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Firms, Wage Dynamics, and Labour Contract Choices

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"An ending fitting for the start"

– The Libertines, *Can't Stand Me Now*

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Note to the reader

This thesis consists of three independent papers written during my doctoral period at Sciences Po. These chapters are preceded by an introduction. A French translation of this introduction follows the English version. While the bibliography of each paper can be found at the end of each chapter, all the appendices appear together at the end of the thesis.

Introduction

English Version

The dynamics of wage inequality and the challenges posed by labour market segmentation - where different groups of workers have different employment protections and conditions - are at the forefront of academic and policy discussions. Wage inequality is a driving force of economic inequality and its rise over several decades in most rich countries is well documented (OECD 2021; Tomaskovic-Devey et al. 2020). Among the drivers of labour market segmentation, the division along the lines of the permanent versus the temporary nature of employment contracts has been one of the most debated issues. To combat rising unemployment and rigid labour markets (OECD 1994), governments have increasingly liberalised fixed-term contracts, avoiding the political complexities of reforming dismissal rules for permanent employment (Bentolila et al. 2020). In these contexts, strict regulation of permanent jobs makes them more protected at the expense of higher instability of temporary jobs, with detrimental effects on labour market outsiders (Barbieri & Cutuli 2016; Bentolila et al. 2012; Boeri & Garibaldi 2007; Cazes & Tonin 2010; Kahn 2007).

This thesis studies the interplay between firms and institutions in determining wage dynamics and labour contract choices. With the increasing accessibility of matched employer-employee datasets, researchers have gained deeper insight into firm behaviors and the impact of changes in labour market regulations. Such administrative datasets, which provide granular insight into wage and employment contract structures, can be instrumental in shedding light on the mechanisms driving between-firm wage disparities and hiring decisions. This work uses a newly constructed and quasi-exhaustive matched employer-employee database for France and two analogous datasets for Italy to analyse wage dynamics and employment contract patterns in these countries.

Methodologically, this thesis combines the estimation of regression and structural models. The seminal work of Abowd et al. (1999) continues to influence the literature on wage determination. This model suggests a single job ladder in the labour market and

rejects the idea of comparative advantage driving job mobility. It also implies that only the current worker-firm match matters, not past job histories. Despite recent advances in modelling the influence of previous employers on wages (Di Addario et al. 2023), the evolution of firm effects (Lachowska et al. 2023), and worker-firm complementarities (Bonhomme et al. 2019), the basic AKM model remains a useful tool for effectively summarising the first-order facts about the role of firms in wage determination (Card et al. 2023). The first chapter proposes a novel method to correct for the well-known "limited mobility" bias in the measurement of sorting through AKM models and applies it to France.

The second and third chapters estimate the impact of an Italian labour market reform using firm-level regressions combined with a structural model allowing for equilibrium effects and policy simulations. Together, these methods provide a comprehensive view of the reform, from its immediate micro and macro effects to the potential consequences of alternative regulatory approaches.

Motivation. The first chapter looks at the role of firms in shaping wage inequality in France. In countries such as Germany (Card et al. 2013) and the USA (Song et al. 2019), firms contribute significantly to growing wage dispersion. In France, however, wage inequality has remained stable or even declined. This chapter provides some insights to understand this French anomaly.

The second chapter examines how firm heterogeneity affects the choice between permanent and temporary contracts in Italy, and how regulatory interventions affect this relationship. Recent studies on dual labour markets have provided tools to understand what drives the choice between temporary and permanent contracts (Berton & Garibaldi 2012; Cahuc et al. 2016; Cao et al. 2010). However, these studies often sideline firms and focus only on the quality of the match between worker and job. This approach has limitations: it neglects the perspective of the firm, which often knows more about its own needs than about individual candidates. Excluding firms also makes it more difficult to link theory to real-world data, especially when studying the impact of policies on employment dynamics. The chapter examines how a firm's fundamentals influence its hiring decisions in terms of contracts and its employment responses to regulation.

In recent years, concerns about the spread of job insecurity have led several countries to gradually reintroduce restrictions on the use of temporary contracts, marking a significant shift from the previous policy regime. The second chapter examines how the labour market responds to this type of reform through adjustments in employment and occupational flows, for which there is already some evidence (Cahuc et al. 2022).

The third chapter examines whether these occupational changes are accompanied by adjustments in wage dynamics, an area where little research has been done.

The main contributions and findings of each chapter are briefly presented below.

In the Land of AKM: Explaining the Dynamics of Wage Inequality in France. This chapter contributes to the existing literature in three main ways. First, it uses a new, quasi-exhaustive matched employer-employee dataset for France, which allows it to provide the first decomposition of the variance of log wages and its evolution from 2002 to 2019. Second, it presents a novel method to correct for the well-known "limited mobility" bias in AKM models (Abowd et al. 2004; Andrews et al. 2008; Bonhomme et al. 2023). Both Card et al. (2013) and Song et al. (2019) acknowledge the mobility bias in sorting measures but assume its stability over time. The corrected results of this chapter challenge this assumption and highlight the potential impact of the mobility bias on the observed dynamics.

Finally, the chapter shows that most of the increase in inequality between firms has been driven by shifts in the composition of firms and changes in the demand for labour in specific occupations. Over the same period, wages at the bottom have grown faster than those at the middle and top percentiles. The chapter provides suggestive evidence that these dynamics are related to the nature of French labour market institutions. While France has managed to contain wage inequalities through institutional measures - namely the mix of a substantial increase in the minimum wage and the elimination of all employer-paid payroll taxes around it - the underlying dynamics driving the rise in both *segregation*, where high-wage workers tend to work more often with similar workers, and *sorting*, where high-wage workers are increasingly employed in high-paying firms, are consistent with those observed elsewhere.

Temporary Employment, Firm Heterogeneity, and Regulation. This chapter contributes to the very broad literature on employment protection legislation and, in particular, to the large body of work examining the trade-offs between temporary and permanent contracts. Over the past two decades, this literature has highlighted two main sets of issues.

First, a substantial body of work has focused on understanding the mechanisms that determine a firm's choice between different contract types. These studies provide explanations for why it might be economically rational for a firm to offer temporary jobs to some workers and permanent jobs to others. The main contribution of the chapter in this direction is to acknowledge the existence of firms as more than just

collections of independent jobs. This is achieved by first showing that the variability in the share of temporary employment in the economy is mostly explained by firm-specific components, while time and industry-specific variability play a less important role. In particular, while there is no correlation between past labour productivity and the share of days worked in temporary employment, highly productive firms within the same industry tend to employ a lower share of workers on fixed-term contracts and offer longer durations for such contracts. This suggests that firm-specific factors, in addition to industry characteristics, significantly influence the demand for temporary work. The structural model introduced provides a mechanism to examine how these micro-level decisions influence the macro-level equilibrium of contract types within the broader economy.

A second body of work has examined the effects of different regulatory regimes for employment contracts. Recent contributions converge on the view that variations in the regulation of fixed-term contracts primarily lead to substitution between contract types rather than to significant variations in overall employment levels (Cahuc et al. 2022; Cappellari et al. 2012; Daruich et al. 2023). Our empirical work sheds light on an unexplored driver of labour demand response to regulation targeting the use of fixed-term contracts: how within-sector productivity differentials influence the extent to which firms substitute temporary jobs for permanent ones. The estimated model can be used to conduct counterfactual exercises to understand how different policy choices affect labour dynamics within heterogeneous firms, and to shed light on the potential consequences of alternative regulatory approaches.

Dignity by Decree? Temporary Jobs Reforms and Workers' Wages. The main objective of this chapter is to examine the wage changes experienced by workers moving from a temporary to a permanent contract. To this end, it focuses on contract conversions, cases in which a worker moves from a temporary to a permanent contract within the same firm, and compares the associated wage dynamics between different policy regimes. Building on existing evidence that has empirically documented the existence of wage increases for such transitions (Daruich et al. 2023; Silva et al. 2018), the chapter presents evidence that reforms that limit the use of temporary contracts can compress this wage premium and provides some tentative explanations for the mechanisms behind this effect.

Version Française

La dynamique des inégalités salariales et les défis posés par la segmentation du marché du travail - où différents groupes de travailleurs ont des protections et des conditions d'emploi différentes - sont au premier plan des discussions académiques et politiques. L'inégalité salariale est un moteur de l'inégalité économique et son augmentation sur plusieurs décennies dans la plupart des pays riches est bien documentée (OECD 2021; Tomaskovic-Devey et al. 2020). Parmi les moteurs de la segmentation du marché du travail, la division selon la nature permanente ou temporaire des contrats de travail a été l'une des questions les plus débattues. Pour lutter contre la montée du chômage et la rigidité des marchés du travail (OECD 1994), les gouvernements ont de plus en plus libéralisé les contrats à durée déterminée, évitant ainsi les complexités politiques d'une réforme des règles de licenciement pour les emplois permanents (Bentolila et al. 2020). Dans ces contextes, la réglementation stricte des emplois permanents les rend plus protégés au détriment d'une plus grande instabilité des emplois temporaires, ce qui a des effets néfastes sur les individus en marge du marché du travail (Barbieri & Cutuli 2016; Bentolila et al. 2012; Boeri & Garibaldi 2007; Cazes & Tonin 2010; Kahn 2007).

Cette thèse étudie l'interaction entre les entreprises et les institutions dans la détermination de la dynamique des salaires et des choix de contrats de travail. Grâce à l'accessibilité croissante des ensembles de données appariées employeur-employé, les chercheurs ont acquis une connaissance plus approfondie des comportements des entreprises et de l'impact des changements dans la réglementation du marché du travail. Ces ensembles de données administratives, qui fournissent des informations détaillées sur les structures des salaires et des contrats de travail, peuvent contribuer à mettre en lumière les mécanismes à l'origine des disparités salariales entre les entreprises et des décisions d'embauche. Ce travail utilise une base de données employeur-employé nouvellement construite et quasi-exhaustive pour la France et deux ensembles de données analogues pour l'Italie afin d'analyser les modèles de dynamique des salaires et des contrats d'emploi dans ces pays.

Sur le plan méthodologique, cette thèse combine l'estimation de modèles de régression et de modèles structurels. Le travail séminal de Abowd et al. (1999) continue d'influencer la littérature sur la détermination des salaires. Ce modèle suggère une seule "job ladder" sur le marché du travail et rejette l'idée que l'avantage comparatif est le vecteur de la mobilité de l'emploi. Il implique également que seule le "match" entre le travailleur et l'entreprise compte, et non les antécédents professionnels. Malgré les avancées récentes dans la modélisation de l'influence des employeurs précédents sur

les salaires (Di Addario et al. 2023), l'évolution des effets de l'entreprise (Lachowska et al. 2023), et les complémentarités travailleur-entreprise (Bonhomme et al. 2019), le modèle AKM de base reste un outil utile pour résumer efficacement les faits de premier ordre sur le rôle des entreprises dans la détermination des salaires (Card et al. 2023). Le premier chapitre propose une nouvelle méthode pour corriger le biais bien connu de "limited mobility" dans la mesure du "sorting" par les modèles AKM et l'applique à la France.

Les deuxième et troisième chapitres estiment l'impact d'une réforme du marché du travail italien à l'aide de régressions au niveau de l'entreprise combinées à un modèle structurel permettant des effets d'équilibre et des simulations de policy. Ensemble, ces méthodes permettent d'obtenir une vue d'ensemble de la réforme, de ses effets micro et macro immédiats aux conséquences potentielles d'autres réglementations.

Motivation. Le premier chapitre s'intéresse au rôle des entreprises dans la formation des inégalités salariales en France. Dans des pays comme l'Allemagne (Card et al. 2013) et les États-Unis (Song et al. 2019), les entreprises contribuent de manière significative à la dispersion croissante des salaires. En France, en revanche, les inégalités salariales sont restées stables, voire ont diminué. Ce chapitre apporte quelques éléments pour comprendre cette anomalie française.

Le deuxième chapitre examine comment l'hétérogénéité des entreprises affecte le choix entre les contrats permanents et les contrats temporaires en Italie, et comment les interventions réglementaires influencent cette relation. Les études récentes sur le dualisme des marchés du travail ont fourni des outils pour comprendre ce qui motive le choix entre les contrats temporaires et permanents (Berton & Garibaldi 2012; Cahuc et al. 2016; Cao et al. 2010). Cependant, ces études mettent souvent de côté les entreprises et se concentrent uniquement sur la qualité de l'appariement entre le travailleur et l'emploi. Cette approche a des limites: elle néglige le point de vue de l'entreprise, qui en sait souvent plus sur ses propres besoins que sur les candidats individuels. L'exclusion des entreprises rend également plus difficile l'établissement d'un lien entre la théorie et les données du monde réel, en particulier lorsqu'il s'agit d'étudier l'impact des politiques sur la dynamique de l'emploi. Ce chapitre examine comment les fondamentaux d'une entreprise influencent ses décisions d'embauche en termes de contrats et ses réponses en matière d'emploi à la réglementation.

Au cours des dernières années, les inquiétudes concernant la propagation de l'insécurité de l'emploi ont conduit plusieurs pays à réintroduire progressivement des restrictions sur l'utilisation des contrats temporaires, marquant ainsi un changement sig-

nificatif par rapport au régime politique précédent. Le deuxième chapitre examine comment le marché du travail réagit à ce type de réforme par des ajustements de l'emploi et des flux professionnels, pour lesquels il existe déjà quelques évidences (Cahuc et al. 2022). Le troisième chapitre examine si ces changements professionnels s'accompagnent d'ajustements dans la dynamique des salaires, un domaine dans lequel peu de recherches ont été effectuées.

Les principales contributions et les résultats de chaque chapitre sont brièvement présentés ci-dessous.

In the Land of AKM: Explaining the Dynamics of Wage Inequality in France. Ce chapitre contribue à la littérature existante de trois manières principales. Premièrement, il utilise un nouvel ensemble de données appariées employeur-employé quasi-exhaustif pour la France, ce qui lui permet de fournir la première décomposition de la variance du logarithme des salaires et son évolution de 2002 à 2019. Deuxièmement, il présente une nouvelle méthode pour corriger le biais bien connu de "limited mobility" dans les modèles AKM (Abowd et al. 2004; Andrews et al. 2008; Bonhomme et al. 2023). Card et al. (2013) et Song et al. (2019) reconnaissent le biais de mobilité dans les mesures de "sorting" mais supposent sa stabilité dans le temps. Les résultats corrigés du chapitre remettent en question cette hypothèse et soulignent l'impact potentiel du biais de mobilité sur la dynamique observée.

Enfin, le chapitre montre que la majeure partie de l'augmentation des inégalités entre les entreprises est due à des changements dans la composition des entreprises et à l'évolution de la demande de main-d'œuvre dans des professions spécifiques. Au cours de la même période, les salaires du bas de l'échelle ont augmenté plus rapidement que ceux des centiles moyens et supérieurs. Le chapitre fournit des preuves suggestives que ces dynamiques sont liées à la nature des institutions du marché du travail français. Alors que la France a réussi à contenir les inégalités salariales grâce à des mesures institutionnelles - à savoir le mélange d'une augmentation substantielle du salaire minimum et l'élimination de toutes les charges sociales payées par l'employeur autour de celui-ci - la dynamique sous-jacente qui conduit à l'augmentation à la fois de *ségrégation,* où les travailleurs à hauts salaires ont tendance à travailler plus souvent avec des travailleurs similaires, et de *sorting*, où les travailleurs à hauts salaires sont de plus en plus employés dans des entreprises à hauts salaires, est cohérente avec celles observées ailleurs.

Temporary Employment, Firm Heterogeneity, and Regulation. Ce chapitre contribue à la très vaste littérature sur la législation relative à la protection de l'emploi et, en

particulier, au grand nombre de travaux examinant les “trade-offs” entre les contrats temporaires et les contrats permanents. Au cours des deux dernières décennies, cette littérature a mis en évidence deux grandes séries de questions.

Tout d’abord, de nombreux travaux se sont concentrés sur la compréhension des mécanismes qui déterminent le choix d’une entreprise entre différents types de contrats. Ces études expliquent pourquoi il peut être économiquement rationnel pour une entreprise de proposer des emplois temporaires à certains travailleurs et des emplois permanents à d’autres. La principale contribution de ce chapitre dans cette direction est de reconnaître l’existence des entreprises comme étant plus que de simples collections d’emplois indépendants. Pour ce faire, nous commençons par montrer que la variabilité de la part de l’emploi temporaire dans l’économie s’explique principalement par des composantes spécifiques à l’entreprise, tandis que la variabilité spécifique au temps et à son secteur d’activité joue un rôle moins important. En particulier, alors qu’il n’y a pas de corrélation entre la productivité passée du travail et la proportion de jours travaillés en emploi temporaire, les entreprises hautement productives d’un même secteur ont tendance à employer une plus faible proportion de travailleurs en contrat à durée déterminée et à proposer des durées plus longues pour ces contrats. Cela suggère que des facteurs spécifiques à l’entreprise, en plus des caractéristiques de l’industrie, influencent de manière significative la demande de travail temporaire. Le modèle structurel proposé fournit un mécanisme permettant d’examiner comment ces décisions microéconomiques influencent l’équilibre macroéconomique des types de contrats au sein de l’économie au sens large.

Un deuxième corpus de travaux a examiné les effets des différents régimes de réglementation des contrats de travail. Les contributions récentes convergent sur l’idée que les variations de la réglementation des contrats à durée déterminée conduisent principalement à une substitution entre les types de contrats plutôt qu’à des variations significatives des niveaux d’emploi globaux (Cahuc et al. 2022; Cappellari et al. 2012; Daruich et al. 2023). Notre travail empirique met en lumière un levier inexploré de la réponse de la demande de travail à la réglementation ciblant l’utilisation des contrats à durée déterminée : comment les différentiels de productivité au sein du secteur influencent la mesure dans laquelle les entreprises substituent des emplois temporaires à des emplois permanents. Le modèle estimé peut être utilisé pour effectuer des exercices contrefactuels afin de comprendre comment différents choix de politiques affectent la dynamique du travail au sein d’entreprises hétérogènes, et pour mettre en lumière les conséquences potentielles d’approches réglementaires alternatives.

Dignity by Decree? Temporary Jobs Reforms and Workers' Wages. L'objectif principal de ce chapitre est d'examiner les changements salariaux subis par les travailleurs qui passent d'un contrat temporaire à un contrat permanent. À cette fin, il se concentre sur les conversions de contrat, les cas dans lesquels un travailleur passe d'un contrat temporaire à un contrat permanent au sein de la même entreprise, et compare la dynamique salariale associée entre les différents régimes politiques. En se basant sur les études existantes qui ont empiriquement documenté l'existence d'augmentations salariales pour ces transitions (Daruich et al. 2023; Silva et al. 2018), le chapitre présente des évidences que les réformes qui limitent l'utilisation des contrats temporaires peuvent comprimer cette prime salariale et fournit quelques explications provisoires sur les mécanismes qui sous-tendent cet effet.

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1. In the Land of AKM: Explaining the Dynamics of Wage Inequality in France*

Abstract

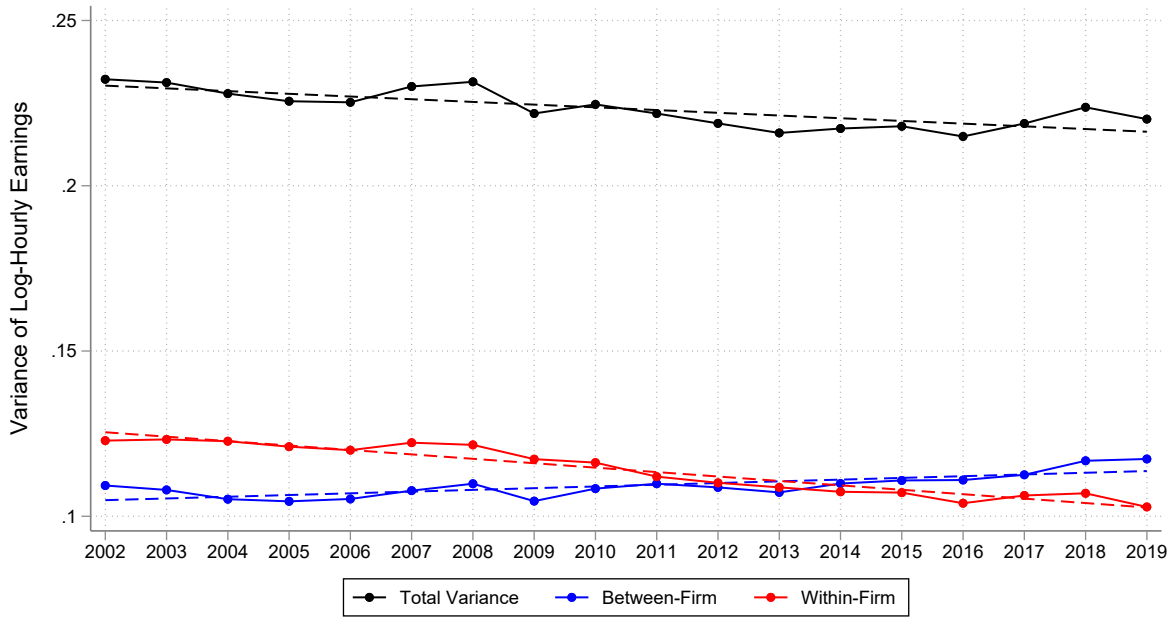
We use a newly built and quasi-exhaustive matched employer-employee database to study firms' contribution to wage inequalities in France. Our analysis covering the period from 2002 to 2019 reveals a significant increase in between-firm inequalities, driven by a growing tendency of high-wage workers to cluster together in high-premium firms. These phenomena are directly associated with changes in firms' demographics and workforce composition. Over the same period, bottom earnings percentiles increased more than the rest of the distribution, in line with the rise in the legal minimum wage. As a result, within-firm inequalities decreased, almost offsetting the rising between-firm inequalities.

*This chapter is a joint work with Damien Babet and Olivier Godechot. We dedicate this work to our professor, Christian Baudelot, who was one of the designers of the first version of the BTS database. We thank Kerstin Holzheu, Horng Wong, Matthieu Lequien, Sébastien Roux, Pierre Cahuc, Pauline Givord, and the seminar participants at the département des études économiques seminar at Insee, and at the Coin meeting. Access to some confidential data, on which this work is based, was made possible within a secure environment provided by CASD – Centre d'accès sécurisé aux données (Ref. 10.34724/CASD). The research and writing of this paper benefited from the monetary support of the following institutions: Agence Nationale de la Recherche (grant ANR-17-CE41-0009-01)

1.1. Introduction

Wage inequality is a driving force of economic inequalities. Its rise for several decades in most rich countries is well documented[2]. Firms play a central role in driving these dynamics, as evidenced by studies conducted in Germany (Card et al. 2013) and the USA (Song et al. 2019), where rising inequality is largely attributed to between-firm wage disparities. France, however, represents an interesting case, with wage inequality remaining stable or even decreasing in recent decades, despite exhibiting similar polarizing dynamics observed in the other countries, characterized by an increase in between-firm inequalities (Figure 1.1).

FIGURE 1.1. Evolution of wage inequality - France



Notes: This figure shows the evolution over time of the variance of log-wage, the between-firm variance of log-wage, and the within-firm variance of log-wage. We compute the overall variance of log-earnings as $\frac{1}{N_t}\sum_i(w_{it}-\overline{w}_t)^2$, the between-firm variance as $\frac{1}{N_t}\sum_f N_{ft}(\overline{w}_{ft}-\overline{w}_t)^2$ and the within-firm variance as $\frac{1}{N_t}\sum_f\sum_{i\in f}(w_{it}-\overline{w}_{ft})^2$, where workers are indexed by i and time by t and firms by f. N_t and N_{ft} denote the number of workers in total and in each firm, respectively; w_{it}, $\overline{w}_t$ and $\overline{w}_{ft}$ are the log worker wage, the overall average log wage and the average log wage within each firm, respectively. All individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included.

To explore the factors contributing to the French exception, we employ the Abowd et al. (1999) model (hereafter AKM) to decompose log-wage variance into between- and within-firm components.

Despite the existence of wage premiums for similar workers across different firms,

[2]Tomaskovic-Devey et al. (2020) and OECD (2021) for a recent international comparison.

their contribution to the observed increase in between-firm wage inequalities is negligible, and if anything negative. Similarly to Germany and the USA, in France, we find that changes in the distribution of workers between firms are the primary drivers of the rise in between-firm inequalities. These changes manifest as a notable increase in *segregation*, wherein high-wage workers tend to work more often with similar workers, and a more limited in magnitude but robust rise in *sorting*, wherein high-wage workers are increasingly employed in high-premium firms.

During the same period, wages at the bottom have grown faster than middle and top percentiles. We provide suggestive evidence that these dynamics are associated with French labor market institutions' nature, reinforcing the findings of recent literature (Bilal & Lhuillier 2021; Bozio et al. 2020; Kramarz et al. 2022). The mix of a significant increase in the minimum wage and the elimination of all employer-paid payroll taxes around it has proven successful at increasing redistribution and balancing the contemporaneous rise in between-firm inequalities.

This paper contributes to the existing literature in three main ways. Firstly, we create a new, quasi-exhaustive matched employer-employee dataset for France which enables us to provide the first decomposition of log-wage variance and its evolution from 2002 to 2019[3]. This dataset allows us to document the increase in segregation and sorting, alongside the decrease in within-firm inequalities.

Secondly, we propose a novel method to correct for the well-known "limited mobility" bias in the measurement of sorting through AKM models described in Abowd et al. (2004), Andrews et al. (2008), and Bonhomme et al. (2023). This bias stems from the limited number of observations available for individual firm and worker parameters. Individual parameter estimations remain consistent, but the variance of the error term is underestimated. We use a split-sampling strategy (building on Chanut (2018), Kline et al. (2020) and others) and provide proof that, under reasonable hypothesis analogous to Kline et al., split-sampling corrects the limited mobility bias on quadratic terms. We also implement Bonhomme et al. (2019) firm clustering method (without random effects) and find the results coherent with split sampling. Both Card et al. (2013) and Song et al. (2019) acknowledge the mobility bias in sorting measurement but assume

[3]The AKM model of log-wages with additive workers and firm fixed effects was originally estimated on French data: a panel sample of 1/24th of French wage earners (without civil servants) from 1976 to 1987. The paper inspired much subsequent work on matched employer-employee datasets, most of them from countries where exhaustive, panelized administrative data was available to researchers: this exhaustivity proved essential to the quality of the estimation for these models. We build such a dataset for France, to bring AKM back to the state of the art in its original land. A first panelization of the BTS was developed in Godechot et al. (2020). We improved the algorithm and made it public for further use cf. Appendix A3.

its stability over time. Our corrected results challenge this assumption, revealing the potential impact of the mobility bias on observed dynamics.

Lastly, to gain a deeper understanding of the rise in segregation and sorting, we employ detailed descriptive statistics. We show that most of the rise in between-firm inequalities is driven by shifts in the composition of firms and changes in labor demand for specific occupations. We observe a stronger growth in two dimensions: the size of the workforce in firms employing high-premium, high-wage workers, and the number of firms offering low premiums. Specifically, the group of high-premium, high-wage firms experienced a consistent expansion over time, characterized by an increased presence of managers and engineers, accompanied by a decline in the proportion of blue-collar workers and associate professionals. On the other hand, the emergence of new firms with low premiums was accompanied by specialization in personal services and increased employment of health and social workers.

These trends reflect a structural evolution in the division of work between firms, arising also through outsourcing dynamics - already outlined in other contexts (Dorn et al. 2018; Drenik et al. 2023; Goldschmidt & Schmieder 2017) - and recently studied in France (Bergeaud et al. 2021; Bilal & Lhuillier 2021; Godechot et al. 2023). We rule out alternative stories potentially related to the rise in between-firm inequalities. We do not find evidence of an increasing return to skills and a change in firms' rent-sharing behavior.

Our findings provide insights into the dynamics of wage inequality and offer an explanation for the apparent divergence between France and other comparable countries. While France has managed to contain wage inequalities through institutional measures, the underlying dynamics driving segregation and sorting remain consistent with those observed elsewhere. The persistence of segregation and sorting in France highlights the strength of these processes, and their successful mitigation within the French context suggests that similar institutional tools could be effective in addressing inequalities elsewhere.

Section 2.2 provides an overview of our data construction process. In Section 1.3, we outline the methods used, including the variance decomposition of log-wage based on the AKM model, and the correction of bias through split-sampling or firm clustering. The results are presented in Section 1.4, followed by a discussion of our methods and findings in Section 1.5.

1.2. Data

1.2.1. Building an Exhaustive Pseudo-panel

We utilize BTS data, which stands for "Base tous salariés", or "all wage-earners file"[4]. It is an exhaustive yearly dataset built from tax returns files provided by firms on their payrolled employees. This dataset serves as the source for French official statistics on wage evolution.

To conduct panel analysis, we need to address the issue of pseudonymity in the data. Each individual is assigned a unique identifying code that changes every year, allowing for cross-sectional analysis but not for long panel analysis. In France, panel analysis on matched employer-employee wage data is typically performed using the "BTS panel" or "DADS panel", which is a narrower panel constructed from a sample of 1/24th of the data before 2002 and 1/12th after. This "narrow panel" samples the same individuals as a permanent demographic panel. The sampling also facilitates additional data quality control and correction work that would be more challenging with the exhaustive data. The oldest years of this narrow panel were the basis for the original AKM. The narrow panel remained the basis for later AKM estimations on French data, notably in Abowd et al. (2006) and Coudin et al. (2018).

However, since 1999, AKM models have been more accurately estimated in countries where researchers have had access to exhaustive panel data, such as the USA, Germany, Sweden, Austria, Italy, Norway, and Denmark. The reduction in sample size and precision due to sampling raises uncertainty in the estimation process. For instance, in the narrow panel, firms need to be approximately 12 times larger to have their firm fixed effects estimated with the same precision as in the exhaustive data. This decrease in precision introduces a larger "limited mobility bias" for variance and sorting estimates. Additionally, the identification of AKM models relies on mobile workers moving between firms, and this is only possible within the group of firms interconnected through such workers. Sampling drastically reduces the proportion of firms belonging to the main connected component and biases the estimation towards larger and more connected firms.

Each yearly BTS file for a given year contains data for both the current year and the

[4]Formerly known as "DADS", which stands for "déclaration annuelle de données sociales" or "annual social data declaration". DADS were the main source for BTS, completed with other administrative sources (public sector data for instance), and was progressively replaced by a new administrative source, the DSN ("déclarations sociales nominatives") starting in 2016.

previous year. This overlap allows for matching between yearly files based on common information, such as establishment ID, gender, number of hours, job duration, start and end dates of the job, municipality of work and residence, earnings, and age. The matching procedure provides a single match for approximately 98% of individuals between 2002 and 2019, except for the period 2016-2018 where it drops to 91%-93%. However, there are rare cases where matching is not possible, such as when all matching variables are identical for several individuals or when individual data is modified between yearly files. Workers who are not matched due to career interruptions or matching misses still appear in the panel, but they are represented by multiple identification numbers. This results in an almost exhaustive pseudo panel that we refer to as the "wide panel."

Before 2002, the matching procedure is not applicable due to the lack of linkage between various jobs of a single individual in the files. It is not possible to track a worker through different employers, even within the same year. Matching is only feasible for workers who held the same job for two consecutive years. As a result, the AKM estimation cannot be performed before 2002[5].

We have computed long-term series on sorting from 1976 to 2019 using the narrow panel, which provides wage and career information since 1976 with some missing years and varying data quality[6].

To supplement the analysis, we incorporate exhaustive firm financial data from administrative sources (FICUS/FARE files), which are matched to the wage files and provide information on value-added per worker and total workforce at the legal unit level. We use legal unit identification numbers (SIREN) as our empirical units for firms[7]. Estimates based on establishment identification numbers (SIRET) are very similar[8].

1.2.2. Sample Restrictions

Following similar works, we exclude public workers, both for comparability and data quality reasons[9]. We focus on ordinary jobs, excluding subsidized contracts, interns, and apprenticeships. Both men and women are included in the analysis. To ensure data preparation simplicity and avoid significant disconnection and high sorting between

[5]The matching procedure is detailed in Annex A3.

[6]Historical series in the Annex, Figure A.1.

[7]Our approach aligns with other studies that have applied the AKM method to analyze French data. More recently, Insee has started to provide datasets of groups, built from financial links between legal units. There is not enough historical depth yet to measure evolution at this observational unit level.

[8]See Table A.3.

[9]Most notably, public servants are not included before 2009.

distant geographic regions, we limit our analysis to metropolitan France.

We divide the data into three adjacent six-year periods: 2002-2007, 2008-2013, and 2014-2019[10]. Each observation consists of a worker / firm / year triplet, where each individual worker is associated with the firm from which she earned the most during the year (or, when equal, for which she worked the most). We refer to these observations as a "wage", or a "job" for convenience. Each worker can appear up to six times during each sample period.

We have information about the number of hours paid, which is rare in this kind of data. Without it, it is common practice in the literature to set a minimum wage for inclusion and exclude women to reduce the risk of misidentifying part-time workers. We can keep both part-time and full-time employees and avoid these exclusions. Our target variable of earnings is the log hourly gross wage (including employees' social contributions, but not employers'). We restrict the sample to individuals employed for the full year to minimize the impact of annualized payments[11]. We limit the impact of possibly erroneous extreme values with some additional selection. We exclude jobs with an hourly wage inferior to 80% of the legal minimum hourly wage for the corresponding year, or above 1000 times the minimum hourly wage, with less than 100 work hours a year, and observations with missing values for sex, age and employer. We keep only workers between the ages of 16 and 70. These restrictions are applied after matching when the wide panel is already constructed, selecting specific observations while retaining individuals who have been working during the year or receiving unemployment benefits. All sample restrictions might incur selection effects, that could change in time. The restriction to workers employed for the full year, for instance, excludes workers that are more frequently at the bottom of the wage distribution.

The estimation of AKM models relies on connected sets of firms and workers. Two sets of firms are considered disconnected if no worker in the data has worked for two different firms, one from each group. A connected set includes all workers who have ever worked in the firms belonging to that set. Estimations are performed on the main connected set for each period. Descriptive information characterizing both the full population and the largest connected set is provided in Table 1.1.

In our historical series computed on the narrow panel, paid hours are not reliable

[10] Card et al. (2013) divide their 1985-2009 data into four overlapping seven-year panels, and Song et al. (2019) divide their 1980-2013 data into five adjacent six-year panels.

[11] We found similar results when widening the selection to all individuals whose main job during the year lasts more than 90 days (Table A.8). In this larger sample, the connectivity is better and the limited mobility bias is reduced.

TABLE 1.1. Summary statistics

				Log-Hourly Wage	
	Person*yr	Individuals	Firms	Mean	Std.Dev.
	Overall Sample				
2002-2007	65,457,069	21,356,960	1,203,830	2.67	0.46
2008-2013	68,998,598	18,595,890	1,232,452	2.81	0.45
2014-2019	67,928,369	20,894,672	1,397,646	2.91	0.46
	Largest Connected Set				
2002-2007	58,666,317	18,842,389	536,814	2.69	0.46
2008-2013	61,413,372	16,266,899	560,778	2.83	0.45
2014-2019	59,550,287	18,065,244	564,113	2.94	0.46

Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. The largest connected set entails the group of firms connected by worker mobility.

before 1996, but job duration in days and an indicator variable for part-time jobs exist since 1976. There were other changes in variables in the 44-year period. For instance, the distinction between the public and private sectors is not consistent[12]. Because of these changes, it is not possible to precisely replicate the selection criteria used in the 2002-2019 wide panel. We use a slightly different selection based on Insee's long-term, private sector series selection. In our long-term series, we also compute sorting on the daily wage of full-time workers. While the level of sorting is lower than the hourly wage narrow panel sorting, the trends are generally parallel in the post-1996 period where both measures are available.

[12]Partly because of nationalization and privatization of major firms, such as banks, airlines, posts, and telecommunications, etc. over the period that is difficult to track in the data.

1.3. Methodology

1.3.1. The AKM Model

We follow AKM with an additive model of log-wages :

$$y_{it} = \beta x_{it} + \theta_i + \psi_{j(i,t)} + u_{it} \tag{1.1}$$

Here y_{it} is the logarithm of the hourly wage of worker $i = 1, 2, ..., N$ during year $t = 1, ..., T$, demeaned by the average log-hourly wage for all workers during year t so that $\bar{y}_t = 0$[13]. Time-varying covariates x_{it} are limited to age and age squared. θ_i is the fixed effect of individual worker i, and ψ_j is the fixed effect of individual firm $j = 1, 2..., J$, both supposed constant in time for the duration of the panel, firm $j(i,t)$ being the employer of worker i during year t. u_{it} is the idiosyncratic error term. We further note $\mathbf{F} = (\mathbf{1}_{j=j(i,t)})$ the $N^* \times J$ matrix of the bipartite graph of workers/firms connections through time, with $N^* = NT$.

This model is built on two notable hypotheses. Firstly, it assumes no interaction effect between firm type and worker type, meaning that the fixed effects are additive in the log-wage. This assumption posits that the firm-specific wage premium will be the same for all workers regardless of their characteristics, such as gender, age, or skill level. Secondly, the model assumes exogenous mobility, where the residual term u_{it} has a null expectation conditional on the variables x_{it}, i, t, and j, as is traditionally assumed. Additionally, it is conditioned on the matrix $\mathbf{F}$. This hypothesis implies that wages before or after a job change, on average, remain the same as if there had been no job change.

Although both hypotheses can appear unrealistic and have been subjected to scrutiny, they seem to provide reasonable approximations. Bonhomme et al. (2019) build a model that allows for interaction and finds only slight departures to the additive linear model. Di Addario et al. (2023) develop an extension of the two-way fixed effects model à la AKM with two firm fixed effects. They add to the fixed effect for the "destination" firm hiring the worker (the classic firm fixed effect) a fixed effect for the "origin" firm,

[13] We also checked with a model inspired by Card et al. (2018), where log-wages are not centered, years are right-hand explanatory variables and age is included as a cubic polynomial constrained to be flat at 40 to avoid collinearity. Results in Table A.7 are generally robust to this change in specification but show a lower rise in sorting. Figures A.7A and A.8A suggest this specification is more sensitive to firm fixed effect changes during the great recession. This alternate specification addresses the same classic underlying problem that age, generation and year effects are not separately identifiable without an additional source of variation or constraining hypothesis.

reflecting the wage level necessary to "poach" a worker from a given firm. They found that destination effects are more than thirteen times as variable as origin effects across firms, implying that a more dynamic specification does not increase much the model explanatory power. Another potential specification error results from the possible evolution of "fixed" effects. Firm premium might change with time, and workers' age profiles are heterogeneous, as shown in the French case in Magnac and Roux (2021) on BTS data. Formally, this simply contradicts the hypothesis of a null conditional expectation for the residual term. Regarding year-to-year variations in firm premium at least, Engbom et al. (2023) and Lachowska et al. (2023) provide some reassurance: firm premium are mostly stable in time, even if their variance appears somewhat procyclic. We analyze this source of heterogeneity in Annex A4.2 and find signs of procyclicality[14]. Still, we find strong evolutions in age effects variance and covariances, that are difficult to interpret. We cannot separately identify age, generation and year effects, and cannot exclude that specification errors interact with real changes, such as changes in firm premium with time, age composition evolution of the population of workers (linked for instance to retirement age reforms), and generational rise in human capital.

1.3.2. Log-wage Variance Decomposition

Following Card et al. (2013) and Song et al. (2019), we take $V(y) = Var(y_{it})$ as a measure of wage inequalities and observe its evolution through 3 six-year periods: 2002-2007, 2008-2013 and 2014-2019. Like them, we also assume the AKM model to correctly describe wages and use it as the basis for variance decomposition. Ignoring for simplicity of exposition the time-varying workers variables x_{it}, we can describe for each period a decomposition of $V(y)$ as a sum of the variances of θ, ψ, u, and their respective covariances, estimated over all worker-years observations:

$$V(y) = V(\theta) + V(\psi) + V(u) + 2Cov(\theta, \psi) \tag{1.2}$$

Song et al. further distinguish within-firms and between-firms components of wage variance, and extend the law of total variance $V(y) = E[V(y|j)] + V[E(y|j)]$ to:

[14] More generally, de Chaisemartin and d'Haultfoeuille (2020) and de Chaisemartin and D'Haultfoeuille (2023) raised an alarm about the consequences of heterogeneity in panel treatment effects or, in our case, fixed effects. The problem does apply here if the AKM model is ill-specified, and we are not aware of any existing solution in this setting.

$$V(y) = \underbrace{V(\bar{y}_j)}_{\text{Between-firm component}} + \underbrace{\sum_j m_j \times V(y_i | i \in j)}_{\text{Within-firm component}} \tag{1.3}$$

$$V(y) = \underbrace{V(\psi) + 2Cov(\bar{\theta}_j, \psi) + V(\bar{\theta}_j)}_{\text{Between-firm component}} + \underbrace{V(\theta_i - \bar{\theta}_j) + V(u)}_{\text{Within-firm component}} \tag{1.4}$$

With $\bar{y}_j = \bar{y}_{j(i,t)}$ and $\bar{\theta}_j = \bar{\theta}_{j(i,t)}$ the respective expectations on i, t in firm j. By hypothesis, the analogous $\bar{u}_j$ is equal to 0. All moments of the distribution of firm variables are weighted by the share m_j of each firm in the total number of observations either directly as in equation 1.3 or implicitely when when computing variance over all (i, t) observations as in equation 1.4. Our interest lies first with the evolution of the sorting component of this decomposition, $2Cov(\theta, \phi)$, which is by construction entirely contained in the between-firm component of wage variance. Following Song et al. (2019), we define segregation in this context as $V(\bar{\theta}_j)$. Segregation captures the extent to which high-wage workers tend to work with one another, and low-wage workers with one another. Like other dimensions of segregation (residential, school, etc.), this has a strong impact on social mixing, but no direct impact on overall wage inequality, since the increased between-firm variance stems from a decrease in within-firm variance, leaving the overall distribution unchanged[15].

1.3.3. Limited Mobility Bias

The so-called "limited mobility bias" could also be seen as an incidental parameter bias, impacting the inference if we were interested in estimating only a few parameters (such as the effect on age), or as overfitting in a statistical learning context. It is similar to the classical bias on variance estimand that one corrects with a finite sample variance correction (i.e. the $N/(N-1)$ factor). The importance of the bias in our context stems from three elements: the very high number of parameters to be estimated with a low number of observations each, our focus on quadratic transformations of these noisy estimates (variance and covariance), and the sparse, centralized and clustered network structure of the design matrix that can be close to collinearity. This last aspect is due to

[15]We also compute Song et al. (2019)'s "Segregation Index" as $Var(\bar{\theta}_j)/Var(\theta_i)$ when possible.

the network of workers' mobility and explains the name of the bias.

More intuitively, if two large groups of firms are connected only through a handful of mobile workers, the identification of the relative premiums of the two groups will rest on these few observations. The parameters' estimates are not biased, but the high estimation errors translate into a bias in our variance terms: individual and, especially, firm effects variance is overestimated, while their covariance, which measures sorting, is underestimated.

