-.0171*** (.0013)	-.0141*** (.0015)	-.0041*** (.0009)
(2) Minimum $\gamma$ for which $\beta$ is significant at the 10% level in eq. (4) using 2SLS	-.0054	-.0148	-.0113	-.0023
(2)/(1)	.74	.87	.80	.57
Observations	5,486,000	8,269,375	11,050,435	15,086,998

Note: The dependent variable is  $\log(\text{wage})$ . Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include yearly dummies for workers' age, whether the individual is a new hire, whether the individual was employed the year before if new hire, as well as individual fixed effects, firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. Monthly wages instead of daily wages are used for Portugal. Standard errors are clustered at the labour-market-by-year level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table E.2 – Labour Market Concentration and Probability of being hired on a Permanent Contract – New hires only – Plausibly exogenous instrument regressions.**

Dep. Var Permanent contract	France	Germany
(1) Reduced-form estimate of $\alpha$ from eq. (5)	-.0158*** (.0034)	-.0271*** (.0092)
(2) Minimum $\gamma$ for which $\beta$ is significant at the 10% level in eq. (4) using 2SLS	-.0101	-.0118
(2)/(1)	.6333	.4333
Observations	3,530,660	4,167,918

Note: The dependent variable is a dummy variable equal to 1 if the individual is hired on a permanent contract and 0 if hired on a temporary contract. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. Standard errors are clustered at the labour-market-by-year level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table E.3 – Labour Market Concentration and Conversions from Temporary to Permanent Contracts – New hires on temporary contracts only – Plausibly exogenous instrument regressions.**

Dep. Var Conversion	Italy	Spain
(1) Reduced-form estimate of $\alpha$ from eq. (5)	-.0168*** (.0033)	-.0026*** (.0007)
(2) Minimum $\gamma$ for which $\beta$ is significant at the 10% level in eq. (4) using 2SLS	-.0112	-.0013
(2)/(1)	.66	.50
Observations	8,927,725	4,089,355

Note: The dependent variable is a dummy variable equal to one when the individual was hired on a temporary contract at year  $t$  and started a permanent employment spell with the same employer by the end of the following calendar year. Local labour markets are defined based on 4-digit occupations and Functional Areas (FAs). Control variables include gender, education (4 categories), yearly dummies for workers' age, whether the individual was employed the year before hiring, whether he/she works full time or not, as well as firm-by-municipality-by-year fixed effects, sector and establishment fixed effects (where different from firm-by-municipality fixed effects), and local labour market fixed effects. Standard errors are clustered at the labour-market-by-year level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## Appendix F – Data Description

### Appendix F.1 – Denmark

The best-suited dataset to study labour market concentration in Denmark is the Danish Integrated Database for Labor Market Research (in Danish, *Den Integrerede Database for Arbejdsmarkedsforskning*, IDA). The database covers the universe of Danish workers, establishments, and firms in all sectors of the economy.

Each worker is observed at a yearly frequency (in November), when information is recorded on his/her employment status, main occupation, and on one secondary activity. A host of information is available for each job, including the occupational category (ISCO-08 plus one additional level of disaggregation), the number of hours worked per week, and the hourly wage.

Worker-level data can be linked to data on both the establishment and the firm in which the worker is employed. Workers, establishments, and firms have unique time-invariant identifiers, which allow for the construction of a panel.

To facilitate the creation of employment histories, the dataset also includes a retrospective and a prospective employment variable. For each worker, the retrospective variable identifies the employment status of the individual in the previous year and provides some additional explanation for changes in status.<sup>57</sup> Similarly, the prospective variable identifies the employment status of the individual in the following year.<sup>58</sup>

At the individual level, the database also includes a host of personal worker characteristics, including age, gender, and education.

For this project, we use data from 2010 to 2018.

### Appendix F.2 – France

The analysis for France builds upon the *Déclarations Annuelles de Données Sociales* (DADS), which are social security records drawn from firm tax declarations. The DADS are available under different formats. The *DADS-Postes* cover the universe of workers and establishments in all industries since 2009 (before that date the DADS did not include agriculture, part of the food-processing industry, so-called rural financial institutions – including Crédit Agricole, which is one of the largest French banks) and the public administration. We have access to these data until 2017, which effectively limits our sample to 2009-2017. The data contain information on each establishment's industry (at the 4-digit NACE level), location (municipality) and the firm to which the establishment belongs. They also contain information on each wage and salary employee who has worked for at least one hour in the establishment over the year. In particular, we know his/her 4-digit occupation according to the PCS-ESE classification, which contains about the same number of categories as the 4-digit breakdown of the ISCO-08 classification.<sup>59</sup> Self-employed are not covered by the dataset, while household employees and interns are dropped even if included in the data.

