Programa de Doctorado en Economía y Gestión Empresarial

ESSAYS ON IMPACT EVALUATION OF LABOUR MARKET REFORMS USING ADMINISTRATIVE DATA

Tesis Doctoral presentada por

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2026

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Alcalá de Henares, julio 2026

Agradecimientos

Esta tesis doctoral es el resultado de cuatro años de trabajo en el Departamento de Economía de la Universidad de Alcalá. Tras estos años, quisiera expresar mi agradecimiento a todos aquellos que han hecho posible el desarrollo de la tesis doctoral.

En primer lugar, el desarrollo de la tesis no habría sido posible sin el apoyo financiero recibido por parte de la Universidad de Alcalá y de la Comunidad de Madrid. Gracias a ello, he podido realizar a tiempo completo todas las actividades necesarias para finalizar este proyecto.

En segundo lugar, quiero agradecer a mis directores de tesis, José María Arranz Muñoz y Carlos García Serrano. Durante estos años, han dedicado gran parte de su tiempo a dirigir este proyecto; sin su compromiso, el desarrollo de la tesis no habría sido posible. Además, ha sido un gran privilegio para mí poder trabajar con ellos durante estos años, aprendiendo de sus conocimientos, consejos, experiencia y ayuda. El progreso académico que he experimentado durante estos años ha sido en gran medida gracias a ellos. Además de todo lo aprendido gracias a su tiempo, han supuesto para mí un gran apoyo. Han confiado siempre en mi trabajo y me han apoyado en todas las decisiones académicas que he ido tomando. Agradezco enormemente su apoyo personal en los momentos más duros de la tesis, especialmente durante la estancia. Me siento muy privilegiada por trabajar con un grupo tan empático y humano. Gracias por la paciencia, el tiempo y el apoyo; creo que no podría haber tenido una mejor dirección.

En tercer lugar, gracias al Departamento de Economía de la Universidad de Alcalá por brindarme la oportunidad de ir a congresos y financiar parte de mi formación. Además, quiero dar las gracias a todas las personas que forman parte de él, especialmente a Guillermo Cabanillas, quien me ha acompañado en mis primeros congresos y me ha aconsejado con sus conocimientos econométricos, y a Dmitry Petrov, por hacerme tan buenas preguntas que han hecho crecer en mí muchas ideas. A ambos les doy las gracias también por ser tan buenos amigos y por todas las risas compartidas. No me puedo olvidar de mi gran amigo y compañero de despacho Miguel Parra, con quien desde el máster compartíamos apuntes. Gracias por las sugerencias

y el tiempo dedicado a enseñarme Stata, pero, sobre todo, gracias por todo el apoyo y por escuchar mis preocupaciones e inquietudes durante este periodo.

En cuarto lugar, quiero agradecer a Cristina García-Herrera y a Miguel Gómez de Antonio por la oportunidad que me brindaron en mi etapa como becaria de formación en el Instituto de Estudios Fiscales, sin ellos, no me habría decidido a hacer la tesis doctoral. Gracias también a Adela Recio, con quien compartí mis primeras inquietudes sobre el mundo de la investigación. Por supuesto, a los demás becarios con los que compartí grandes momentos y que espero algún día se conviertan en coautores: Brais, Giovanni, Celia, Iván y Ricardo.

En quinto lugar, gracias a todas las personas que he conocido en congresos y que me han ayudado a mejorar mis investigaciones, especialmente a María González Cabrera, Carolina Calatayud, Marco Pérez, Ruben González y, por supuesto, a Marina Romaguera de la Cruz. Por último, deseo expresar mi gratitud a Daniel Fernández Romero por su generosidad al compartir su tiempo, conocimientos e ideas conmigo.

Por último, gracias a San Diego State University por acogerme durante mi estancia. Gracias a todos los compañeros del Departamento de Economía que me ayudaron a integrarme y aprender tanto de esta experiencia, especialmente a Eduardo Polo y a Elena Pardo. Por supuesto, gracias a Catalina Amuedo-Dorantes por darme la oportunidad de ir a San Diego y poder trabajar con ella, ha sido un auténtico placer. Finalmente, gracias a Esther, a quien conocí durante mi estancia y quien fue un gran apoyo.

Querría detenerme también a agradecer a todas las personas de fuera de la academia sin las cuales no habría evolucionado tanto. Gracias a mis amigas Andrea, Berta, Laura, Marta, Sara y José, por su apoyo incondicional. Gracias a mis amigos economistas Andrea Guadalajara, Sara y Guille, que me acompañaron en el descubrimiento de la Economía como ciencia. Gracias a todas las amigas que he hecho en Crossfit, especialmente a Isabel, por todas las risas y por ayudarme a superar mis límites.

Gracias a toda mi familia: A mis tías Mario y Luisi, por sostenerme cuando estaba a miles de kilómetros; a mis primos, que me inspiran todos los días; a mis tíos, que me observan y quieren con prudencia; a mis abuelas, que siempre me animaron a estudiar y me mostraron qué es la resiliencia. Una mención especial a mi tía Ana, que me animó a hacer la tesis y me mostró que era válida para ello; sin ella no estaría aquí. No puedo olvidarme de mis padres, María José y Alejandro, a quienes debo todo lo que soy. Gracias por vuestro amor incondicional, por enseñarme la importancia del esfuerzo y la perseverancia. Gracias por confiar siempre en mí y en apoyar todos y cada uno de mis pasos. Sois mi gran ejemplo a seguir. Gracias a mi hermana

Lucía, por cuidarme, escucharme y ser siempre un referente. Eres el mayor regalo que me ha dado la vida.