Several correction strategies have been put forward in the literature. Andrews et al. (2008) directly correct estimates with a bias correction factor derived from an estimate of the error term variance, with a hypothesis of homoscedasticity that is unrealistic because of the networked nature of the estimation error (Jochmans & Weidner 2019). Borovičková and Shimer (2017) model heterogeneity as random effects rather than fixed effects and find a much higher sorting than previous estimates, but while fixed effects models allow for further study of the distribution of this heterogeneity, it is much more difficult with random effects models. Bonhomme et al. (2019) cluster firms based on the distance between their wage distributions, then estimate a wage model where workers' effects are treated as random effects. The clustering creates a dense mobility network with many observations per cluster, that allows for the estimation of richer models, including interaction and dynamic terms, at the price of the additional hypothesis that clusters are correctly identified. Kline et al. (2020) leave-one-out strategy amounts to the bias correction factor method of Andrews et al. (2008) compatible with heteroscedasticity but is complex and computationally costly on large datasets. We use an analogous but much simpler split-sampling strategy by applying only one split to our data, rather than a leave-one-out method with as many splits as observations. The idea is that fixed effects are estimated independently in each split so that covariance between firm effects estimated in one sample and workers effects estimated in the other is a debiased measure of sorting. Chanut (2018) introduces split-sampling in an identical setting with French data and shows, on a toy example, that it corrects the bias. Other authors in similar settings implement split sampling either for instrumental variable estimation or to compare various groups of workers, typically without explicitly mentioning that it corrects the limited mobility bias: Drenik et al. (2023), Goldschmidt and Schmieder (2017), Gerard et al. (2021), Sorkin (2018) and Schoefer and Ziv (2022). In 1.3.3 we add to these works by generalizing the idea and providing proof that, under reasonable hypothesis analogous to Kline et al., split-sampling corrects the limited mobility bias on quadratic terms. We also implement Bonhomme et al. (2019) cluster method in 1.3.3

(without random effects) and find the results coherent with split sampling.

Formally, we follow Kline et al. (2020) for the description of this bias. They provide a simple framework that neatly generalizes to any quadratic form of the estimated parameters. We start with a simplified notation of our linear model with only one parameter vector and one observation index i (replacing the (i, t) couple in the detailed model):

$$y_i = z_i'\alpha + u_i \tag{1.5}$$

With $\alpha = (\beta, \theta, \psi)$ our parameter vector of length $k = 2 + N + J$ and z_i the non-random regressors vector of the (worker * year) i's observation characteristics, including the indicator vector for worker and firm. We note $S_{zz} = \sum_{i=1}^{N^*} z_i z_i'$ the design matrix (with full rank when we limit the sample to the main connected set). Our objects of interest are (weighted) variances and covariances of parts of the α vector and can be described as quadratic forms $\omega = \alpha' A \alpha$ for a chosen symmetric matrix $A \in \mathbf{R}^{k \times k}$. We can choose A so as to compute the quantities studied here, weighted by the number of worker-year observations[16].

Our naive plug-in estimator for ω is thus $\hat{\omega}^{PI} = \hat{\alpha}' A \hat{\alpha}$ with $\hat{\alpha}$ an OLS estimate $\hat{\alpha} = {S_{zz}}^{-1} \sum_{i=1}^{N^*} z_i y_i = \alpha + {S_{zz}}^{-1} \sum_{i=1}^{N^*} z_i u_i$. $\hat{\alpha}$ contains $\hat{\theta}$ and $\hat{\psi}$ which we later name *WFE* (for workers fixed effects) and *FFE* (for firms fixed effects). The estimation error in $\hat{\alpha}$ will result in a systematic bias in $\hat{\omega}^{PI}$ equal to a linear combination of the unknown and possibly heteroscedastic variances σ_i^2 of the error terms u_i. From classic results on quadratic forms, Kline et al. deduce :

$$\mathbf{E}[\hat{\omega}^{PI}] - \omega = trace(A\mathbf{V}[\hat{\alpha}]) = \sum_{i=1}^{N^*} B_{ii}\sigma_i^2 \tag{1.6}$$

With $B_{ii} = z_i' S_{zz}^{-1} A S_{zz}^{-1} z_i$ representing the influence of each (squared) error term on the plug-in estimator. This bias exists for all linear models, but usually for a small parameter dimension k the S^{-2} term insures relatively fast convergence. Here however k is large, and so is, potentially, B_{ii}. Moreover, the complex structure of the design

[16]For instance, var(WFE) is computed with a matrix A filled with 0 except for the $N \times N$ square corresponding to the N WFE estimates of the parameter vector, which we fill with a generic term $-1/N$ and a diagonal term $1 - 1/N$. Covariances objects are built with analogous matrices, with for instance a $1 - m_j$ term for the $(2 + i, 2 + N + j)$ and $(2 + N + j, 2 + i)$ positions in the matrix if the worker i is in firm j and a generic $-m_j$ term if the worker i is not working in firm j.

matrix, reflecting the complex network of worker / firm connections, is present both in matrices S and A when computing $cov(\theta, \psi)$, leaving way to even stronger bias.

This expression for the bias leads Kline et al. to an obvious correction strategy: estimating the σ_i^2 error terms, and thus the bias itself. This can be done with a leave-one-out strategy that is computationally costly, adding a factor of the order $N^* = NT$ to the computation. Kline et al. provide a more tractable estimation method through a high number of random projections (in the hundreds). Bonhomme et al. (2023) still finds the method demanding and further approximates it, though they worry the succession of approximate estimations (with those usual in AKM models) might have consequences that are not well understood.

For purely computational reasons, we favor a split-sampling strategy that only demands two estimations on two half-samples, at worst doubling computing time[17].

Split-sampling Bias Correction

With $\hat{\omega}^{SP}$ the split-sampling estimate of any quadratic form ω of the parameters, we show in Annex A4 that the bias is

$$\mathbf{E}[\hat{\omega}^{SP}] - \omega = trace(AS_{zz,1}^{-1}\mathbf{E}(B)(S_{zz,0}^{-1})') \tag{1.7}$$

With the indices 1 and 0 indexing the two split samples I_1 and I_0 and B the matrix with the generic term:

$$b_{l\,m} = \sum_{i \in I_1}^{N_1} u_i z_{l,i} \sum_{j \in I_0}^{N_0} u_j z_{m,j}$$

This term has null expectation under mild conditions identical to Kline et al.: 1. null conditional expectation $\mathbf{E}[u|z] = 0$ and 2. independence of $u_i, i \in I_1$ and $u_j, j \in I_0$. If the variance-covariance matrix of u is diagonal, the bias disappears whatever the matrices A and S_i might be. The second condition might be violated if for instance u_i are correlated for different years of the same employer / employee pair, which is likely. We do not derive here the complete conditions for consistency, nor the convergence rate when the number of parameters rises with the number of observations. We follow

[17] We also leveraged the lower computation time of the R package *fixest* (Bergé 2018), up to ten times faster than the R package *lfe* (Gaure 2013) or the Stata package *reghdfe* (Correia 2016), although we used all three packages depending on the setting.

in this most of the AKM decomposition literature, which implicitly relies on the big size of the data used. However, consistency and convergence depend on the variance of error terms but also on relations between the quadratic form matrix A and the design matrix S, hence on the mobility network. We refer to the proofs and discussion in Kline et al. in the different but related leave-one-out context.

Of course, there is an additional cost of split-sampling in increased uncertainty coming from the reduced effective sample size ($S_{zz,s}$ has half the observations of S_{zz}). With our data, we observe that this uncertainty is small compared to the size of the bias reduction effect. One can reduce this uncertainty by repeatedly estimating the quadratic form through split-sampling and averaging the results. The procedure reaches arbitrary precision, only limited by the computational cost. We also report Monte Carlo experiment results and standard deviations computed on multiple random splits in Annex A4, which confirms the stability of the procedure and offers some reassurance about consistency and convergence rate.

The main limitation of split sampling is the impact of the split on the bipartite graph and its main connected set, and the sample-splitting strategy has to be considered in this regard. In each split sample, the main connected set is smaller than in the original sample and both are distinct so that the common sample of workers and firms belonging to the main connected set in both split samples is reduced and so is the corresponding parameter vector of individual effects.

The most simple split strategy is a direct random split of observations in two equally sized samples. By balancing the sampling by workers, and splitting for each worker the periods of observation, one increases the odds that each worker is present in both samples' main connected set. We dub this method "period splitting". On the contrary, by splitting individuals rather than observations, one increases the connectivity in each set (because individual careers are kept intact), but each worker's fixed effect is estimated only once: one loses the capacity to correct the $var(\theta)$ and $var(u)$ quadratic forms through split sampling. If this splitting of individuals is balanced by firm, it increases the odds that each firm's fixed effect is estimated in each sample. We dub this method "firm splitting"[18]. With firm splitting, each firm with two workers or more is present in both samples and belongs to each main connected set if it remains connected with each random half of its employees.

Period splitting might not completely correct the limited mobility bias for the reason mentioned above: it is likely that u_i are correlated for several observations of the same

[18]Chanut (2018) describes an algorithm to compute this split.

employer / employee pair. This problem is attenuated by the specifics of our setting. Because we keep only observations with full-year jobs, movers are generally observed only five years or less among the six in the panel. It is less likely that after the random split of these five observations or less, an individual worker would remain a mover in both samples. Because the estimation relies exclusively on movers, a given individual residuals would generally not be correlated to errors on both sides of the split. Still, by keeping all observations of one individual on one side of the split, the firm splitting method avoids entirely this drawback. Consequently, we favor firm splitting to compute a debiased sorting effect, but use period splitting to compute the complete variance decomposition.

Firm Clustering

We also implemented Bonhomme et al. (2019) strategy (Table A.6). We ran a firm-clustering algorithm with 1000 clusters (explaining around 90% of the between-firm dispersion in earnings for each period) before estimating AKM on firm clusters (rather than individual firms), with the hypothesis that firms' fixed effects are discretely distributed with a small number of values. The mobility network between clusters is very dense and each cluster's fixed effect estimate has very low variance, thus correcting the limited mobility bias. The clustering algorithm is a kmeans clustering based on quantiles of the wage distribution, as the identification of clusters can not rely on firm mean wage and must use higher moments of the distribution of wages[19]. Even so, it remains plausible that the segregation of workers could bias the clustering, with firms being clustered based on some combination of their own fixed effects and their average workers' fixed effects. An AKM estimation following this procedure would then show higher sorting, and lower cluster effect variance than is really the case. Bonhomme et al. (2019) acknowledge the risk, mention job-market models that satisfy the conditions for cluster identification,[20] and provide in-depth robustness analysis that suggests it is of limited impact in practice. More generally, Bonhomme et al. (2022) provides theoretical conditions and convergence results for the *two-step grouped fixed-effects* (GFE) method, where the clusters are viewed as an approximation of an underlying continuous

[19] Following the original Bonhomme et al. (2019) specification, we do not add additional firm variables to feed the clustering algorithm. Such developments are possible.

[20] "In some environments without firm capacity constraints, such as Postel–Vinay and Robin (2002), the upper bound of earnings in the firm is increasing in firm productivity, so firm-specific distributions are all different and firms may be consistently classified based on their earnings distributions. It is difficult to obtain similar guarantees in models with capacity constraints" (p. 217).

unobserved heterogeneity. The relevant condition here is that there is an injective relationship between this unobserved heterogeneity and the moments of the distributions of the observed variables used in the k-means algorithm.

Compared to split-sampling, firm-clustering comes with this additional hypothesis and the added approximation and computing cost from the clustering step but allows for more versatile models and uses the whole data[21]. And while split-sampling is fast and only relies on AKM hypotheses, it further reduces the estimation sample to individuals or firms belonging to the main connected components in each split. We view the convergence of both methods as a strong argument in favor of the robustness of our results.

1.4. Results

1.4.1. A Robust Rise in Segregation and Sorting

To conduct the baseline variance decomposition, Table 1.2 applies two variance decompositions (Equations 1.2 and 1.4) to the estimates from AKM, corrected using firm split sampling. The correction involves splitting the data at the individual level and balancing it by firm (see Section 1.3.3). However, our baseline correction method does not allow for the computation of corrected and distinct estimates of $Var(u)$ and $Var(\theta)$. To provide a comprehensive analysis of their evolution, we rely on several alternative specifications, which are presented in Annex A1.

As shown in Figure 1.1, the analysis reveals an increase in between-firm inequalities and a decrease in within-firm inequalities. The overall log-hourly wage variance slightly increased from 2002-2007 to 2014-2019[22]. France's quasi-stable wage inequalities during this period are atypical among developed countries. Nevertheless, the rise in between-firm wage inequalities aligns with the findings of Song et al. for the US and Card et al. for Germany.

[21] Following BLM, we cluster firms using the empirical distribution of earnings of stayers only. As a consequence, only firms with at least one stayer in each period are selected. This explains the discrepancy in Table A.6's last row and Table 1.1' overall sample person/yr observations.

[22] It is important to note a qualitative discrepancy between the overall trend and the specific yearly log-hourly wage variance as depicted in Figure 1.1 This discrepancy can be attributed to small methodological breaks in the original data, which were corrected in the graphical evidence but could not be accounted for in the AKM estimations.

TABLE 1.2. Decomposition of wage variance and its evolution - Split-sampling with firm split

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.211		0.207		0.219		0.008	
	Var (θ)	**	**	**	**	**	**	**	**
	Var (ψ)	0.014	6.7	0.014	6.8	0.013	5.8	-0.001	-19.3
	Var(Xb)	0.019	9	0.025	12.1	0.040	18.2	0.021	272.5
	Var(u)	**	**	**	**	**	**	**	**
	2*Cov(θ.ψ)	0.025	12	0.026	12.6	0.028	13.0	0.003	40.8
	2*Cov(θ.Xb)	-0.007	-3.3	-0.017	-8.2	-0.039	-17.8	-0.032	-419.6
	2*Cov(ψ.Xb)	0.002	0.8	0.001	0.7	0.001	0.6	0.000	-5.8
Between-firm variance	Var($\bar{y}$)	0.088	41.5	0.094	45.3	0.103	46.9	0.015	197.7
	Var ($\bar{\theta}$)	0.041	19.6	0.049	23.6	0.058	26.5	0.017	218.7
	Var (ψ)	0.014	6.7	0.014	6.8	0.013	5.8	-0.001	-19.3
	Var($\bar{X}\bar{B}$)	0.003	1.4	0.004	1.9	0.006	2.7	0.003	39.1
	2*Cov($\bar{\theta}$.ψ)	0.025	12.0	0.026	12.6	0.028	13.0	0.003	40.9
	2*Cov($\bar{\theta}$.$\bar{X}\bar{B}$)	0.000	0.2	-0.002	-0.9	-0.005	-2.4	-0.006	-73.8
	2*Cov(ψ.$\bar{X}\bar{B}$)	0.002	0.8	0.001	0.7	0.001	0.6	0.000	-5.8
Within-firm variance	Var($y - \bar{y}$)	0.124	58.5	0.113	54.7	0.116	53.1	-0.007	-97.7
	Var ($\theta - \bar{\theta}$)	**	**	**	**	**	**	**	**
	Var($Xb - \bar{X}\bar{b}$)	0.016	7.6	0.021	10.3	0.034	15.5	0.018	233.3
	Var(u)	**	**	**	**	**	**	**	**
	2*Cov($\theta - \bar{\theta}$.$Xb - \bar{X}\bar{b}$)	-0.007	-3.4	-0.015	-7.2	-0.033	-15.2	-0.026	-340.4
	2*Cov($\theta - \bar{\theta}$, u)	**	**	**	**	**	**	**	**
	2*Cov($Xb - \bar{X}\bar{b}$,u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.2
N* (firms in both largest connected sets)		51,932,308		54,530,553		52,921,242			

Note: "Comp." is the component of variance. "Share" is the share (in %) of total $var(y)$, or total change in $var(y)$ in the last column. All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Estimation on firms present in both main connected components in each split sample. Decomposition based on Equations 1.2, 1.3 and 1.4. The split-sampling method is described in Section 1.3.3.
** : These parameters' estimates are not corrected by firm-split

Decomposition of the Rise in Between-Firm Variance

The increase in between-firm variance can be decomposed into several components (Equation 1.4): the rise in $Var(\bar{\theta})$ reflecting segregation, the rise in $2Cov(\theta, \psi)$ capturing sorting, the decrease in firm premium variance $Var(\psi)$, and age effects. The dynamics of age effects are uncertain and difficult to interpret, as already mentioned. Focusing on the

robust findings, it can be concluded that the growing gaps between firms are primarily driven by the rise in segregation and sorting. In 2002-2007, segregation accounted for 19.6% of total log-hourly wage variance, increasing to 26.5% in 2014-2019. Sorting represented 12% of inequalities in the first period and 13% in the last.

The rise in segregation is strong and consistent across different specifications. This finding complements previous research of Godechot et al. which shows a consistent increase in workplace segregation in 12 advanced capitalist economies (including France), with the top 10 and 1 percent earners increasingly isolated from the rest of the wage distribution. The increase in sorting, while comparatively limited in size, is also robust across various checks conducted.

Limited Mobility Bias and Correction

The rise in sorting can be observed even before correcting for the limited mobility bias or when restricting the analysis to firms with more than 20 observations per year. Figure 1.2 presents the results obtained from the principal corrections discussed in Sections 1.3.3 and 1.3.3[23].

The measure of sorting is affected by a substantial limited mobility bias. While the AKM estimate attributes 1% to 5% of overall wage inequality to sorting, the baseline corrected estimates attribute 12% to 13%. The firm-clustering method produces results that closely align with the split sampling method and confirm the magnitude of the bias.

Importantly, the limited mobility bias is not neutral. The intensity of the rise in sorting is lower in the corrected estimates, suggesting an overall reduction in the limited mobility bias during the period, possibly due to increased mobility and connectivity in the firm-workers network. This reduction in bias is primarily concentrated in the smallest firms (see Table 1.3).

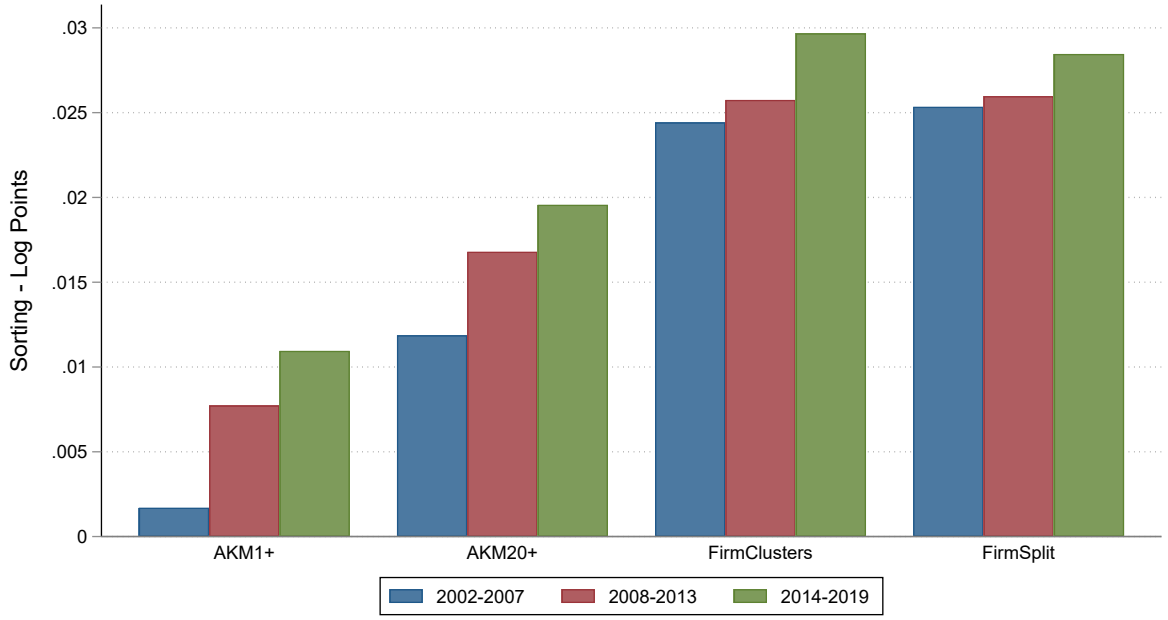
These findings highlight the importance of using corrected estimates to capture the dynamics of sorting accurately.

Historical Trends

To provide further insights, we document the chronology of the rise in sorting and segregation from 1976 onward using long-term series (see Figure A.1) and yearly decom-

[23]See Tables A.1, A.2, 1.2, A.5 and A.6 for the full decomposition uncorrected and with the different corrections.

FIGURE 1.2. Sorting over time - Baseline and selected correction strategies



Notes: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. This figure reports the estimates of sorting - by period - coming from our baseline AKM estimate, an AKM estimate limited to firms with at least 20 employees, and the two bias correction strategies described in Sections 1.3.3 and 1.3.3.

TABLE 1.3. Sorting decomposition by firm size group and over time

		2002-2007				2014-2019			
		< 20	20 – 200	200 – 1000	> 1000	< 20	20 – 200	200 – 1000	> 1000
Split-Sampling	Overall			0.0254				0.0285	
	Between			0.0020				0.0023	
	Within	0.0233	0.0228	0.0238	0.0237	0.0245	0.0224	0.0262	0.0293
AKM	Overall			0.0017				0.0109	
	Between			0.0026				0.0027	
	Within	-0.0912	-0.0070	0.0205	0.0235	-0.0678	0.0025	0.0240	0.0296

Notes: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Split sampling with the firm split method. Firm size is computed on the observed workers employed by the firm in the panel. Sorting is computed as elsewhere as $2Cov(\theta, \psi)$, while between sorting is $2Cov(\bar{\theta}, \bar{\psi})$ with the average computed for each size group, and within is sorting computed separately for each size group.

positions from rolling panels in our main period of interest (see Figures A.7A and A.9A). Although long-term series are imperfect, they provide a clear indication of past trends.

The rise in sorting began in the mid-nineties, predating the period under study. The trend of increased segregation dates back even further, manifesting itself from the early eighties. The rise in sorting exhibits signs of pro-cyclicality in the most recent period, linked to the pro-cyclicality of the variance of firm premium. The variance of firm effects, measured for the same years on subsequent, overlapping panels, diminishes around 2008 and again in 2012. This decline likely reflects changes in firms' pay policies during and after the financial crisis and the Eurozone crisis (see Figure A.8A). The decrease in premium variance explains the decline in estimated sorting for a given year when measured in subsequent, overlapping panels around the time of the financial crisis. In contrast, segregation has been steadily increasing without breaks around the financial crisis (see Figure A.9A). These findings suggest that the rise in sorting resulting from pure matching and composition effects is likely even stronger than measured.

1.4.2. Explaining the Rise in Between-Firm Inequalities

In this section, we test which factors are behind the between-firm empirical trends we find. We move beyond variance and covariance and study other, non-quadratic distributional statistics of fixed effects[24]. For this reason, we use uncorrected estimates[25]. We examine several empirical arguments to sort through potential economic mechanisms. We find that the rise in segregation and sorting is mostly accounted for by composition effects in the population of firms and shifts in their occupational structure.

Firm Demography and Composition Effects

We get a first look at composition effects with a simple partition of all firms in the first and third periods into four categories. Fixed effects estimates are centered on 0 for each period. We partition firms depending on the sign of their estimated fixed effect (averaged over the two periods for firms present in both periods) and of the mean workers' fixed effect of their employees (similarly averaged). The partition distinguishes four "quadrants": covariance-increasing quadrants (high-wage firms with high-wage workers and low-wage firms with low-wage workers) and covariance-decreasing quadrants (low-

[24]We also experimented with panel regressions of firm-level workers and firms' fixed effects on firm-level variables, followed by period-specific covariance decomposition. The method is complex and generates numerous, hard-to-interpret interaction terms. Even so, most of the rise in sorting was accounted for by changes in the size and population of firms with given fixed effects, rather than in firm-specific changes in fixed effects. We focus here on simpler results conveying the same message.

[25]Other correction strategies might be available in this context, such as Jochmans and Weidner (2022). The split-sampling method could also possibly be adapted. We leave these corrections for future research.

wage firms with high-wage workers and high-wage firms with low-wage workers). Table 1.4 additionally differentiates between firms that are present in both periods and firms present either in 2002-2007 or in 2014-2019. The first column captures the number of workers where both average worker (W) and firm fixed effects (F) are above zero (F>0, W>0). Other columns map the rest of the possible combinations for F and W values.

TABLE 1.4. Composition effects: firm demographics and mean fixed effects by sorting quadrant

Firms present in both periods (2-period)

	Person-Year Observations				**Mean Worker Fixed Effects**				**Mean Firm Fixed Effects**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	F>0, W>0	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***
2002-2007	14,003,660	8,892,099	13,122,069	5,748,544	0.1834	-0.1235	-0.1419	0.1528	0.1166	0.0727	-0.1028	-0.1103
2014-2019	15,379,346	9,252,809	15,582,805	6,931,931	0.2433	-0.1389	-0.1817	0.1171	0.1105	0.0732	-0.0824	-0.0951
Diff	1,375,686	360,710	2,460,736	1,183,387	0.0599	-0.0153	-0.0398	-0.0357	-0.0061	0.0005	0.0204	0.0152

Firms present only in one period (1-period)

	Person-Year Observations				**Mean Worker Fixed Effects**				**Mean Firm Fixed Effects**			
	F>0, W>0	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***
2002-2007	3,609,701	4,516,577	5,585,022	3,188,645	0.2319	-0.2046	-0.1757	0.2022	0.1300	0.1222	-0.2065	-0.1179
2014-2019	2,612,327	2,636,805	4,357,807	2,796,457	0.2701	-0.2265	-0.1999	0.2138	0.1283	0.1205	-0.1960	-0.1236
Diff	-997,374	-1,879,772	-1,227,215	-392,188	0.0382	-0.0218	-0.0242	0.0116	-0.0017	-0.0017	0.0105	-0.0057

Notes: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. The largest connected set firms present in both periods and present either in 2002-2007 or in 2014-2019 are analyzed separately. Firms are further divided into four quadrants according to the value of the estimated worker (W) and firm fixed effects (F) (see Equation 1.1), averaged over both periods for staying firms. Worker and firm fixed effects are normalized so they are comparable across periods. Columns 1, 5, and 9 refer to the number of workers where both worker (W) and firm fixed effects (F) are above zero (F>0, W>0). The other columns map the rest of the possible combinations for F and W values. For firms present in both periods, the (not employment-weighted) 2-period average of firm and worker fixed effects are considered in the allocation to the four quadrants. Entries in columns 5-8 and 9-12 are the average worker and firm fixed effects, respectively, by quadrant.

Stayers firms grew in size,[26] remarkably so in the areas where both FEs are both positive and negative. In other words, even if fixed effects in every firm were the same in both periods, sorting would have increased simply because the size of stayers firms increased more in the sorting groups than in the non-sorting groups[27]. Notably, the

[26] By construction, the number of 2-period firms is the same in both periods.

[27] This is not due to a general change in size distribution. A significant shift in the size distribution over time could explain the rise in between-firm inequalities. Figure A.2 plots the fraction of firms below a given size, by period (2002-2007 vs 2014-2019). The two distributions overlap almost perfectly.

groups of firms that experienced significant growth in size were already characterized by relatively higher employment (Table A.9). This observation is further supported by the split-sampling findings in Table 1.3.

1-period firms in 2014-2019 have fewer person-year observations than those in 2002-2007. Firm age is likely to play a role here: 1-period firms in the 2014-2019 period are young and small, whereas 1-period firms in 2002-2017 are larger but shrinking before facing dissolution. Indeed, 1-period firms in 2014-2019 are on average smaller but more numerous, remarkably so in the low-F (F<0) quadrants (Table A.9).

The evolution of the distribution of jobs over the four firms' quadrants can be summarized as follows: low-premium firms grew in share of employment, especially low-W ones, increasing sorting. Meanwhile, the drop in high-premium firms entirely occurred among low-W ones, especially one-period firms, again increasing sorting.

Table 1.4 also uses the four quadrants to describe the evolution of fixed effects for a given group of firms between the periods. The change in the mean workers' fixed effects for a firm between 2002 and 2019 might come from workers' turnover, or from a change in the WFE estimation of a given worker in two different panels, like the change in FFE for a given firm. Columns 5 to 12 in Table 1.4 show a notable increase in the average worker fixed effects in positive-sorting, positive-FEs stayer-firms while a decrease in the positive-sorting, negative-FEs stayer-firms. On the contrary, firm effects demonstrate remarkable stability in high-F (F>0) firms and exhibit a slight upward trend in low-F (F<0) firms.

Table 1.5 presents simple descriptive data that help to investigate the role of occupations. We use the fixed effect quadrants already mentioned and map the evolution of the occupational structure for the two groups with the most important growth in size: surviving firms with either high-premium and high-wage workers or low-premium and low-wage workers. The color gradient allows for a visual representation of the magnitude and direction of the changes observed, highlighting the areas of significant shifts in the data: darker shades of red represent more negative changes and darker shades of green represent more positive changes. The group of high-wage, stayer firms have more managers and engineers in period 3 than in period 1, and fewer blue-collar workers and associate professionals. Low-wage, stayer firms, instead, have a very stable occupational structure.

TABLE 1.5. Change in the occupational structure by type of firm

		2-period, F>0, W>0			2-period, F<0, W<0		
Occupation	CS	2002-2007	2014-2019	Diff	2002-2007	2014-2019	Diff
Entrepreneurs	20	0.0050	0.0055	0.0005	0.0080	0.0059	-0.0022
Professionals	32	0.0076	0.0116	0.0040	0.0076	0.0196	0.0119
Artists and media professionals	35	0.0087	0.0093	0.0006	0.0008	0.0011	0.0003
Managers	37	0.1634	0.2163	0.0528	0.0356	0.0385	0.0028
Engineers	38	0.1460	0.2084	0.0624	0.0145	0.0169	0.0024
Primary school teachers	42	0.0041	0.0047	0.0006	0.0106	0.0186	0.0080
Health and social workers	43	0.0062	0.0075	0.0013	0.0663	0.0742	0.0080
Public administration intermediates	45	0.0005	0.0010	0.0005	0.0001	0.0003	0.0002
Business administration intermediates	46	0.1572	0.1229	-0.0343	0.0541	0.0576	0.0035
Technicians	47	0.0979	0.1001	0.0022	0.0201	0.0248	0.0047
Intermediate supervisors	48	0.0444	0.0328	-0.0116	0.0233	0.0200	-0.0033
Public administration clerks	52	0.0029	0.0033	0.0004	0.0631	0.0756	0.0126
Security agents	53	0.0030	0.0023	-0.0007	0.0174	0.0196	0.0022
Business administration clerks	54	0.1151	0.1031	-0.0120	0.0860	0.0827	-0.0033
Retail salespersons	55	0.0100	0.0123	0.0023	0.1317	0.1242	-0.0075
Personal service employees	56	0.0053	0.0049	-0.0004	0.0999	0.0857	-0.0142
Skilled manufacturing workers	62	0.1335	0.0843	-0.0492	0.0870	0.0650	-0.0220
Skilled artisans	63	0.0191	0.0189	-0.0002	0.0571	0.0565	-0.0006
Drivers	64	0.0059	0.0044	-0.0014	0.0740	0.0785	0.0045
Handling, transport skilled workers	65	0.0220	0.0189	-0.0031	0.0281	0.0261	-0.0019
Unskilled manufacturing workers	67	0.0388	0.0231	-0.0157	0.0565	0.0472	-0.0093
Unskilled artisans	68	0.0028	0.0040	0.0012	0.0544	0.0559	0.0015
Farm workers	69	0.0004	0.0004	0.0000	0.0038	0.0056	0.0018

Notes: All firms and individuals in firms with at least 2 employees are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Firms are divided into types as described in Table 1.4. We consider only the types that experienced a remarkable change in the person-year observations across time (see Table 1.4). We report the incidence of a certain 2-digits occupation in 2002-2007 vs 2014-2019, and the difference (in percentage points), by type. The color code in the visualization indicates the intensity of the change, with darker shades of red representing more negative changes and darker shades of green representing more positive changes. The second column displays the two-digit code corresponding to the *catégories socioprofessionnelles*, which can be further explored in detail here.

We extend our analysis by replicating the same investigation on 1-period firms, as presented in Table A.10. By considering these additional findings, we uncover several significant trends associated with de-industrialization, the rise of low-end services, labor market polarization, and privatization of public services. In the low-W (W<0), 1-period firms quadrants, firms with a high share of manufacturing workers have been replaced by firms with more personal services employees. Furthermore, we observe a decrease in business administration occupations within the high-W quadrants, while there is an increase in the presence of managers and engineers within the same quadrants. Additionally, there is observable growth in the number of health and education workers employed by low-F, 1-period firms.

This descriptive evidence hints towards a structural evolution in the division of work between firms. Goldschmidt and Schmieder (2017) show for Germany that the rise in sorting is explained by the outsourcing of low productivity activities. Bilal and Lhuillier (2021)[28] find evidence for France that contractor firms pay lower wages and hire less from employment, while firms that outsource are on average more productive. Further, they document that a decline in the cost of outsourcing leads firms to scale up and increase labor demand. The structural estimation of a framework that rationalizes these findings highlights the link between outsourcing and labor market sorting. Additionally, Godechot et al. (2023) demonstrate that restructuring events in France, such as outsourcing, layoffs, offshoring, and subcontracting, contribute to increased segregation. Specifically, workplaces implementing these restructuring actions show a concentration of the top 10 percent of earners.

Our composition results are very compatible with such mechanisms, and more generally with any dynamics of firm specialization taking place through firm growth, creation, and destruction, and shifts in labor demand rather than changes in firm premiums.

Potential Alternative Channels

The economic literature has dealt extensively with skill-biased technological change (Acemoglu & Autor 2011). A rise in skill premium could explain the increase in sorting and segregation we document. Indeed, worker fixed effects dispersion Var(θ) diverges significantly during the study period (Annex A1), entirely driven by its between-firm

[28] Both papers define contractor firms and outsourcing events from detailed firms' industry, occupations, and joint mobility of clusters of workers. Bilal and Lhuillier (2021) additionally use a firm survey (EAE) on intermediate inputs to measure expenditure on external workers.

component Var($\bar{\theta}$). However, an increase in the skill premium would likely impact the link between occupations and worker fixed effects[29]. The relationship between occupations and worker fixed effects is very stable between the periods and does not show evidence of an increase in occupational effects variance. [30] However, occupations associated with higher or lower worker fixed effects grew more than occupations with average fixed effect. Overall, the findings suggest that the increase in worker unobserved heterogeneity dispersion is not about a change in skill returns but rather a transformation in the occupational structure associated with increased occupational segregation in specific categories of firms.

Firm pay premium's variance has been slightly decreasing[31]. To further investigate this trend, we analyze the employment-weighted relationships between value added per worker and estimated firm fixed effects at the firm level across different periods, as illustrated in Figure A.3. Our findings reveal that, for a given level of period-demeaned productivity, the distribution of shared rents exhibits a slight increase at the bottom during the most recent period compared to the previous one, while converging towards the top. These results are consistent with the evidence presented in columns 9 to 12 of Table 1.4. This suggests that the overall decrease in the variance of firm effects can be attributed to an increase in rents at the bottom end of the firm rent distribution. However, the period fitting lines' slopes are visually very similar and statistically non-different from each other. We conclude that the decrease in the variance of firm effects, and the relatively small increases in rents at the bottom end of the distribution do not significantly contribute to explaining the observed increase in sorting. On the contrary, their influence might be in the other direction.

1.4.3. Explaining the Decline in Within-Firm Inequalities

To investigate the decrease in within-firm inequalities, we examine the average change in earnings for employees across different percentiles, ranging from the top 1%-paid employee down to the employees in the first percentile (Figure A.4). This simple statistic provides valuable insights into the dynamics of wage disparities within firms over time. In contrast to findings in the United States, particularly in mega-firms (Song et

[29]To be distinguished from the evolution of the distribution of occupations in firms, discussed in the previous section.

[30]Table A.13 in Annex A5 gives the results.

[31]This decline is more pronounced without bias correction, which corroborates the idea that the bias decreased with time. The decline of premium variance is compatible with an "eclipse of rent-sharing" as recently documented by Acemoglu et al. (2022) for Denmark and the US.

al. 2019), our analysis reveals that wages at the bottom percentile in France have not stagnated. Instead, they have experienced a higher growth rate compared to the rest of the distribution across almost all firm-size classes. Figure A.5 shows how this pace of growth is strongly correlated with hourly minimum wage growth, which experienced a steep increase in the first year of our panel following the need for harmonization after the 35-hours-a-week Reform. This suggestive evidence points towards a potential role for labor market institutions in the French context. The link between P1 and hourly minimum wage is partly mechanical though for two main reasons. First, it is mandatory to pay at least the SMIC in most sectors and for most employees. Second, our population selection is partially linked to the hourly minimum wage as explained in Section 1.2.2.

On a similar note, Bozio et al. (2020) shows that since the 1970's the redistributive effects of payroll taxation have regularly increased. Considered before tax, labour cost inequalities have increased in France at a comparable rate as in other countries.

Kramarz et al. (2022) concludes as well that the early 2000s' significant increase in the minimum wage—followed by the workweek's reduction to 35 hours and the elimination of all employer-paid payroll taxes around the minimum wage—translates into a significant rise in the bottom percentiles of the earnings distribution in the 2000s. They observe that this mix of policies was particularly beneficial for women, young people, and workers in rural and remote municipalities.

Finally, Bilal and Lhuillier (2021) show that in the context of their model, an increase in the minimum wage can mitigate earnings losses arising from substantial worker reallocation across firms.

1.5. Discussion

Despite the stable log-wage variance in France throughout 2002-2019, our results demonstrate that both sorting and segregation increased, similar to the trends observed in the USA and Germany. The rise in between-firm inequalities in France is consistent with a structural evolution in the division of work between firms. Alternative explanations related to increasing returns to skills and changes in firms' rent-sharing behavior were not supported by our analysis. In parallel, wages at the bottom percentiles have experienced faster growth compared to the middle and top percentiles. This phenomenon is closely linked to the nature of French labor market institutions, which aligns with recent findings in the literature. France experienced the same mechanisms raising inequalities elsewhere, only to be contained by increasing redistribution.

The AKM model has provided valuable insights into wage inequalities. The model's estimation is subject to the well-known limitation of the limited mobility bias, which results in the underestimation of sorting. We show that the bias also affects the measurement of the evolution of sorting, likely because mobility intensity and patterns do evolve over time. These findings underscore the importance of employing corrected estimates to capture the precise dynamics of sorting.

There are additional substantive and methodological questions that warrant further investigation. On the substantive side, there is more to explore regarding the interplay between French institutional features and segregation and sorting. Wage inequalities in France have been effectively controlled, primarily through an increase in the redistributive power of payroll taxes and periodic increments in the minimum wage. These phenomena inevitably impact wage distribution and likely interact with firm pay policies, the job market, and sorting. While our focus has been on wages rather than labor costs, as it provides better insights into workers' incentives and observed inequalities, replicating our analysis on labor costs in addition to wages could enhance our understanding of the French exception and its underlying dynamics. Finally, the increased concentration of specific workers in particular firms raises questions about firm specialization, externalization, and job polarization, which merit further exploration.

Methodologically, there are limitations that require a deeper understanding, such as the non-fixed nature of fixed effects. The implications of this specification error are complex, but they could potentially affect the measurement of sorting and other components of the decomposition, particularly during periods of significant short-term economic fluctuations. Another question pertains to accurately measuring the age and experience components of wages when disentangling yearly effects, age, and individual effects. Fluctuations in the interactions between these terms suggest possible estimation artifacts that need to be addressed through alternative specifications.

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2. Temporary Employment, Firm Heterogeneity, and Regulation*

Abstract

We investigate how firm heterogeneity affects the choice between permanent and temporary contracts, and show how regulatory interventions affect this relationship. First, we use detailed matched employer-employee records from Italy to document how firms' hiring strategies vary with their productivity. Firms with higher productivity are less likely to create temporary jobs, but the temporary contracts they do offer are significantly longer in duration, leading to a lack of correlation between productivity and the share of temporary jobs at the firm level. We then exploit a 2018 reform that tightened the legislation on temporary work in Italy to show how firms' response to regulatory interventions depends on their characteristics: while high-productivity firms react by substituting between contract types, switching from temporary to permanent jobs, less productive units end up reducing their labour demand. We turn to a search and matching model of the labour market with endogenous contract choice to rationalise these facts. In the model, a firm's share of temporary jobs is determined by its productivity and the expected duration of the productive period of each match. The estimated model provides a good fit to the pre-reform economy in terms of the use of temporary employment.

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Introduction

Temporary jobs are controversial. Supporters claim that they provide a significant increase in occupational opportunities and serve as a stepping stone for more stable working relationships; detractors maintain that they constitute a precarious form of employment with little security for the workers, who end up unduly absorbing part of the business risk of their firm. This trade-off between employment gains and job security has shaped the political debate on the regulation of temporary contracts in the last decades.

One question stands as the starting point of the discussion: *why do we use fixed-term contracts for some employment relationships and open-ended ones for others?* In this paper we propose a new way of approaching this question, suggesting that firm characteristics are a key element to be taken into account when analyzing temporary employment dynamics.

Recent research on dual labor markets has proposed useful tools to study the microeconomic mechanisms that drive the choice between temporary and permanent employment contracts. Importantly, however, existing contributions generally leave firms on the sidelines by modelling contract choice as a match-by-match decision problem with an outcome that depends solely on the "quality of the match". Although analytically very convenient, this shortcut has two drawbacks. On the conceptual side, it overlooks the fact that - when making a hiring decision - a firm usually has limited information about the candidates for a job, while it can probably rely on a better degree of knowledge about its own performance and outcomes. From a practical point of view, omitting firms makes it much more difficult to translate theoretical predictions into empirical analysis and quantitative exercises. This is especially true if, in addition to describing labour market equilibrium, we are interested in understanding how different policies and regulatory interventions might affect occupational dynamics. The aim of this paper is to address these shortcomings by analysing how firms' characteristics (in our case, productivity) ultimately influence their choice of contract type and their response to regulatory interventions.

We structure our paper into two main parts. First, we provide empirical support for our claims, showing that firm heterogeneity plays a crucial role in predicting temporary employment utilization through its effect on firms' hiring strategies and firms' response to regulatory interventions. The second part presents a theoretical framework that allows us to rationalize these patterns. Rather than proposing a completely new

structure, we modify the reference model of contract type choice and adapt it to include firms. This allows us to clearly describe and quantify the role of firm heterogeneity in the hiring process, separating the exogenous characteristics that inform each hiring decision from the endogenous outcomes it produces; it also allows us to quantify the weight of regulation in firms' decisions.

Summary. Our study begins with a descriptive analysis, based on a matched employer-employee panel dataset covering the universe of firms and workers in the Italian region of Veneto between 2017 and 2019. We first show that the variability in the share of temporary employment in the economy is mostly explained by firm-specific components, while time variability and industry-specific variation play a less important role. Notably, while there is no correlation between past labor productivity and the share of days worked in temporary employment, highly productive firms within the same industry tend to hire a lower proportion of workers on fixed-term contracts and offer longer durations for such contracts. This suggests that firm-specific factors, in addition to industry characteristics, have a significant impact on the demand for temporary jobs.

Next, we examine the impact of the *Dignity Decree* - a 2018 reform that imposed stronger legal constraints on the use of fixed-term contracts in Italy - on firms' workforce composition and hiring strategies. We observe a significant shift in the workforce composition of exposed firms, characterised by a reduction in the share of days worked on fixed-term contracts, mainly replaced by an increase in permanent employment through contract conversions. Importantly, the response to the reform varies according to firms' ex-ante labour productivity, with more productive firms experiencing a more pronounced substitution from permanent to temporary employment. In contrast, less productive firms face more significant disemployment effects. Taken together, these empirical results suggest that a firm's decisions regarding its workforce composition may be influenced not only by the idiosyncratic characteristics of the matches (workers) it hires, as assumed in much of the previous literature, but also by firm-level characteristics, such as its productivity.

Our final step is to examine the links between firm heterogeneity, the use of temporary contracts, and regulation through the lens of a search and matching model of the labor market. We propose a modified version of the framework first introduced by Cahuc et al. (2016), in which firms hire workers to exploit production opportunities of random duration. Each job created must be formalised by an employment contract, which may be permanent or temporary. In choosing the type of contract to offer for each job, firms face a trade-off: by offering a permanent contract, they ensure themselves

the chance to exploit each production opportunity until it is exhausted, but face the risk of bearing deadweight labour costs in the event of a shock; by offering a temporary contract, firms limit their exposure to productivity shocks, but also reduce their profits, since each new contract entails a fixed cost for the firm.