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<sup>57</sup> If the worker was not in the same establishment in the previous year, the variable records whether he/she moved to his/her current post from another establishment of the same firm, a different firm, unemployment, outside the labour force, abroad, or a period of leave.

<sup>58</sup> If the individual is not present in the same establishment in the following year, the variable records whether he/she moved to another establishment within the same firm, another firm, unemployment, outside the labour force, abroad, a period of leave, or died.

<sup>59</sup> 412 categories in the PCS-ESE classification as compared to 436 categories in the ISCO-08 classification.

Each line in this dataset is a job match (establishment-by-employee-by-year). Establishments have a unique identifier which is invariant over time, except when the establishment changes location (or simply changes mail address), in which case it is assigned a new identifier. By contrast, for the sake of anonymity, workers' identifiers are changed every year (but they are unique in a given year, even if they change establishment). However, for any given wave (corresponding to a given year), the previous year's record corresponding to the same job match (if any) is also reported (with a random noise on quantitative variables, so that these couples of observations for different waves cannot be chained and used to reconstruct a long panel).<sup>60</sup> Information concerning dissolved matches that existed in the previous year is also reported. Using these pieces of information, we can identify whether a worker is a new hire under the different definitions of firm used in the paper.

For the subset of workers in the *DADS-Postes* who are born in October of each year, there exists a panel which maintains the same identifier over time for each worker and hence allows to follow workers across various employers and years. This panel (*Panel tous salariés*) reports the day when the worker was hired and when the employment spell terminates. It also reports, for each match, the annual gross wage, a full-time/part-time indicator and annual hours worked. The data also report workers' age, gender, municipality of residence, type of contract, and 4-digit occupation.

The *Echantillon Démographique Permanent* (EDP) is used to retrieve information on education. It contains a random sample of individuals updated with Census data (which are updated on a rotating basis for 1/10 of the population each year). However, as this information is updated every ten years, it only allows to define a rough, time-invariant variable based on the highest diploma, as the precise dates of change in educational status cannot be identified. Moreover, since not all workers in the *Panel tous salariés* are part of the EDP, information on educational attainment can be retrieved only for a minority of the workers in our sample.

### **Appendix F.3 – Germany**

The analysis for Germany builds on the Employment History (BeH V10.05.01-201912) of the Institute for Employment Research (IAB), which is equivalent to the employment information in the IAB Integrated Employment Biographies – IEB, described in Müller and Wolter (2020). The BeH contains employer declarations about the universe of workers who are subject to social security contributions. Thus, the data give full account of private sector employment for regular workers, marginal workers, and apprentices in the German labour market.<sup>61</sup> The BeH is available from 1975 onwards for West Germany and 1993 onwards for East Germany. In this project, we use the universe of employment spells in the years 2012–2018.

Information on employment spells is available at the daily level. Wages are averaged over the spells' duration and reported by calendar year for spells that cover several calendar years. When establishments report multiple spells for the same worker in the same year, we select the spell with the highest wage.<sup>62</sup> The information is thus organised in worker-by-establishment-by-year triplets (i.e., job-year combinations). An establishment typically comprises all establishments

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<sup>60</sup> For example, for a given establishment A and worker B, the 2010 wave reports both the variables corresponding to 2010 and 2009, with a small random noise added to the 2009 values. If all variables for the previous year (2009 in the example) are missing, this means that worker B was not in establishment A in that year (therefore it would be unambiguously a new hire in the current year – 2010 in the example). If no record involving worker B can be found in the previous year, this means that he/she was not working as a salaried employee the year before.

<sup>61</sup> Being exempt from social security records, civil servants, self-employed persons and family workers do not enter the IEB.

<sup>62</sup> If there is a tie, we choose the spell with the longest duration.

of a firm within a municipality, but we do not observe whether establishments in different municipalities belong to the same firm.

As regards workers, the data include the daily wage (up to the social security contribution ceiling), job duration, social security status, gender, age, occupation, the employer's location and industry, and an indicator for full-time and part-time employment, but not the precise number of working hours. Furthermore, the data contain information on whether a worker was hired on a fixed-term or a permanent contract, though we cannot observe later conversions.

Occupations are recorded using the German Classification of Occupations (KldB 2010) with 1,286 distinct occupations, which we map onto 4-digit ISCO-08 occupations via a standard crosswalk (German Federal Employment Agency, 2011). Moreover, we follow standard practice and impute top-coded wages using a two-step procedure (see Card et al., 2013).<sup>63</sup>

Information on collective bargaining coverage is retrieved from the IAB Establishment Panel, which contains a sample of about 15,500 establishments per year (Bellmann et al., 2021). The survey reports whether each establishment is subject to a sector-level or a firm-level collective agreement and whether there is a works council. The survey's sampling scheme is based on a partition of the economy in 19 industries (an intermediate level between 1 and 2 digits of the NACE rev.2 classification – see Ellguth et al., 2014) and is therefore representative of each of these industries.