In this paper, we extend the original framework by formally introducing firms into the model. We do this by introducing exogenous productivity shifters at the firm level. These shifters affect the surplus generated by each worker within a firm and affect the composition of matches that the firm encounters. Notwithstanding these common factors, different jobs within a firm can still be regulated by different contract types based on the random shock arrival rate that characterises each match. This extension allows for the incorporation of both *between-firm* heterogeneity, related to the composition and size of the workforce, and *within-firm* heterogeneity, related to the choice of contract type, duration and conversion probabilities for individual workers. Building on this framework, we then introduce the regulation of fixed-term contracts by modelling the costs of writing such contracts.

The model, estimated on forty moments from our panel dataset, provides a good fit to the hiring patterns that characterised the pre-reform economy: the estimates accurately reproduce the relationships between firm productivity and various measures of temporary work utilisation (contract type composition of employment flows and stocks, average contract duration) predicted by the model's equations.

The estimated model can be used to conduct counterfactual exercises. Firstly, we ask ourselves how the effects of the reform would change depending on the prevailing economic conditions (e.g. an aggregate change in the shock arrival rates), keeping fixed the productivity distribution. Secondly, we can explore how the impact of diverse regulatory interventions can vary depending on the distribution of firm characteristics. These counterfactual exercises will enable us to understand how different policy choices influence workforce dynamics within heterogeneous firms, shedding light on the potential consequences of alternative regulatory approaches.

Related Literature. This paper contributes to the very broad literature on employment protection legislation, and in particular to the large body of work examining the trade-offs between temporary and permanent contracts. This literature, well summarised in Bentolila et al. (2020), has given prominence to three main sets of questions over the last two decades.

First, a substantial body of work has focused on understanding the mechanisms that determine a firm's choice between different contract types. These studies provide

explanations for why it might be economically rational for a firm to offer temporary jobs to some workers and permanent jobs to others. Recently, a significant shift in perspective has emerged from this research, recognising that temporary jobs are not simply a low-firing-cost counterpart to permanent jobs: instead, they involve a combination of ex ante commitments (e.g. the duration of the employment relationship) and ex post decisions (e.g. whether to convert to permanent status) that make the hiring choice on the part of firms far from trivial. Cahuc et al. (2016) propose a search and matching model of the labor market that can be considered a cornerstone of this perspective. In this model, the type of contract chosen for each job is endogenously determined by the firm taking into account the expected duration of the productive spell and the costs of starting a new job. Our paper builds on this framework and extends it by allowing for the presence of firms with more than one worker. Our main contribution in this direction is to acknowledge the existence of firms as more than just collections of independent jobs. We embed heterogeneity in firm performance, specifically labor productivity, and illustrate its substantial role in shaping a firm's hiring strategies. This is achieved by modeling the firm's hiring decisions as a function of its productivity. This perspective enables us to explore decisions made on both extensive (number of contracts per period) and intensive (contract duration) margins at the firm level. Ultimately, our approach provides a mechanism to examine how these micro-level decisions influence the macro-level equilibrium of contract types within the broader economy.

A second line of work has examined the effects of different regulatory regimes for employment contracts. The key question here is to understand what degree of rigidity (or flexibility) on either side of dual labour markets would strike the right balance between fostering employment growth and protecting workers from precarious employment conditions and job instability. Recent contributions converge on the view that variations in the regulation of temporary contracts primarily affect the contract composition of the labour force. In essence, regulatory changes lead to substitution between contract types rather than to significant variations in overall employment levels. (Cahuc et al. 2022; Cappellari et al. 2012; Daruich et al. 2023)[2]. Our empirical work sheds light on an unexplored driver of labour demand responses to regulation targeting the use of fixed-term contracts: how within-sector productivity differentials affect the extent to

[2]Cahuc et al. (2022) show that the true occupational effects crucially depend on accounting for spillovers between firms that are subject to regulation and firms that are not. Since the reform studied in our paper did not generate a clear quasi-experimental setting that could be used to study its effect, we do not delve into this type of analysis.

which firms substitute temporary jobs for permanent ones.

The last set of works steps outside the contract-choice realm to instead discuss the consequences of using temporary jobs for various economic outcomes. This corpus includes macroeconomic investigations into the occupational effects of dual labor markets (Bentolila et al. 2012; Pijoan-Mas & Roldan-Blanco 2022), as well as microeconomic investigations of how employment protection legislation affects firm outcomes such as demand for skills (Grasso & Tatsiramos 2023), capital intensity (Cappellari et al. 2012), and productivity (Bassanini et al. 2009). There are also analyses of its impact on worker outcomes (Booth et al. 2002). Departing from this body of work, our analysis proposes a reversal of perspective: we argue that productivity, through its impact on the relative attractiveness of different contract types, is a determinant of the equilibrium contract composition in the labor market, and we further analyze the implications of this relationship for total labor flows and employment dynamics.

Outline. The remainder of this paper is structured as follows. Section 2.1 provides some institutional background on temporary contracts and on the reform. Section 2.2 presents the data that we use. Section 2.3 reports the empirical facts that motivated our study and presents our regression models at the firm-level. Section 2.4 presents the model. Finally, Section 2.6 draws our preliminary conclusions and describes the next steps in our strategy.

2.1. Institutional Background

2.1.1. Temporary Contracts in Italy

In Italy, as in other European countries, the most typical way to formalize an employment relationship is the permanent (or *open-ended*) contract: in 2017, approximately 85% of the stock of dependent employment was permanent. The remaining share of dependent employees work under temporary contracts, contractual arrangements that envisage an employment spell with a pre-determined (usually short) duration. Despite their limited weight in total employment stocks, reliance on temporary contracts has been growing steadily over the last three decades and today they account for more than half of total labor flows during periods of labor market stability.

The standard way to govern a temporary employment relationship in Italy is the fixed-term contract (*Contratto a tempo determinato*). This type of contract was first introduced in 1962 to address specific instances in which labor demand would be temporary

in nature: (i) seasonal activities; (ii) substitution of permanent workers for a limited time-span; (iii) specific and predetermined needs deriving from extraordinary activities[3]. Over time, the rules regulating the use of the fixed-term contract were subject to numerous modifications (see Appendix Section B8.2).

While some differences emerge when comparing different legislations, the Italian fixed-term contract shares with the most used temporary contracts in other European countries a number of relevant characteristics. First, fixed-term contracts can be initiated by the firm itself or through an employment agency; in this paper, we focus on the former case, the most common. Second, a temporary contract can be extended: an extension arises when, at or before the contract expiration date, the firm and the worker immediately decide to prolong the employment relationship (for a limited time-span) without changing the terms of the contract[4]. Finally, fixed-term contracts can be converted into permanent ones at any time before their expiration date or during an extension. Contract conversions constitute one of the primary forms of transition from temporary to permanent employment, as they allow to preserve the existing employment relationship without interruptions or bureaucratic burdens.

2.1.2. The 2018 Reform

The so-called *Dignity Decree* (Law Decree 2018, n.87) was the first labor market reform to invert the trend of temporary contracts liberalization that characterized Italy and other major European countries starting from the 1990s (see Appendix Section B8.1). The decree, introduced in July 2018, severely tightened the set of rules governing the use of fixed-term contracts in Italy through a combination of mandatory limitations ("hard-bounds"), additional contributory costs and increased regulatory requirements. More specifically, following the introduction of the D.D.: (i) the maximum duration of a temporary employment relationship (incl. all extensions) is reduced from 36 to 24 months, (ii) fewer extensions are allowed (up to 4), (iii) a justification is required when the employment relationship is longer than 12 months (incl. extensions) and for all recalls and (iv) every recall triggers a 0.5% increase in the social security contributions by the employer. Other minor changes were included[5].

[3] Law 1962, n. 230

[4] Firms can also *recall* workers: this occurs whenever a worker experiences two separate temporary employment spells at the same firm, with some time elapsing between the end of the first and the start of the subsequent one.

[5] The D.D. additionally establishes an increase in the statute of limitations to start litigation regarding a temporary contract (from 120 to 180 days after its end) and in the compensation due for an unlawful

A first glimpse of the relevance of this reform in strengthening temporary contracts regulation can be obtained by looking at the evolution in the OECD "Employment Protection Legislation Index" for both temporary (Figure B.1A) and permanent (Figure B.1B) positions: while EPL did not vary much for permanent contracts over the last decade, between 2018 and 2019 the strictness of temporary jobs regulation in Italy jumps to the highest level observed in the four major European economies since 2008.

2.2. Data

This section introduces the main data sources used in our empirical analysis, the *Mercurio* and *Cerved* datasets. A description of the additional data used in the aggregate descriptive analysis is provided in Section B9.

Mercurio - Veneto Lavoro. Mercurio is a matched employer-employee dataset covering the universe of dependent employment relationships for the Italian region of Veneto between 1990 and 2021. This microdata are derived from mandatory private firms' notifications to the local public labour market agency (*Comunicazioni Obbligatorie*).

One record in the dataset corresponds to one employment spell. For each employment spell, we observe three main classes of variables: variables describing the employment relationship (e.g. contract type, starting and termination/conversion dates, occupation); variables describing the firm (e.g. sector of activity, age, location); and variables describing the worker (e.g. age, sex, education).

Cerved. The Cerved dataset is derived from standardized reports filed annually by Italian limited liability corporations. It provides information on – among others – sales, value added, labor costs, and profits for these firms. The data excludes private partnerships and sole proprietorships, which are common among smaller firms. The information from the Cerved dataset is matched with Mercurio using the tax identifier[6].

2.2.1. Setting

The reform studied in this paper was enacted by the newly elected Italian government in the summer of 2018. To study it, we consider the window of 36 months starting in January 2017 and ending in December 2019, which is conveniently centered around the reform. This time span is a good choice because it allows us to avoid considering 2015

dismissal of a permanent worker (from 4-24 to 6-36 monthly wages)

[6]The match between datasets was carried out by the data providers to preserve anonymity.

and 2016, two years in which the Italian labor market was flooded with hiring subsidies (see Section B8.2 and Figure B.2). At the same time, ending our observation window in 2019 is necessary to avoid the disruptions caused by the Covid-19 pandemic, which affected in a very asymmetric way workers with different types of contracts.

The D.D. reform applies to the entire Italian territory. The aggregate trends described in Section B10 rely on national-level aggregate data. Instead, the micro-data used for the empirical analyses in Section 2.3 refer just to the region of Veneto. Veneto is a large ($\simeq$ 5M inhabitants) region in northeast Italy. It has a high employment rate (75% in 2017) and a low estimated incidence of the informal market (9% in 2017) relative to the rest of Italy, and it constitutes one of the most productive areas of the country with a very strong manufacturing base.

2.2.2. Sample selection

We work with a selected sub-sample of the records contained in the original micro-data. Following Sestito and Viviano (2018), we drop firms in the agricultural sectors (for the high seasonality of their activity) and domestic workers (who are subject to specific regulatory provisions). We additionally limit our sample to workers under 65 years of age, and to firms with at least 5 employees over our entire period of analysis.

In our regression analyses, we rely on long-run differences in labor productivity and in the demand for temporary jobs at the firm level for identification purposes. We hence exclude those firms that appeared in the dataset in 2017 or later and/or have missing value-added information. As a result of these exclusions, our final sample consists of more than 14,000 firms.

The final dataset based on this sample of firms is structured as a monthly panel over the period 2017-2019. In addition to a series of descriptive variables characterizing each firm, it includes monthly labor flows, the average stated duration of temporary contracts starting each month and the share of temporary employment on total employment, calculated on the basis of the days worked by all workers in the firm under each type of contract.

2.3. Empirical Analysis

This section presents our empirical analysis, structured into three parts: first, a descriptive account of firms' reliance on temporary jobs and and its relationship with firm productivity; second, a series of regression models that analyze the effects of the reform

on firms' hiring strategies and workforce composition; third, an examination of the heterogeneous response to the reform across firms based on their productivity.

2.3.1. Firms and Temporary Employment

The first step in our empirical analysis is to analyze how the reliance on temporary jobs varies across sectors, firms and time. To achieve this, we restrict our attention to the pre-reform period (from January 2017 to June 2018) and focus on share of temporary employment, denoted π and measured as the share of days worked in temporary jobs for each firm-month pair.

In the eighteen months prior to the reform the temporary employment share has averaged 11% in our sample, with a standard deviation of 15 percentage points. Variability in temporary contract utilization can be traced back to many different factors. We rely on a schematic breakdown along three relevant axes: *within-firm* changes in the share of days worked in temporary employment over time, *between-firms* and *between-industries* differences in the average utilization rate. Quantifying the contribution of each of these factors to the overall variability in temporary employment is a first step towards understanding and correctly modelling the economic decisions behind the use of temporary contracts.

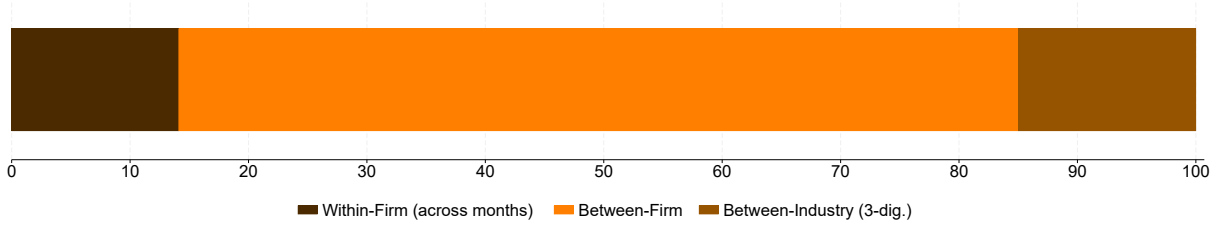
For this reason, we decompose the variance of π as follows:

$$\text{Var}(\pi_{t,i,s}) = \text{Var}(\pi_{t,i,s} - \bar{\pi}_{i,s}) + \text{Var}(\bar{\pi}_{i,s} - \bar{\pi}_s) + \text{Var}(\bar{\pi}_s - \bar{\pi}) \tag{2.1}$$

, where t is a month, i is a firm and s is a 3-digit industry. Applying this decomposition to our sample of firms for the period between January 2017 and June 2018, we find that most of the variability is driven by between-firm differences in the share of temporary contracts used (70% of the total variance), while time-series variability (15%) and between-sector variability (15%) play a smaller role[7] (Figure 2.1).

[7] Figure B.3 plots the average share of temporary days worked in firms by 3-digit industry over the same period.

FIGURE 2.1. Variance Decomposition – π



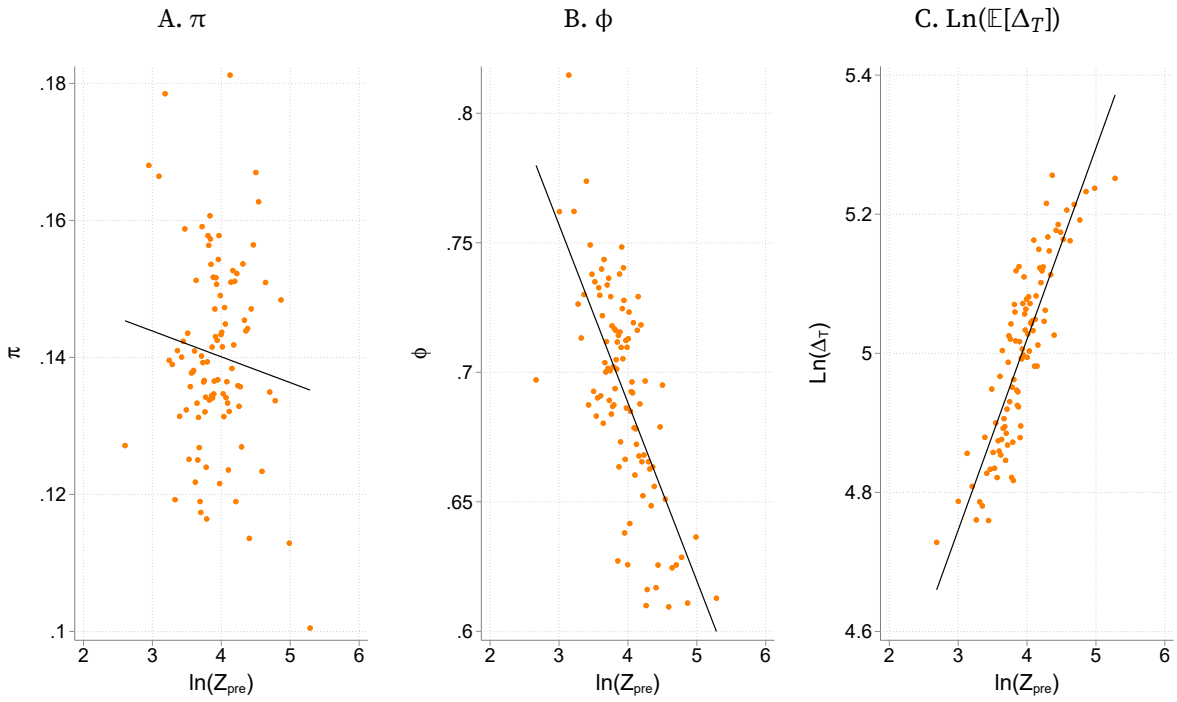
Notes: The graph presents the decomposition (see Equation 2.1) of the variance of π, which represents the ratio of days worked in temporary employment to total days worked, at the firm-month level between January 2017 and June 2018. The variance of π is decomposed into three components. The Within-Firm (across months) component captures the extent to which firms change their share of days worked in temporary employment from month to month. The Between-Firm component accounts for the variation in temporary contract utilization across different firms within a given industry. The Between-Industry component reflects the variation in temporary contract utilization across different three-digit industries.
Source: Mercurio, see Section 2.2

This first result suggests that firms play a significant role in guiding temporary employment dynamics, regardless of the industry to which they belong. While the variation in the use of temporary contracts across industries is relatively easy to rationalize, due to the large structural differences in production processes that arise across sectors, the substantial within-industry variability that we document is less recognized in the literature but appears to be very influential.

We then examine whether firms' heterogeneous use of temporary contracts is associated with their performance, which we measure in terms of past labor productivity. Specifically, for each firm we define $\ln(Z_{pre})$ as the average annual log value added per worker measured from 2012 to 2016[8]. In cases where a firm appears later in the data, we calculate the average log value added per worker from its first year to 2016. We pool the data from January 2017 to June 2018, aggregating observations at the firm level to obtain one observation per firm. We then regress the pre-reform π on $\ln(Z_{pre})$, controlling for industry fixed effects. The result of this exercise (Figure 2.2A) suggests a very weak and statistically insignificant correlation between the two measures.

[8]Figure B.4 illustrates the distribution of the resulting variable.

FIGURE 2.2. Within-Industry Relationship Between Productivity and the Use of FT Contracts



Notes: The graphs provide evidence on the firm-level relationship between past labor productivity – measured as the average yearly log value added per worker from 2012 to 2016 – and various measures of fixed-term (FT) contracts utilization, measured between January 2017 and June 2018. All variables have been residualized with respect to 3-digit industry fixed effects. The orange dots represent the conditional mean of each outcome variable at each percentile of residualized firm past labor productivity, and the black line represents the fitted regression line. Panel A: share of days worked under fixed-term contracts in total days worked (π). Panel B: share of hirings in fixed-term contracts (ϕ). Panel C: average duration of new fixed-term contracts (expressed in log). For detailed regression results, refer to Table B.1.
Source: Mercurio, see Section 2.2

n order to gain insight into the factors behind this non-relationship, we introduce two additional measures of fixed-term contract utilization. The first measure, denoted as ϕ for *flows*, captures the relative entries into temporary employment and is calculated as the total number of entries into a fixed-term contract divided by the total number of entries over the whole period[9]. The second measure, denoted as $\mathbb{E}[\Delta_T]$, represents the average *duration* (in days) of new fixed-term contracts as reported by firms[10]. Interestingly, we find a significant relationship between past labor productivity and both ϕ and $\mathbb{E}[\Delta_T]$ for firms within the same industry. More specifically, more productive firms within a given industry tend to hire a smaller proportion of workers on fixed-term contracts (lower ϕ, Figure 2.2B); at the same time, these firms tend to offer longer

[9]It is important to note that we focus specifically on contracts started anew and do not include conversions and renewals in the calculation of this measure.

[10]Each fixed-term contract reports an initial duration, chosen at the time of signing.

durations for their fixed-term contracts (higher $\mathbb{E}[\Delta_T]$) compared to less productive ones (Figure 2.2C)[11]. Since the share of temporary jobs (π) necessarily depends on how many temporary contracts are signed each period (ϕ) and on the average duration of these temporary contracts ($\mathbb{E}[\Delta_T]$), these opposing forces drive the non-relationship between past labor productivity and the use of temporary jobs.

It is important to stress that these within-industry relationships are not only statistically significant, but also relevant in terms of their economic magnitude: moving from a firm at the 10th to the 90th percentile of the distribution reduces the predicted share of monthly new jobs with a fixed-term contract by 28 percentage points and increases the predicted average contract duration by two and a half months.

In summary, our analysis suggests a subtle relationship between firm-level past labor productivity and the utilization of temporary contracts. While there is no significant correlation between past labor productivity and the overall share of days worked on fixed-term contracts, we find that more productive firms within specific industries tend to hire a lower proportion of workers on fixed-term contracts and to offer longer durations for these contracts. These findings suggest that firm-specific factors, over and above industry characteristics, play a crucial role in shaping the demand for temporary jobs. The contrasting effects of relative entries and average contract durations highlight the need for a more nuanced understanding of the underlying mechanisms driving the use of temporary contracts.

2.3.2. The Effect of the Reform on Stocks and Flows

Our panel dataset reports all hiring, firing and contract conversion events experienced by a firm for each month between January 2017 and December 2019, as well as total days worked by contract type. From this, a regression model is built to compare the time variation in occupational outcomes across firms with different exposure to the reform. Unlike other previous reforms, the D.D. does not have characteristics that would provide a clear quasi-experimental setting that could be used to identify its causal effects. Therefore, our investigation relies on an event study type of analysis, which allows us to exploit the fine-grained (i.e. monthly) time variability of our data while controlling for common unobservable time-invariant and period-specific factors. The specification is as follows:

$$Y_{i,t} = \alpha_i + \tau_t + \beta_t I(\pi_{i,Pre\text{–}2017} > 0) + \varepsilon_{i,t} \tag{2.2}$$

[11] Full regression results can be found in Table B.1.

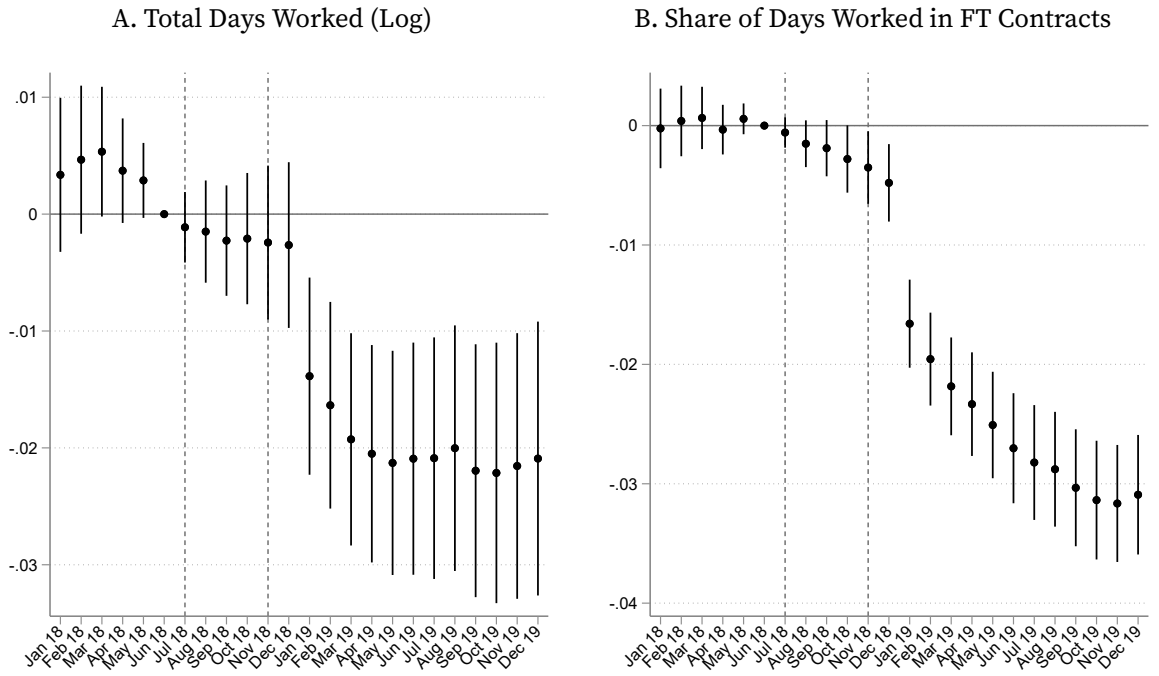
where $Y_{i,t}$ is a firm-level occupational outcome at time $t = \{y, m\}$. The terms α_i and τ_t denote firm and period fixed effects respectively. The key element of heterogeneity across firms is $\pi_{i,Pre\text{–}2017}$, which represents each firm's exposure to the reform. We measure this exposure by relying on long-run differences in the demand for temporary workers at the firm level. We reintroduce π, which is conceptually the same as in Section 2.3.1, and measures the incidence of temporary employment relative to total employment. In our preferred specification, π is calculated for each firm as the average of the monthly share of days worked in temporary positions (w.r.t. total days worked) between 2012 and 2016[12]. By specifying the model as shown in Equation (2.2), we perform a simple exercise of comparing the average employment outcomes of two groups of firms: those that never relied on temporary employment before 2017 (the "non-exposed") and those that did (the "exposed"). The categorization is summarized by the indicator function $I(\pi_{i,\text{Pre-2017}} > 0)$. Table B.2 presents descriptive statistics on flows and stocks by type of contracts for these two groups of firms in the pre-reform period.

We present a series of figures reporting the β_t parameters from this regression, along with their respective confidence intervals, using different variables as outcomes. We identify everything with respect to June 2018 (the last month before the reform). In each figure, the first vertical line indicates July 2018, the month in which the government presented the D.D., and the second line indicates the date of its full enactment after parliamentary approval.

We start by looking at the dynamics of employment stocks. Figure 2.3 shows the results for total days worked and the share of days worked on fixed-term contracts.

[12] Figure B.5 displays the distribution of the resulting variable.

FIGURE 2.3. Employment Stocks - Exposed vs Non-exposed Difference



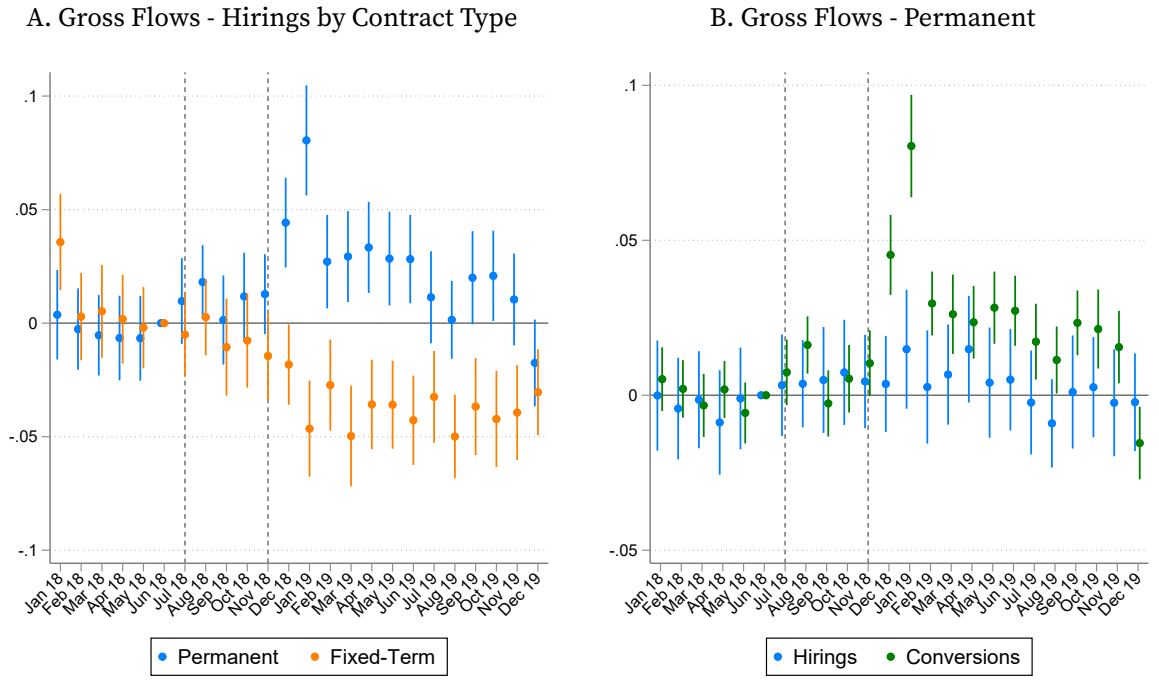
Notes: The graph in Panel A reports the average difference in monthly total days worked (in log terms) between exposed and non-exposed firms (estimates of β_t from the model in Equation 2.2). The graph in Panel B reports the average difference in the monthly share of total days worked in fixed-term (FT) contracts between exposed and non-exposed firms. The outcomes are first residualized with respect to calendar months to control for seasonal patterns. Standard errors are clustered at the firm level.
Source: Mercurio, see Section 2.2.

he coefficients in Figure 2.3A suggest that, on average, the reform had moderate disemployment effects. In addition, Figure 2.3B highlights a significant shift in the composition of the workforce of exposed firms. They experienced a significant decline in the share of days worked in fixed-term contracts, starting right after the decree announcement and intensifying after its enactment. This trend stabilized in mid-2019. Overall, these graphs and the additional ones in the Appendix indicate that the days worked in fixed-term contracts were replaced mostly with permanent employment, with few employment losses and a small degree of substitution towards other contract types (Figure B.6).

We next delve into gross flows to gain a deeper understanding of the discontinuity in hiring strategies that drove the observed substitution pattern. Figure 2.4 shows the results when examining hiring by contract and entry type. The monthly gross flows data have a significant number of zero values and a skewed distribution. To deal with this appropriately, we apply the inverse hyperbolic sine (IHS) transformation. This transformation helps stabilize the variance and allows us to better handle the

distribution's characteristics, leading to more reliable results in our analysis[13].

FIGURE 2.4. Difference in hiring patterns over time: exposed vs non-exposed firms



Notes: The graph in Panel A presents the estimated average monthly difference in gross monthly hirings by type of contract between exposed and non-exposed firms (estimates of β_t from the model in Equation 2.2). Permanent gross flows include both new permanent employment relationships and conversions of existing temporary contracts. The graph in Panel B displays the same average difference in hirings between exposed and non-exposed firms, separating between new permanent employment relationships and conversions. The outcomes are first transformed using the inverse hyperbolic sine (IHS) transformation and then residualized with respect to calendar months to control for seasonal patterns. Standard errors are clustered at the firm level.
Source: Mercurio, see Section 2.2.

The hiring strategies of the two groups show a statistically significant difference after July 2018: firms exposed to the reform experience a surge in permanent-contract hiring. This increase is mainly driven by the conversion of existing temporary contracts (Figure 2.4B). Exposed firms also demonstrate a substantial relative reduction in temporary entries in the aftermath of the reform, with the effect mainly concentrated in 2019. We further investigate whether the post-reform substitution between permanent and temporary positions might also mask a discontinuity in firms' contract termination strategies, but the evidence does not support this scenario (Figure B.7).

[13]Based on recent work by Chen and Roth (2023), the interpretation of the coefficients avoids presenting them as percentage differences between the two groups (exposed and non-exposed) in the pre-post reform period. This decision is taken to avoid arbitrary interpretations that could be influenced by the specific units of the outcome variable, especially when dealing with extensive margin effects, as in the case of monthly gross flows. We are primarily interested here in investigating the existence of a discontinuity in firms' hiring behavior specifically around the introduction of the reform.

In summary, the reform generated moderate dis-employment effects and led to a remarkable shift in workforce composition, with exposed firms experiencing a substantial reduction in the share of days worked in fixed-term contracts. This reduction was replaced with increased permanent employment, primarily through the conversion of existing temporary contracts. These findings are consistent with the more descriptive analysis conducted on national data, as reported in Section B10.

2.3.3. The Heterogeneous Response to the Reform

This section investigates the heterogeneous occupational effects of the reform across firms. To this end, we modify the event-study regression of equation (2.2) by introducing an additional term: we interact the exposure dummy with the dummy variable P, taking value 1 for all the post-reform months, and with the firm-specific measure of labor productivity ($Z_{Pre\text{–}2017}$) introduced in Section 2.3.1 (the average of annual log value added per worker between 2012 and 2016, residualized with respect to industry fixed effects). The resulting regression takes the following form:

$$Y_{i,t} = \alpha_i + \tau_t + \beta_t I(\pi_{i,Pre\text{–}2017} > 0) + \gamma Z_{i,Pre\text{–}2017} I(\pi_{i,Pre\text{–}2017} > 0) P + \varepsilon_{i,t} \tag{2.3}$$

The coefficient of interest, γ in equation (2.3), reflects how a marginal increase in the productivity of exposed firms affects the average post-reform gap in occupational outcomes between the exposed and the non-exposed groups. We begin by examining employment stocks in Table 2.1. The results indicate significant heterogeneity in firms' employment responses to the reform based on their past within-industry labor productivity. While a reduction in the number of days worked on fixed-term contracts is observed on average for all firms, the substitution between temporary and permanent employment is more pronounced for firms that were more productive ex-ante. Consequently, the dis-employment effects are more pronounced for enterprises that were less productive prior to the reform.

TABLE 2.1. Heterogeneous Response to the Reform - Stocks

	(1) Days Worked - Total	(2) Share - FT	(3) Days Worked - P	(4) Days Worked - FT
γ	0.038***	-0.000	0.039***	0.005
	(0.004)	(0.002)	(0.005)	(0.014)
N	518,130	518,130	516,702	328,154
N of firms	14,437	14,437	14,407	12,346
Within R^2	0.009	0.027	0.007	0.022

Notes: The table reports the γ coefficient values derived from Model 2.3 for different outcomes. Column (1): total days worked. Column (2): share of total days worked in fixed-term (FT) contracts. Column (3): total days worked under permanent contracts. Column (4): total days worked under fixed-term contracts. Outcomes in columns (1), (3), and (4) are expressed in logarithmic form. Consequently, we exclude observations with zero values in the outcome, leading to a reduction in the number of firm-month observations and total firms. Results in (3) and (4) should be interpreted as focusing solely on intensive margin effects. All outcomes are first residualized with respect to calendar months to control for seasonal patterns. Standard errors are clustered at the firm level.
Source: Mercurio, see Section 2.2.

The economic magnitude of this heterogeneity is meaningful. On average, exposed firms in 2019 experienced a reduction in total days worked of about 2 log points, equivalent to a 2% reduction relative to non-exposed firms. However, this effect ranges from -3.85% to -0.01% when moving from the 10th to the 90th percentile of firms in the (ex-ante, within-industry) past labor productivity distribution.

These results are further supported by Table B.3, which presents findings on flows, demonstrating that more productive firms had relatively more entries into permanent jobs (particularly through conversions) while experiencing, on average, the same reduction in temporary hiring as less productive firms.

Taking Stock. Overall, our empirical analysis highlights the importance of firm-specific factors in driving differences in the use of temporary jobs and the response to regulatory changes. Firms with higher labor productivity tend to hire a lower proportion of workers on fixed-term contracts and to offer longer durations for these contracts within the same industry. Regarding the impact of the reform, which was primarily aimed at reducing the use of fixed-term contracts for long employment relationships, we observe that more productive firms responded by substituting temporary positions with permanent ones, while less productive firms experienced higher job destruction, leading to more significant employment losses. The next step is to integrate these findings into a coherent theoretical framework.

2.4. Model

In this section, we use a search and matching model of the labor market to complement the evidence provided by our regression results. Our objective is to study how firm heterogeneity interacts with (i) the choice between permanent and temporary employment; (ii) the differential use of fixed-term contracts; (iii) the impact of different regulatory interventions.

As shown by the empirical results of Section 2.3.1, firms play a central role in explaining the variability in temporary employment utilization. Consequently, we organize our modeling effort as follows: we start from Cahuc et al. (2016)'s framework (Section 2.4.1) and extend it by formally introducing firms into the model (Section 2.4.2). Equipped with a structure that can rationalize firms' contract-type choices, we then add regulation by modeling the cost of writing a temporary contract (Section 2.4.3). Section 2.4.4 presents a partial equilibrium version of the model with firm heterogeneity and temporary contracts regulation. Section 2.4.5 develops the full labor market equilibrium.

2.4.1. Benchmark

In order to study the choice between temporary and permanent contracts, we start from the model proposed by Cahuc et al. (2016)[14].

The model represents a continuous-time economy with a continuum of risk-neutral workers and firms. Firms use labor as their only production input to exploit the production opportunities that they encounter, all of which produce the same output y per unit of time. Each production opportunity is associated with a measure of its riskiness, a shock arrival rate λ which defines the probability that the opportunity will cease to exist at any given point in time[15]. When a worker and a firm meet, they first observe the realization of their match-specific shock-arrival rate. If, given this information, the parties decide to start a new employment relationship, this needs to be formalized through an employment contract, which can be permanent or temporary.

Permanent contracts have no expiry date, set a wage $w(\lambda)$ that cannot be renegotiated without mutual agreement, and entail a red-tape firing cost F. Temporary contracts, on the contrary, are characterized by a predetermined duration Δ (determined at the time of signing the contract) and a wage $w(\lambda, \Delta)$, and they cannot be terminated before their

[14] Here we briefly summarize the main features of this benchmark framework. For definitions, proofs and detailed explanations, please refer to the original paper.

[15] Each firm uses exactly one worker in this baseline specification so that λ can be considered to be associated with one firm or one match

expiry date. If a temporary contract expires before any shock hits it, the firm and the worker can decide to preserve the match and convert it into a permanent contract or to interrupt the employment relationship.

In choosing the type of contract used to formalize the employment relationship, firms and workers trade off the costs and benefits of the permanent and temporary options. What generates the tradeoff? By offering a permanent contract, firms secure the chance to exploit each production opportunity until it lasts, but face the risk of bearing dead-weight labor costs (or the firing cost) if a shock arrives; by offering a temporary contract, instead, firms limit their exposure to productivity shocks but also limit their potential profits, given that the signing of each new contract is subject to a contracting cost c[16].

This reference model has very desirable properties: it endogenizes the contract-type choice and it makes a credible case for the use of permanent positions also in the absence of temporary contracts regulation. In equilibrium, permanent contracts are used for jobs with low-shock arrival rates, while temporary contracts are used for more risky positions, and only a fraction of them is converted to permanent ones at the end of their term.

2.4.2. Introducing Firms

In the benchmark version of the model, each firm offers exactly one job. This has two important implications: first, the model behaves as if all shocks were idiosyncratic across workers, with no role for firm-level disturbances; second, the model is unable to speak of differences in the hiring behavior of different firms and their link to basic firm characteristics such as productivity.

We extend the original "one firm-one job" framework by formally introducing firms. First, we introduce an exogenous productivity shifter z at the firm level, which quantifies output per worker at any point in time and thus affects the surplus generated by all matches of a firm in the same direction. Second, we allow the distribution of production opportunities to depend on z, so that more productive firms encounter, on average, less risky job matches. Third, we allow firms to create an arbitrary number of vacancies in each period. In equilibrium, firm size is jointly determined by the expected surplus from opening a vacancy (given the firm's productivity level), the firm-specific distribution

[16] If a firm were to replace a permanent contract with a sequence of temporary contracts, it would incur a cost for each of them, whereas if it were to terminate the employment relationship after the first temporary contract expired, it would forego some profits.

of production opportunities, and a convex vacancy cost function that prevents more productive firms from tending towards infinite size.

2.4.3. Temporary Contracts Regulation

As a second step, we introduce temporary contracts regulation. We model the cost of writing a temporary contract as a decreasing function of the productivity shock λ. We do so by adding a regulation-driven component of costs c_R that relates the contractual burden to the frequency of the shocks λ and applies only to temporary contracts. We assume $c_R = 0$ in the pre-reform period and $c_R > 0$ in its aftermath, and a higher costs for matches with low λ. This choice stems from the set of rules imposed by the reform studied in this paper, which aimed to discourage the use of temporary contracts for long employment relationships.

2.4.4. Job Creation

In choosing which type of contract to use to formalize the employment relationship, firms and workers trade off the costs and benefits of the permanent and temporary options. This trade-off between the two types of contract is summarized mathematically by comparing the functions

$$S_P(z, \lambda) = J_P(z, \lambda) + W_P(\lambda) - U \tag{2.4}$$

$$S_T(z, \lambda, \Delta) = J_T(z, \lambda, \Delta) + W_T(\lambda, \Delta) - U \tag{2.5}$$

that represent, respectively, the surplus generated by signing a permanent or temporary contract compared to the alternative of leaving the match with no job creation. Each of these functions is the sum of the surpluses generated on the firm side, $J(\cdot)$, and on the worker side, $W(\cdot) - U$. Both functions are decreasing in λ.

We assume that productivity shocks strike productivity down to zero once and for all. In partial equilibrium, we are interested to unveil the relationships between the shock arrival rate λ, the firm-level productivity shifter z and job creation.

Permanent Contracts' Surplus. The expected surplus of a permanent contract is

$$
\begin{aligned}
S_P(z,\lambda) &= J_P(z,\lambda) + W_P(\lambda) - U \\
&= \int_0^\infty \left[\int_0^\tau [z - w(z,\lambda)] e^{-rt} \mathrm{d}t - F e^{-r\tau} \right] \lambda e^{-\lambda\tau} \, \mathrm{d}\tau - c \\
&\quad + \int_0^\infty \left[\int_0^\tau [w(z,\lambda)] e^{-rt} \mathrm{d}t + U e^{-r\tau} \right] \lambda e^{-\lambda\tau} \, \mathrm{d}\tau - U
\end{aligned} \tag{2.6}
$$

where $z > 0$ is output per job, *firm specific* and constant over time; $w(\cdot)$ represents workers' wage function; F represents a *red-tape* firing cost (i.e. no severance); U represents the value of unemployment; τ represents the random date at which a job becomes unproductive; c represents general contract-costs to start permanent positions.

When a shock hits and productivity drops from z to zero, the job is destroyed if the total surplus becomes negative. We assume that $F < U$ to ensure that unproductive jobs are indeed destroyed. By solving Equation 2.6, we obtain:

$$
S_P(z,\lambda) = \frac{z - rU - \lambda F}{\lambda + r} - c \tag{2.7}
$$

The surplus associated with starting a permanent job decreases in λ. There is only one value, λ_P, where the surplus is zero. To the left of this value, the surplus is always positive, which means that signing a permanent contract would generate some value. To the right of it, however, it is never optimal to formalize the match into a permanent contract. Productivity z acts as a shifter: for any given λ, a higher z implies a higher value for the surplus of starting a permanent job. The lower the λ, the greater the effect of the shifter. This means that the productivity premium enjoyed by workers in high-z firms translates into smaller and smaller surplus gains as the shock arrival rate of their match increases, due to the lower expected duration of the production opportunity (see Section B11.1 for proof).

Temporary Contracts' Surplus. The expected surplus of a temporary contract is

$$
\begin{aligned}
S_T(z,\lambda,\Delta,c_R) &= J_T(z,\lambda,\Delta,c_R) + W_T(\lambda,\Delta) - U \\
&= \int_0^\Delta [e^{-\lambda\tau} z - w(z,\lambda,\Delta)] e^{-r\tau} \, \mathrm{d}\tau + \max\{J_P(z,\lambda), 0\} e^{-(r+\lambda)\Delta} - c - c_1 e^{-\lambda} \\
&\quad + \int_0^\Delta [w(z,\lambda,\Delta) - rU] e^{-r\tau} \, \mathrm{d}\tau + \max\{W_P(\lambda,\Delta), U\} e^{-(r+\lambda)\Delta} + U(1 - e^{-(r+\lambda)\Delta})
\end{aligned} \tag{2.8}
$$

where Δ represents the (optimally chosen) contract duration. We define the cost of

writing a temporary contract as $c + c_1 e^{-\lambda}$, where c is a component associated to writing a generic employment contract and c_R is a component dependent on the frequency of the shocks. We assume $c_R = 0$ in the pre-reform period and $c_R > 0$ in its aftermath, reflecting the intention of the regulator to discourage the use of temporary contracts to govern employment relationships with a long expected duration (i.e. those that are not expected to be hit by shocks). While most of the D.D.'s regulatory requirements kick in at specific duration thresholds (12 and 24 months), we choose a cost function that is continuous in λ to allow for a more tractable and interpretable structure.

The optimal duration of a temporary job is chosen so as to maximise its expected surplus.

$$\Delta^* = \begin{cases} \frac{1}{\lambda} \ln\left(\frac{rU+\lambda F+(r+\lambda)c}{rU}\right) & \text{if } \lambda < \lambda_P(z) \\ \frac{1}{\lambda} \ln\left(\frac{z}{rU}\right) & \text{if } \lambda \geq \lambda_P(z). \end{cases} \tag{2.9}$$

The optimal duration value Δ^* is a continuous decreasing function of λ, ranging from infinite (when $\lambda \to 0$) to zero (when $\lambda \to \infty$). It displays a kink at λ_P, generated by the possibility to convert a temporary contract into a permanent one at expiration.

If $\lambda < \lambda_P$, there is a positive surplus from converting the temporary contract in the future: Δ^* is thus obtained by substituting $S_P(z, \lambda)$ (from equation 2.7) into F.O.C. in Equation B.3. When $\lambda \geq \lambda_P$, no positive surplus can emerge from converting the temporary contract at expiration, so the duration is calculated by considering only the value of the temporary contract itself.

Having derived the optimal contract duration, we can discuss the properties of the surplus associated with starting a temporary position. $S_T(z, \lambda, c_R)$ is continuous and monotically decreasing in λ for sufficiently small values of c_R. In this case, there exists a unique threshold λ_T such that $S_T(\lambda_T) = 0$ and $S_T(\lambda_T) > 0$ if and only if $\lambda < \lambda_T$. An increase in firm productivity also affects the surplus associated with temporary positions: for any given λ, a higher z implies a higher value for the surplus of starting a temporary position. The marginal impact of z when $\lambda < \lambda_P(z)$ is the same as the one associated with permanent positions. When $\lambda \geq \lambda_P(z)$, the marginal impact of z is still positive, but smaller. Finally, an increase in c_R reduces the surplus generated by a temporary position, and the effect is more pronounced for lower λ. This feature creates a similarity between the model and the actual reform, which was mostly targeted at stable (long-duration) temporary contracts (see Section B11.1 for proofs).

The Contract-type Choice

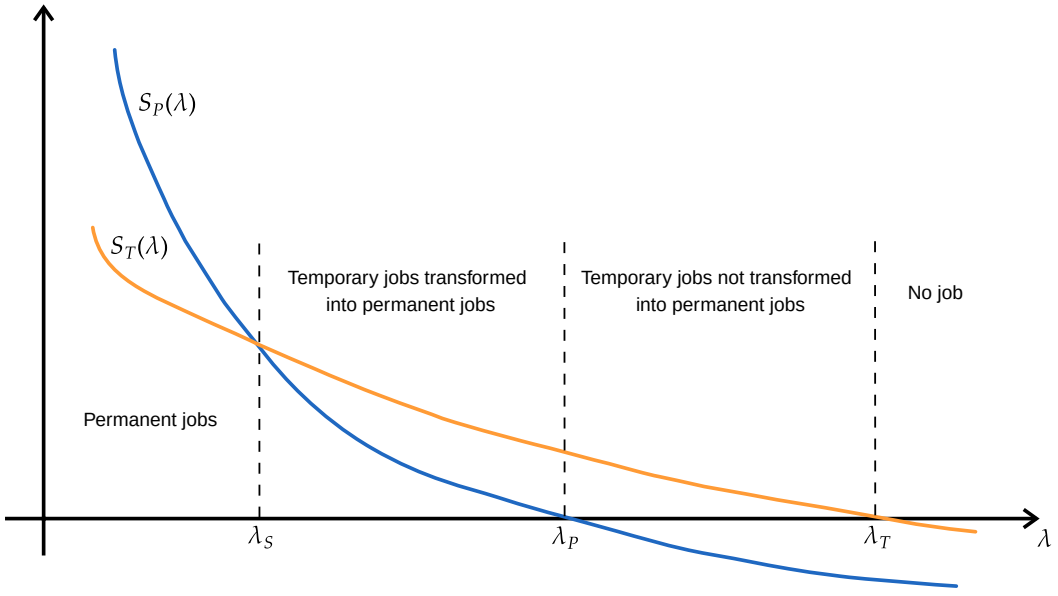
Whenever a job is created, firms and workers choose the type of contract with the highest surplus given the draw of λ of the match[17]. To model this choice, we define function $h(\cdot)$ as the surplus-gap between temporary and permanent contracts:

$$h(\lambda, c_R) = S_T(z, \lambda, c_R) - S_P(z, \lambda) = \frac{\lambda F - \lambda U(1 - e^{-r\Delta^*})}{r + \lambda} - c_1 e^{-\lambda} \tag{2.10}$$

The decision on which contract to use depends on the sign of $h(\cdot)$. This function starts from a negative value, is first decreasing, and then increasing. The value λ_S such that $S_T(\lambda_S) = S_P(\lambda_P)$ is unique (see Section B11.2 for proofs).

When $S_T(\lambda_P) > 0$, $h(\lambda_P) > 0 = h(\lambda_S)$. Since $h(\lambda, c_R)$ is positive for $\lambda > \lambda_S$, and negative when $\lambda < \lambda_S$, then $\lambda_P > \lambda_S$. Moreover, given that $S_T(\cdot)$ is monotonically decreasing in λ, $S_T(\lambda_P) > 0 = S_T(\lambda_T)$ implies $\lambda_T > \lambda_P$. Therefore $0 < \lambda_S < \lambda_P < \lambda_T$. This condition guarantees the equilibrium briefly described in Section 2.4.1 and represented in Figure 2.5.

FIGURE 2.5. Surplus as a function of the shock arrival parameter λ with z given

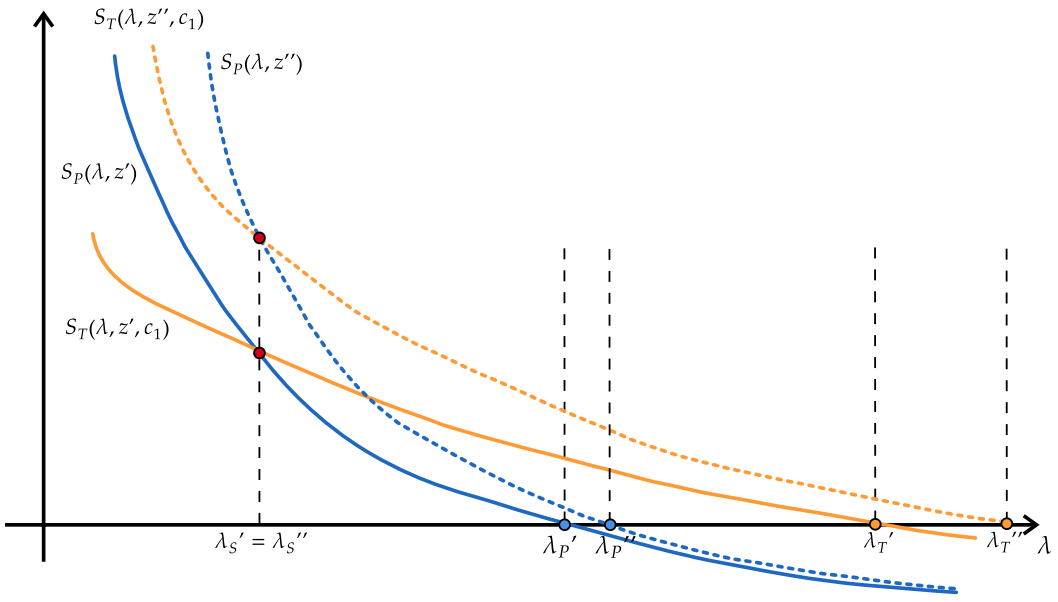


[17]We focus on the empirically relevant case when $c > 0$ and $S_T(\lambda_P) > 0$. See Cahuc et al. (2016) for a discussion of the other cases

Permanent contracts are used for jobs with low-shock arrival rates ($\lambda < \lambda_S$), while temporary contracts are used for more risky positions ($\lambda > \lambda_S$). However, not all temporary jobs face the same fate at the end of the contract: for the fraction of temporary jobs with a relatively low shock arrival rate ($\lambda_S < \lambda < \lambda_P$), the fact that $S_P(\lambda) > 0$ implies that it will be beneficial to continue the employment relationship by converting the temporary job to a permanent position when the contract expires (conditional on no shock having hit the match). On the contrary, for temporary jobs with $\lambda_P < \lambda < \lambda_T$ the expiration of the temporary contract will imply the breaking of the match.

The Productivity Shifter. Figure 2.5 analyses the equilibrium for a given value of the productivity-shifter z. We now turn to studying how firm heterogeneity affects job creation. In equilibrium, λ_S is not affected by z, while λ_P and λ_T both increase when z increases slightly (see Section B11.2 for proofs). Job creation for a low-z firms (z') against a high-z firm (z'') is represented in Figure 2.6.

FIGURE 2.6. Surplus as a function of λ for low (z') and high (z'') productivity firms



The Distribution of the Shock Arrival Rate. We assume that the arrival rates of productivity shocks follow a Weibull distribution with a scaling factor z. This implies that the

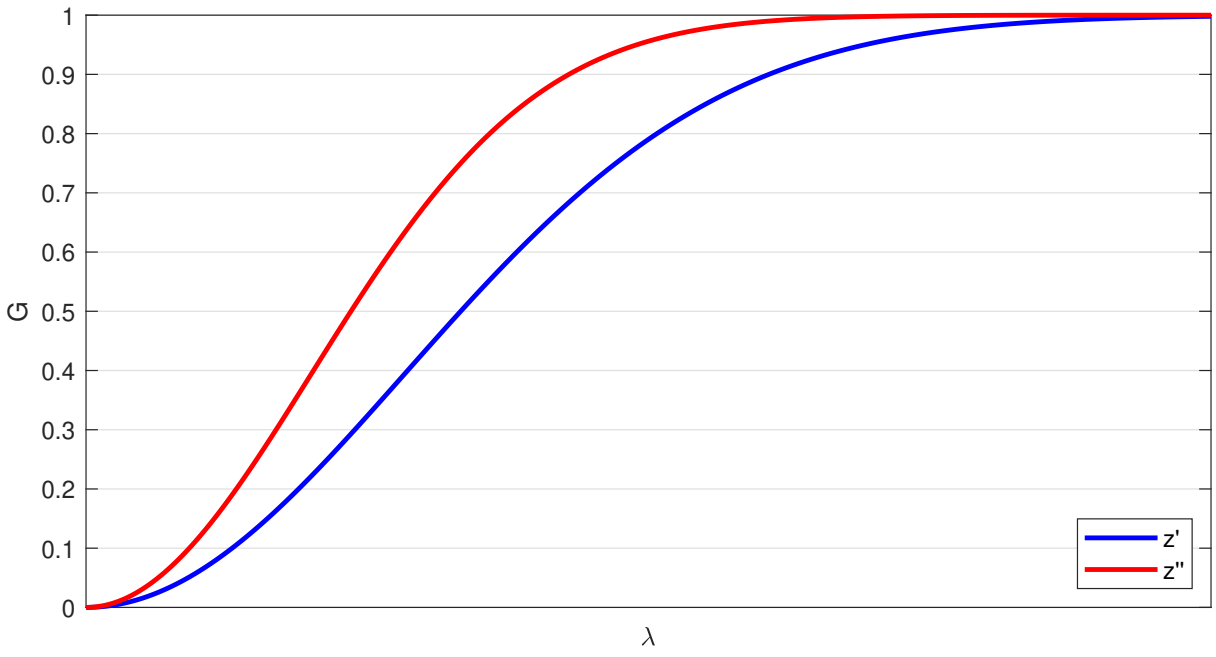
density function $g(\lambda, z)$ and the cumulative distribution function $G(\lambda, z)$ of the arrival rates have the following form:

$$G(\lambda, z) = 1 - e^{-z\left(\frac{\lambda}{\mu}\right)^{\sigma}} \tag{2.11}$$

$$g(\lambda, z) = \left(\frac{z\sigma}{\mu}\right) \cdot \left(\frac{\lambda}{\mu}\right)^{\sigma-1} \cdot e^{-z\left(\frac{\lambda}{\mu}\right)^{\sigma}} \tag{2.12}$$

Here, μ represents the scale parameter of the Weibull distribution, σ represents the shape parameter, λ represents the productivity shock arrival rate, and z is the productivity shifter at the firm level. The scaling factor z modifies the shape and scale of the Weibull distribution, allowing for variations in the arrival rates of productivity shocks based on the firm's productivity level. The derivative of the cumulative distribution function $G(\lambda, z)$ with respect to z is always positive (see Section B11.2). This suggests that higher productivity is associated with a higher incidence of low-λ jobs.

FIGURE 2.7. $G(\lambda, z)$ as a function of λ for low (z') and high (z") productive firms



The role of the firm-specific productivity shifter (z) in job creation is twofold: firstly, it expands the set of λ with a positive surplus, allowing for more temporary jobs to be converted into permanent positions and increasing the overall number of contracts that

are beneficial to start; secondly, it influences the distribution of productivity shocks, leading more productive firms to encounter relatively more low-risk job opportunities.

The Impact of the Reform on Job Creation

The reform increases contract costs of temporary jobs. In the model, this is represented by an increase in the regulation-driven cost component c_R We start by investigating the impact of the reform on permanent hirings, i.e. we analyze how λ_S is affected by c_R in equilibrium. Studying the impact of the reform on λ_S implies understanding what portion of the support of λ will translate into the direct signing of a permanent contract upon matching. By deriving, we obtain

$$\frac{\partial \lambda_S}{\partial c_R} > 0$$

We conclude that the reform increases the share of λ values where $S_P(\lambda) > S_T(\lambda)$. We then turn to investigate the impact of the reform on temporary hirings. We first derive the effect of a change in c_R on λ_T taking z as given.

$$\frac{\partial \lambda_T}{\partial c_R} < 0 \tag{2.13}$$

An increase in c_R decreases temporary hirings, i.e. it restricts the set of λ with positive temporary-contract surplus.

2.4.5. Labor Market Equilibrium

In this section, we determine the labor market equilibrium by formally describing the process of matching between workers and jobs in heterogeneous firms.

The Value of Vacancies and Unemployment

Each firm is characterized by a level of productivity z, drawn once and for all from the distribution $H(z)$ at the moment of its incorporation, which determines the level output-per-unit-of-time of each worker in the firm. We assume that the number of firms is given. To produce, a firm must hire workers, and to do so it needs to open vacancies. Each vacancy creates the chance for a match with an unemployed worker. Each match is characterized by a specific shock-arrival rate λ drawn from the distribution $G(\lambda, z)$,

which determines the expected duration of the production span associated with the match.

To open v vacancies a firm needs to pay a cost, that is determined by the convex function $C(v) = (\frac{\gamma}{\alpha+1}v^{\alpha})v$ with elasticity $\alpha > 0$ and $\gamma > 0$ as a scale parameter. A firm opens vacancies up to the point where the marginal cost of an additional vacancy equals its expected value, conditional on $\{z, \lambda\}$:

$$\gamma v(z)^{\alpha} = \int max\{V(z, \lambda), 0\} dG(\lambda, z) \tag{2.14}$$

where $V(z, \lambda)$ corresponds to the value of a type-$\{z, \lambda\}$ vacant job.

The matching between unemployed workers and job vacancies is governed by a constant return to scale matching technology $q(\theta)$, strictly decreasing in θ ($q'(\theta) < 0$), where the aggregate $\theta = \frac{N}{u}$ denotes the labor market tightness and is defined as the ratio of total vacancies over total unemployment. The bargaining process between workers and firms allows workers to obtain a share $\beta \in (0, 1)$ of the combined job surplus. Therefore, from the perspective of a firm, the value of a type-$\{z, \lambda\}$ vacant job today is

$$rV(z, \lambda) = q(\theta)[(1 - \beta)S(z, \lambda) - V(z, \lambda)] \tag{2.15}$$

The equation states that the discounted value of having a vacancy (left side of the equation) is equal to the expected net benefit from that vacancy (right side of the equation). The last term is composed of the expected surplus from a match, minus the cost of maintaining the vacancy, all multiplied by the job-filling rate.

We are left with deriving the value of unemployment. Denoting by ζ the unemployment utility flow, which can be positive or negative (Dey & Flinn 2005), we can write

$$rU = \zeta + \theta q(\theta)\beta \int_{z} \left[\int_{\lambda_{min}}^{\lambda_T(z)} \frac{S(z, \lambda)}{G(\lambda_T(z))} dG(\lambda, z) \right] dH(z) \tag{2.16}$$

The outer integral captures the variation in productivity in the labor market. The inner integral calculates the expected surplus from a job in a firm with productivity z. $\theta q(\theta)$ is the job-finding rate. This equation expresses that the value of unemployment is equal to the unemployment benefits plus the expected value of finding a job, where the expectation is taken over both the distribution of firm types and the distribution of production opportunities within each firm type. Combining Equations 2.14, 2.15 and

2.16, we get

$$rU = \zeta + \frac{\beta\theta[r + q(\theta)]}{(1-\beta)} \int_z \frac{\alpha\gamma\nu(z)^{\alpha-1}}{G(\lambda_T(z), z)} dH(z) \tag{2.17}$$

An increase in labor market tightness improves the expected gains of unemployed workers, as it increases the arrival rate of job offers. Conversely, it decreases the value of a type-$\{z, \lambda\}$ vacant job for a firm since the vacancy-filling rate gets reduced. We study the case with both contracts in place, i.e. when firing costs F are sufficiently large. Combining equations 2.14 and 2.15, the free entry condition for a firm with productivity z becomes

$$\gamma\nu(z)^{\alpha} = \frac{q(\theta)(1-\beta)}{r + q(\theta)} \left[\int_{\lambda_{min}}^{\lambda_S} S_P(z, \lambda) dG(\lambda, z) + \int_{\lambda_S}^{\lambda_T(z)} S_T(z, \lambda) dG(\lambda, z) \right] \tag{2.18}$$

Intuitively, high-z firms create more vacancies. The marginal cost of opening a vacancy is strictly increasing, preventing more productive firms from opening infinitely many vacancies. In Appendix B11.4, we demonstrate that the equilibrium value of θ is consistently unique across the distribution of z. The underpinning economic rationale is the presence of a universal market tightness that balances the market for all firms, irrespective of their individual productivity levels. Although high-productivity firms experience greater surpluses, they also encounter elevated vacancy costs. These factors counterbalance each other, ensuring that a singular θ can bring every firm to equilibrium.

Employment Flows

For any given z, labor market flows are given by:

$$\begin{aligned} \Phi_P(z, \theta) &= q(\theta)\nu(z) \int_{\lambda_{min}}^{\lambda_S} dG(\lambda, z) \\ \Phi_{TC}(z, \theta) &= q(\theta)\nu(z) \int_{\lambda_S}^{\lambda_P(z)} dG(\lambda, z) \\ \Phi_{TNC}(z, \theta) &= q(\theta)\nu(z) \int_{\lambda_P(z)}^{\lambda_T(z)} dG(\lambda, z) \end{aligned} \tag{2.19}$$

The equations describe the flows of workers into different types of employment contracts at different firms. The subscript P represents permanent contracts, while the

subscript T denotes temporary contracts, which are further divided into fixed-term contracts that are converted at expiration (C) and those that are not converted (NC). The term $q(\theta)v(z)$ represents the total rate at which vacancies are filled. The integral terms denote the proportion of jobs within a certain range of λ. An increase in labor market tightness (θ) reduces the job-filling rate, resulting in decreased flows into all contract types. Conversely, an increase in productivity (z) leads to higher flows into both permanent and converted temporary contracts, but the effect on non-converted temporary contracts is ambiguous (see Section B11.4). The relative flows, obtained by dividing each flow by the total flows, correspond to the theoretical counterpart of the ϕ introduced in Section 2.3.1. The effect of z on relative entries is also ambiguous and requires quantitative evaluation for a better assessment.

Employment Stocks

The equation for the size of a firm, denoted as $L(z, \theta)$, represents the total days worked within the firm as a function of its productivity z and the labor market tightness θ:

$$L(z, \theta) = q(\theta)v(z) \left[\int_{\lambda_{min}}^{\lambda_P(z)} \frac{1}{\lambda} dG(\lambda, z) + \int_{\lambda_P(z)}^{\lambda_T(z)} \Delta(z, \lambda) dG(\lambda, z) \right] \tag{2.20}$$

In the first integral, $\frac{1}{\lambda}$ represents the average duration of a permanent job, implying that such a job will continue until it becomes unproductive. The integral calculates the sum of the durations of all permanent jobs, weighted by their respective probabilities given by the distribution $G(\lambda, z)$. The second integral reflects the sum of the durations of non-converted temporary jobs, also weighted by their respective probabilities.

Aggregate Outcomes

If N is total population, exploiting 2.20 the unemployment stock u can be retrieved as

$$u = N - L = N - \int_z L(z, \theta) dH(z) \tag{2.21}$$

while the total number of vacancies is

$$v = \int_z v(z, \theta) dH(z) \tag{2.22}$$

2.5. Model Estimation

In this section, we present the estimation strategy for our model. We need to determine the values for 12 parameters. Of these, 3 are directly parameterized while the remaining 9 are estimated jointly. The estimation is carried out using data for the eighteen-month period preceding the introduction of the reform, specifically from January 2017 to June 2018. We first discuss the assumptions regarding functional forms, then move on to detail the baseline parameter values and conclude by discussing the identification and the estimation of the other parameters.

Functional Forms and Assumptions. Drawing from the approach in Cahuc et al. (2022), we assume a Cobb-Douglas matching function that is homogeneous of degree one. Consequently, the probability of filling a vacancy, denoted as $q(\theta)$, is formulated as $q(\theta) = m\theta^{-\eta}$, where η represents the elasticity of the matching function concerning unemployment, and $m > 0$ acts as a scaling parameter. For the productivity shifter z we assume a discrete distribution: we split firms in our sample into ten groups based on the empirical deciles of within-industry past labor productivity[18] and use them as direct inputs into the structural framework.

Parametrization of Baseline Parameters. We set our time frame at 1 month and therefore set the discount rate r at 0.41%. This corresponds to an annual discount rate of 5%. In line with the established literature (Cahuc et al. 2016; Pissarides 2009; Pissarides & Petrongolo 2001), we assign both the bargaining power parameter β and the elasticity of the matching function of η a value of 0.5.

Estimation and Identification of the Remaining Parameters. We estimate the model in the pre-reform period, so we set $c_R = 0$ for the moment. This leaves us with nine parameters to estimate: the firing costs F; the cost of writing a generic contract c; parameters μ and σ that dictate the distribution of productivity shock arrival rates; α and γ, which are parameters of the vacancy cost function; the scale parameter m of the matching function; the instantaneous utility of unemployment ζ. We define the vector $\Omega = \{U, F, c, \mu, \sigma, \alpha, \gamma, m, \zeta\}$. This vector also contains the value of unemployment, which is a combination of the other parameters. Our estimation approach uses a two-step Simulated Method of Moments (SMM) procedure. The objective is to identify the

[18] Past productivity is measured as the average yearly value added per worker from 2012 to 2016, adjusted to a monthly rate by dividing by 12. See Section 2.3.1

$\hat{\Omega}$ that satisfies the following expression:

$$\hat{\Omega} = \arg\min_{\Omega} \left[\mathbf{p} - \mathbf{p}(\Omega)\right]' \Lambda^{-1} \left[\mathbf{p} - \mathbf{p}(\Omega)\right] \tag{2.23}$$

Here $\mathbf{p}$ is a vector of forty elements: four moments from our data each evaluated ten times, once for each tenth of the past labor productivity distribution. The chosen moments are the average monthly hirings for each of the three contract "types" identified by our model (permanent, fixed-term that get a conversion, fixed-term that are terminated at expiration, see Figure 2.5) and the average duration of fixed-term contracts. The vector $\mathbf{p}(\Omega)$ represents its theoretical equivalent, as determined by the model. The matrix Λ^{-1} is a symmetric and positive definite weighting matrix. We present in Appendix B12 a comprehensive overview of the estimation procedure.

In essence, the model parameters are jointly identified using the gross flows by contract type and the average duration for new temporary entries, all in relation to the distribution of productivity z. The estimated parameter values, along with their standard errors, are detailed in Table 2.2. The present value of unemployment is estimated to be around 900 euros. Firing costs represent approximately 30% of the average annual labour costs per employee[19]. The cost to write a contract is roughly 2 cents. The parameters of the vacancy cost function are in line with the literature (Gavazza et al. 2018). The average time to fill a vacancy $1/q(\theta)$ is 43 days. The flow utility of unemployment is negative as in Dey and Flinn (2005) and Cahuc et al. (2022).

The Hansen over-identification test presented at the bottom of Table 2.2 does not reject the null hypothesis. The validity of our approach is further confirmed by the model's fit illustrated in Figure 2.8. In this figure, we see that our model closely mirrors the empirical distribution of gross flows for different contract types, as well as the average duration of new temporary contracts, both of which are plotted against deciles of past *within-industry* labor productivity.

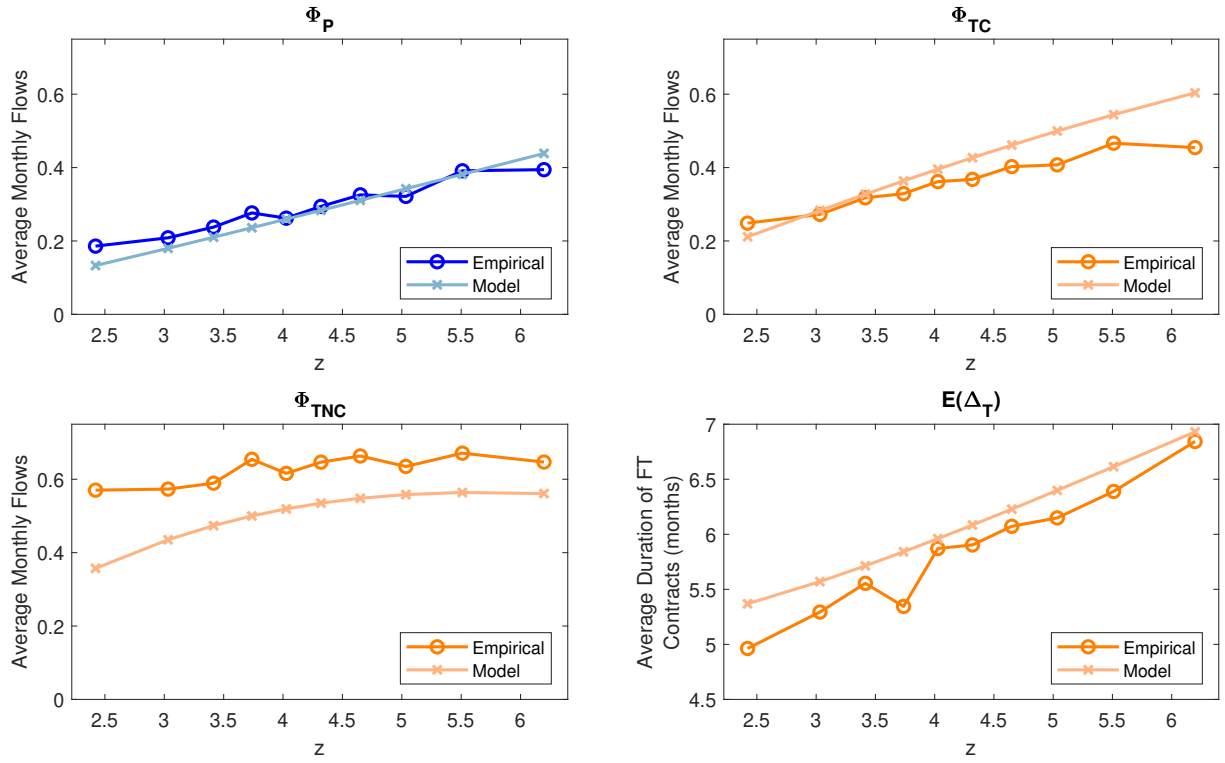
[19] corresponding to 41,081 euros in 2020 according to Istat.

TABLE 2.2. SMM Estimation Results

Parameter	**Notation**	**Value**
Baseline Parameters		
Discount rate	r	0.0041
Workers' bargaining power	β	0.5
Elasticity of the matching function	η	0.5
Estimated Parameters		
Value of Unemployment	U	190.441
		(0.062)
Firing costs	F	10.722
		(0.025)
Cost to write a contract	c	1.6e-05
		(1.2e-06)
Weibull scale	μ	323.471
		(0.014)
Weibull shape	σ	0.072
		(0.001)
Scale parameter of the vacancy cost function	γ	5.259
		(0.008)
Elasticity of the vacancy cost function	α	5.208
		(0.198)
Scale parameter of the matching function	m	0.610
		(0.007)
Flow utility of unemployment	ζ	-81.344
		(0.002)
Hansen statistic		5.387
		$\chi^2(40 - 9)$

Notes: This table reports the point estimate and the standard errors (in parentheses) for the model's parameters. The value of unemployment, the firing costs, and the cost of writing a contract are expressed in thousands of euros. The bottom row presents the value of the Hansen statistics, which is compared to a chi-squared distribution obtained by subtracting the number of parameters from the number of moment conditions. For more details on the estimation process, refer to Section 2.5 and Appendix B12.

FIGURE 2.8. Goodness of Fit: Empirical vs. Model Moments



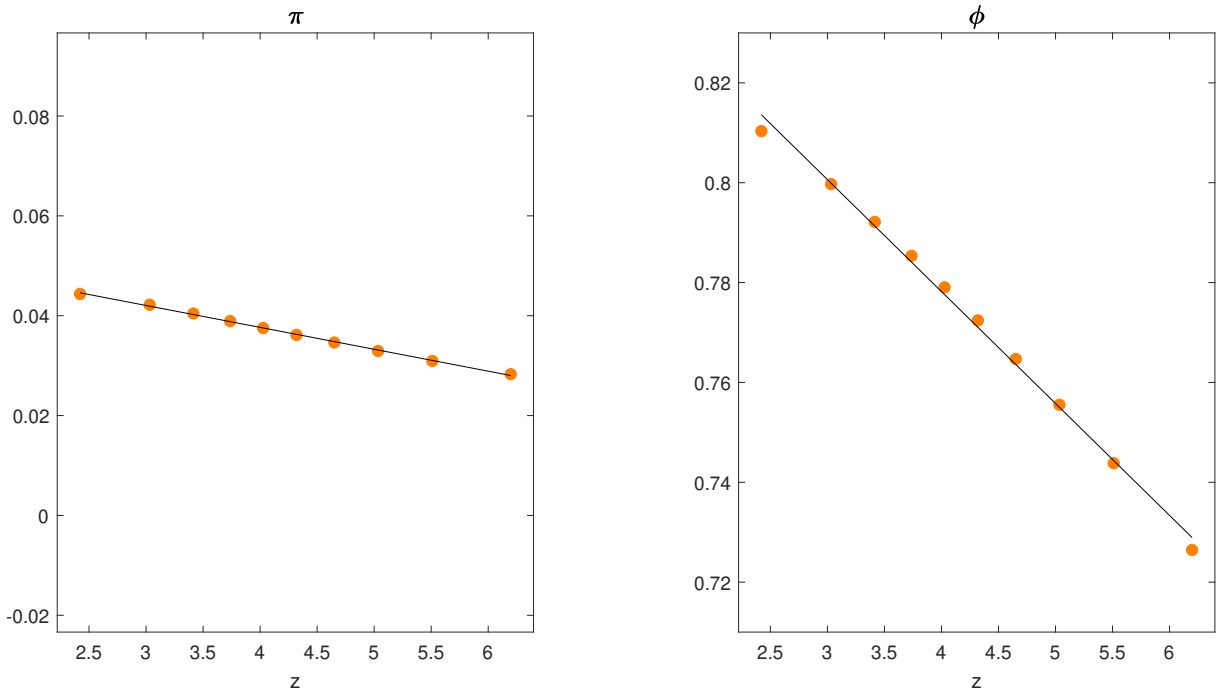
Notes: The graph presents the empirical and theoretical distributions of the average monthly number of gross flows by contract type and the average duration (in months) of new temporary contracts. These are evaluated across the deciles of *within-industry* past labor productivity z, which is expressed in thousands of euros. The subscript P represents permanent contracts, while the subscript T denotes temporary contracts, which are further divided into fixed-term contracts that are converted at expiration (C) and those that are not converted (NC). The duration Δ_T represents a weighted average of durations for both converted and non converted fixed-term contracts. For more details on the estimation process, refer to Section 2.5 and Appendix B12.

Figure 2.9 examines the relationships derived from our model between z and the two key metrics associated with the use of FT contracts, as discussed in Section 2.3.1. The left panel of the figure displays the share of temporary job stocks in relation to the total employment stocks (π). The right panel depicts the share of flows into fixed-term contracts (ϕ). The data points, represented by orange dots, indicate the average values of both variables at each decile of z. The estimated model suggests a weak correlation between productivity and the relative shares of employment stocks, but a strong negative correlation between productivity and relative flows. Remarkably, the two scales[20] and the two slopes of these relationships resemble their empirical counterparts presented in Figure 2.2. The estimated model is thus able to closely replicate the empirical relationships uncovered in Figure 2.2. This congruence is particularly noteworthy given

[20]The observed discrepancy between the scales of the empirical and model-generated values of π could possibly be due to the assumption of exogenous separations within our model. This could lead to an overestimation of the average tenure associated with permanent job positions.

that two of the metrics (π and ϕ) were not direct targets of our estimation process.

FIGURE 2.9. Estimated Model Predictions: Temporary Jobs Utilization vs. Productivity



Notes: The figure depicts the relationships, as estimated from the model, between the *within-industry* past labor productivity (z) and two metrics associated with the utilization of fixed-term (FT) contracts. Orange dots represent the average values for each outcome variable at each decile of z, while the black line shows the regression fit. The left panel depicts the proportion of temporary job stocks to the total employment stocks (π), and the right panel presents the proportion of flows into fixed-term contracts (ϕ).

2.6. Discussion and Next Steps

Temporary contracts have expanded greatly in many European countries over the last three decades, as a result of the progressive deregulation of their use chosen by policymakers. In recent years, governments in many countries have attempted to reverse this trend by re-imposing fiscal and regulatory restrictions on the use of temporary contracts.

In this paper, we study how firm heterogeneity interacts with the choice between permanent and temporary contracts and how firms' response to regulatory interventions differs depending on this heterogeneity. We first provide empirical evidence that firm productivity is an important determinant of contract-type choices. We first provide empirical evidence that firm productivity is an important determinant of the choice of contract type. We show that past labour productivity, although uncorrelated with the firm-level share of temporary workers at a given point in time, is strongly associated

with their turnover, with highly productive firms offering fewer temporary contracts of longer average duration. In addition, we show that firms with different levels of productivity respond differently to changes in the stringency of temporary contract regulation. We then rationalise these patterns by incorporating them into a model that allows us to identify separately the exogenous and endogenous characteristics that influence the choice of contract that firms make for each job.

We use this model to analyse the effects of a change in the regulation of temporary contracts. An increase in the regulatory burden on temporary contracts leads to a generalised increase in permanent hiring; on the contrary, firm heterogeneity plays a role in determining the strategy towards temporary contracts: all firms reduce hiring, but more productive firms to a lesser extent. Overall, the model is able to generate different hiring patterns for heterogeneous firms, consistent with what is observed in the empirical analysis. The next steps are to estimate c_R, i.e. the monetary cost of the reform, and to use the model to carry out counterfactual exercises.

A limitation of our study is its focus on firms that always had 5 or more employees throughout our observation period. Given the notoriously right-skewed distribution of firm sizes in Italy, this restriction excludes the vast majority of firms and a substantial share of employment from our sample. However, given that one of the aims of this study is to identify the extent to which the variability of temporary employment is driven by firm-specific factors, we consider it appropriate to focus only on firms with a non-negligible number of dependent employees. Another limitation is the short time window that we analyse: if productivity is indeed a profound driver of temporary employment decisions, its effects would be best studied over a long period of time. Unfortunately, the almost continuous stream of legislative changes, subsidies and other policy interventions that characterise the Italian context, together with the arrival of the Covid-19 pandemic, limits our useful observation window to the period 2017-2019. We argue that the use of monthly data partially addresses this shortcoming. The relatively high frequency of these data also helps to support the claim that the discontinuities observed in our regression estimates are indeed picking up the effect of the reform, and not some other trend.

Despite these drawbacks, we believe that our study sheds light on the main mechanisms behind the dynamics of temporary employment, with three important takeaways.

(a) **Firms cannot be ignored when studying temporary work**. Most of the variability in the use of temporary agency work can be explained by firm-specific factors rather than by characteristics of the sector, the worker or the match.

(b) **Firms determine *how*, rather than *how much*, temporary contracts are used.**
More productive firms offer contracts of longer duration and convert many more of them into permanent jobs. In other words, firm productivity is a strong predictor of whether a temporary job turns out to be a stepping stone or a dead end for the worker.

(c) **The effects of regulatory interventions depend on the way firms use temporary work.**
When faced with restrictions on the use of fixed-term contracts, productive firms will be more likely to convert workers into permanent jobs, while less productive firms will tend to let the worker go. This is consistent with the different scope for using this type of contractual arrangement. Ultimately, therefore, firms are also key to explaining what happens after changes in employment protection legislation.

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3. Dignity by Decree? Temporary Jobs Reforms and Workers' Wages*

Abstract

In recent years, several European countries have modified their policies on fixed-term contracts, oscillating between liberalization and restriction in order to balance employment flexibility and job stability. We analyze a 2018 reform that tightened the legislation on temporary contracts in Italy, and examine its impact on wage dynamics. Our analysis, grounded in extensive administrative data covering the universe of dependent employees in Italy, reveals a sizeable decline in the starting wage of workers whose contract gets converted from fixed-term to permanent. We suggest two possible explanations for this decline. First, the new policy regime could induce changes in the composition of the pool of converters. Second, the decline in wages could be caused by a decrease in the surplus generated by each job, either through a direct increase in labor costs or through an increase in uncertainty about the quality of the worker. Our results suggest the existence of some degree of market power on the part of firms, with increased security for the workers being partly compensated by lower wages.

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3.1. Introduction

Temporary contracts play an important role in the labor markets of several European countries, accounting for a wide proportion of entries into employment (Cahuc et al. 2016). However, they are a subject of contention among labor economists and policymakers. Their advocates claim that they provide considerable employment gains and serve as a stepping stone to more stable and better jobs, while their critics argue that they represent a precarious form of employment, leading to excessive worker turnover and dead-end jobs.

In recent years, concerns about job instability have led several countries to gradually reintroduce restrictions on the use of temporary contracts, marking a significant shift from the previous policy regime. A number of studies have examined how the labor market responds to this type of reform through adjustments in employment, occupational flows and skill demand (Cahuc et al. 2022; Grasso & Tatsiramos 2023; Palladino & Sartori 2023). However, there is still little evidence on whether these occupational changes are accompanied by adjustments along the other relevant margin, *wages*.

This paper provides new insights into this issue, examining the dynamics of workers' salaries around a 2018 reform that restricted the terms of use for temporary contracts in Italy. To evaluate the effects of the reform, we leverage administrative records that link Italian firms to their employees. These data allow us to track workers' wages at a monthly frequency. Moreover, they allow us to observe numerous firm-level characteristics as well as several individual characteristics over his entire employment history.

Our primary objective is to examine the wage changes experienced by workers who move from a fixed-term to a permanent contract. To this end, we focus on *contract conversion* events, instances in which a worker moves from a temporary to a permanent contract within the same firm, and compare the associated wage dynamics under different policy regimes. Our empirical approach is based on a regression framework that allows for firm heterogeneity - in terms of wage premia and the volume of conversions - and also flexibly controls for a rich set of individual characteristics - capturing differences in worker composition and earnings potential. This framework allows us to identify the change in the wage premium associated with contract conversions, controlling for both observed and unobserved factors.

Our main finding is that contract conversions become substantially less rewarding in monetary terms as a consequence of the reform: after July 2018, we observe a significant decline in the starting wage of open-ended contracts that are generated from a

conversion – of between 2 and 5 log points, depending on the specification. Importantly, the estimates suggest that composition effects play a role in determining this result: adding a rich set of worker-level controls and accounting for heterogeneity in earnings potential, which we proxy with pre-transformation wages, shrinks the estimated earnings gap by about half its size. However, even after controlling for such differences, we still observe a large and persistent post-transformation wage penalty for converted workers.

In principle, this result can be reconciled with two different types of explanation: (1) changes in the *surplus* generated by each job; or (2) changes in the way the gains from each job are shared through *rent sharing*. The first avenue posits that the new policy regime creates additional costs and risks that are shared by the firm and the worker: on the one hand, the reform imposes new direct costs on firms offering temporary jobs (see sections 3.2 and (Palladino & Sartori 2023) for a more detailed description); on the other hand, the reform pushes firms to anticipate contract conversions for individuals deemed riskier ex ante, with a shortening of the period in which the worker can be evaluated and a consequent internalisation of uncertainty about the quality of the match by the firm.

The second possible explanation suggests a redefinition of how firms and workers share the surplus generated by each game. If the reform increased firms' bargaining power (without increasing costs), we could still observe a reduction in workers' pay. However, given the characteristics of the reform, which imposed additional constraints on firms, we do not consider this explanation credible. At present, therefore, our interpretation of the results is mainly linked to a contraction in surplus. What remains to be investigated is whether firms with greater market power were able to force a larger reduction in workers' wages and what part of the wage reduction can be attributed to uncertainty effects. We intend to address this issue as part of the further development of the paper (see Section 3.6).

Related Literature. The paper contributes to three strands of the literature. First, we contribute to the literature that empirically documents the existence of wage increases for workers transitioning from temporary to permanent contracts. Using a linked employer-employee data set for Italy, Daruich et al. (2023) find an increase in log daily wages of about 6% when switching to a permanent contract. Similarly, using administrative data for Portugal, Silva et al. (2018) provide evidence that temporary workers undergoing contract conversion experience significantly higher hourly wage growth at the time of conversion than their non-converted counterparts. We contribute to this body of work

by providing evidence that reforms limiting the use of fixed-term temporary contracts can compress the wage premium resulting from conversion from a temporary to a permanent contract.

Second, we contribute to the body of work that examines the effects of restricting fixed-term contracts. Cahuc et al. (2022) investigate the employment effects of one such reform implemented in Portugal. Using a reduced-form approach, they find that it succeeded in increasing the number of new permanent jobs, although it was accompanied by a larger reduction of temporary jobs, thus leading to an overall negative employment effect. However, using a directed search and matching model, they find ambiguous effects on total employment. This is due to general equilibrium spillover effects whereby smaller firms not directly targeted by the reform can increase employment. Palladino and Sartori (2023) analyze the effect of the reform studied in this paper on firms' hiring strategies. Their results suggest that the main effect of the reform was a substitution between temporary and permanent employment, which was almost entirely driven by conversions of existing fixed-term contracts. In addition, Grasso and Tatsiramos (2023) examine the effects of the same Italian reform on the demand for labor and skills, using data from online job postings. They find that it led to a substitution in hiring intentions consisting of a fall in the share of job postings advertising positions under a temporary contract and a corresponding increase in the share of vacancies offering jobs with a permanent contract. They also show that, at the same time, the reform raised the skill requirements for entry into jobs, as firms demanded more signals of worker quality — such as higher educational qualifications, more work experience, and social skills — to compensate for higher screening and labor costs. Our contribution is to uncover the wage effects of restricting the use of temporary contracts.