#### ***Appendix F.4 – Italy***

The analysis for Italy builds on *Comunicazioni Obbligatorie (CO)*, a contract-level dataset maintained for administrative purposes by the Ministry of Labour and Social Policies and made available thanks to a research agreement with ANPAL (*Agenzia Nazionale Politiche Attive Lavoro*). The dataset contains all employers' declarations about activations, terminations and conversions of contracts that took place in Italy since 2010.

For each contract, it reports rich information about its characteristics: the (anonymised) identity of the employer and that of the employee; the start date; the conversion date (i.e. the date when a non-permanent contract is converted into a permanent one), the termination date, if applicable; and the number of contract extensions (for non-permanent contracts only). Furthermore, the dataset contains the following information: the type of contract (permanent, temporary, or apprenticeship), the business sector (6-digit NACE Rev.2 classification); the working hour arrangements (i.e. full-time or part-time); the occupation (5-digit CP-2011 classification which we map onto 4-digit ISCO-08 using a hand-created crosswalk); and the municipality where the employer is located.

Alongside information about the contract, the following socio-demographic characteristics of the employee are available: gender, age, and education.

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<sup>63</sup> First, we use fitted wages from a Tobit regression at the worker level to calculate average daily wages for each establishment-year combination (excluding the top-coded observation at hand). In a second step, we repeat the Tobit regression with this leave-one-out variable as an additional regressor which delivers the final imputations. Specifically, we adopt Schmucker et al.'s (2018) implementation and regress log daily wages on age, a quadratic of log establishment size, the share of low-skilled and high-skilled workers within the establishment, the share of censored observations excluding the observation at hand, as well as dummy variables for German nationality, workplace in East Germany, one-person establishments and establishments with more than ten full-time employees. Separate Tobit models are estimated for 56 cells defined according to the year (7 groups), gender (2 groups) and individual qualification (4 groups).

Against these unique advantages, the dataset has the drawback that it does not record information about the hiring wage and the size of the firm.

### ***Appendix F.5 – Portugal***

The data for Portugal come from the *Quadros de Pessoal* (QP), an administrative panel with matched firm, establishment, and worker data for all firms operating in the private sector. Since 2010, the QP is part of a broader compulsory survey on firms, the *Relatório Único*. All firms employing at least one worker in a given year have to deliver the *Relatório Único* to the Ministry of Labour by April of the following year. When filling the QP part of the *Relatório Único* for a given year, firms must report detailed information on their activity and on every worker who was employed in the firm at some point in October of that year. Workers hired after October of year  $t$  and who leave before October of year  $t+1$  will not be included in this record. The QP is therefore a snapshot of the Portuguese labour market as of October of each year.

Importantly for us, the QP provides us with the location of firms and all their establishments at the municipality-level, as well as their economic activity, following the 5-digit Portuguese Classification of Economic Activities (CAE Rev.3), which is harmonised and directly comparable with NACE-Rev.2 until the fourth digit.

At the worker level, the QP includes information on gender, date of birth, education, the month and year of admission in the firm, gross monthly wages (base, bonuses, and overtime components separately), base and overtime hours worked in October, whether or not the individual works full-time or part-time, his/her type of contract (permanent vs temporary), and occupation at the 4 digit-level of the 2010 Portuguese Classification of Occupations (CPP/2010), which is harmonised and directly comparable with ISCO-08. The QP also reports collective agreements regulating the employment contract of the worker, distinguishing among: i) agreements between one firm and one or more unions; ii) agreements between two or more firms and one or more unions; iii) agreements between one employer association and one or more unions; and iv) contracts regulated by working condition ordinances (issued by the Government) and/or no collective agreement.

Firms, establishments and workers all have identifiers that enable us following them longitudinally. This allows us to define a new hire both according to the firm and to the firm-by-municipality concept.

Since the QP does not provide the number of days worked in the firm, in order to ensure that new hires have at least one month of tenure, we exclude all new hires whose admission in the firm dates back to September of each year.<sup>64</sup>

While the QP is available since 1985, we focus on 2010-2019 for three reasons. First, to work on a time period that is contemporaneous to the ones used in other countries. Second, because the introduction of the *Relatório Único* in 2010 led to some changes in the data as compared to 2009. Third, in 2010, occupations were recoded according to the CPP/2010, in replacement of the National Classification of Occupations dating back to 1994. Hence, focusing on 2010 onward ensures that we are consistently defining local labour markets.

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<sup>64</sup> We are not able to assess the number of days of tenure of two workers who would be hired in September and would leave the firm during the month of October, even though it is possible that one of them completed 1 month of tenure while the other one did not.