Third, this paper is related to the broader set of works on two-tier labor markets, which study the divide between temporary and permanent jobs and its impact on different groups of workers (Barbieri & Cutuli 2016; Bentolila et al. 2012; Blanchard & Landier 2002; Boeri & Garibaldi 2007; Cappellari et al. 2012; Cazes & Tonin 2010; Kahn 2007; Saint-Paul 1996).

Outline. The remainder of the paper is organized as follows: Section 3.2 introduces the institutional setting and describes the reform. Section 3.3 presents the data and some descriptive statistics. Section 3.4 illustrates the empirical strategy used to conduct the analysis, the results of which are presented in Section 3.5. Lastly, Section 3.6 presents a discussion and outlines the next steps envisaged for the future development of the paper.

3.2. Institutional setting

The *Decreto Dignità* reform was enacted by the newly elected Italian government in July 2018. It consisted of a set of changes to labor market legislation aimed at reducing the recourse to temporary employment in favor of permanent jobs. Its provisions mainly concern the repeated and prolonged use of fixed-term contracts.

3.2.1. Temporary Employment Agreements in Italy

In Italy, the main method of establishing a working relationship is through a permanent or *open-ended* contract. Despite representing a small share of the total employment stock, the use of temporary contracts has increased steadily over the last three decades and now accounts for a significant majority of labor flows.

The *Contratto a tempo determinato*, or fixed-term contract, is the predominant form of temporary employment in Italy. First introduced in 1962, this type of contract was designed to meet specific, temporary labor needs, such as seasonal work, temporary replacement of permanent staff and meeting specialised needs linked to unusual activities[2].

Fixed-term contracts in Italy have many features in common with other legislation in neighbouring European countries. Firstly, fixed-term contracts can be initiated either directly by the firm or through a recruitment agency. We focus on the more common case where the company itself initiates the contract. Secondly, these contracts are characterised by their flexibility, often including renewal options that allow both employer and employee to extend the duration of employment without amending the existing contract, typically within a limited timeframe[3]. Finally, these contracts can be converted to permanent contracts at any time before they expire or during an extension, facilitating a smooth transition to stable, long-term employment without bureaucratic complications.

A worker employed on a fixed-term contract is entitled to receive the same minimum wage as workers with open-ended contracts, set by the relevant National Collective Labor Agreement (*CCNL*), and proportional to the duration of the work performed. Despite this, the average hourly wage of permanent employees was 15% higher than that of workers on fixed-term contracts in Italy in 2017[4].

[2] Law 1962, n. 230

[3] Firms also have the option of recall, which involves re-employing individuals for several separate temporary periods with gaps in between.

[4] Istat, *Registro annuale su retribuzioni, ore e costo del lavoro per individui e imprese* (RACLI)

3.2.2. The 2018 Regulatory Shift

In July 2018, the newly-formed Italian government introduced a labor market law known as the "Decreto Dignità" or the *Dignity Decree* (Law Decree 2018, n.87), which marked a significant shift in labor policies that promoted the liberalization of temporary contracts in Italy and other major European nations since the 1990s. The central aim of this decree was to reduce precarious employment and increase the stability of employment contracts.

This reform, which drastically increased the relative cost of fixed-term contracts without changing the legal rules for permanent contracts, introduced several important changes. Specifically, the new rules introduced the following changes: (i) the maximum duration for a fixed-term work relationship, including all extensions, was shortened from 36 to 24 months, (ii) the number of allowable extensions for the termination date was reduced from 5 to 4 times, (iii) the social security contribution rate increased for *recalls*, essentially for cases where these weren't converted to permanent contracts[5], (iv) the decree reinstated the requirement to provide a broad motivation for opting for a fixed-term work relationship in cases where the contracts last for 12 months or longer. Other minor changes were included[6].

3.3. Data

The source of the data used in this paper comprises population-level administrative records linking Italian firms and dependent workers, provided by the Italian Social Security Institute (INPS) as part of the *Visitinps* project.

3.3.1. Matched Employer-Employee Data

We use the INPS administrative records, specifically the monthly reports known as *rapporti di lavoro mensili Uniemens*. The dataset covers all non-agricultural private-sector dependent workers from 1974 to 2021. We restrict our focus to the period between 2017 and 2019.

We use information on the total annual wage of each employee, the type of contract (permanent vs. temporary, full-time vs. part-time) and the total number of days and

[5]This rate went up from 1.4% to 1.9% of pre-tax earnings.

[6]The D.D. additionally establishes an increase in the statute of limitations to start litigation regarding a temporary contract (from 120 to 180 days after its end) and in the compensation due for an unlawful dismissal of a permanent worker (from 4-24 to 6-36 monthly wages)

weeks worked in a given year. To calculate the full-time equivalent (FTE) daily wage, we first adjust the total number of days worked in a year by the percentage of part-time employment. This percentage is 1 for full-time and vertical part-time employment and less than 1 for horizontal part-time employment. [7] We then derive FTE daily earnings by dividing total annual gross earnings by the total number of adjusted days worked in the year.

We use *Unilav* reports to look at individual transitions from fixed-term to permanent contracts. These reports are submitted to the INPS by employers whenever there's a change in the employment relationship, whether it's establishment, extension (for fixed-term contracts), conversion (for contractual changes) or termination. Within these reports, employers also indicate the occupation of the employee, identified by a 5-digit occupational code from the ISTAT CP2011 classification. The worker's level of education is also reported.

Sample Construction. We organise the final dataset on a monthly basis, with each entry representing a conversion from a fixed-term contract to a permanent contract that occurred within a given month. For each such event, we collect relevant information about the worker at the time of the conversion, including details such as daily wage, gender, experience (expressed as the number of months since first entering the labour market), tenure (expressed as the number of months since first joining the firm), as well as age, occupation and level of education. In addition, we record the average daily wage earned by the worker during the entire period of temporary employment before the conversion. We observe the firm in which the conversion took place. Our analysis is restricted to firms that have documented at least one conversion both before and after the reform. This allows us to account for potential selection bias in the composition of converting firms after the reform.

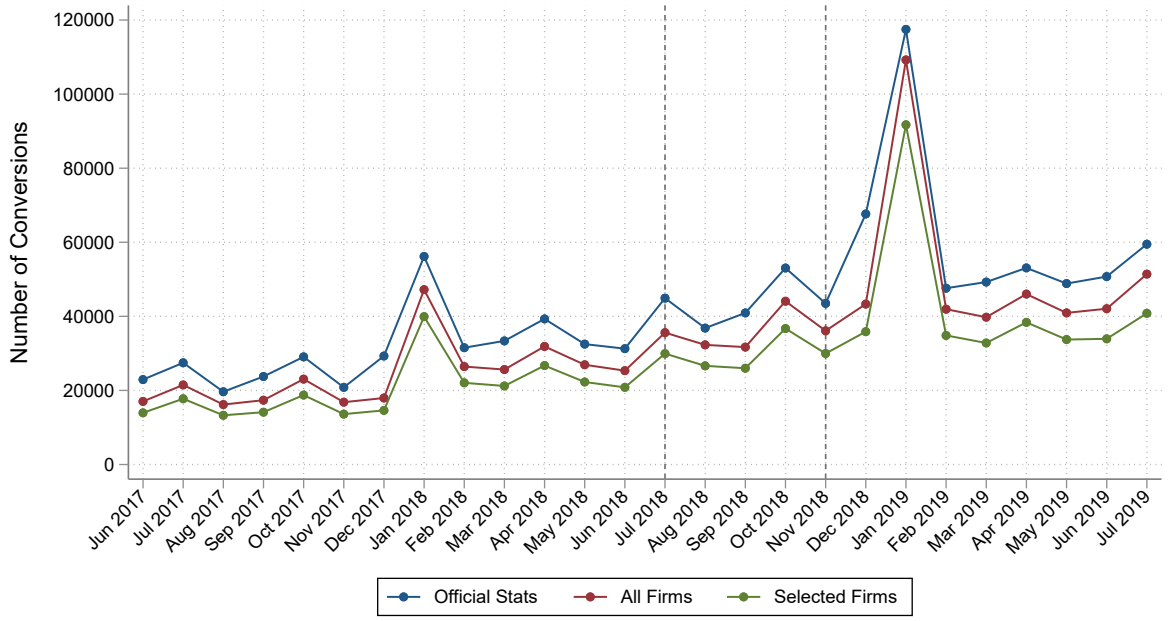
Figure 3.1 shows the trend in contract conversions over the selected period using three different data series. The blue series represents the official statistics as detailed in the INPS report[8]. The red series is derived from our initial sample, before narrowing it down to firms that recorded at least one conversion both before and after the policy

[7] A full-time contract is usually a 40-hour week (8 hours a day, 5 days a week), unless modified by collective agreements. Vertical part-time contracts are less than 40 hours a week, but retain an 8-hour day, spread over fewer than 5 days a week. These days are already counted as FTE. On the other hand, horizontal part-time contracts are less than 40 hours spread over 5 days. For example, a 75% contract means 30 hours per week or 6 hours per day, which is equivalent to 3.75 rescaled FTE days worked. Mixed part-time contracts, which account for about 1% of the observations, are excluded from our data.

[8] https://www.inps.it/it/it/dati-e-bilanci/osservatori-statistici-e-altre-statistiche/report-cartaceo.html

change. The green series represents the data after applying this selection criterion. Two vertical lines in the figure signal critical periods in the policy timeline: the first indicates July 2018, when the government introduced the decree, and the second marks the time of its full implementation after parliamentary approval. As can be seen, there is a significant increase in the number of conversions following the implementation of the reform. Notably, the trends shown by our refined sample closely match the trajectories shown in the other two series, highlighting its accuracy in capturing the shifts that occurred in the wake of the reform.

FIGURE 3.1. Monthly Number of Conversions of Fixed-Term to Permanent Contracts



Notes: The figure shows changes in the monthly number of contract conversions over time. The blue line comes from official INPS report data. The red line shows data from our first sample of firms. The green line shows data from a smaller group of companies that had at least one conversion before and after the policy was introduced. The first vertical line marks when the regulation started in July 2018. The second line shows when this policy was fully put into place.

3.4. Empirical Strategy

Our objective is to study how the wages of those workers experiencing a within-employer transition from a temporary to a permanent contract change in response to the reform. To answer this question, we use a pooled cross-section of transformation events and estimate the following regression model:

$$W_{T\to P(i,j,t)} = \Psi_j + \phi X_i + \gamma \overline{W}_{T,i} + \beta_t + \epsilon_{i,t} \tag{3.1}$$

where $W_{T\to P(i,j,t)}$ is the wage of worker i after a transformation from a temporary (T) to a permanent (P) contract at firm j in month t. This is computed as the log daily wage smoothed over the time window from the transformation month t until $t+6$. In addition, Ψ_j is a firm effect capturing differences in compensation policies across employers; X_i is a vector of individual characteristics measured at the time of conversion, which includes a quadratic function of age, experience, and tenure, and interactions of 5-digit occupation codes with 7-level education and gender dummies[9]; while $\overline{W}_{T,i}$ is the average log daily wage for individual i over the duration of the temporary employment spell before the transformation.

Therefore, the coefficients of interest, $\{\beta_t\}$, identify the evolution of post-transformation wages for similar incumbent temporary workers. In particular, we are interested in measuring how post-transformation wages differ in each month relative to June 2018, the month prior to the implementation of the reform, which we use as a baseline category. To derive our estimates, we consider the 24-month window from July 2017 to July 2019, which is conveniently centred around the reform.

The set of controls is particularly important for accurately identifying the factors influencing possible shifts in wage dynamics following the reform. We restrict our attention to firms that recorded at least one contract conversion both before and after the implementation of the reform, and we consider conversions within the employer to account for changes in both the intensive and extensive margins of conversion at the firm level. We control for a rich set of observable characteristics at the worker level to account for changes in the marginal converted workers based on this set. Finally, we account for differences in earnings potential in the composition of the converted by controlling for the previous wage of the converted workers. Table C.1 presents summary statistics illustrating the differences in these variables before and after the reform. The average log daily wage at the time of conversion decreased by almost 8 percentage points after the reform. Similarly, the average log daily wage over the duration of temporary employment before conversions decreased by about 6.5 percentage points after the implementation of the reform. The data also show a small increase in both average tenure in months and average years of experience after the reform. The fraction of converted females experienced a small rise, while the fraction holding a degree slightly diminished post-reform.

[9]These interactions amount to 2,878 occupation-education-gender cells.

3.5. Results

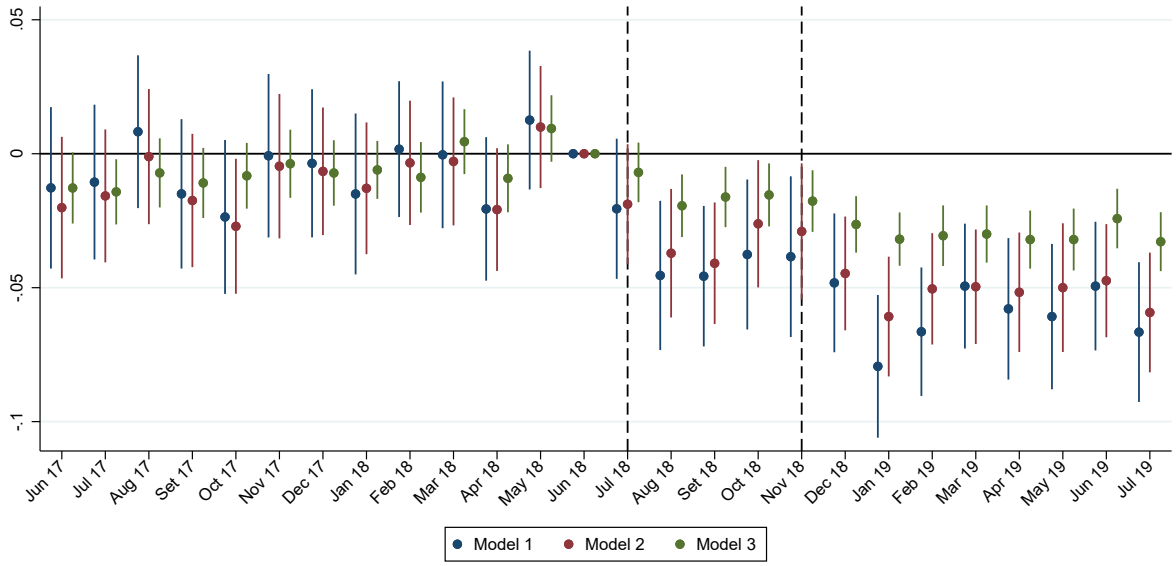
Figure 3.2 shows coefficient estimates for $\{\beta_t\}$, along with 95% confidence intervals, obtained by estimating three different specifications of Equation (3.1). The estimates displayed with the blue markers (Model 1) are derived from our most parsimonious specification, which includes only firm effects. These coefficients show that, conditional on firm composition, workers who transitioned to a permanent contract after the reform experienced a significant reduction in their post-transformation wage ($\approx$ -5 log points) compared to their counterparts who experienced a contract transformation before the reform. Moreover, they show that post-transformation wages do not show any significant trend in the pre-reform period.

The next specification, whose estimates are shown in red (Model 2), also flexibly controls for a rich set of observable individual characteristics that capture wage differences between narrowly defined groups of workers. However, the estimated coefficients are only marginally different from those in the previous specification, suggesting that the small changes in the composition of converted workers do not explain much of the difference in pre- and post-reform post-transformation wages.

Finally, the green markers (Model 3) show the estimates obtained with our third specification, which expands the set of covariates to include individual pre-transformation wages. The magnitude of the coefficients in the post-reform period is slightly smaller, suggesting that part of the difference in pre- and post-reform post-transformation wages is explained by heterogeneity in pre-transformation wages. This specification can be thought of as controlling for latent match-specific factors that could be due to unobserved characteristics of the worker or the job.

Even after controlling for these differences, the post-transformation wage penalty for workers converted after the reform remains large and persistent. Specifically, the estimated coefficients indicate a reduction in post-transformation wages of around 2 log points. Overall, these results provide evidence that the reform negatively affected the wages of similar workers transitioning from a temporary to a permanent contract. To put this in figures, assuming that people work 312 days a year, this wage decrease translates into an annual loss of between €271.5 (according to Model 3) and €589.8 (according to Model 1) for individuals who experienced a post-reform conversion.

FIGURE 3.2. Post-transformation Log-Hourly Wages Before and After the Reform



Notes: The figure displays estimates of β_t from different specifications of Equation 3.1 that show changes in log-daily wages of workers converted from temporary to permanent contracts. The blue markers (Model 1) represent data considering only firm effects, the red markers (Model 2) include individual characteristics, and the green markers (Model 3) factor in the pre-conversion wages as well. The first vertical line indicates the month when the government presented the regulation, and the second line indicates the date of its full enactment after parliamentary approval. Standard errors are clustered at the firm level.

3.6. Discussion and Next Steps

This paper investigates the wage dynamics surrounding the 2018 reform in Italy, which tightened the legislation on fixed-term contracts. Using extensive administrative data, we examine the wage changes experienced by workers transitioning from fixed-term to permanent contracts. Our results show a notable decline in the starting wage of workers whose contracts get converted from fixed-term to permanent post-reform. This decline is substantial, ranging between €271.5 and €589.8, depending on the specification.

Our findings contribute to the broader literature on temporary contracts and their implications for labor markets. Temporary contracts offer flexibility for employers and potential stepping stones for employees. However, the associated wage dynamics can be influenced significantly by policy changes.

There are two main explanations for this decline in wages. First, the new policy regime induces changes in the composition of the pool of converters. This means that firms convert contracts for riskier workers sooner than they would have in the absence of the reform. Second, the decline in wages is associated to a decrease in the surplus generated by each job. This could be due to a direct increase in labor costs or an increase

in uncertainty about the quality of the worker.

The next steps in this paper will be twofold. First, we will perform a sanity check on our main result by analyzing the wage dynamics of direct permanent hires, i.e. those workers who obtained a permanent contract without first going through a temporary employment phase. If there are no significant spillovers, we should not observe any discontinuity in the pattern followed by this group of workers. The second step is to try to quantify the role of labour market power in shaping the trade-off between job security and pay. We expect that, other things being equal, more concentrated markets (as defined by geography-occupation cells) will show more pronounced declines in the starting wages of converted workers.

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Appendices

A. Appendix for Chapter 1

A1. Appendix Tables

TABLE A.1. Decomposition of wage variance and its evolution - Firms with 1+ employees (uncorrected AKM)

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.210		0.204		0.216		0.006	
	Var (θ)	0.157	74.6	0.153	74.8	0.169	78.5	0.013	217.9
	Var (ψ)	0.028	13.4	0.024	11.9	0.023	10.7	-0.005	-86.4
	Var(Xb)	0.020	9.4	0.025	12.3	0.040	18.6	0.020	346.0
	Var(u)	0.010	4.7	0.010	4.7	0.010	4.8	0.000	7.3
	2*Cov(θ.ψ)	0.002	0.8	0.008	3.8	0.011	5.1	0.009	157.4
	2*Cov(θ.Xb)	-0.008	-3.8	-0.017	-8.3	-0.040	-18.4	-0.032	-539.0
	2*Cov(ψ.Xb)	0.002	0.9	0.002	0.9	0.002	0.8	0.000	-3.1
Between-firm variance	Var($\bar{y}$)	0.089	42.2	0.094	46.1	0.103	48.0	0.015	252.3
	Var ($\bar{\theta}$)	0.054	25.6	0.058	28.5	0.067	31.1	0.013	224.9
	Var (ψ)	0.028	13.4	0.024	11.9	0.023	10.7	-0.005	-86.4
	Var($\bar{X}B$)	0.003	1.6	0.004	2.1	0.007	3.1	0.003	57.0
	2*Cov($\bar{\theta}$.ψ)	0.002	0.8	0.008	3.8	0.011	5.1	0.009	157.4
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0.000	0.0	-0.002	-1.1	-0.006	-2.7	-0.006	-97.5
	2*Cov(ψ.$\bar{X}B$)	0.002	0.9	0.002	0.9	0.002	0.8	0.000	-3.1
Within-firm variance	Var($y - \bar{y}$)	0.121	57.8	0.110	53.9	0.112	52.0	-0.009	-152.3
	Var ($\theta - \bar{\theta}$)	0.103	49	0.094	46.3	0.102	47.4	0.000	-6.9
	Var($Xb - \bar{X}b$)	0.016	7.9	0.021	10.2	0.033	15.5	0.017	289.0
	Var(u)	0.010	4.7	0.010	4.7	0.010	4.8	0.000	7.3
	2*Cov($\theta - \bar{\theta}$.$Xb - \bar{X}b$)	-0.008	-3.7	-0.015	-7.3	-0.034	-15.7	-0.026	-441.5
	2*Cov($\theta - \bar{\theta}$, u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	-0.1
	2*Cov($Xb - \bar{X}b$, u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	-0.1
Segregation Index	$\frac{Var(\bar{\theta}_j)}{Var(\theta_i)}$	0.344		0.381		0.396		0.052	
N* (largest connected set)		58,666,316		61,413,372		59,550,288			

Notes: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Decomposition based on Equations 1.2, 1.3 and 1.4, weighted by worker-year observations. The largest connected set entails the group of firms connected by worker mobility.

TABLE A.2. Decomposition of wage variance and its evolution - Firms with 20+ employees

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.209		0.205		0.217		0.008	
	Var (θ)	0.153	73.3	0.152	73.9	0.169	77.6	0.015	185.4
	Var (ψ)	0.021	10.1	0.018	9.0	0.017	8.0	-0.004	-45.5
	Var(Xb)	0.018	8.6	0.025	12.1	0.039	17.7	0.021	250.2
	Var(u)	0.010	4.6	0.009	4.6	0.010	4.7	0.001	8.2
	2*Cov(θ.ψ)	0.012	5.7	0.017	8.2	0.020	9.0	0.008	93.4
	2*Cov(θ.Xb)	-0.006	-3	-0.017	-8.4	-0.038	-17.4	-0.032	-384
	2*Cov(ψ.Xb)	0.002	0.7	0.001	0.6	0.001	0.4	-0.001	-7.6
Between-firm variance	Var($\bar{y}$)	0.086	41.0	0.092	44.7	0.101	46.5	0.015	184.9
	Var ($\bar{\theta}$)	0.048	23.1	0.054	26.2	0.063	28.9	0.015	178
	Var (ψ)	0.021	10.1	0.018	9.0	0.017	8.0	-0.004	-45.5
	Var($\bar{X}B$)	0.003	1.3	0.004	1.8	0.006	2.5	0.003	34
	2*Cov($\bar{\theta}$.ψ)	0.012	5.7	0.017	8.2	0.020	9.0	0.008	93.4
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0.000	0.2	-0.002	-1.0	-0.005	-2.4	-0.006	-67.5
	2*Cov(ψ.$\bar{X}B$)	0.002	0.7	0.001	0.6	0.001	0.4	-0.001	-7.6
Within-firm variance	Var($y - \bar{y}$)	0.123	59.0	0.113	55.3	0.116	53.5	-0.007	-84.9
	Var ($\theta - \bar{\theta}$)	0.105	50.3	0.098	47.7	0.106	48.6	0.001	7.4
	Var($Xb - \bar{X}b$)	0.015	7.3	0.021	10.3	0.033	15.2	0.018	216.2
	Var(u)	0.010	4.6	0.009	4.6	0.010	4.7	0.001	8.2
	2*Cov($\theta - \bar{\theta}$.$Xb - \bar{X}b$)	-0.007	-3.1	-0.015	-7.4	-0.033	-15.0	-0.026	-316.6
	2*Cov($\theta - \bar{\theta}$,u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.0
	2*Cov($Xb - \bar{X}b$,u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.0
Segregation Index	$\frac{Var(\bar{\theta}_j)}{Var(\theta_i)}$	0.315		0.354		0.373		0.052	
N* (largest connected set)		53,378,336		55,173,311		54,723,634			

Note: In each period, all firms and individuals in firms with at least 20 different employees over the period, and in the main connected component, are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Decomposition based on Equations 1.2, 1.3 and 1.4.

TABLE A.3. Decomposition of wage variance and its evolution - Establishments with 20+ employees

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.212		0.209		0.22		0.009	
	Var (θ)	0.152	71.7	0.152	72.6	0.169	76.8	0.017	202.4
	Var (ψ)	0.027	12.6	0.023	11.1	0.022	9.8	-0.005	-59.4
	Var(Xb)	0.017	7.9	0.024	11.4	0.037	17.0	0.021	240.0
	Var(u)	0.009	4.3	0.009	4.4	0.01	4.5	0.001	8.7
	2*Cov(θ.ψ)	0.011	5.2	0.017	8.1	0.019	8.5	0.008	90.2
	2*Cov(θ.Xb)	-0.005	-2.5	-0.017	-8.1	-0.037	-16.9	-0.032	-371.3
	2*Cov(ψ.Xb)	0.001	0.7	0.001	0.5	0.001	0.2	-0.001	-10.6
Between-firm variance	Var($\bar{y}$)	0.098	46.5	0.105	50.4	0.113	51.4	0.015	173.5
	Var ($\bar{\theta}$)	0.056	26.6	0.063	30.2	0.073	33.0	0.016	190.3
	Var (ψ)	0.027	12.6	0.023	11.1	0.022	9.8	-0.005	-59.4
	Var($\bar{X}B$)	0.003	1.3	0.004	1.9	0.006	2.8	0.003	39.2
	2*Cov($\bar{\theta}$.ψ)	0.011	5.2	0.017	8.1	0.019	8.5	0.008	90.2
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0.000	0.1	-0.003	-1.3	-0.006	-2.9	-0.007	-76.2
	2*Cov(ψ.$\bar{X}B$)	0.001	0.7	0.001	0.5	0.001	0.2	-0.001	-10.6
Within-firm variance	Var($y - \bar{y}$)	0.113	53.5	0.104	49.6	0.107	48.6	-0.006	-73.5
	Var (θ – $\bar{\theta}$)	0.096	45.2	0.089	42.5	0.097	43.9	0.001	12.1
	Var($Xb - \bar{X}b$)	0.014	6.6	0.02	9.5	0.031	14.1	0.017	200.8
	Var(u)	0.009	4.3	0.009	4.4	0.01	4.5	0.001	8.7
	2*Cov(θ – $\bar{\theta}$.$Xb - \bar{X}b$)	-0.005	-2.6	-0.014	-6.8	-0.031	-14.0	-0.025	-295.1
	2*Cov(θ – $\bar{\theta}$, u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.1
	2*Cov($Xb - \bar{X}b$, u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.0
Segregation Index	$\frac{Var(\bar{\theta}_j)}{Var(\theta_i)}$	0.371		0.415		0.429		0.059	
N* (largest connected set)		49,378,243		50,278,039		49,994,151			

Note: All establishments and individuals in establishments with at least 20 employees are included. Only individuals employed for at least 360 days by the same establishment during the year are included for a given year. Individuals and establishments in public administration are not included. Decomposition based on Equations 1.2, 1.3 and 1.4.

TABLE A.4. Decomposition of wage variance and its evolution - Split-sampling correction with firm split, with an approximation for missing estimates

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.211		0.207		0.219		0.008	
	** Var (θ)	0.142	67.2	0.143	69.1	0.159	72.8	0.017	227.8
	Var (ψ)	0.014	6.7	0.014	6.8	0.013	5.8	-0.001	-19.3
	Var(Xb)	0.019	9	0.025	12.1	0.040	18.2	0.021	272.5
	** Var(u)	0.016	7.5	0.014	7	0.016	7.4	0.000	4.3
	2*Cov(θ.ψ)	0.025	12	0.026	12.6	0.028	13.0	0.003	40.8
	2*Cov(θ.Xb)	-0.007	-3.3	-0.017	-8.2	-0.039	-17.8	-0.032	-419.6
	2*Cov(ψ.Xb)	0.002	0.8	0.001	0.7	0.001	0.6	0.000	-5.8
Between-firm variance	Var($\bar{y}$)	0.088	41.5	0.094	45.3	0.103	46.9	0.015	197.7
	Var ($\bar{\theta}$)	0.041	19.6	0.049	23.6	0.058	26.5	0.017	218.7
	Var (ψ)	0.014	6.7	0.014	6.8	0.013	5.8	-0.001	-19.3
	Var($\bar{X}B$)	0.003	1.4	0.004	1.9	0.006	2.7	0.003	39.1
	2*Cov($\bar{\theta}$.ψ)	0.025	12.0	0.026	12.6	0.028	13.0	0.003	40.9
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0.000	0.2	-0.002	-0.9	-0.005	-2.4	-0.006	-73.8
	2*Cov(ψ.$\bar{X}B$)	0.002	0.8	0.001	0.7	0.001	0.6	0.000	-5.8
Within-firm variance	Var($y - \bar{y}$)	0.124	58.5	0.113	54.7	0.116	53.1	-0.007	-97.7
	** Var ($\theta - \bar{\theta}$)	0.101	47.6	0.094	45.5	0.101	46.3	0.001	9
	Var($Xb - \bar{X}b$)	0.016	7.6	0.021	10.3	0.034	15.5	0.018	233.3
	** Var(u)	0.016	7.5	0.014	7	0.016	7.4	0.000	4.3
	2*Cov($\theta - \bar{\theta}$.$Xb - \bar{X}b$)	-0.007	-3.4	-0.015	-7.2	-0.033	-15.2	-0.026	-340.4
	2*Cov($\theta - \bar{\theta}$, u) **	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.0
	2*Cov($Xb - \bar{X}b$,u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.2
Segregation Index	** $\frac{Var(\bar{\theta}_j)}{Var(\theta_i)}$	0.291		0.341		0.364		0.073	
N* (largest connected set)		51,932,308		54,530,553		52,921,242			

Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Estimation on firms present in both main connected components in each split sample. Decomposition based on Equations 1.2, 1.3 and 1.4. The split-sampling method is described in Section 1.3.3.

** : These parameters' estimates are NOT directly corrected by firm split. $Var(u)$ is approximated from the within-worker variance of wages, net from age and firm effects estimated on the other split. $Var(\theta)$ and Var ($\theta - \bar{\theta}$) are then deduced by substraction, assuming $cov(u, \theta) = 0$

TABLE A.5. Decomposition of wage variance and its evolution - Split-sampling correction with period split

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.212		0.209		0.221		0.009	
	Var (θ)	0.144	67.9	0.147	70.4	0.162	73.1	0.018	196.1
	Var (ψ)	0.014	6.5	0.013	6.4	0.011	5.1	-0.002	-27.5
	Var(Xb)	0.018	8.3	0.025	12.0	0.038	17.1	0.020	223.3
	Var(u)	**	**	**	**	**	**	**	**
	2*Cov(θ.ψ)	0.025	12.0	0.025	12.1	0.027	12.2	0.002	17.8
	2*Cov(θ.Xb)	-0.005	-2.5	-0.017	-8.1	-0.035	-16	-0.030	-332.4
	2*Cov(ψ.Xb)	0.001	0.7	0.001	0.6	0.001	0.4	-0.001	-7
Between-firm variance	Var($\bar{y}$)	0.087	41.3	0.094	44.9	0.102	46.3	0.015	165.3
	Var ($\bar{\theta}$)	0.044	20.5	0.052	25.1	0.062	28.3	0.019	209.4
	Var (ψ)	0.014	6.5	0.013	6.4	0.011	5.1	-0.002	-27.5
	Var($\bar{X}B$)	0.003	1.2	0.004	1.7	0.005	2.4	0.003	29.2
	2*Cov($\bar{\theta}$.ψ)	0.026	12.0	0.025	12.1	0.027	12.3	0.002	17.5
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0.001	0.4	-0.002	-0.9	-0.005	-2.1	-0.005	-58.9
	2*Cov(ψ.$\bar{X}B$)	0.001	0.7	0.001	0.6	0.001	0.4	-0.001	-7
Within-firm variance	Var($y - \bar{y}$)	0.125	58.7	0.115	55.1	0.119	53.7	-0.006	-65.3
	Var ($\theta - \bar{\theta}$)	0.100	47.3	0.094	45.2	0.100	45.1	-0.001	-7.8
	Var($Xb - \bar{X}b$)	0.015	7.1	0.021	10.3	0.033	14.7	0.018	194.1
	Var(u)	**	**	**	**	**	**	**	**
	2*Cov($\theta - \bar{\theta}$.$Xb - \bar{X}b$)	-0.006	-2.7	-0.015	-7.1	-0.031	-13.8	-0.025	-273.6
	2*Cov($\theta - \bar{\theta}$,u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.0
	2*Cov($Xb - \bar{X}b$,u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.0
Segregation Index	$\frac{Var(\bar{\theta}_j)}{Var(\theta_i)}$	0.302		0.356		0.387		0.084	
N* (largest connected set)		47,529,687		50,395,656		49,395,499			

Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Estimation on firms and, when necessary, individuals present in both main connected components in each split sample. Decomposition based on Equations 1.2, 1.3 and 1.4. The split-sampling method is described in Section 1.3.3.
** : These parameters' estimates are not corrected by period-split

TABLE A.6. Decomposition of wage variance and its evolution - Firm Clustering

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.208		0.201		0.211		0.003	
	Var (θ)	0.156	74.7	0.152	75.2	0.165	77.8	0.009	280.7
	Var (ψ)	0.005	2.4	0.005	2.4	0.005	2.5	0.000	7.6
	Var(Xb)	0.021	9.9	0.024	12.1	0.038	18.1	0.018	553.6
	Var(u)	0.011	5.1	0.010	5.1	0.011	5.2	0.000	7.7
	2*Cov(θ.ψ)	0.024	11.7	0.026	12.8	0.030	14	0.005	164.6
	2*Cov(θ.Xb)	-0.010	-4.7	-0.017	-8.4	-0.039	-18.4	-0.029	-914.6
	2*Cov(ψ.Xb)	0.002	0.8	0.001	0.7	0.002	0.8	0.000	0.4
Between-firm variance	Var($\bar{y}$)	0.078	37.6	0.082	40.9	0.093	44.2	0.015	472.6
	Var ($\bar{\theta}$)	0.042	20.4	0.046	22.8	0.052	24.7	0.010	307.4
	Var (ψ)	0.005	2.4	0.005	2.4	0.005	2.5	0.000	7.6
	Var($\bar{X}B$)	0.001	0.3	0.001	0.3	0.001	0.4	0.000	10.2
	2*Cov($\bar{\theta}$.ψ)	0.024	11.7	0.026	12.8	0.030	14	0.005	164.6
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0.004	2.1	0.004	1.8	0.004	1.8	-0.001	-17.7
	2*Cov(ψ.$\bar{X}B$)	0.002	0.8	0.001	0.7	0.002	0.8	0.000	0.4
Within-firm variance	Var($y - \bar{y}$)	0.130	62.4	0.119	59.1	0.118	55.8	-0.012	-372.6
	Var ($\theta - \bar{\theta}$)	0.113	54.3	0.105	52.4	0.112	53.1	-0.001	-26.7
	Var($Xb - \bar{X}b$)	0.020	9.7	0.024	11.9	0.038	17.7	0.017	543.4
	Var(u)	0.011	5.1	0.010	5.1	0.011	5.2	0.000	7.7
	2*Cov($\theta - \bar{\theta}$.$Xb - \bar{X}b$)	-0.014	-6.8	-0.021	-10.2	-0.043	-20.2	-0.029	-896.9
	2*Cov($\theta - \bar{\theta}$, u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.8
	2*Cov($Xb - \bar{X}b$, u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	-0.4
Segregation Index	$\frac{Var(\bar{\theta}_j)}{Var(\theta_i)}$	0.273		0.304		0.317		0.045	
N*		61,925,099		66,706,199		65,410,886			

Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Decomposition based on Equations 1.2, 1.3 and 1.4. Firm clustering method is described in Section 1.3.3.

TABLE A.7. Decomposition of wage variance and its evolution - Year fixed effects - Firm-split correction

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.213		0.208		0.220		0.006	
	Var (θ)	**	**	**	**	**	**	**	**
	Var (ψ)	0.014	6.5	0.014	6.7	0.013	5.8	-0.001	-18.2
	Var(Xb)	0.003	1.4	0.002	1.1	0.003	1.1	-0.001	-8.2
	Var(u)	**	**	**	**	**	**	**	**
	2*Cov(θ.ψ)	0.026	12.4	0.027	12.8	0.029	13.2	0.002	37.9
	2*Cov(θ.Xb)	0.000	0.0	0.000	0.2	-0.002	-0.8	-0.002	-25.5
	2*Cov(ψ.Xb)	0.001	0.3	0.001	0.3	0.001	0.3	0.000	0.1
Between-firm variance	Var($\bar{y}$)	0.088	41.1	0.094	45.0	0.103	46.7	0.015	233
	Var ($\bar{\theta}$)	0.043	20.4	0.05	23.8	0.057	26.1	0.014	213.2
	Var (ψ)	0.014	6.5	0.014	6.7	0.013	5.8	-0.001	-18.2
	Var($\bar{X}B$)	0.000	0.1	0.000	0.1	0.000	0.1	0.000	-0.9
	2*Cov($\bar{\theta}$.ψ)	0.026	12.4	0.027	12.8	0.029	13.1	0.002	37.1
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0.001	0.6	0.001	0.6	0.001	0.7	0.000	3.9
	2*Cov(ψ.$\bar{X}B$)	0.001	0.3	0.001	0.3	0.001	0.3	0.000	0.1
Within-firm variance	Var($y - \bar{y}$)	0.126	58.9	0.114	55.0	0.117	53.3	-0.009	-133
	Var (θ – $\bar{\theta}$)	**	**	**	**	**	**	**	**
	Var($Xb - \bar{X}b$)	0.003	1.3	0.002	1.0	0.002	1.1	0.000	-7.3
	Var(u)	**	**	**	**	**	**	**	**
	2*Cov(θ – $\bar{\theta}$.$Xb - \bar{X}b$)	-0.001	-0.5	-0.001	-0.4	-0.003	-1.4	-0.002	-29.3
	2*Cov(θ – $\bar{\theta}$, u)	**	**	**	**	**	**	**	**
	2*Cov($Xb - \bar{X}b$,u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.0
N* (largest connected set)		51,932,308		54,530,553		52,921,242			

Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Estimation on firms present in both main connected components in each split sample. Decomposition based on Equations 1.2, 1.3 and 1.4. The split-sampling method is described in Section 1.3.3.
AKM estimation with fixed effects for years and a cubic function of age flat at 40. No selection for age. Bias corrected with firm split.
** : These parameters' estimates are not corrected by firm-split

TABLE A.8. Decomposition of wage variance and its evolution - Split-sampling correction with firm split - 90+ days worked in a year

		2002-2007		2008-2013		2014-2019		Change from 2002-2007 to 2014-2019	
		Comp.	Share	Comp.	Share	Comp.	Share	Comp.	Share
Total variance	Var(y)	0.210		0.200		0.211		0.001	
	Var (θ)	**	**	**	**	**	**	**	**
	Var (ψ)	0.015	7.1	0.014	7.2	0.013	5.9	-0.002	-291.8
	Var(Xb)	0.040	19.3	0.043	21.5	0.068	32.1	0.027	3190.6
	Var(u)	0.025	12	0.023	11.5	0.026	12.3	0.001	105.6
	2*Cov(θ.ψ)	0.024	11.5	0.024	12.2	0.026	12.5	0.002	258.8
	2*Cov(θ.Xb)	-0.027	-12.6	-0.034	-17.2	-0.073	-34.4	-0.046	-5384
	2*Cov(ψ.Xb)	0.004	1.7	0.004	2.1	0.004	2	0.001	79.3
Between-firm variance	Var($\bar{y}$)	0.089	42.5	0.092	46.0	0.101	48.0	0.012	1388.8
	Var ($\bar{\theta}$)	0.037	17.6	0.041	20.4	0.05	23.6	0.013	1486.4
	Var (ψ)	0.015	7.1	0.014	7.2	0.013	5.9	-0.002	-291.8
	Var($\bar{X}B$)	0.008	3.8	0.008	4.2	0.014	6.4	0.006	647.3
	2*Cov($\bar{\theta}$.ψ)	0.024	11.5	0.024	12.2	0.026	12.5	0.002	250.2
	2*Cov($\bar{\theta}$.$\bar{X}B$)	0	-0.2	-0.002	-1.1	-0.007	-3.4	-0.007	-797.1
	2*Cov(ψ.$\bar{X}B$)	0.004	1.7	0.004	2.1	0.004	2	0.001	79.3
Within-firm variance	Var($y - \bar{y}$)	0.121	57.5	0.108	54.0	0.110	52.0	-0.011	-1288.8
	Var ($\theta - \bar{\theta}$)	**	**	**	**	**	**	**	**
	Var($Xb - \bar{X}b$)	0.032	15.5	0.034	17.3	0.054	25.7	0.022	2543.2
	Var(u)	**	**	**	**	**	**	**	**
	2*Cov($\theta - \bar{\theta}$.$Xb - \bar{X}b$)	-0.026	-12.3	-0.031	-15.7	-0.064	-30.4	-0.038	-4478.0
	2*Cov($\theta - \bar{\theta}$, u)	**	**	**	**	**	**	**	**
	2*Cov($Xb - \bar{X}b$, u)	0.000	0.0	0.000	0.0	0.000	0.0	0.000	0.4
N* (largest connected set)		84,772,337		89,060,613		91,320,821			

Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 90 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Estimation on firms present in both main connected components in each split sample. Decomposition based on Equations 1.2, 1.3 and 1.4. The split-sampling method is described in Section 1.3.3.

** : These parameters' estimates are not corrected by firm-split

TABLE A.9. Composition effects: number of firms and average size by sorting quadrant

Firms present in both periods (2-period)

	Person-Year Observations				Number of Firms				Average Size			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	F>0, W>0	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***
2002-2007	14,003,660	8,892,099	13,122,069	5,748,544	29,114	41,287	79,037	51,230	480.99	215.37	166.02	112.21
2014-2019	15,379,346	9,252,809	15,582,805	6,931,931	29,114	41,287	79,037	51,230	528.25	224.11	197.16	135.31
Diff	1,375,686	360,710	2,460,736	1,183,387	0	0	0	0	47.25	8.74	31.13	23.10

Firms present only in one period (1-period)

	Person-Year Observations				Number of Firms				Average Size			
	F>0, W>0	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***	***F>0, W>0***	***F>0, W<0***	***F<0, W<0***	***F<0, W>0***
2002-2007	3,609,701	4,516,577	5,585,022	3,188,645	34,278	82,244	120,218	99,406	105.31	54.92	46.46	32.08
2014-2019	2,612,327	2,636,805	4,357,807	2,796,457	37,580	81,401	131,086	113,378	69.51	32.39	33.24	24.66
Diff	-997,374	-1,879,772	-1,227,215	-392,188	3,302	-843	10,868	13,972	-35.79	-22.52	-13.21	-7.41

Notes: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. The largest connected set firms present in both periods and present either in 2002-2007 or in 2014-2019 are analyzed separately. Firms are further divided into four quadrants according to the value of the estimated worker (W) and firm fixed effects (F) (see Equation 1.1), averaged over both periods for staying firms. Worker and firm fixed effects are normalized so they are comparable across periods. Columns 1, 5, and 9 refer to the number of workers where both worker (W) and firm fixed effects (F) are above zero (F>0, W>0). The other columns map the rest of the possible combinations for F and W values. For firms present in both periods, the (not employment-weighted) 2-period average of firm and worker fixed effects are considered in the allocation to the four quadrants. Entries in columns 5-8 and 9-12 are the average worker and firm fixed effects, respectively, by quadrant.

TABLE A.10. Change in the occupational structure by type of firm – 1-period firms

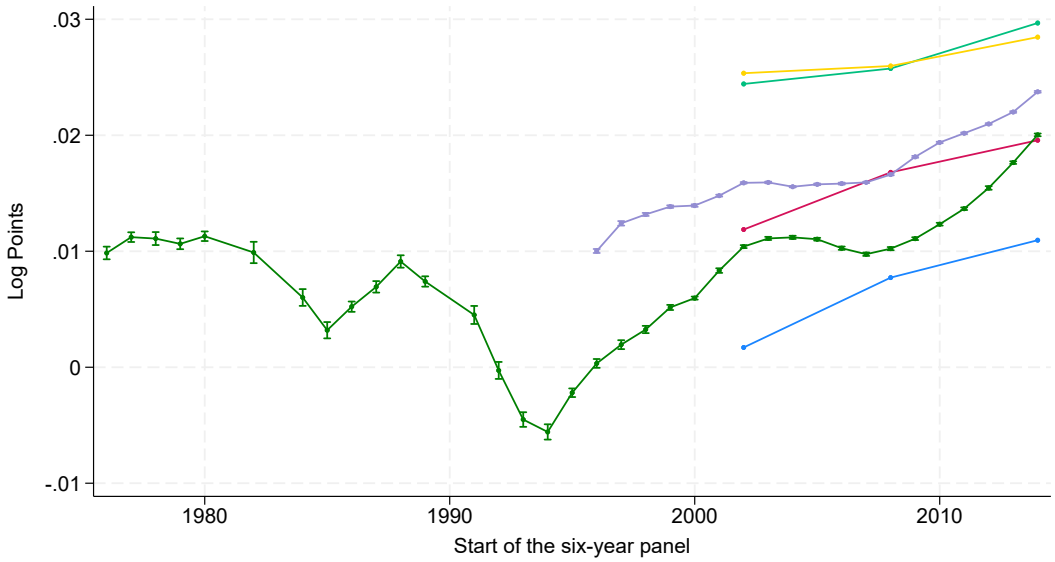
		1-period, F>0, W>0			1-period, F>0, W<0			1-period, F<0, W<0			1-period, F<0, W>0		
Occupation	CS	2002-2007	2014-2019	Diff	2002-2007	2014-2019	Diff	2002-2007	2014-2019	Diff	2002-2007	2014-2019	Diff
Entrepreneurs	20	0.0103	0.0163	0.0060	0.0112	0.0163	0.0051	0.0112	0.0130	0.0018	0.0277	0.0283	0.0006
Professionals	32	0.0083	0.0104	0.0021	0.0044	0.0067	0.0022	0.0028	0.0078	0.0050	0.0136	0.0295	0.0159
Artists and media professionals	35	0.0175	0.0081	-0.0095	0.0028	0.0032	0.0004	0.0008	0.0012	0.0004	0.0087	0.0071	-0.0016
Managers	37	0.1676	0.1965	0.0289	0.0479	0.0544	0.0065	0.0319	0.0322	0.0003	0.1053	0.1165	0.0112
Engineers	38	0.1986	0.2255	0.0269	0.0325	0.0342	0.0017	0.0160	0.0114	-0.0046	0.0645	0.0886	0.0241
Primary school teachers	42	0.0047	0.0042	-0.0005	0.0040	0.0113	0.0072	0.0042	0.0210	0.0168	0.0153	0.0293	0.0140
Health and social workers	43	0.0091	0.0094	0.0002	0.0207	0.0253	0.0046	0.0229	0.0399	0.0170	0.0421	0.0477	0.0056
Public administration intermediates	45	0.0014	0.0003	-0.0012	0.0002	0.0002	0.0000	0.0005	0.0001	-0.0004	0.0012	0.0004	-0.0008
Business administration intermediates	46	0.1308	0.0906	-0.0402	0.0561	0.0618	0.0057	0.0505	0.0442	-0.0063	0.1055	0.0871	-0.0184
Technicians	47	0.1041	0.1017	-0.0024	0.0445	0.0456	0.0011	0.0233	0.0195	-0.0038	0.0657	0.0667	0.0010
Intermediate supervisors	48	0.0310	0.0506	0.0197	0.0410	0.0301	-0.0108	0.0299	0.0151	-0.0148	0.0219	0.0153	-0.0066
Public administration clerks	52	0.0053	0.0024	-0.0029	0.0174	0.0185	0.0011	0.0281	0.0502	0.0221	0.0151	0.0185	0.0034
Security agents	53	0.0027	0.0022	-0.0004	0.0108	0.0066	-0.0042	0.0258	0.0259	0.0001	0.0020	0.0026	0.0006
Business administration clerks	54	0.1365	0.1055	-0.0311	0.1096	0.1187	0.0091	0.0975	0.0993	0.0018	0.2034	0.1721	-0.0313
Retail salespersons	55	0.0058	0.0143	0.0085	0.0775	0.0708	-0.0066	0.1154	0.1160	0.0006	0.0470	0.0505	0.0035
Personal service employees	56	0.0052	0.0108	0.0056	0.0343	0.0697	0.0354	0.0888	0.1514	0.0626	0.0393	0.0585	0.0192
Skilled manufacturing workers	62	0.1051	0.0949	-0.0101	0.2063	0.1146	-0.0917	0.1237	0.0442	-0.0795	0.0807	0.0345	-0.0462
Skilled artisans	63	0.0185	0.0198	0.0014	0.0785	0.1130	0.0344	0.0648	0.0840	0.0192	0.0651	0.0761	0.0110
Drivers	64	0.0051	0.0045	-0.0006	0.0461	0.0539	0.0079	0.0804	0.0756	-0.0048	0.0191	0.0135	-0.0056
Handling, transport skilled workers	65	0.0162	0.0123	-0.0040	0.0456	0.0426	-0.0030	0.0278	0.0172	-0.0106	0.0201	0.0110	-0.0091
Unskilled manufacturing workers	67	0.0125	0.0128	0.0002	0.0903	0.0584	-0.0320	0.0728	0.0362	-0.0366	0.0210	0.0157	-0.0053
Unskilled artisans	68	0.0029	0.0050	0.0022	0.0162	0.0289	0.0127	0.0768	0.0774	0.0006	0.0132	0.0170	0.0038
Farm workers	69	0.0007	0.0019	0.0012	0.0020	0.0152	0.0133	0.0042	0.0172	0.0130	0.0024	0.0132	0.0108

Notes: All firms and individuals in firms with at least 2 employees are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Firms are divided into types as described in Table 1.4. We report the incidence of a certain 2-digits occupation in 2002-2007 vs 2014-2019, and the difference (in percentage points), by type. The color code in the visualization indicates the intensity of the change, with darker shades of red representing more negative changes and darker shades of green representing more positive changes. The second column displays the two-digit code corresponding to the *catégories socioprofessionnelles*, which can be further explored in detail here.

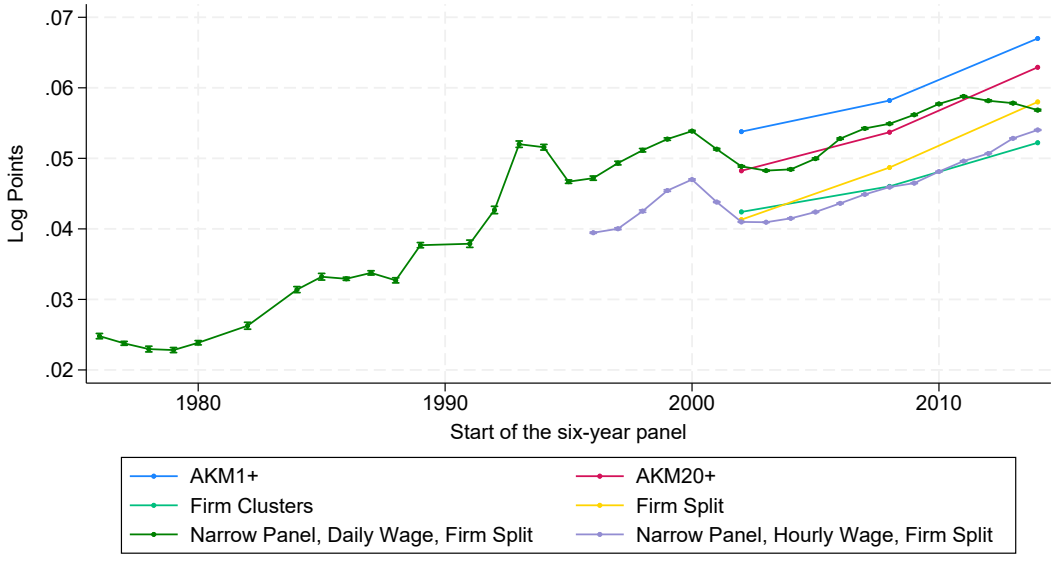
A2. Appendix Figures

FIGURE A.1. Historical Series

A. Sorting

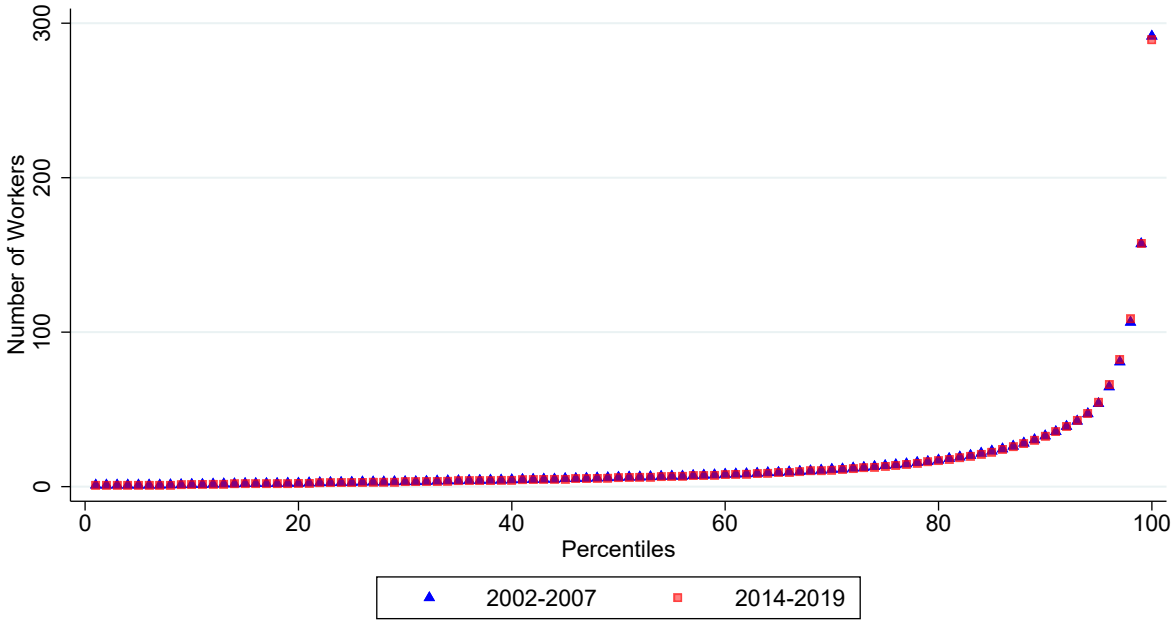


B. Segregation



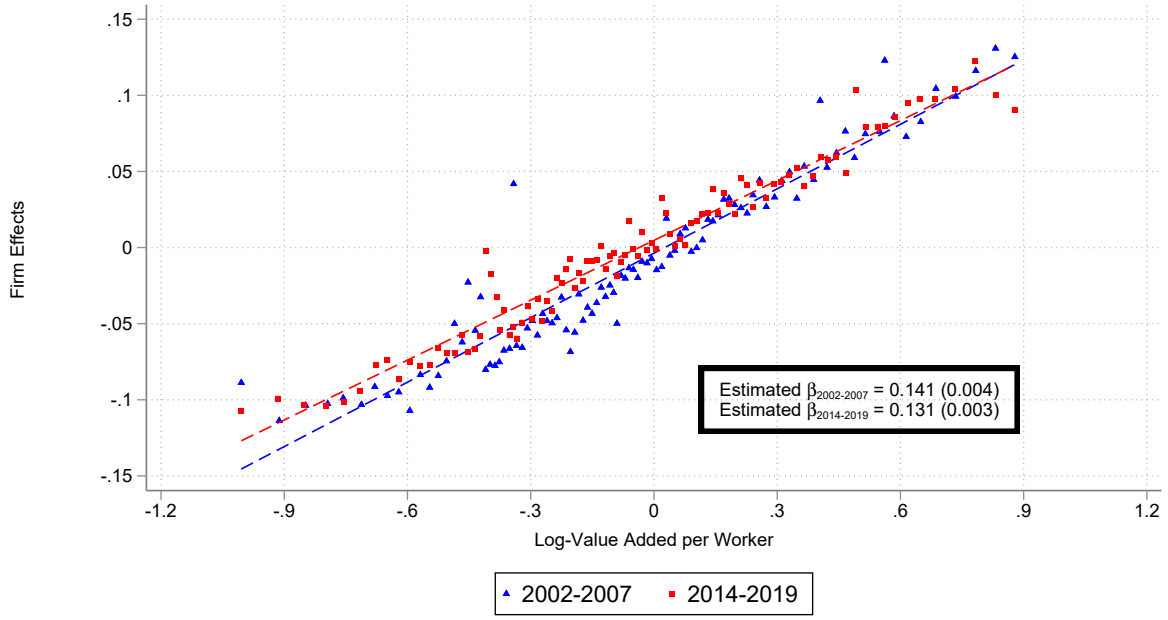
Note: Only individuals employed for at least 360 days for the exhaustive panels, and 90 days for the narrow panel, by the same firm during the year are included for a given year. The wider selection on the narrow panel aims at increasing connectivity. Individuals and firms in public administration are not included. Sorting – $2 * Cov(\theta, \psi)$ – and Segregation – $Var(\theta)$ – estimates by six-year periods. Long-term series are computed on the narrow panel, on rolling six-year periods, corrected by firm-splitting, with mean estimate and confidence intervals computed on repeated (split) sampling with 20 repetitions, reflecting only the noise stemming from the randomness of the split. Estimates on the narrow panel are particularly affected by sampling error and the selection of bigger and more connected firms. The years 1981, 1983, and 1990 are missing. There have been several changes in scope and variable definition since 1976.

FIGURE A.2. Cumulative firm size distribution



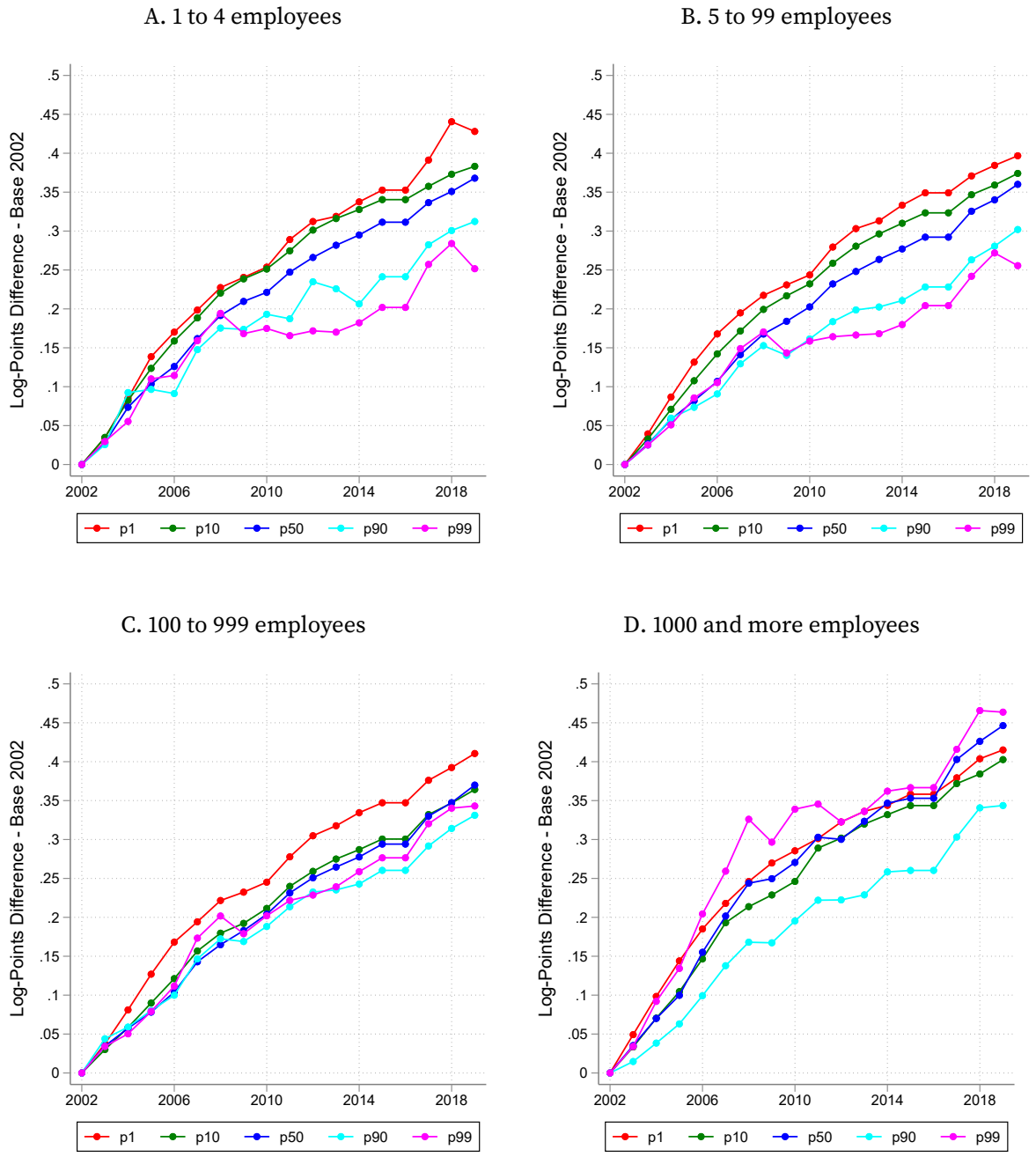
Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. This figure shows the fraction of firms below a given size, by period.

FIGURE A.3. Rent-Sharing - Firm Fixed Effects vs Log Value Added/Worker



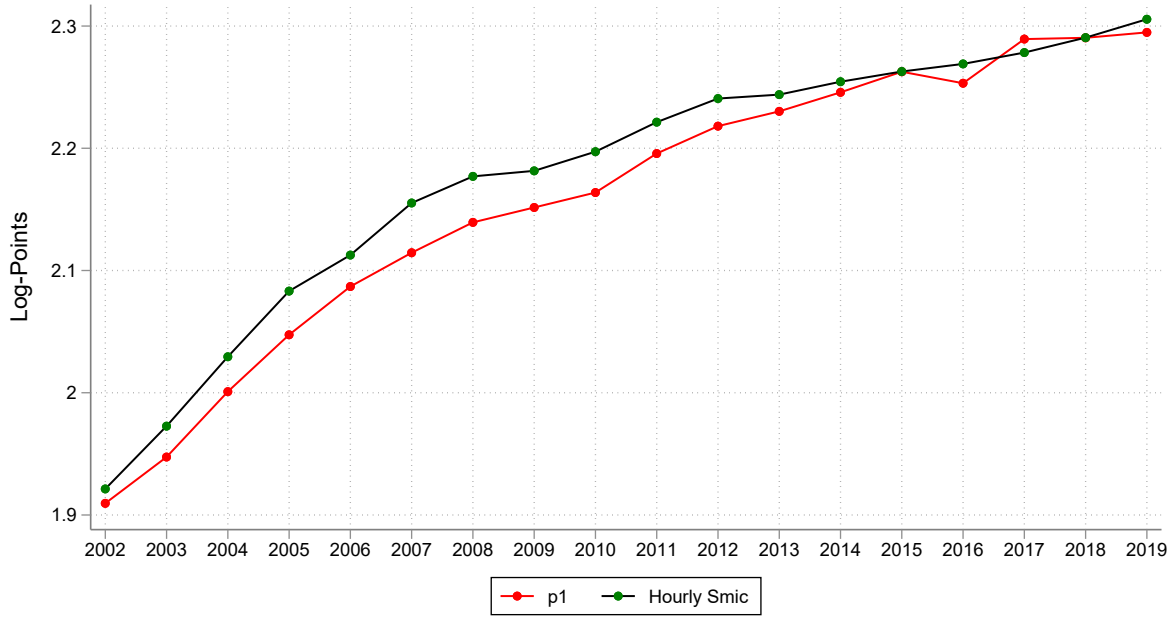
Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Points shown represent mean estimated firm fixed effects from AKM models, averaged across firms in 100 percentile bins of the period-demeaned log value added per worker. Period best-fitting lines coming from employment-weighted OLS are reported.

FIGURE A.4. Change in Percentiles of Real Earnings Relative to 2002 - By Firm Size Class



Note: This figure illustrates the average change in log-earnings for employees across different percentiles, relative to the year 2002. The data is categorized into four distinct firm size classes, based on measurements taken on December 31st of each year. The percentiles range from the top 1% of earners to the employees in the first percentile.

FIGURE A.5. Evolution of P1 of real earnings and of real hourly minimum wage



Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. The figure plots the evolution over time of the first percentile of real earnings and of the real hourly minimum wage (in log points).

A3. Constructing a BTS full panel

A3.1. Chaining the yearfiles

The French BTS is not a proper panel dataset as there are no individual IDs before 2002 and after 2002 the individual IDs are specific to each yearfile. However, each yearfile y contains information both on the current year t and the preceding year $t-1$ (variables for the year $t-1$ end with "_1"). We therefore take advantage of this overlap to build a pseudo-panel based on common information between year t of yearfile $y-1$ and year $t-1$ of yearfile y.

We obtained Insee's authorization to chain the BTS yearly files in order to create a full panel of the wage-earning population between 1994 and 2020. Between 1994 and 2001, as there are no individual IDs enabling to track mobilities within the yearfiles, we can only match the "stayers", employees as long as they stay in the same establishment: for instance, an individual who moves from workplace j to workplace k will be given two different IDs. After 2002, individual IDs in the original yearfiles, even if yearfile specific, enables us to match both stayers and movers.

In order to conduct the match, we used the following variables: sex (`SEXE`), firm ID (`SIREN`), establishment ID (`NIC`), number of hours (`NBHEUR` or `NBHEUR_1`), starting day of the job during the year (`DATDEB` or `DATDEB_1`), ending day of the job during the year (`DATFIN` or `DATFIN_1`), number of days between starting and ending day (`DUREE` or `DUREE_1`), municipality of residence (`COMR` or `COMR_1`), municipality of work (`COMT` or `COMT_1`), being part of the sample used for the DADS panel (`SONDE` or `SONDE_1`), and gross wage (`S_BRUT` or `S_BRUT_1`) and age (`AGE`).

We run the match with a SAS script at the regional level, using the BTS regional files[1]. Within the regional file, we keep the job for which a worker i has the highest pay.

We create the following keys for the year t of yearfile $y-1$:

```
pseudoid=COMPRESS(SEXE!!"#"!!SIREN!!"#"!!NIC!!"#"!!ROUND(NBHEUR,1)!!"#"!!
DATDEB!!"#"!!DATFIN!!"#"!!DUREE !! "#" !!COMR!!"#"!!COMT !! "#" !! SONDE);
```

and the following for the year $t-1$ of yearfile y:

```
pseudoid_b=COMPRESS(SEXE!!"#"!!SIREN!!"#" !!NIC!!"#"!!ROUND(NBHEUR_1,1)!!"#"!!
DATDEB_1!!"#"!!DATFIN_1!!"#"!!DUREE_1!! "#"!! COMR_1!!"#"!!COMT_1!!"#" !! SONDE_1);
```

[1] In the current project, we restricted the match to mainland France and excluded overseas departments (DOM).

However, as there are some discrepancies in the ages and the wages reported for the same year in yearfile $y-1$ and y, we do not use them directly in the matching key. We use the HAVING property of the SQL procedure, in order to select the match with the minimal difference between the two wages and an absolute age difference below two years.

```
PROC SQL;
    CREATE TABLE ab (DROP=pseudoid pseudoid_b S_BRUT S_BRUT_1 AGE)
        AS SELECT * FROM a1 (KEEP=pseudoid s_brut IDENT_S ID2 REGT
        AGE NBHEUR) AS aa
    FULL JOIN b1 (keep=pseudoid_b s_brut_1 IDENT_S ID2_B AGE
        DEP_NAISS NBHEUR_1 rename=(IDENT_S=IDENT_S_B AGE=AGE_B))
        AS bb
    ON aa.pseudoid=bb.pseudoid_B
    GROUP BY aa.S_BRUT,aa.PSEUDOID
    HAVING ABS(aa.s_brut-bb.s_brut_1)=MIN(ABS(aa.s_brut-bb.s_brut_1))
    AND (0<=bb.AGE_B-aa.AGE<2 or AGE_B=. or AGE=.)
    ORDER BY aa.PSEUDOID, bb.s_brut_1;
QUIT;
```

This code was adapted to account for fileyear specificity.

- For years before 2002 ($y < 2002$ and $y-1 < 2001$), we create an individual ID based on the initial row numbers in each regional file, to which we add at the end the regional code. For instance: the ID for the 10th observation of the Paris region (code: 11) will be 1011.
- In 2013 ($y = 2013$ and $y-1 = 2012$), the `SONDE` variable leads to some mismatch and is excluded from the pseudoid key.
- After 2013 ($y > 2013$ and $y-1 > 2012$), we found that the number of hours for the same year differed between yearfile $y-1$ and y. We thus excluded the number of hours from the matching key and we added the minimal difference in the number of hours in the having clause.

We count the number of matches based on the procedure and we attribute the same ID only to workers with a single match. Finally, we chain the different IDs starting from the first year of the DADS (1994). The ID files (*PSID*_1994 to *PSID*_2020) contain the ID of

the year (IDENT_S) and a permanent ID (IDENT_ALL), which is based on the initial ID of an employee when she first appears in the DADS, to which we add the year of the first appearance on two digits. The full SAS script pseudo_id.sas is available at the following address:
http://olivier.godechot.free.fr/hopfichiers/pseudo_id.zip

It comes with three additional SAS scripts for creating DADS files with the identifier IDENT_ALL included, for creating and adding seniority variables, and for correcting information on workers' location of birth and citizenship.

A3.2. Quality of the identification

To avoid false identification, we opted for a conservative procedure in order to identify two individuals as the same person, by using the maximal available overlapping information. When the procedure leads to multiple matches, we do not impute any identification. However, these duplicates remain rare, around 0.4% of the observations. Most matching failures are due to observations for which we don't find any match.

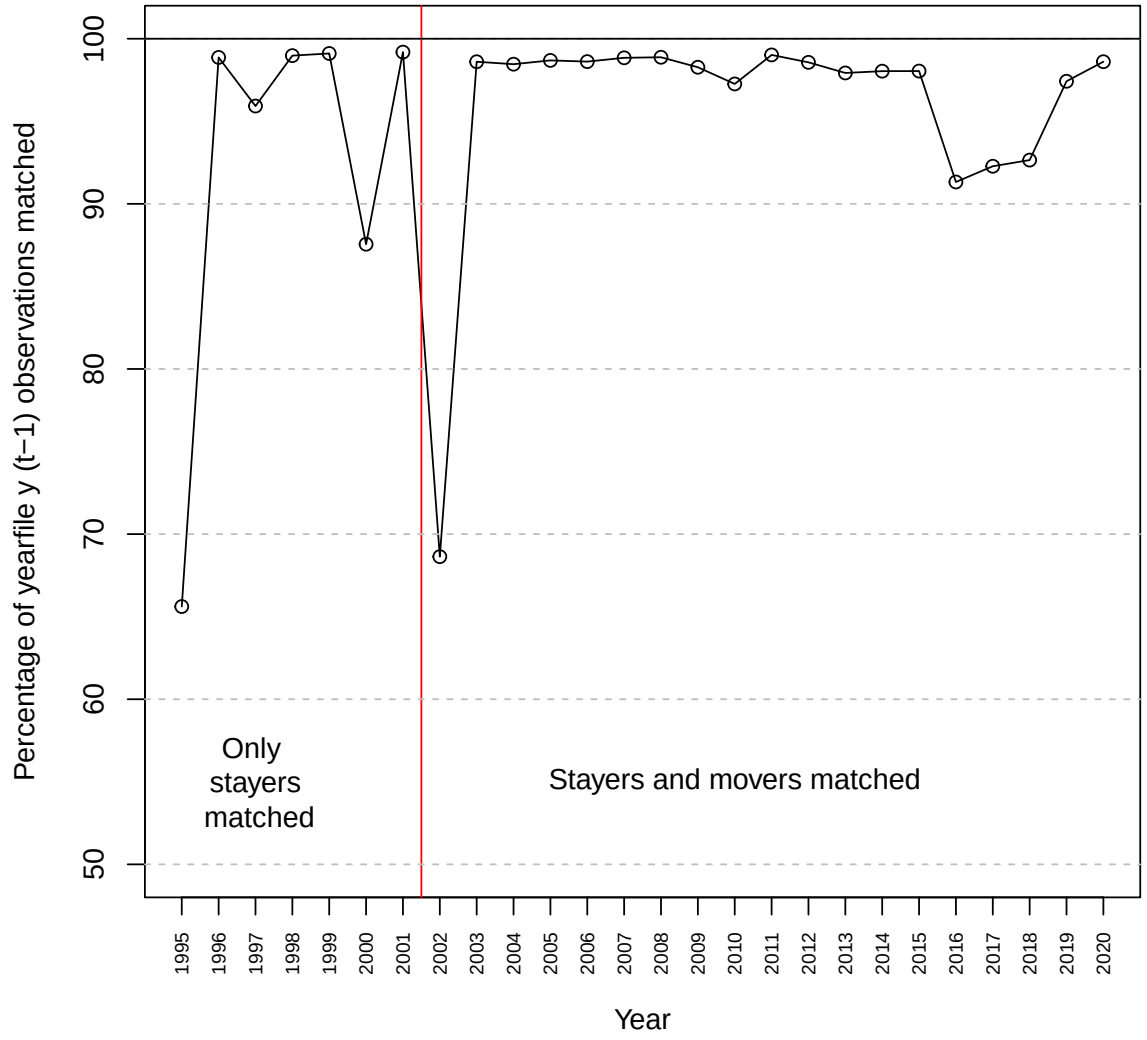
Figure A.6 gives a first proxy of the quality of the matching. Generally, we find a single match for 98% of the observations of the overlapping years of two yearfiles. The quality of the matching declines between 2016 and 2018, dropping to 91-93% and resumes back to 97% in 2019, probably as a result of the switch from DADS to the DSN[2]. With the existing procedure, the match is poor for yearfile 2002 (and similarly in 1995), as the consequence of the major transformation of the BTS between the 1994-2001 series and the 2002-2020 series[3].

Despite a high level of matches, the matching has some limitations. We must bear in mind, that a false positive is still possible (but unlikely). Second, in order to be identified as the same person, an employee needs to be present each year as a wage earner in the BTS. This means that we cannot link the initial ID of an employee who was either further unemployed, self-employed or civil servant (before 2009) for more than a calendar year to her subsequent employment periods. Overall, the quality of the match seems sufficiently good to run AKM panel regressions.

[2]The "déclaration sociale nominative" is a new monthly administrative source replacing the "déclarations annuelles de données sociales". INSEE produces from the DSN a yearly datafile on the BTS format for series continuity.

[3]One could probably improve the match for these years by eliminating variables in the matching key which are incomplete or coded differently. We already dropped the number of hours for 2002 and this increased the matching rate from 60 to 68%.

FIGURE A.6. Quality of the match



Note: The figure present the proportion of observations from the year $t-1$ of yearfile y for which we found a single match in the year t of yearfile $y-1$. Before 2002, the lack of individual ID in the initial dataset makes it impossible to follow the movers. In 2002 and after, we can match both stayers and movers.

A3.3. How to use the ID files

Hence, in order to add the permanent ID to a given datafile (for instance a file `b2010` for the year 2010), the procedure is as follows:[4]

```
PROC SQL;
 CREATE TABLE b2010b
 AS SELECT * FROM b2010 AS aa
```

[4]The script pseudo_id_use.sas also provides a macro program to run these steps automatically.

```
 LEFT JOIN psid.psid_2010 AS bb
 ON aa.ident_s=bb.ident_s;
QUIT;

data b2010c; set b2010b;
 if Missing(ident_all) then ident_all=ident_s*100+substr(AN,3,4);
run;
```

Before 2002, in order to get permanent IDs necessary for the match with the `PSID_yyyy` files, one needs to create an ID in each regional file (prior to any selection) as follows (for instance for Paris Region in 1997):

```
DATA b1197; SET po1997.post1197;
 ident_s=_N_*100+REG;
RUN;
```

A4. Proof of split-sampling bias correction

We suppose that observations are randomly split into two half samples and that each sample retains the same connectivity as the full sample. In practice, we reduce the data to firms and individuals (for period-split, or only firms for firm-split) that belong to both main connected components in each split sample. This condition is analogous to the Kline et al. 2020 leave-one-out condition that the main connected sample is not disconnected when any one observation is removed. We start again with a simplified notation of the AKM model in equation 1.5:

$$y_i = z_i'\alpha + u_i \tag{A.1}$$

With $\alpha = (\beta, \theta, \psi)$ our parameter vector of length $k = 2 + N + J$ and z_i the non-random regressors vector of the (worker * year) i's observation characteristics, including the indicator vector for worker and firm. For a given symetric matrix A corresponding to a given quadratic form ω of interest, our split-sampling plug-in estimator becomes $\hat{\omega}^{SP} = \hat{\alpha}_0' A \hat{\alpha}_1$ with $\hat{\alpha}_s$ an OLS estimate in the sample I_s, $s = 0, 1$ of size N_s : $\hat{\alpha}_s = S_{zz,s}^{-1} \sum_{i \in s}^{N_s} z_i y_i = \alpha + S_{zz,s}^{-1} \sum_{i \in s}^{N_s} z_i u_i$, with $S_{zz,s} = \sum_{i \in s}^{N_s} z_i z_i'$ the split sample design matrix (with full rank when we limit the sample to the split sample main connected set). We can express $\hat{\alpha}_s$ as

$\hat{\alpha}_s = \alpha + \epsilon_s$. We calculate the bias by expressing a scalar as the trace of a (1,1) matrix, as in the classic demonstration of the expectation of quadratic forms

$$\begin{aligned}
\mathbf{E}[\hat{\omega}^{SP}] &= \mathbf{E}[\hat{\alpha}_0' A \hat{\alpha}_1] = \mathbf{E}[trace(\hat{\alpha}_0' A \hat{\alpha}_1)] \\
&= \mathbf{E}[trace(\hat{\alpha}_1 \hat{\alpha}_0' A)] && \text{by propriety of trace()} \\
&= \mathbf{E}[trace(A \hat{\alpha}_1 \hat{\alpha}_0')] && \text{idem} \\
&= trace(A \mathbf{E}[\hat{\alpha}_1 \hat{\alpha_0}'']) && \text{A non-random, trace() is linear}
\end{aligned}$$

We further have:

$$\begin{aligned}
\mathbf{E}[\hat{\alpha}_1 \hat{\alpha_0}''] &= \mathbf{E}[(\alpha + \epsilon_1)(\alpha + \epsilon_0)'] \\
&= \alpha\alpha' + \mathbf{E}[\epsilon_1 \epsilon_0']
\end{aligned}$$

And: $trace(A\alpha\alpha') = \omega$

The bias is thus equal to:

$$\mathbf{E}[\hat{\omega}^{SP}] - \omega = trace(A\mathbf{E}[(S_{zz,1}^{-1} \sum_{i \in I_1}^{N_1} z_i u_i)(S_{zz,0}^{-1} \sum_{j \in I_0}^{N_0} z_j u_j)'])$$

$$\mathbf{E}[\hat{\omega}^{SP}] - \omega = trace(A\mathbf{E}[(S_{zz,1}^{-1} \sum_{i \in I_1}^{N_1} u_i z_i)(S_{zz,0}^{-1} \sum_{j \in I_0}^{N_0} u_j z_j)'])$$

$$= trace(AS_{zz,1}^{-1} \underbrace{\mathbf{E}[(\sum_{i \in I_1}^{N_1} u_i z_i)(\sum_{j \in I_0}^{N_0} u_j z_j)']}_{\text{matrix } (b_{lm})} (S_{zz,0}^{-1})')$$

with generic term:

$$b_{lm} = \sum_{i \in I_1}^{N_1} u_i z_{l,i} \sum_{j \in I_0}^{N_0} u_j z_{m,j}$$

The variance of the split sampling estimator of a quadratic form stems from the random errors u_i, but also from the randomness of the split. We can abstract from this last source by considering only a given split, and the corresponding sample of firms and individuals connected in both split samples. The size of the variance then depends on the quadratic form matrix A and the design matrices $S_{zz,s}$ and their relation, so additional hypotheses are needed for this variance to be of finite value. Kline et al. 2020 discuss these conditions in the context of leave-one-out. When introducing the randomness of the split, however, A and $S_{zz,s}$ become random matrices. Finally,

to study the consistency and convergence of our estimator, we need to consider the series of these random matrices when some index n of the number of observations grows. We leave the study of the precise conditions for consistency and convergence to further research. Instead, we checked that the estimator showed reasonable stability over multiple random splits, and recovered known values in Monte Carlo experiments.

A4.1. Simulations

We generated simulated workers' and firms' fixed effects with sorting and noise, calibrated on measured distributions, to get simulated wages on the observed match. It is important to keep the real mobility network, on which the bias depends. In Table A.11, split sample correction recovers the true value on the sample of firms belonging to a connected component in both splits, which slightly differs from the full sample true values because firms are bigger and there is fewer firm variance in fixed effects and mean workers effects. This kind of simulation cannot however reproduce potential selection effects in the sample reduction.

TABLE A.11. Simulated wage: true fixed effects and estimations

	(1)	(2)	(3)	(4)
Var(θ)	0.1494	0.1543	0.1488	
Var(ψ)	0.0138	0.0164	0.0136	0,0137
2Cov(θ,ψ)	0.0248	0.0199	0.0243	0.0243
N of obs	58,666,317	58,666,317	52,157,735	52,157,735

Simulation on 2002-2007 data, corrected estimates with period split method. First column: ground truth on AKM estimation sample (true quadratic terms computed on simulated fixed effects). Second column: AKM estimates (on simulated fixed effects). Third column: ground truth on the split-sampling estimation sample. Fourth column: slip-sampling estimates.

A4.2. Multiple random splits

We checked the stability of the (firm split) split sample estimators with multiple random splits on two different data sets. First on the long-term historical series, which are computed on the smaller and less connected "narrow panel" and show more variability due to splitting. We plot the means of 20 split sample estimations and a confidence interval on this mean in Figure A.1A.

On our main estimates, we limited the multiple random split experiments to the firm-split in the first and third periods for computational reasons. In Table A.12 we report the mean and standard deviation of 20 estimations.

TABLE A.12. Decomposition of wage variance - Mean and standard deviation over 20 firm split estimations

		2002-2007		2014-2019	
		Mean	SD	Mean	SD
Total variance	Var(y)	0.211	0.00003	0.219	0.00003
	Var(ψ)	0.014	0.00004	0.013	0.00005
	Var(Xb)	0.019	0.00001	0.040	0.00002
	2Cov(θ,ψ)	0.026	0.00008	0.029	0.00006
	2Cov(θ,Xb)	-0.007	0.00002	-0.039	0.00003
	2Cov(ψ,Xb)	0.002	0.00001	0.001	0.00002
Between-firm variance	Var($\bar{y}$)	0.087	0.00002	0.103	0.00003
	Var($\bar{\theta}$)	0.041	0.00007	0.058	0.00006
	Var($\bar{X}B$)	0.003	0.00000	0.006	0.00000
	2Cov($\bar{\theta}$,ψ)	0.026	0.00008	0.029	0.00007
	2Cov($\bar{\theta}$,$\bar{X}B$)	0.000	0.00001	-0.005	0.00002
	2Cov(ψ,$\bar{X}B$)	0.002	0.00001	0.001	0.00002
Within-firm variance	Var($y - \bar{y}$)	0.124	0.00002	0.116	0.00001
	Var($Xb - \bar{X}b$)	0.016	0.00001	0.034	0.00002
	2Cov($\theta - \bar{\theta}$,$Xb - \bar{X}b$)	-0.007	0.00001	-0.033	0.00003
N of obs		52,149,132	12,361.51	53,142,360	8,563.32

Note: Mean and standard deviations computed on 20 estimations similar to table 1.2, on firms belonging to both main connected components

The standard deviations are very small relative to the estimates, the sizes of the bias correction, and the evolution between periods. Our split sampling corrected results do not stem from random split noise. This exercise can also be interpreted as a bootstrap, indicative more generally of the high stability of AKM decomposition statistics in our large dataset.

A4. Log-wage variance decomposition from alternate specifications

We conduct multiple robustness experiments with alternate specifications.

A4.1. Model with year fixed effects and a cubic function of age

Card et al. 2018's AKM model specification differs from ours. While we demean wages per year, they use year fixed effects. To avoid collinearity between year effects, workers effects, and age, they suppress the linear term in age and keep only a quadratic and a cubic term in (age-40). We implement this specification and present results in Table A.7, with split sampling bias correction with the firm-split method (variance of workers' effects and residual are not corrected).

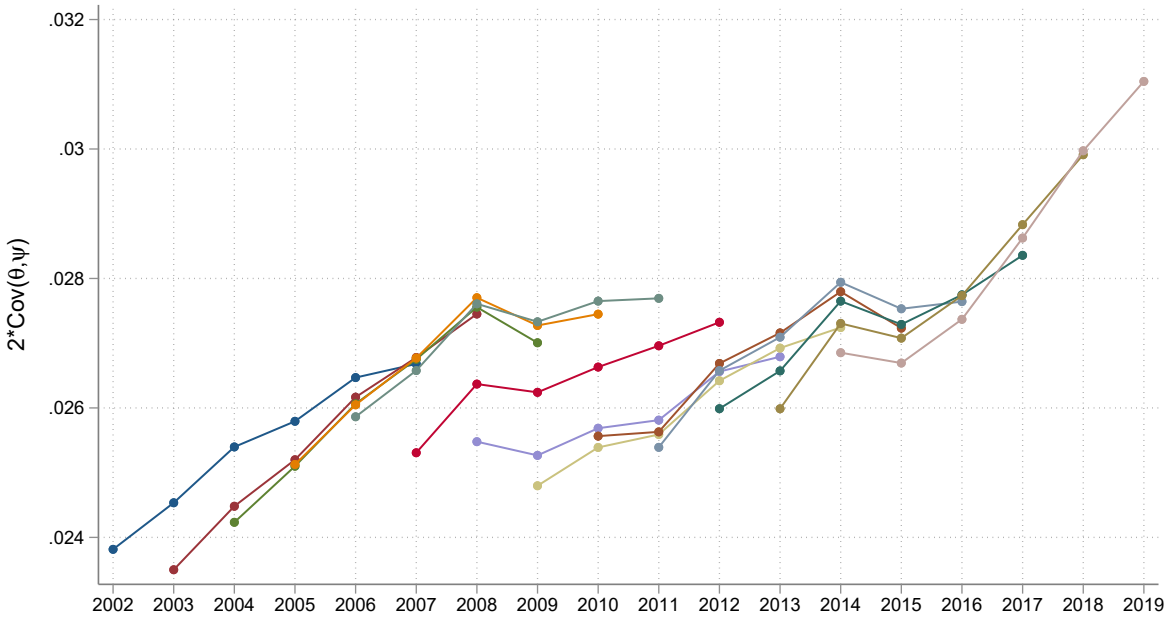
With this specification, the covariance between workers' effects and age functions is close to zero. The rise in sorting is still visible but less important.