## **Appendix F.6 – Spain**

The analysis for Spain uses confidential information on the universe of contracts recorded by the National Public Employment Service (*Servicio Público de Empleo Estatal* or SEPE), an independent agency ascribed to the Ministry of Labour and Social Economy. Our monthly dataset contains anonymised records for the universe of regular private sector employment contracts including apprenticeship. The data are available starting in 2007, but for this project we use the universe of contracts signed between 2010 and 2017.

The administrative record for each contract contains information about the worker, including gender, age, an anonymised ID and his/her level of education. It also includes a unique firm and an establishment-level ID, the 4-digit sector (according to the 2009 National Classification of Economic Activities) in which the firm is active and the location of the establishment. As regards the labour contract itself, it provides information on the 4-digit occupation, regular working hours, as well as the starting date and type of the contract. In contrast, our dataset does not contain information on wages and only provides information on the expected end dates for fixed-term contracts.

Occupations are recorded using the 4-digit Spanish Classification of Occupations (CNO 2011). We map this data onto the 4-digit ISCO-08 classification using a crosswalk provided by the Spanish National Statistical Institute (INE).

Formally, our dataset is a matched employer-employee panel with information on the universe of contracts. As such, we can distinguish between initial contracts, new contracts and the conversion of temporary or fixed-term contracts into permanent contracts.

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# Conclusion

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## Concluding remarks

Space is a crucial factor in determining access to healthcare facilities and to job opportunities. This thesis shows how it limits workers' access to jobs of quality and the way it affects not only the abortion conditions of women but even their ability to abort. On this last point, Chapter 1 finds that the farther away women live from an abortion service, the worse are the conditions under which they abort. It also documents how regions that become far from an abortion provider experience fewer abortions, suggesting that long distances to abortion care impede women from obtaining an abortion. Thus, such regional disparities in access have the potential to harm the well-being of women in multiple dimensions, in particular, their reproductive rights and their health. The importance of space also extends to the labour market. Chapters 2 and 3 document that workers living in areas with fewer potential employers work in poorer-quality jobs. Indeed, employers in more concentrated markets are able to pay them lower wages and offer them more precarious contracts.

In an era of increased political polarization, where parts of society, and particularly those living in rural areas, may feel left behind, it is essential that policymakers include the fight against spatial disparities in their agenda. Promoting equitable access to abortion across space can be achieved through cost-efficient solutions such as increasing the transparency of the functioning of the abortion care system. This can be achieved by providing women with accessible information on how and where to access abortion services. Similarly, when assessing mergers and acquisitions, it is important to assess them not only on the basis of their potential impact on product markets but also on local labour markets. In addition, implementing policies that foster unionization is another manner to help level the field between workers and monopsonistic employers. In conclusion, disregarding space is detrimental to society. To place the well-being of citizens as the central priority of public policy, space needs to be at the center of decisions too.



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## RÉSUMÉ

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L'espace délimite naturellement les êtres humains. Cette thèse examine son interaction avec deux dimensions importantes du bien-être subjectif : la santé et le travail. Le premier chapitre examine comment l'espace détermine l'accès à l'IVG et les soins d'IVG que les femmes reçoivent. Il montre que les femmes qui vivent loin d'un pourvoyeurs de IVG ont moins de possibilités d'avorter et risquent davantage d'être confrontées à des conditions d'avortement plus difficiles. L'espace est également au cœur des deuxième et troisième chapitres, qui montrent comment le fait de vivre à proximité de marchés du travail non compétitifs nuit à la qualité de l'emploi, comme le montrent les salaires inférieurs et la sécurité de l'emploi des travailleurs sur des marchés du travail plus concentrés. Dans l'ensemble, cette thèse souligne l'importance de concevoir des politiques qui s'attaquent aux disparités spatiales dans l'accès aux soins de santé et aux opportunités d'emploi afin d'améliorer le bien-être des citoyens.

## MOTS CLÉS

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Espace, Accès aux opportunités d'emploi, Accès aux services de santé, Monopsonie, Interruption volontaire de grossesse (IVG)

## ABSTRACT

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Space naturally bounds human beings. This thesis examines its interplay with two important dimensions of subjective well-being: health and work. The first chapter examines how space determines abortion access and the abortion care that women receive. It finds that women living far from an abortion provider are less able to get an abortion and at a higher risk of experiencing worse abortion conditions. Space is also at the core of the second and third chapters, which document how living nearby uncompetitive labour markets is detrimental of job quality, as the lower wages and job security of workers in more concentrated labour markets show. Overall, this thesis points to the importance of designing policies tackling spatial disparities in access to healthcare facilities and job opportunities as means of improving the well-being of citizens.

## KEYWORDS

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Space, Access to job opportunities, Access to health facilities, Monopsony, Abortion