A4.2. Rolling panels and yearly log-wage variance decomposition

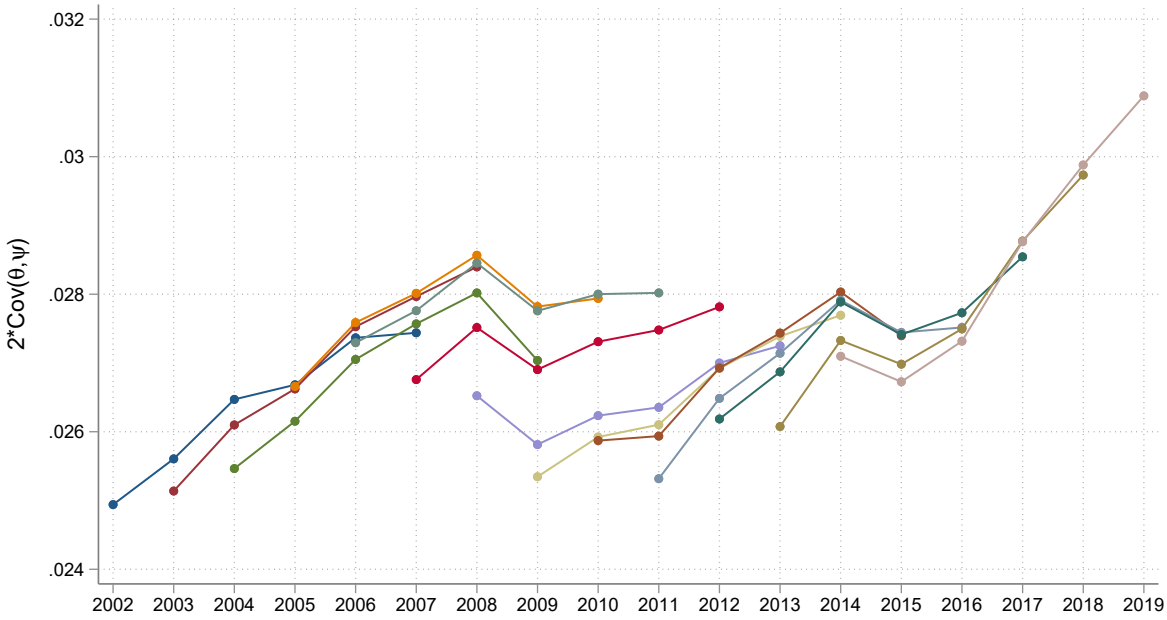
Are fixed effects really fixed? To better understand their stability or lack thereof, we implement the rolling AKM (R-AKM) method of Lachowska et al. 2023 by estimating firm-split corrected AKM on six-year panels for every possible starting year in our data. We then apply log-wage variance decomposition separately for each year of each panel estimation. Within each six-year panel, year-to-year variations in sorting reflect composition effects (year-to-year changes in the populations of firms and workers) and pure sorting (changes in worker-to-firm matching). If fixed effects were fixed, and the AKM estimation was correct, the yearly results would be the same whatever the panel used for estimation. On the contrary, we observe systematic shifts between different panels' estimates, for a given year (Figure A.7). Sampling differences between panels due to the main connected component selection might explain some of the shift, but it is most likely due to changes in the fixed effects of a given firm or a given worker, estimated in different, overlapping panels. This, in turn, could be a mechanical consequence of the evolution of individuals' fixed effects themselves, from year to year.

FIGURE A.7. Sorting - Rolling Panels

A. Baseline



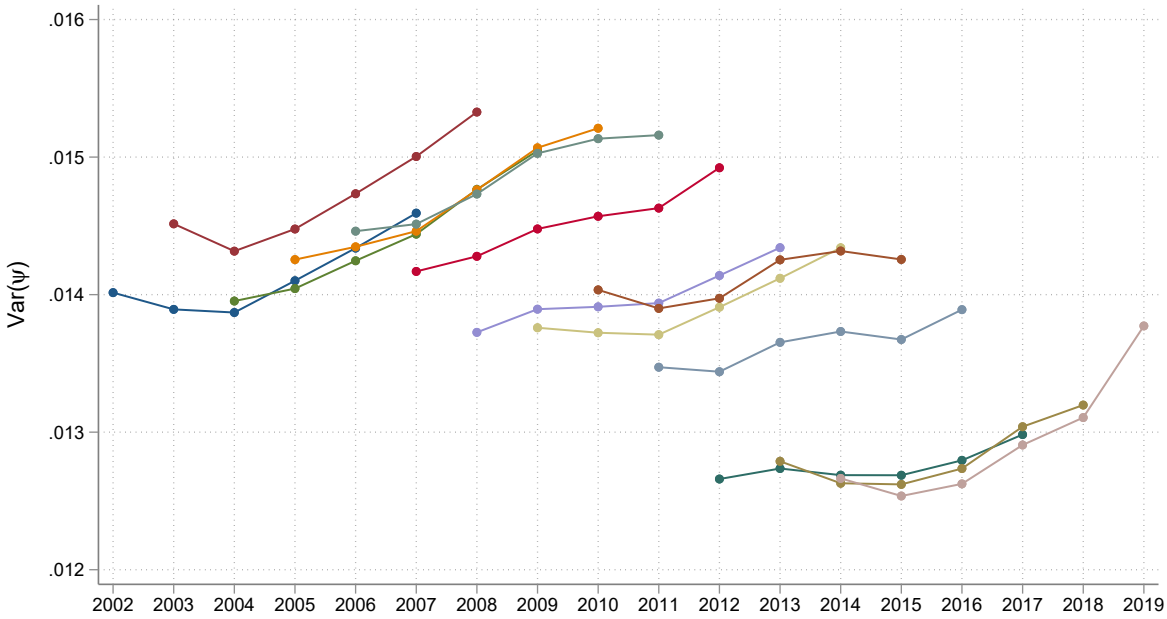
B. Year fixed effects and cubic function of (age-40)



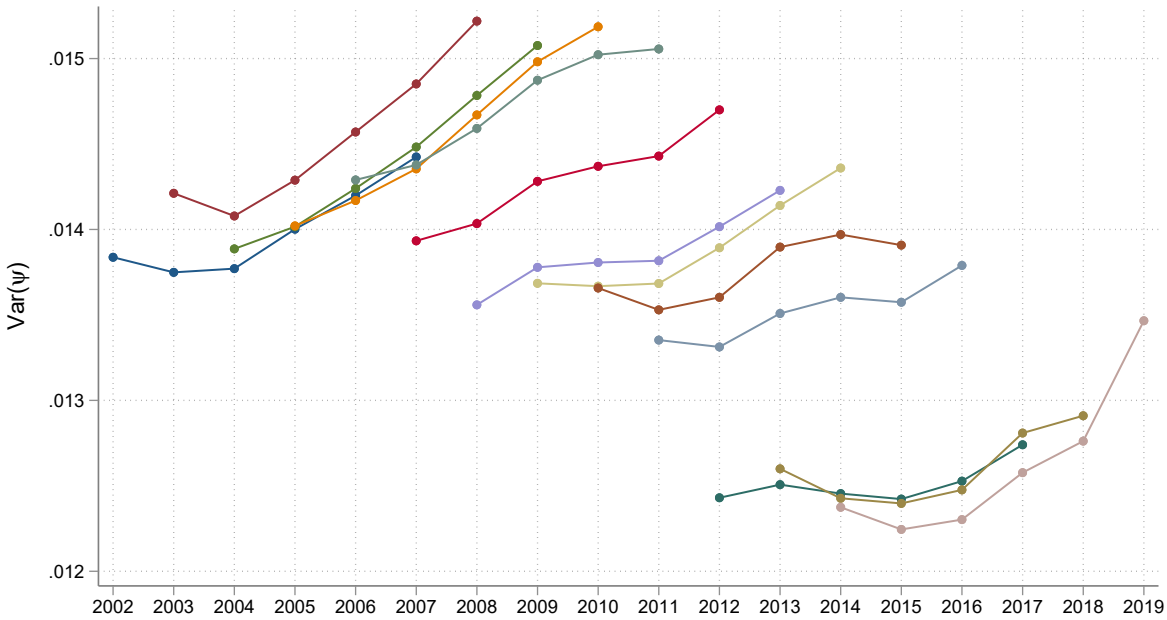
Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Fixed effects estimates by a panel of six-year periods split sample bias correction with firm splitting (Panel a). In Panel b, we further include year fixed effects and cubic function of (age-40). For each panel, log wage variance decomposition is computed separately for each year.

FIGURE A.8. Variance of Firm Effects - Rolling Panels

A. Baseline



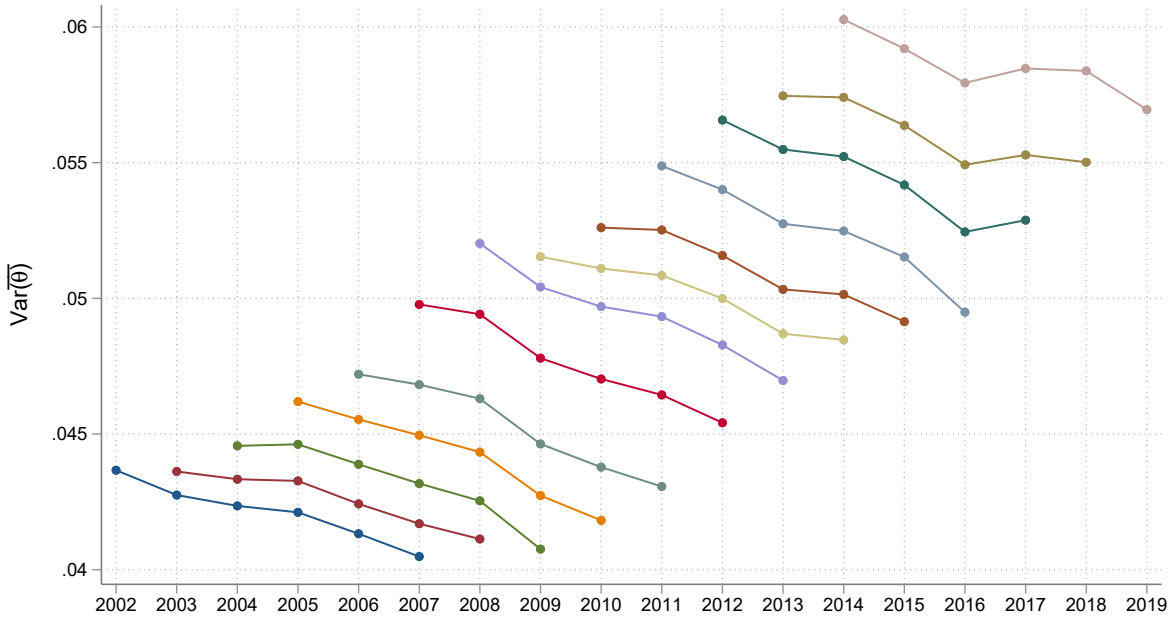
B. Year fixed effects and cubic function of (age-40)



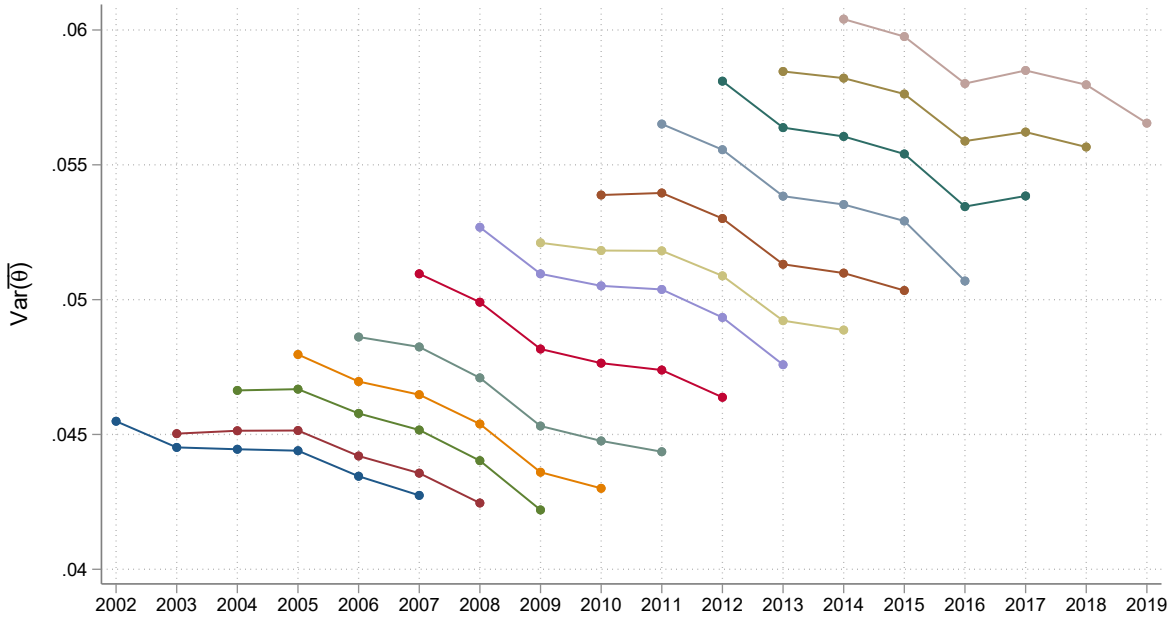
Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Fixed effects estimates by a panel of six-year periods split sample bias correction with firm splitting (Panel a). In Panel b, we further include year fixed effects and cubic function of (age-40). For each panel, log wage variance decomposition is computed separately for each year.

FIGURE A.9. Segregation - Rolling Panels

A. Baseline



B. Year fixed effects and cubic function of (age-40)



Note: All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included. Fixed effects estimates by a panel of six-year periods split sample bias correction with firm splitting (Panel a). In Panel b, we further include year fixed effects and cubic function of (age-40). For each panel, log wage variance decomposition is computed separately for each year.

A5. Fixed effects regressions

We regressed at the observation level the estimated firms and workers fixed effects (without correction) on occupation, sex, and industry (Table A.13). This is similar to a firm-level regression of FFE on industry, occupational shares, and proportion of women, weighted by worker-year observations, or an individual-level regression of WFE on occupation, sex, and industry shares (in a given year), weighted by worker-year observations. Estimates are relatively stable from period 1 to period 3. We do not observe strong patterns where a change in the association between an occupation, industry, or sex and each fixed effect would imply an increase in sorting, though these results are mostly descriptive. They do not account for interactions between terms (if, for instance, women's shares in industries changed over the period) and compositional effects (for instance, because sex is associated with sorting, an increase in the share of the minority would increase sorting).

TABLE A.13. Fixed effects regressions on occupation, sex and industry

		2002-2007						2014-2019					
		freq. (%)	WFE	SE		FFE	SE		freq. (%)	WFE	SE		FFE	SE	
Occupation															
20	Entrepreneurs	0.91	0.962	0.008	***	0.001	0.008		0.90	0.794	0.008	***	-0.010	0.007	
32	Professionals	0.83	0.802	0.009	***	0.076	0.009	***	1.68	0.628	0.022	***	0.013	0.010	
35	Artists and media professionals	0.48	0.516	0.010	***	0.121	0.010	***	0.44	0.456	0.015	***	0.071	0.010	***
37	Managers	8.93	0.708	0.010	***	0.091	0.008	***	10.71	0.647	0.008	***	0.082	0.007	***
38	Engineers	7.23	0.646	0.008	***	0.098	0.008	***	9.24	0.589	0.009	***	0.080	0.007	***
42	Primary school teachers	0.85	0.277	0.011	***	0.018	0.010	*	1.39	0.279	0.008	***	-0.014	0.007	*
43	Health and social workers	3.24	0.312	0.006	***	0.043	0.008	***	3.98	0.308	0.008	***	0.024	0.007	***
45	Public admin. intermediates	0.04	0.230	0.020	***	-0.007	0.010		0.05	0.197	0.030	***	0.074	0.012	***
46	Business admin. intermediates	9.32	0.261	0.006	***	0.073	0.008	***	8.28	0.240	0.007	***	0.047	0.007	***
47	Technicians	5.75	0.193	0.007	***	0.078	0.008	***	6.08	0.178	0.008	***	0.051	0.007	***
48	Intermediate supervisors	3.37	0.197	0.006	***	0.058	0.008	***	2.76	0.171	0.008	***	0.048	0.007	***
52	Public admin. clerks	2.57	0.006	0.006		0.045	0.008	***	3.28	0.013	0.008	*	0.031	0.007	***
53	Security agents	0.92	-0.073	0.011	***	-0.005	0.013		0.96	-0.063	0.010	***	-0.032	0.011	***
54	Business admin. clerks	11.76	0.105	0.006	***	0.047	0.007	***	10.78	0.114	0.007	***	0.020	0.007	***
55	Retail salespersons	6.48	0	ref		0	ref		6.87	0	ref		0	ref	
56	Personal service employees	4.38	-0.057	0.005	***	-0.014	0.008	*	4.86	-0.034	0.007	***	-0.007	0.007	
62	Skilled manufacturing workers	12.93	-0.035	0.008	***	0.049	0.008	***	8.63	-0.028	0.007	***	0.041	0.007	***
63	Skilled artisans	4.80	-0.040	0.006	***	0.011	0.007		5.22	-0.017	0.007	**	-0.006	0.007	
64	Drivers	3.98	-0.108	0.011	***	-0.022	0.009	**	4.08	-0.149	0.014	***	-0.035	0.008	***
65	Handling, transport skilled workers	2.92	-0.092	0.007	***	0.055	0.008	***	2.56	-0.085	0.008	***	0.041	0.007	***
67	Unskilled manufacturing workers	5.58	-0.133	0.008	***	0.032	0.009	***	3.93	-0.122	0.008	***	0.018	0.007	**
68	Unkilled artisans	2.55	-0.162	0.006	***	-0.040	0.010	***	2.80	-0.172	0.008	***	-0.038	0.007	***
69	Farm workers	0.20	-0.093	0.010	***	-0.065	0.010	***	0.51	-0.058	0.008	***	-0.072	0.008	***
Industry															
AC	Farming and industry	29.57	0.020	0.006	***	0.057	0.004	***	20.95	0.037	0.004	***	0.041	0.003	***
DE	Utilities	2.52	0.056	0.011	***	0.086	0.005	***	2.41	0.089	0.027	***	0.093	0.010	***
F	Construction	6.14	0.022	0.003	***	0.066	0.003	***	6.80	0.027	0.003	***	0.064	0.003	***
G	Commerce	17.31	0	ref		0	ref		17.54	0	ref		0	ref	
H	Transport	5.92	0.039	0.015	***	0.025	0.007	***	6.02	0.039	0.017	**	0.025	0.005	***
I	Hotels, tourism, catering	2.98	0.004	0.005		-0.020	0.005	***	3.56	-0.023	0.006	***	-0.025	0.004	***
J	Media	3.65	0.006	0.007		0.078	0.006	***	4.90	0.019	0.015		0.027	0.007	***
K	Financial services	7.37	0.037	0.010	***	0.115	0.009	***	5.71	0.072	0.007	***	0.099	0.006	***
LM	Real estate, professional services	5.69	0.022	0.004	***	0.016	0.005	***	8.46	0.082	0.005	***	0.004	0.004	
N	Administrative services	4.36	-0.006	0.004		0.002	0.014		6.56	-0.020	0.004	***	-0.005	0.007	
OPQ	Health, educ. and public admin.	12.49	-0.012	0.003	***	-0.030	0.003	***	14.28	-0.058	0.005	***	-0.076	0.003	***
R	Arts and recreation	0.51	0.034	0.014	**	0.018	0.012		0.99	0.009	0.008		-0.008	0.007	
STU	Other	1.48	-0.023	0.005	***	-0.032	0.005	***	1.81	-0.032	0.006	***	-0.054	0.005	***
Sex															
	Women	40.94	-0.140	0.002	***	-0.004	0.001	***	43.17	-0.120	0.001	***	-0.001	0.000	*

Note: Significance levels : $p < 0.01$ ***; $p < 0.05$ **; $p < 0.10$ *
All firms and individuals in firms with at least 1 employee are included. Only individuals employed for at least 360 days by the same firm during the year are included for a given year. Individuals and firms in public administration are not included.

B. Appendix for Chapter 2

B6. Appendix Tables

TABLE B.1. Relationship between Productivity and the Use of FT Contracts

	π	π	ϕ	ϕ	$\text{Ln}(\mathbb{E}[\Delta_T)]$	$\text{Ln}(\mathbb{E}[\Delta_T])$
$\text{Ln}(Z_{pre})$	-0.0406***	-0.0038	-0.1140***	-0.0687***	0.2920***	0.2750***
	(0.0029)	(0.0033)	(0.0057)	(0.0072)	(0.0120)	(0.0147)
Industry FE		✓		✓		✓
N	14,428	14,417	12,200	12186	10,884	10,871
R^2	0.017	0.163	0.031	0.116	0.057	0.120

Notes: The table presents the results of the regression analysis examining the firm-level relationship between past labor productivity, measured as the average yearly log value added per worker from 2012 to 2016, and the use of fixed-term (FT) contracts in the pre-Reform period (January 2017 to June 2018). The first two columns of the table display the results for the dependent variable π, representing the ratio of days worked in temporary employment to total days worked. The third and fourth columns present the results for the dependent variable ϕ, capturing the relative entries into temporary employment. The fifth and sixth columns display the results for the dependent variable $\text{Ln}(\mathbb{E}[\Delta_T])$, representing the average duration of new fixed-term contracts (in log). The row labeled N provides the sample size for each regression analysis, indicating the number of firms used in the estimation. The third and fourth rows have fewer firms because firms with zero entries in employment during the whole period are excluded from the analysis. The fifth and sixth columns have even fewer firms because firms with zero entries in temporary employment during the whole period are excluded. Robust standard errors are reported in parentheses. The significance levels are denoted by asterisks: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Source: Mercurio, see Section 2.2

TABLE B.2. Monthly Averages – Exposed vs. Non-exposed Firms

	$\pi_{i,Pre\text{–}2017} = 0$	$\pi_{i,Pre\text{–}2017} > 0$
N of firms	1,278 (9%)	13,172 (91%)
Flows		
Hirings – Permanent	0.051 (0.388)	0.159 (1.152)
Hirings – FT	0.046 (0.320)	0.947 (8.174)
Conversions – FT $\rightarrow$ P	0.006 (0.095)	0.116 (0.774)
Hirings – Other Contracts	0.085 (0.615)	0.309 (2.403)
Terminations – Permanent	0.087 (0.543)	0.321 (1.545)
Terminations – Temporary	0.018 (0.186)	0.692 (8.050)
Stocks		
Days Worked - Permanent	329.2 (527.5)	780.6 (2,754.7)
Days Worked - FT	6.6 (24.1)	158.5 (728.0)
Days Worked - Other Contracts	42.1 (106.0)	99.6 (817.1)
Days Worked - Total	377.8 (543.6)	1,038.7 (3,364.7)

Notes: The table presents monthly averages and standard deviations – measured from January 2017 to June 2018 – of various employment indicators for two groups of firms: those with $\pi_{i,Pre\text{–}2017} = 0$ (non-exposed firms) and those with $\pi_{i,Pre\text{–}2017} > 0$ (exposed firms). To measure the exposure, we rely on long-run differences in the demand for temporary workers at the firm level. This exposure variable, denoted as $\pi_{i,Pre\text{–}2017}$, captures the average monthly incidence of days worked in temporary employment over the total days worked between 2012 and 2016. Figure B.5 displays the distribution of this variable. The term Other Contracts refers to various alternative types of employment, including (i) apprenticeship, (ii) consultancy (so-called *parasubordinato*), (iii) intermittent worker, (iv) internship (so-called *tirocinio*) and (v) seasonal worker.
Source: Mercurio, see Section 2.2

TABLE B.3. Heterogeneous Response to the Reform - Flows

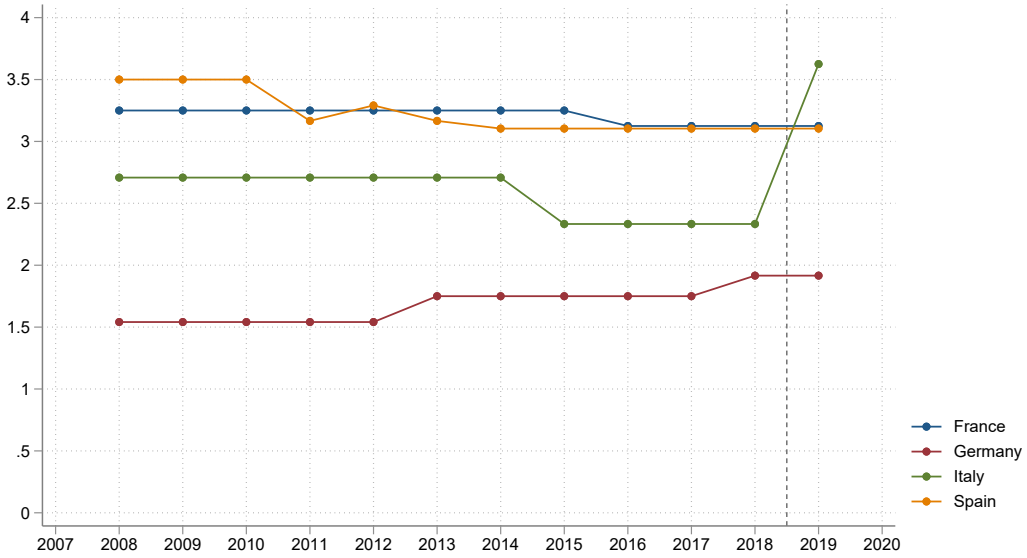
	(1) Hirings - P	(2) Conversions - FT → P	(3) Hirings - FT
γ	0.0067*	0.0191***	-0.0074
	(0.0032)	(0.0034)	(0.0052)
N	518,130	518,130	518,130
N of firms	14,437	14,437	14,437
Within R^2	0.0016	0.0150	0.0016

Notes: The table reports the γ coefficient values derived from Model 2.3 for different outcomes. Column (1) provides the coefficient for entries in permanent employment. Column (2) presents the coefficient for conversions of existing fixed-term (FT) contracts into permanent ones. Column (3) shows the coefficient for FT hirings. The outcomes are first transformed using the inverse hyperbolic sine (IHS) transformation and then residualized with respect to calendar months to control for seasonal patterns. Standard errors are clustered at the firm level.
Source: Mercurio, see Section 2.2.

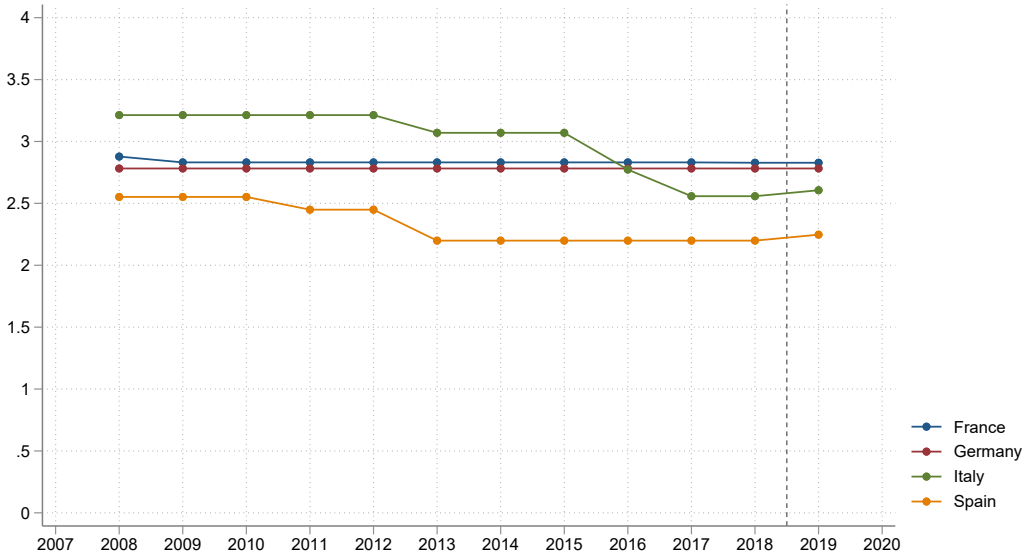
B7. Appendix Figures

FIGURE B.1. Strictness of EPL in Europe

A. Restrictions on temporary contracts

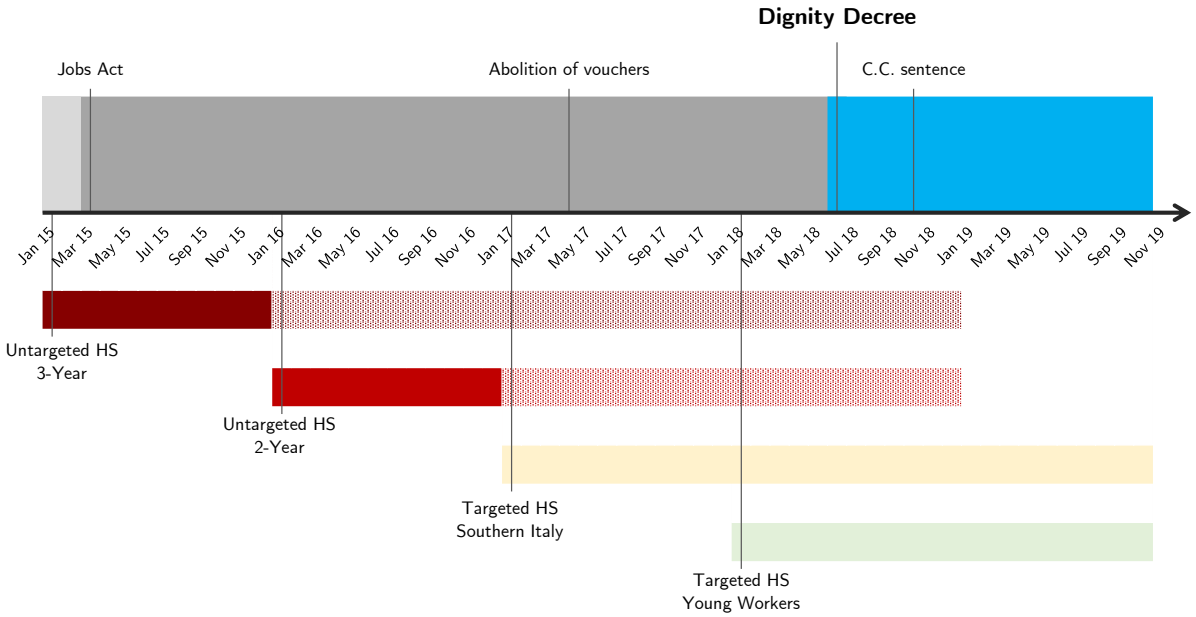


B. Protection for permanent contracts



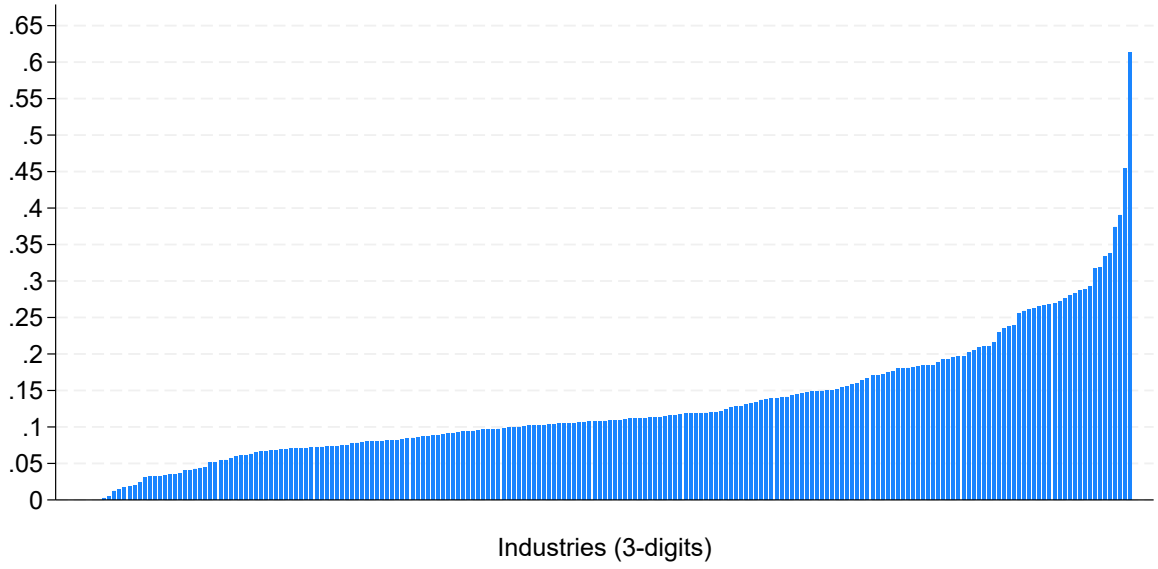
Notes: Strictness of the Employment Protection Legislation regarding permanent and temporary jobs for France, Germany, Italy and Spain. The indicator is a synthetic measure of the level of protection against dismissals for permanent contracts and of the level of regulation for the use of temporary contracts.
Source: OECD

FIGURE B.2. Reforms and interventions in the Italian labor market



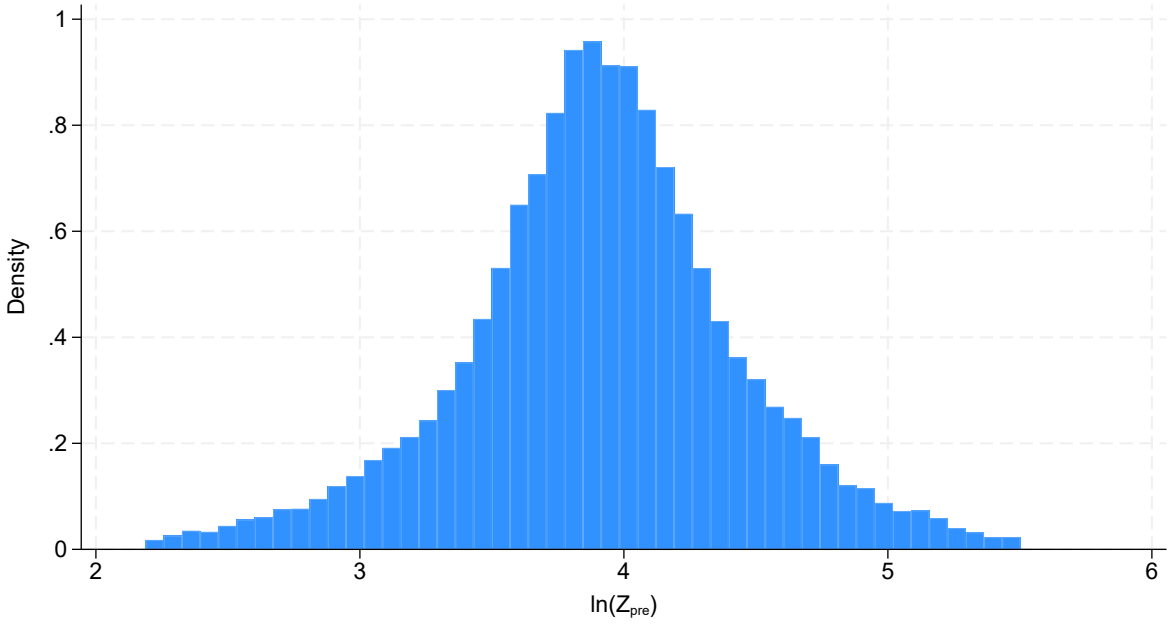
Notes: Timeline of reforms and interventions in the Italian labor market (2015-2019). Structural reforms are reported on top of the timeline while hiring subsidies are at the bottom. For subsidies, filled bars indicate periods in which it is possible to gain entitlement to the subsidy, and pattern-filled bars indicate the benefit duration

FIGURE B.3. Average share of temporary days worked – by 3-digit Industry



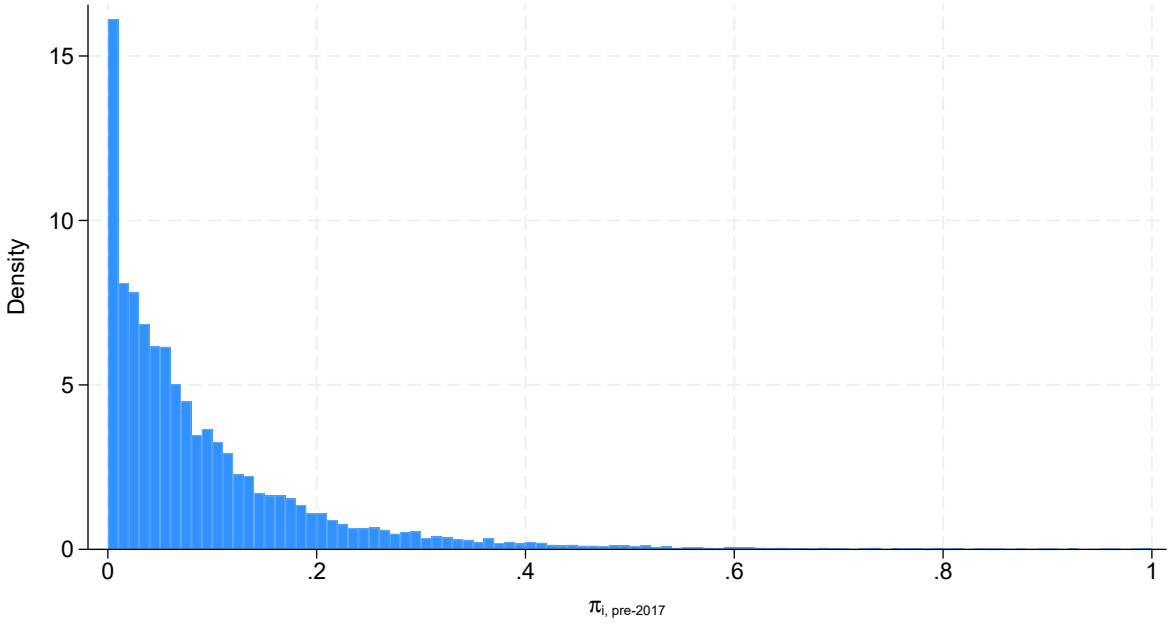
Notes: Average of the firm-level share of days worked in temporary contracts over total days worked calculated with monthly frequency (January 2017 - June 2018).
Source: Mercurio, see Section 2.2

FIGURE B.4. Distribution of Firm Productivity



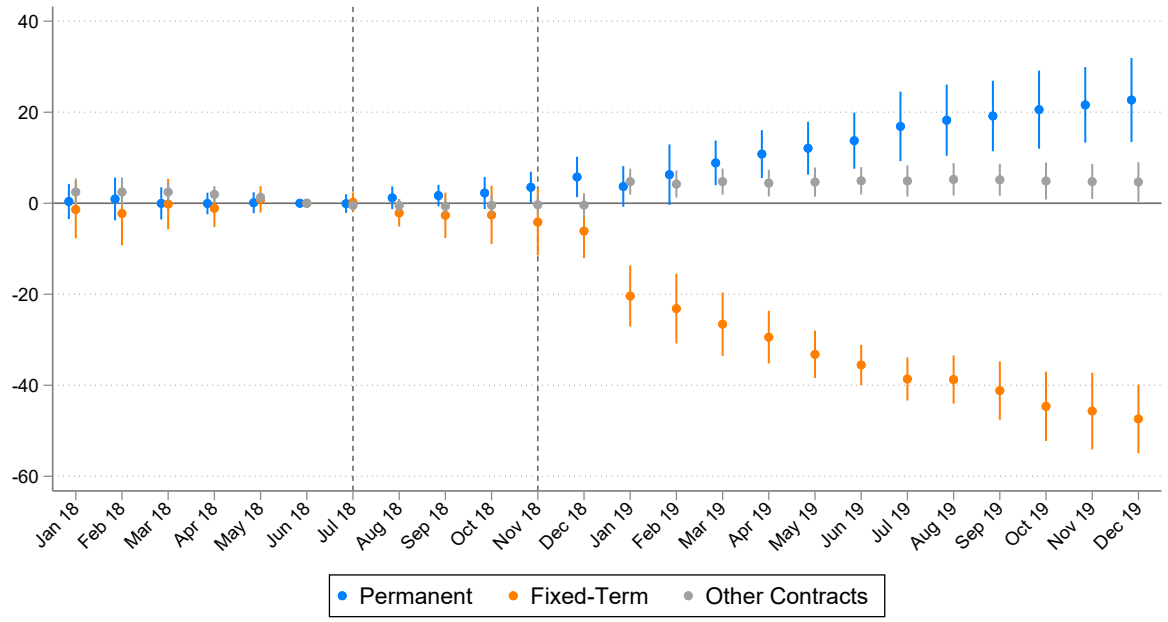
Notes: The figure presents the distribution of (the log of) firms' past labor productivity, denoted as $\ln(Z_{pre})$. Past labor productivity is calculated as the average yearly log value added per worker from 2012 to 2016. In cases where a firm appears later in the data, we calculate the average log value added per worker from their first year until 2016. The distribution is trimmed with respect to outliers by eliminating observations below percentile 1 and above percentile 99.
Source: Mercurio, see Section 2.2.

FIGURE B.5. Distribution of Firm-Level Temporary Employment Share



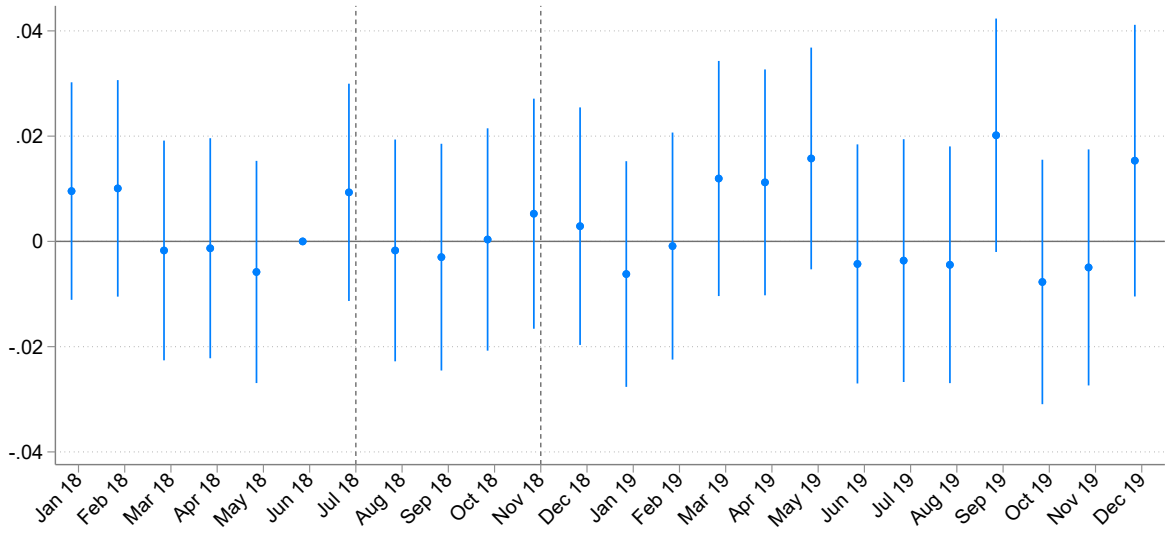
Notes: The figure presents the distribution of the firm-level temporary employment share. This is calculated as the average monthly percentage of days worked in fixed-term contracts ($\pi_{i,Pre\text{-}2017}$) at the firm level for the pre-reform period (January 2017 - June 2018).
Source: Mercurio, see Section 2.2.

FIGURE B.6. Employment Stocks by Contract Type - Exposed vs. Non-exposed Firms



Notes: The graph reports the average difference in monthly total days worked between exposed and non-exposed firms (estimates of β_t from the model in Equation 2.2). Other contracts include (i) apprenticeship, (ii) consultancy (so-called *parasubordinato*), (iii) intermittent worker, (iv) internship (so-called *tirocinio*) and (v) seasonal worker. The outcomes are first residualized with respect to calendar months to control for seasonal patterns. Standard errors are clustered at the firm level.
Source: Mercurio, see Section 2.2.

FIGURE B.7. Termination Pattern for Permanent Contracts - Exposed vs. Non-exposed Firms



Notes: The graph presents the estimated average monthly difference in gross terminations for permanent contracts (estimates of β_t from the model in Equation 2.2). The outcome is first transformed using the inverse hyperbolic sine (IHS) transformation and then residualized with respect to calendar months to control for seasonal patterns. Standard errors are clustered at the firm level.
Source: Mercurio, see Section 2.2.

B8. Background

B8.1. The international context

Temporary contracts have enjoyed a great diffusion during the last three decades. When during the 1980s and '90s unemployment rates started to pick up and remain high for prolonged periods in many European countries, part of the blame was ascribed to the very low degree of flexibility prevailing in European labor markets at the time. As a reaction, policymakers in these countries started reforming their body of Employment Protection Legislation (EPL) with interventions aimed at revamping labor market dynamism and leaving the so-called *Eurosclerosis* behind. Rather than embarking on the politically-costly endeavor of reforming the legislation covering permanent employment contracts, however, many governments opted for an alternative: adjusting the degree of labor market flexibility at the margin by facilitating the use of temporary employment contracts, considered a less-binding form of working relationship between employer and employee.

The rationale behind this move was simple: matching the marginal labor demand – for which the very rigid and protected permanent contracts would be too costly – with a new slice of supply, drawn directly out of the ranks of the unemployed. To many policymakers this looked like a "free lunch" policy, which would increase total employment without the need to touch the rights of incumbent permanent workers. In countries such as Italy and Spain, the de-regulation of temporary contracts came to be considered a standard tool to boost employment up to the mid-2010s.

Today, after three decades and many reforms abiding by this principle, the era of temporary work liberalizations might have finally come to an end: governments have started to give more weight to the downsides of job precariousness and some of the countries characterized by very high usage of temporary contracts – Portugal, Italy and Spain – have recently taken step backs to discourage systematical recourse to these contractual arrangements.

B8.2. Labor market reforms in Italy

The progressive diffusion of temporary contracts in Italy dates back to the 1990s when a number of reforms started a process of temporary contracts liberalization that continued throughout the 2000s. As underlined in Daruich et al. 2023, most of these legislative measures were *partial*, meaning that, while the regulation of temporary contracts was

largely relaxed, the employment protection measures associated with permanent positions remained unchanged. Over the 2010s, on the contrary, three major reforms significantly intervened in the Italian labor market by acting on the legislation concerning both types of contract.

In 2012, the *Fornero Reform*[1] attempted to reduce the regulatory gap between permanent and temporary contracts by acting on both sides: on one hand, it reduced the uncertainty about firing costs associated to permanent positions[2]; on the other hand, it made more costly the renewal of temporary contracts[3].

In 2015, the *Jobs Act* brought even further the reduction of protection for permanent positions[4] but at the same time strongly liberalized the use of temporary contracts[5].

The third of these reforms was the Dignity Decree, the object of interest of our study (see Section 3.2.2 for details). The policy pitch of this reform was clearly stated by its political promoters: "putting a stop to rampant precariousness" and marking a clear discontinuity with respect to previous reforms, which had operated according to "the myth of flexibility and the bugbear of rigidity"[6].

B8.3. Hiring subsidies

Despite the period between the introduction of the Jobs Act in 2015 and the signing of the D.D. in 2018 was a relatively calm one on the side of changes in labor market legislation, the various governments in charge during those years still took a very interventionist stance with respect to the incentivization of permanent employment: four waves of hiring subsidies flooded the Italian labor market, providing monetary benefits to firms hiring workers through permanent employment contracts[7].

[1] *Law 28 June 2012, n. 92*. Not to be confused with the *Fornero Law* (pension system reform from the same minister)

[2] Fornero Reform eliminated the automatic reinstatement of a worker in the job in some cases of unlawful dismissal (the judge could opt for monetary compensation)

[3] Fornero Reform introduced the requirement of an explicit justification for the recourse to temporary employment (except for the first contract) and increased the fiscal contribution for renewals by 1.4%

[4] Jobs Act introduced the *graded-security* contract, which limits the reinstatement to discriminatory dismissals and fixes a predetermined level for the compensation for unfair dismissals, proportional to job tenure. In September 2018, the Constitutional Court Sentence 194/2018 stroke down this pillar of Jobs Act by determining that a judge can once again step in to decide the amount of the severance

[5] *Poletti Decree* eased the process of entry and renewals associated to temporary positions alongside and removed the requirement of a justification for any temporary employment relationships up to 36 months and/or to 5 extensions

[6] Min. of Labor Luigi Di Maio, speech to the Chamber of Deputies, July 30, 2018

[7] Untargeted subsidy programs: (i) Total exemption from employer's social security contributions (36 months duration, €8,060 yearly cap); (ii) 40% exemption from employer's social security contributions (24 months duration, €3,250 yearly cap). Targeted subsidy programs: (iii) Firms in the South: total exemption

While the first two waves of subsidies – those of 2015 and 2016 – were very large and directed to the entire labor force, the subsequent two waves – starting in 2017 – were much more restricted in size and targeted to specific groups of workers: those working in a firm located in regions of Southern Italy and those under 35 years of age.

To better visualize the whole institutional picture, in Figure B.2 we report the timeline of the reforms and interventions undertaken in the Italian labor market starting from 2015.

from social security contributions (12 months duration, €8,060 yearly cap); (iv) Under-35 workers: 50% exemption from social security contributions (36 months duration, €3,000 cap).

B9. Additional Data

Osservatorio sul Precariato - INPS. In Section B10 we describe the behavior of aggregate occupational flows (hirings, terminations, conversions) in Italy before and after the reform. We make use of publicly available aggregate data from the INPS observatory *Osservatorio sul Precariato* – based on social security records.

Labor Force Survey - Istat. In Section B10 we provide information regarding the evolution of the stock of dependent employment in the Italian labor market. We use aggregate quarterly data derived from the Labor Force Survey conducted by the Italian statistical institute (Istat). One caveat when comparing these data with the flow ones is that stock data aggregate under the label "temporary contracts" all contract types that envisage a pre-established duration (including, e.g., internships), forcing us to abide by this definition. In the rest of the paper, we generally use the term "fixed-term contracts" when talking specifically about "*Contratto a tempo determinato*".

B10. Aggregate Trends

Since the end of 2013, Italy has experienced a prolonged period of steady employment growth, that continued – with basically no interruption – until the arrival of the Covid-19 pandemic in 2020 (Figure B.8).

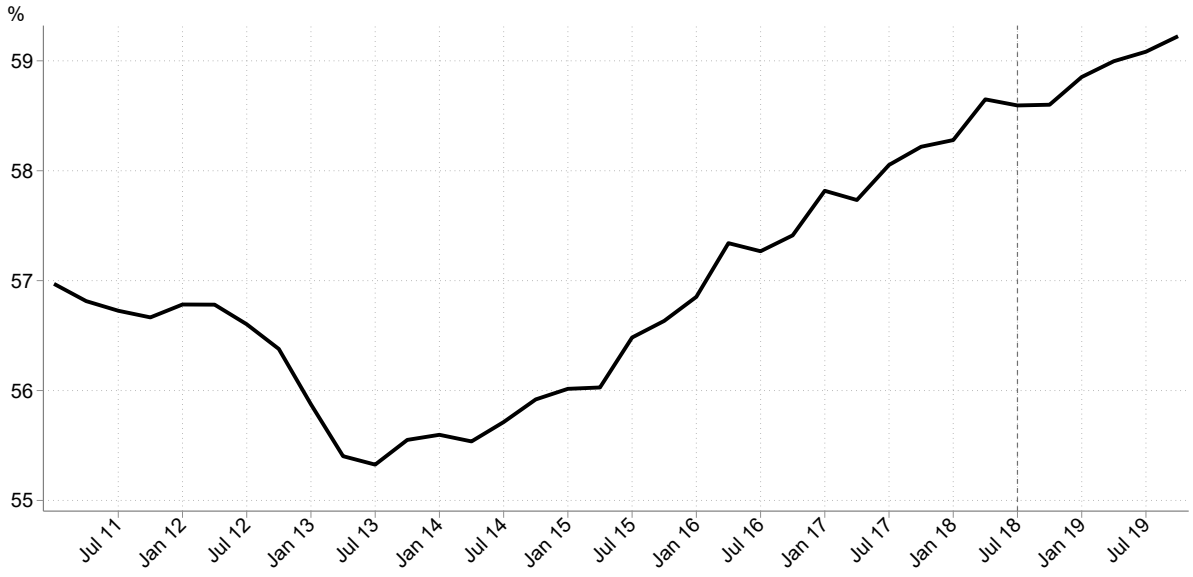
Nevertheless, the source of this occupational expansion has been far from homogeneous over time: the relative contribution of new permanent and temporary jobs to total employment growth has indeed varied a lot over time, with an alternation of periods of permanent employment expansion and periods when temporary contracts were the main drivers of change (Figure B.9).

Starting in 2016[8], this occupational expansion was driven more and more by the growth of temporary jobs, the share of which rose from 14% to 17% of total employment in a little more than two years. This expansionary trend for temporary jobs came to a sudden stop in the third quarter of 2018, the quarter in which the D.D. reform was signed into law by the Italian government (Figure B.10).

The natural question arising from looking at this line is whether this change was the product of a large-scale substitution across contract types, or whether it can be ascribed

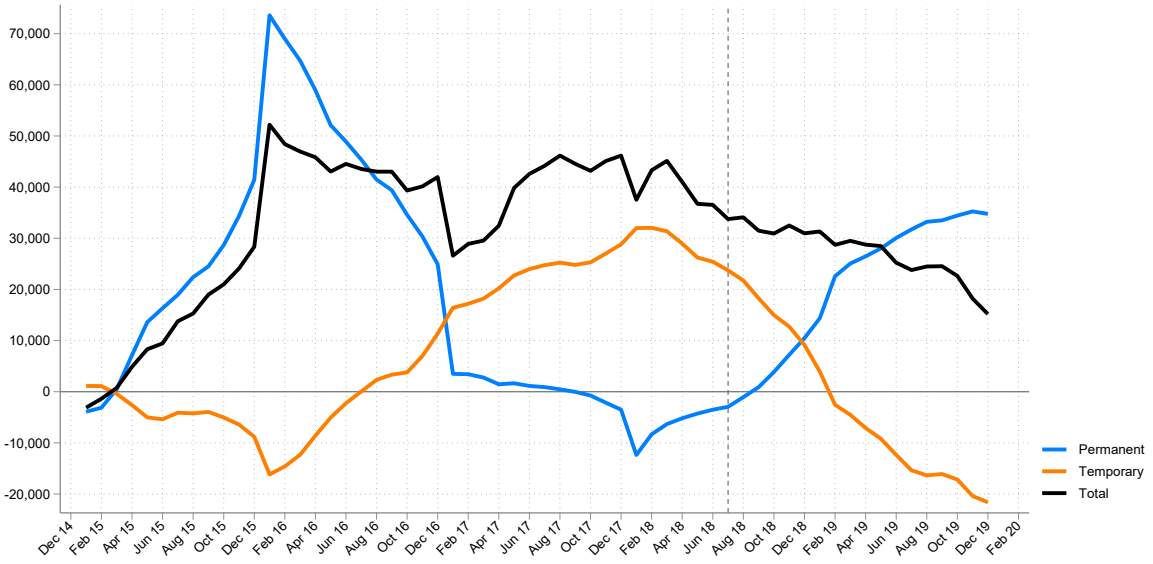
[8]January 2016 marked the end of a very large wave of hiring subsidies in favor of firms offering new permanent-contract jobs.

FIGURE B.8. Employment rate in the working-age population (Italy, 2010-2019)



Notes: Employment rate for people aged 15-64 in Italy (2010-2019).
Source: FRED

FIGURE B.9. Net labor flows: 12-months backward-looking M.A

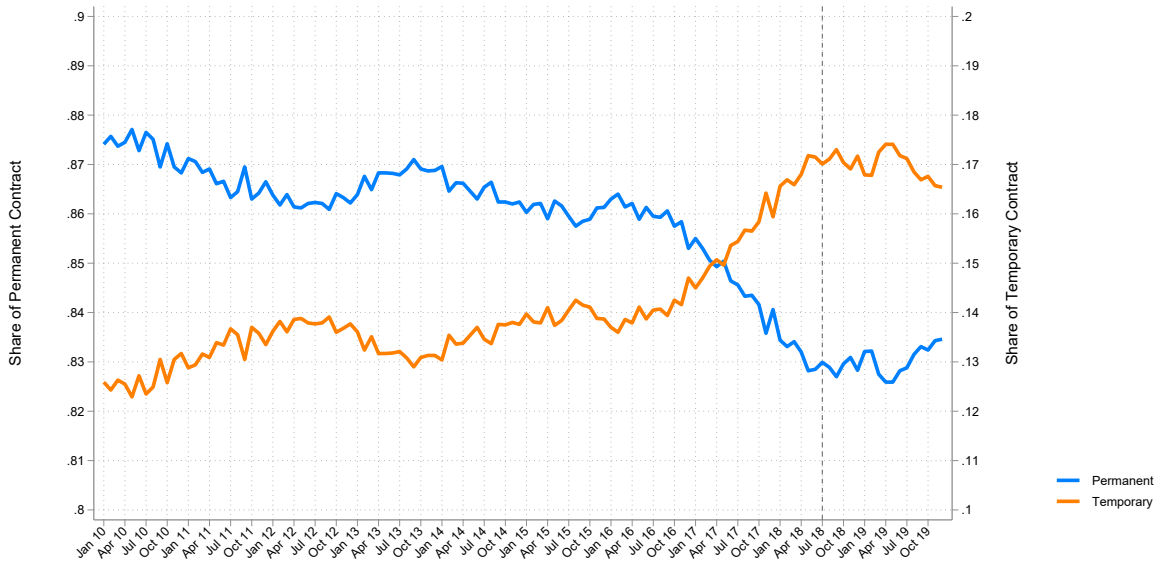


Notes: Net occupational flows by type of contract (2015-2019). Net flows = hirings - terminations +/- conversions. Total flows also include other temporary contracts. Flows are reported as 12-month backward-looking moving averages to handle calendar effects. The vertical line indicates the month in which the D.D. is introduced.
Source: INPS, see Section B9.

to growth and reallocation mechanisms in the economy.

We provide an answer to this question by means of a decomposition (equation B.1).

FIGURE B.10. Shares of total employment by contract type



Notes: Shares of total employment by type of contract (2010-2019). The left axis refers to permanent positions, and the right one to temporary positions. The vertical line indicates the month in which the D.D. is introduced.
Source: ISTAT, see Section B9.

We first write the total stock of temporary[9] jobs in Italy at any given point in time (T_t) as the sum of the stocks utilized by k different sectors in the economy. We then write each element of this sum as the product of three factors: total employment in the economy, the weight of each sector (in percentage terms w.r.t. total employment E_t) and the share of temporary jobs in each sector. The blue, red and green lines in Figure B.11 represent the fraction of variability that is explained by changes in each of the three factors in equation B.1.

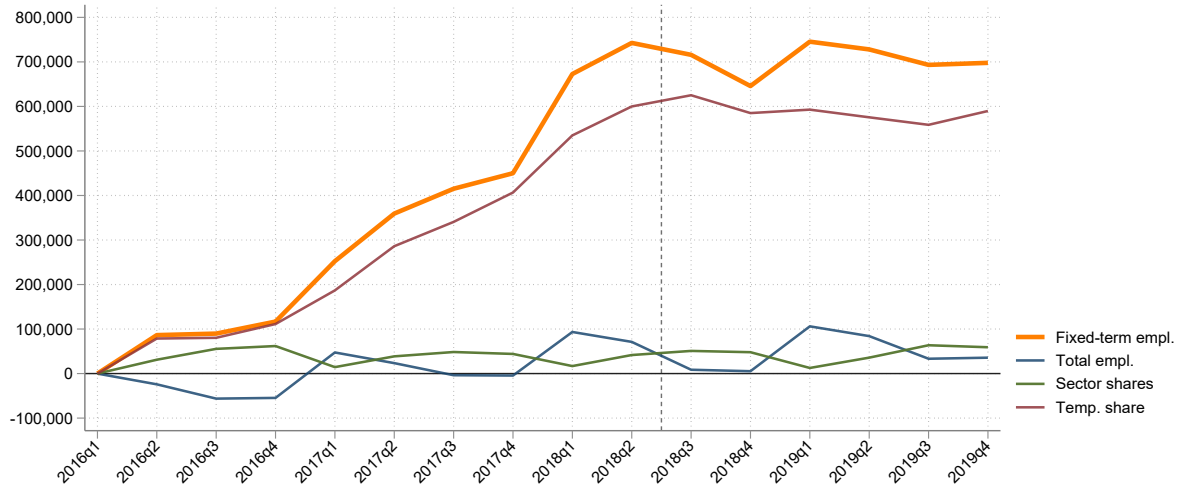
$$T_t = \sum_k T_{k,t} = \sum_k \underbrace{E_t}_{\text{Total empl.}} * \underbrace{\frac{E_{k,t}}{E_t}}_{\text{Sector share}} * \underbrace{\frac{T_{k,t}}{E_{k,t}}}_{\text{Temp. share}} \tag{B.1}$$

The main driver of the growth in temporary employment over the 2016-2020 period is an increase in the within-sector share of jobs governed by temporary contracts (red line). On the contrary, the contributions coming from labor reallocation across sectors (green line) and from the overall employment growth in the country (blue line) are very

[9] Data on labor stocks do not separate fixed-term contracts from other kinds of temporary jobs (see Section B9)

limited and display no significant discontinuity over time. Moreover, Figure B.11 clearly identifies mid-2018 as the key moment for a change in this substitution pattern across contracts in the different sectors of the economy.

FIGURE B.11. Temporary-jobs growth in Italy (2016-2019)



Notes: Absolute variation in the deseasonalized stock of fixed-term jobs, differences from 2010. Blue, green and red lines represent the contribution from the three factors from equation (B.1).
Source: ISTAT, see Section B9.

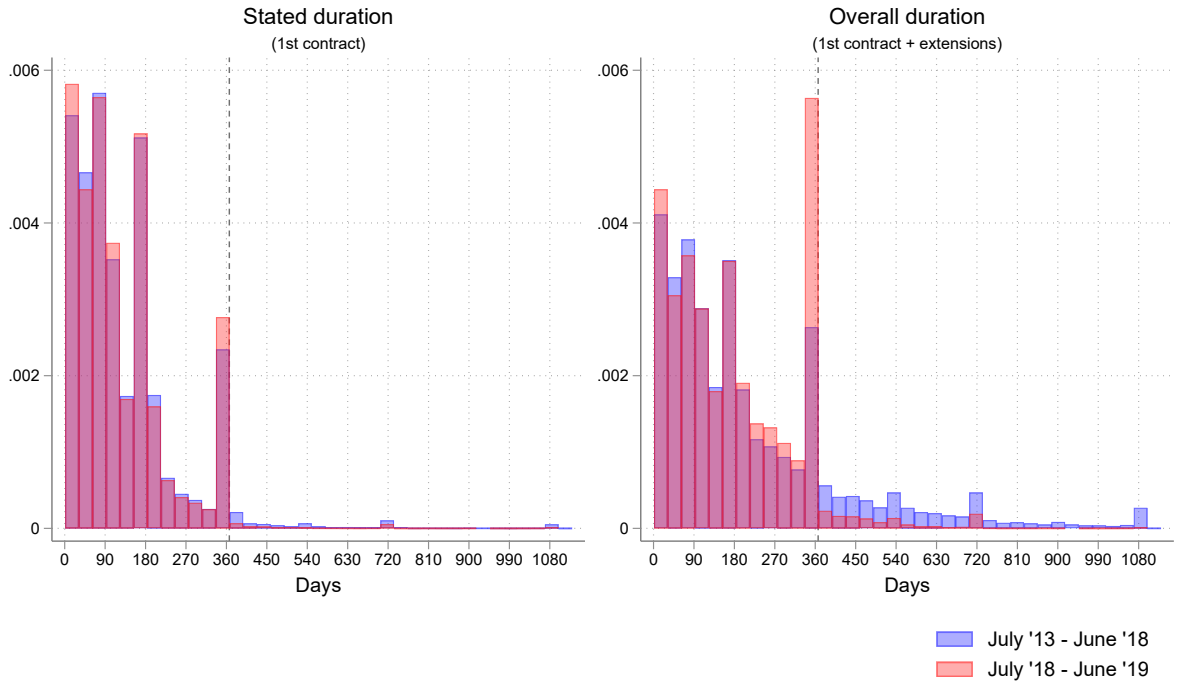
This simple decomposition points to substitution across contract types as the main driving force behind the surge and subsequent halt of temporary employment growth in Italy.

We turn to providing some empirical evidence on the intensive margin of temporary contracts utilization. We focus on the duration and number of extensions and compare temporary contracts signed before and after the 2018 reform. As this type of data is not available at the national level, we rely on the Mercurio dataset once again and narrow our focus to the Veneto region.

Figure B.12 displays the distribution of temporary contracts with respect to their duration, limiting the sample to those contracts that regulate the first employment relationship between a worker and a firm (i.e. excluding recalls[10], the number of which is very stable over time).

[10]Recalls arise when workers return to work for a company which had already employed them with a temporary contract in the past after a "cooling-off" period.

FIGURE B.12. Duration for 1st temporary contracts



Notes: Duration histogram of temporary employment contracts before and after the introduction of the D.D.
Left panel: stated duration – the duration that is written on the contract when it is first signed.
Right panel: overall duration – the duration of the entire employment relationship, also including contract extensions.
Source: Mercurio, see Section 2.2.

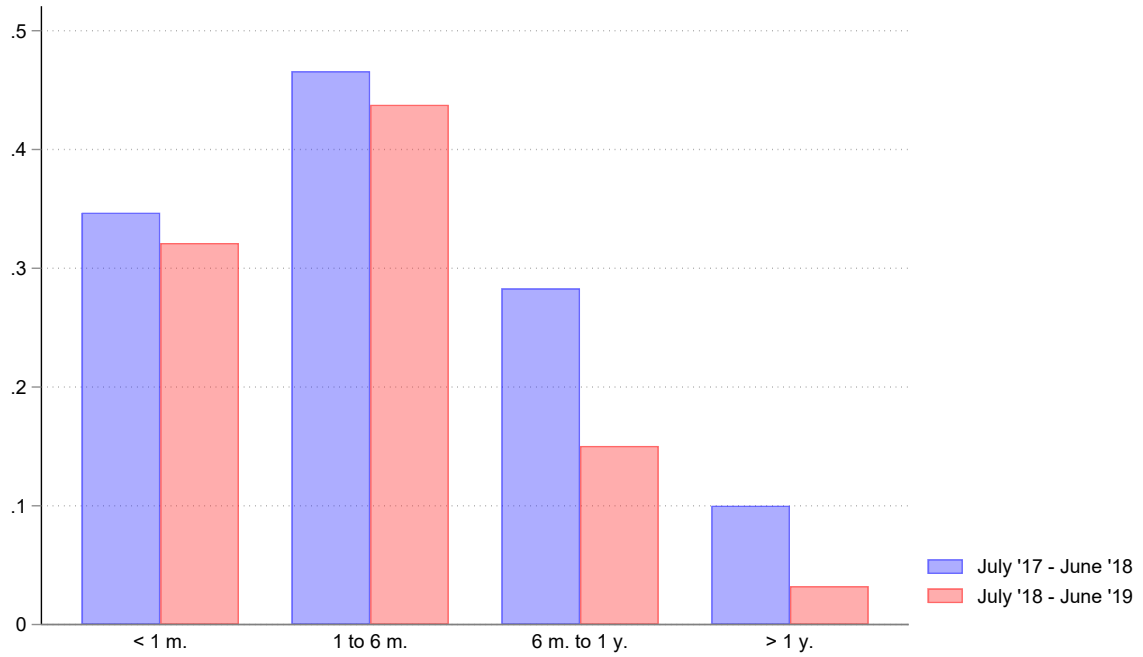
Looking at the overall duration (r.h.s.), a clear difference emerges from a before-after comparison of the distributions: after the D.D. we observe a marked increase in the concentration of durations just below the 1-year threshold, with a fall in the share of very long temporary employment spells and also a reduction in the fraction of contracts lasting less than six months overall. These changes can be reconciled with the design of the reform, which entails significant increases in costs for temporary employment spells that go over one year of duration and discourages the repeated use of short-term extensions[11].

[11] One important disclaimer applies to this comparison. In the first months of 2020 nearly all temporary contracts in the Italian economy were left to expire by firms due to the effect of the Covid-19 pandemic on labor demand: this implies that some of the temporary employment relationships that would have been extended in the absence of the pandemic were instead dissolved upon contract expiration. This shortening effect is bound to be more relevant the closer a temporary contract is signed with respect to 2020.

Nevertheless, two elements seem to support the meaningfulness of our before-after comparison. First, similar distributional effects can be observed for the stated contract duration (l.h.s. panel), which – being established at the moment of signing – does not suffer from the pandemic-induced bias. Second, pandemic-related "censoring" alone would have caused a much more uniform shift of the duration

Shifting to contract extensions, a coherent story emerges: after the reform the fraction of temporary contracts that get extended at expiration is reduced, and this holds across all stated-duration groups (Figure B.13). The most substantial decrease, however, is observed for contracts between six months and one year of duration, those that have the highest likelihood of crossing the one-year threshold when opting for an extension. This is coherent with a strategic reduction in the number of extensions, aimed at avoiding the additional costs implied by the reform for temporary employment spells over one year of duration[12].

FIGURE B.13. Share of extended temporary contracts



Notes: Share of extended temporary contracts before and after the introduction of the D.D., by class of stated duration.
Source: Mercurio, see Section 2.2.

distribution towards lower values, while we observe a spike just below the 1-year threshold.

[12]The same right-censoring problem outlined for duration applies here, and it affects more contracts of longer duration

B11. Model Details

This section contains many mathematical steps, that were omitted in the main body for brevity purposes.

B11.1. Properties of Surplus Functions and Optimal Duration

Properties of $S_P(z, \lambda)$ with respect to λ. The surplus associated with starting a permanent position is decreasing in λ. There is only one value λ_P where the surplus is zero. To the left of this value, the surplus is always positive, implying that signing a permanent contract would generate some value. On the right of it, in contrast, it is never optimal for the match to be formalized into a permanent employment contract.

$$\frac{\partial S_P}{\partial \lambda} = \frac{-z + rU - rF}{(r+\lambda)^2} \leq 0$$

$$S_P(\lambda = 0) = \frac{z}{r} - U - c$$

$$\lim_{\lambda \to \infty} = -c - F$$

$$\exists! \quad \lambda_P(z) = \frac{z - rU - rc}{F + c} \quad \text{such that} \quad S_P(\lambda) \lesseqgtr 0 \leftrightarrow \lambda \gtreqless \lambda_P$$

Properties of $S_P(z, \lambda)$ with respect to z. The productivity parameter z acts as a shifter for permanent contracts' surplus. For any given λ, a higher z implies a higher value for the surplus of starting a permanent position. The lower the λ, the higher the impact of the shifter. This means that the productivity premium that workers in high-z firms enjoy translates to lower and lower surplus gains as the shock arrival rate of their match increases, due to the lower expected duration of the production opportunity.

$$\frac{\partial S_P}{\partial z} = \frac{1}{(r+\lambda)} > 0$$

$$\frac{\partial^2 S_P}{\partial \lambda \partial z} = -\frac{1}{(r+\lambda)^2} < 0$$

Optimal duration. For fixed-term contracts, optimal contract duration is decreasing in λ. Solving the integrals in equation (2.8) gives (for a given duration Δ):

$$S_T(z, \lambda, \Delta, c_R) = z\left[\frac{1 - e^{-\Delta(\lambda+r)}}{\lambda + r}\right] - U(1 - e^{-\Delta r}) + e^{-(r+\lambda)\Delta} \max\{S_P(z, \lambda), 0\} - c - c_1 e^{-\lambda} \tag{B.2}$$

The optimal duration of a temporary job is chosen so as to maximise its expected surplus. This yields the first order condition[13]:

$$S'_T(\Delta, z) = e^{-\lambda\Delta} z - rU - (r+\lambda)e^{-\lambda\Delta} \max\{S_P(z,\lambda), 0\} = 0 \tag{B.3}$$

which yields

$$\Delta^* = \begin{cases} \frac{1}{\lambda} \ln\left(\frac{rU+\lambda F+(r+\lambda)c}{rU}\right) & \text{if } \lambda < \lambda_P(z) \\ \frac{1}{\lambda} \ln\left(\frac{z}{rU}\right) & \text{if } \lambda \geq \lambda_P(z). \end{cases} \tag{B.4}$$

Properties of $S_T(z, \lambda, c_R)$ with respect to λ. The surplus associated with starting a temporary position is decreasing in λ. Having derived the optimal contract duration, the F.O.C. in equation (B.3) can be used to obtain:

$$\max\{S_P(z,\lambda), 0\} = \frac{z - rUe^{\lambda\Delta^*}}{r+\lambda}$$

Substituting this term into (B.2) gives an expression for S_T:

$$S_T(z, \lambda, c_R) = \max_{\Delta}\{S_T(z, \lambda, \Delta, c_R)\} = \frac{z}{\lambda + r} - U + \frac{\lambda U e^{-r\Delta^*}}{\lambda + r} - c - c_1 e^{-\lambda} \tag{B.5}$$

Using this expression, we can study the relationship between S_T and λ on both sides of $\lambda_P(z)$. When $\lambda < \lambda_P(z)$:

$$\frac{\partial S_T}{\partial \lambda} = z\frac{e^{-(\lambda+r)\Delta^*}[(\lambda+r)\Delta^* + 1] - 1}{(\lambda+r)^2} + c_1 e^{-\lambda} - e^{-(\lambda+r)\Delta^*}[\Delta^* S_P(z,\lambda) - S'_p(\lambda)]$$

$$\lim_{\lambda \to 0} S_T = \frac{z}{r} - U - c - c_1 e^{-\lambda}$$

The derivative is negative for sufficiently small values of c_R. When $\lambda \geq \lambda_P(z)$:

$$\frac{\partial S_T}{\partial \lambda} = z\frac{e^{-(\lambda+r)\Delta^*}[(\lambda+r)\Delta^* + 1] - 1}{(\lambda+r)^2} + c_1 e^{-\lambda}$$

$$\lim_{\lambda \to \infty} S_T = -c$$

The derivative is negative again for sufficiently small values of c_R.

[13]The second-order condition is always fulfilled as in Cahuc et al. 2016

Properties of $S_T(z, \lambda, c_R)$ with respect to z. The productivity parameter z acts as a shifter also for fixed-term contracts' surplus. Deriving S_T in equation (B.5) with respect to z gives

$$\frac{\partial S_T}{\partial z} = \begin{cases} = \frac{1}{\lambda+r} = \frac{\partial S_P}{\partial z} > 0 & \text{if } \lambda < \lambda_P(z) \\ \frac{1}{\lambda+r}\left[1 - e^{-(\lambda+r)\Delta^*}\right] > 0 & \text{if } \lambda \geq \lambda_P(z). \end{cases}$$

Properties of $S_T(z, \lambda, c_R)$ with respect to c_R. An increase in c_R decreases the surplus generated by a temporary position:

$$\frac{\partial S_T}{\partial c_1} = -e^{-\lambda} < 0$$

The effect is more pronounced for lower λ:

$$\frac{\partial^2 S_T}{\partial \lambda \partial c_1} = e^{-\lambda} > 0$$

B11.2. Job Creation

The decision on which contract to use depends on the sign of $h(\cdot)$ (see equation 2.10), that is characterized by:

$$\lim_{\lambda \to 0} h(\lambda) = -c_R < 0$$

$$\lim_{\lambda \to \infty} h(\lambda) = F > 0$$

$$h'(\lambda) = \frac{-\lambda U r \Delta'(\lambda) e^{-r\Delta^*}}{\lambda + r} + \frac{r(F-U)}{(\lambda+r)^2} + \frac{rUe^{-r\Delta^*}}{(\lambda+r)^2} + c_1 e^{-\lambda}$$

$$\lim_{\lambda \to 0} h'(\lambda) = \frac{F-U}{r} + c_R$$

The last derivative is negative for sufficiently small values of c_R. By continuity, there exists at least one value of λ_S when $F = U(1 - e^{-r\Delta^*(\lambda_S)}) + (\lambda_S + r)c_1 e^{-\lambda_S}$ such that $h(\lambda_S) = 0$. Since:

$$h'(\lambda_S) = \frac{-\lambda_S U r \Delta'(\lambda_S) e^{-r\Delta^*(\lambda_S)}}{r + \lambda_S} \geq 0 \tag{B.6}$$

the value is unique.

Optimal Contract Type and Productivity. λ_S is not affected by z. Indeed, $h(\lambda, c_R)$ starts being a function of z only for $\lambda > \lambda_P$ (see equation 2.10). On the contrary, λ_P and λ_T are affected by the productivity shifter z in equilibrium. We know that

$$S_P(\lambda_P, z) = 0 \quad \text{defines} \quad \lambda_P = f(z)$$
$$S_T(\lambda_T, z) = 0 \quad \text{defines} \quad \lambda_T = g(z)$$

Relying on the Implicit Function Theorem, we can write:

$$\frac{d\lambda_P}{dz} = f'(z) = -\frac{\frac{\partial S_P}{\partial z}}{\frac{\partial S_P}{\partial \lambda_P}} > 0$$
$$\frac{d\lambda_T}{dz} = g'(z) = -\frac{\frac{\partial S_T}{\partial z}}{\frac{\partial S_T}{\partial \lambda_T}} > 0$$

In equilibrium, λ_P and λ_T both increase when z increases marginally.

The Distribution of Shock Arrival Rates and Productivity. In our model, firm productivity affects not only the surplus produced per unit of time but also the composition of a firm's matches (shock arrival rates). To understand how, we derive equation (2.12) with respect to the productivity shifter z. Applying the chain rule, we get:

$$\frac{\partial G(\lambda, z)}{\partial z} = \left(\frac{\lambda}{\mu}\right)^{\sigma} \cdot e^{-z \cdot \left(\frac{\lambda}{\mu}\right)^{\sigma}}$$

Given the restrictions imposed on our parameters, this derivative is always positive.

B11.3. Regulation and Job Creation

The Impact of the Reform on Permanent Hirings. λ_S is defined as the value that verifies $h(\lambda_S, c_R) = 0$. By making use of the implicit function theorem again, we can write:

$$\frac{\partial \lambda_S}{\partial c_R} = -\frac{\frac{\partial h}{\partial c_R}}{\frac{\partial h}{\partial \lambda}}$$

The numerator of this expression is negative:

$$\frac{\partial h}{\partial c_R} = -e^{-\lambda} < 0$$

The sign of the denominator is not as straightforward:

$$\frac{\partial h}{\partial \lambda} = \frac{-\lambda_S U r \Delta'(\lambda_S) e^{-r\Delta^*}}{r+\lambda_S} + \frac{r(F-U)}{(r+\lambda_S)^2} + \frac{rUe^{-r\Delta^*}}{(r+\lambda_S)^2} + c_1 e^{-\lambda_S}$$

We know from Section 2.4.4 that the sign of this derivative for a generic λ is ambiguous as $F - U < 0$, but also that $h'(\lambda_S) > 0$ (equation B.6).

The Impact of the Reform on Temporary Hirings. We derive the effect of a change in c_R on λ_T taking z as given. By using the implicit function theorem, we can write:

$$\text{(B.7)} \qquad \frac{\partial \lambda_T}{\partial c_R} = -\frac{\frac{\partial S_T}{\partial c_R}}{\frac{\partial S_T}{\partial \lambda}} = \frac{(\lambda_T(z)+r)^2 \cdot e^{-\lambda_T(z)}}{z\left\{e^{-(\lambda_T(z)+r)\Delta^*}[(\lambda_T(z)+r)\Delta^*+1]-1\right\} + (\lambda_T(z)+r)^2 \cdot c_1 e^{-\lambda_T(z)}}$$

This derivative is negative for sufficiently small values of c_R: an increase in contract costs associated with temporary contracts reduces the share of matches for which $S_T(\lambda) > 0$.

B11.4. Labor Market Equilibrium

Equilibrium Labor Market Tightness θ**.** To prove the uniqueness of the equilibrium value of θ across the distribution of z, we reformulate the free entry condition in Equation 2.18 as a function of $\{z, \theta\}$:

$$\Gamma(z,\theta) = \alpha\gamma\nu(z)^{\alpha-1} - \frac{q(\theta)(1-\beta)}{r+q(\theta)}\left[\int_{\lambda_{min}}^{\lambda_S(\theta)} S_P(z,\lambda)dG(\lambda,z) + \int_{\lambda_S(\theta)}^{\lambda_T(z,\theta)} S_T(z,\lambda)dG(\lambda,z)\right]$$

where the threshold $\{\lambda_S, \lambda_P, \lambda_T\}$ can be expressed as a function of θ through Equation 2.16. We now differentiate Γ with respect to θ. The marginal cost of creating a vacancy is not dependent on the market tightness, so we are essentially looking at how the expected benefits of a vacancy change with θ.

$$\Gamma_\theta(z,\theta) = -(1-\beta)\left[\frac{q'(\theta)r}{(r+q(\theta))^2}\right]\left(\int_{\lambda_{\min}}^{\lambda_s(\theta)} S_P(z,\lambda)dG(\lambda,z) - \int_{\lambda_s(\theta)}^{\lambda_t(\theta)} St(z,\lambda)dG(\lambda,z)\right)$$
$$- \frac{q(\theta)(1-\beta)}{r+q(\theta)}\int_{\lambda_{\min}}^{\lambda_s(\theta)} \frac{dS_P(z,\lambda)}{d\theta}dG(\lambda,z) - \int_{\lambda_s(\theta)}^{\lambda_t(\theta)} \frac{dSt(z,\lambda)}{d\theta}dG(\lambda,z).$$

Since the unemployment value is increasing in θ, it follows that $\frac{dS_P(z,\lambda)}{d\theta}$ and $\frac{dSt(z,\lambda)}{d\theta}$ are both negative for each z. As a consequence, $\Gamma_\theta(z,\theta)$ is always positive across the

distribution of z, so we can conclude that there exists a unique equilibrium θ for every z.

Employment Flows and Productivity. The derivatives of gross labor flows (hirings) with respect to z are:

$$\frac{\partial \Phi_P(z)}{\partial z} = q(\theta)\nu(z)\int_{\lambda_{min}}^{\lambda_S}\frac{\partial G(\lambda,z)}{\partial z}d\lambda + q(\theta)\frac{d\nu(z)}{dz}\int_{\lambda_{min}}^{\lambda_S}G(\lambda,z)$$

$$\frac{\partial \Phi_{TC}(z)}{\partial z} = q(\theta)\nu(z)\left[\int_{\lambda_S}^{\lambda_P(z)}\frac{\partial G(\lambda,z)}{\partial z}d\lambda + G(\lambda_P(z),z)\frac{d\lambda_P(z)}{dz}\right] + q(\theta)\frac{d\nu(z)}{dz}\int_{\lambda_S}^{\lambda_P(z)}G(\lambda,z)$$

$$\frac{\partial \Phi_{TNC}(z)}{\partial z} = q(\theta)\nu(z)\left[\int_{\lambda_P(z)}^{\lambda_T(z)}\frac{\partial G(\lambda,z)}{\partial z}d\lambda - G(\lambda_P(z),z)\frac{d\lambda_P(z)}{dz} + G(\lambda_T(z),z)\frac{d\lambda_T(z)}{dz}\right] + q(\theta)\frac{d\nu(z)}{dz}\int_{\lambda_P(z)}^{\lambda_T(z)}G(\lambda,z)$$

While the first two derivatives are always positive, the last term's sign is ambiguous.

B12. Model Estimation Details

In this section, we describe the procedure employed for the estimation of model parameters. These are obtained by finding the values that minimize the quadratic form in (2.23)

The estimation process begins by computing the empirical moments and by setting the values for the baseline parameters (see Table 2.2). We then define a grid of potential values for the 9 target parameters to be estimated and a second grid of values for U, a quantity which is linked to our model's parameters through equation (2.17). For each potential U value, we identify one vector of parameters by minimizing the objective function expressed by the quadratic form in (2.23) using the identity matrix as a starting choice for the weighting function[14].

Given this first set of local minima, we focus on constructing the optimal weighting matrix Λ^{-1}. This matrix assigns weights to the discrepancies between the empirical and model-implied moments so as to ensure that moments with lower variance ("higher reliability") have more influence on the final estimates. This is obtained by first evaluating the discrepancy between $\mathbf{p}$ and its model counterpart $\mathbf{p}(\Omega)$ at each of the N local minima previously computed. We then compute an empirical covariance matrix for

[14]We use the Interior-Point Algorithm in Matlab.

this set of vectors:

$$\Lambda = \frac{1}{N}\sum_{i=1}^{N}[\mathbf{p} - \mathbf{p}(\Omega)][\mathbf{p} - \mathbf{p}(\Omega)]'$$

After a regularization step, this matrix is finally inverted to obtain the optimal weighting matrix.

We then re-estimate the model parameters, minimizing once again the quadratic form in (2.23) but using the optimal weighting matrix. We use the local minima identified in our first step estimation as starting points for our parameter values in this refined estimation.

Subsequently, we derive standard errors for the best-estimated parameters. To compute these, we first derive a Jacobian matrix, denoted as J. This matrix captures the sensitivity of the model-implied moments $\mathbf{p}(\Omega)$ to infinitesimal variations in the parameters of Ω. Specifically, the element J_{ij} of the Jacobian matrix is given by:

$$J_{ij} = \frac{\partial \mathbf{p}_i(\Omega)}{\partial \Omega_j}$$

where $\mathbf{p}_i(\Omega)$ is the ith moment and Ω_j is the jth parameter in the vector Ω. The asymptotic variance $Var(\hat{\Omega})$ of the parameter estimates $\hat{\Omega}$ is given by:

$$Var(\hat{\Omega}) = (J'\Lambda^{-1}J)^{-1}J'\Lambda^{-1}J(J'\Lambda^{-1}J)^{-1}$$

The standard errors for the parameter estimates are then the square root of the diagonal elements of the asymptotic variance matrix:

$$SE(\hat{\Omega}_j) = \sqrt{Var(\hat{\Omega}_j)}$$

Where $SE(\hat{\Omega}_j)$ is the standard error of the jth parameter estimate and $Var(\hat{\Omega}_j)$ is the jth diagonal element of $Var(\hat{\Omega})$.

The procedure ends with an overidentification test, assessing the validity of the model's assumptions. We compute the test statistic leveraging the squared relative errors between the empirical moments and those implied by the model for the best-estimated parameters. We then employ the chi-squared distribution to compute the p-value for this statistic which provides insights into the model's fit to the data.

C. Appendix for Chapter 3

TABLE C.1. Summary Statistics – Before and After the Reform

	Before	After
Avg. Log-Daily Wage At Conversion	3.634	3.555
	(1.058)	(1.088)
Avg. Log-Daily Wage Before Conversion	3.556	3.491
	(1.070)	(1.088)
Avg. Tenure – Months	13.847	14.634
	(11.486)	(10.080)
Avg. Experience – Years	14.394	14.694
	(11.107)	(11.409)
Avg. Age	36.615	37.101
	(10.043)	(10.384)
Fraction of Females	0.361	0.379
	(0.480)	(0.485)
Fraction with Degree	0.140	0.126
	(0.347)	(0.332)

Notes: The table shows the summary statistics of variables analyzed in the study. It reports the average log-daily wage at the time of contract conversion and during the temporary employment phase, the average tenure in months, the average years of experience, and the average age both before and after the reform, measured at the time of conversion. The table also indicates the fractions of converted females and individuals holding a degree during the same time frames.