Por último, a Alex, gracias por estar a mi lado en cada paso de este camino. Gracias por tu paciencia, por tu amor y por creer en mí incluso cuando yo dudaba. Gracias por las risas, por los abrazos en los momentos difíciles y por compartir conmigo tanto lo cotidiano como lo extraordinario. Gracias también a toda tu familia, especialmente a Carmen, por acogerme con tanto cariño.

Abstract

This dissertation analyses how two central institutions of the labour market—the minimum wage and the unemployment benefit system—influence employment dynamics and unemployment outcomes in Spain, a country characterised by high structural unemployment and marked cyclical sensitivity. The main objective is to understand how reforms in these areas affect job retention, labour turnover and the duration of unemployment, taking into account both heterogeneity of workers and macroeconomic context.

The first two chapters focus on the national minimum wage. Chapter 2 evaluates the impact of the substantial 22.3% (21.6% in real terms) increase implemented in 2019 on job retention and employment permanence within the same firm. To this end, robust impact evaluation techniques are used—specifically, inverse probability weighted difference-in-differences—applied to individual administrative data from the *Muestra Continua de Vidas Laborales* (MCVL). The results show no negative effects on job retention. On the contrary, they reveal modest and short-lived positive effects—limited to the first months after the minimum wage increase—particularly in low-wage regions and among full-time workers as well as temporary workers.

Chapter 3 extends the analysis to gross employment flows—hiring and separations—at the provincial level between 2015 and 2023. To this end, the MCVL administrative records are also used, but aggregated at the provincial level. In provinces with greater exposure to the minimum wage (i.e., with a higher percentage of workers earning the minimum wage), both hires and separations decrease, thus attenuating labour turnover. However, the effect on net employment is close to zero. The results differ according to working hours: turnover increases among part-time workers and decreases among full-time workers.

Chapter 4 evaluates two legislative changes in the unemployment benefit system and their effects on the duration of unemployment. The first reform, implemented in 2012 during the Great Recession, consisted of a reduction in the replacement rate. The second, in 2023, resulted in an increase in that rate, taking advantage of the favourable economic context. This analysis

is particularly relevant due to the use of administrative records showing the entire universe of unemployed individuals (from the *Servicio Público de Empleo Estatal*, SEPE) and the use of quasi-experimental techniques combined with duration models. The findings show that the reduction in the replacement rate in 2012 had a negative and significant effect on the duration of unemployment. However, the increase in the replacement rate in 2023 led to a moderate increase in duration. The effects are heterogeneous in both reforms: workers with higher benefits and shorter potential duration react more intensely, while for certain population subgroups the effects are negligible.

Overall, this dissertation provides robust causal evidence on recent and highly relevant reforms of the Spanish labour market and offers an integrated assessment of how institutions operate across the hiring, retention, and separation margins. The results underscore the importance of designing labour policies that are sensitive to the economic cycle and the heterogeneity of workers, providing relevant implications for both academic research and policy design in economies with high unemployment.

Resumen

Esta tesis doctoral analiza cómo dos instituciones centrales del mercado de trabajo —el salario mínimo y el sistema de prestaciones por desempleo— influyen en la dinámica del empleo y en los resultados del desempleo en España, un país caracterizado por un elevado desempleo estructural y una marcada sensibilidad cíclica. El objetivo principal es comprender cómo las reformas en estas áreas afectan a la retención del empleo, la rotación laboral y la duración del desempleo, teniendo en cuenta tanto la heterogeneidad de los trabajadores como el contexto macroeconómico.

Los dos primeros capítulos se centran en el salario mínimo interprofesional. El Capítulo 2 evalúa el impacto del gran aumento del 22,3% (21,6% en términos reales) implementado en 2019 sobre la retención del empleo y la permanencia en la misma empresa. Para ello se utilizan técnicas de evaluación de impacto robustas, diferencias en diferencias ponderadas por probabilidad inversa, aplicadas sobre datos administrativos individuales procedentes de la Muestra Continua de Vidas Laborales (MCVL). Los resultados no muestran efectos negativos sobre la permanencia en el empleo e incluso existen efectos positivos modestos y de corta duración (solo en los primeros meses tras la subida del salario mínimo) en regiones de bajos salarios y entre trabajadores a tiempo completo y con contratos temporales.

El Capítulo 3 amplía el análisis a los flujos brutos de empleo —contrataciones y separaciones— a nivel provincial entre 2015 y 2023. En las provincias con mayor exposición al salario mínimo (es decir, con mayor porcentaje de trabajadores cobrando el SMI), tanto las contrataciones como las separaciones disminuyen, atenuando así la rotación laboral. Sin embargo, el efecto sobre el empleo neto es cercano a cero. Los resultados difieren según la jornada laboral: la rotación aumenta entre los trabajadores a tiempo parcial y disminuye entre los trabajadores a tiempo completo.

El Capítulo 4 evalúa dos cambios legislativos en el sistema de prestaciones por desempleo y sus efectos sobre la duración del paro. La primera reforma, implementada en 2012 durante

la Gran Recesión, consistió en una reducción de la tasa de sustitución. La segunda, en 2023, se tradujo en un incremento de la tasa de sustitución, aprovechando el contexto económico favorable. Este análisis tiene especial relevancia por el uso de registros administrativos que muestran el universo de desempleados (procedentes del Servicio Público de Empleo Estatal, SEPE) y por la utilización de técnicas cuasi-experimentales combinadas con modelos de duración. Las conclusiones del estudio demuestran que la reducción de la tasa de sustitución en 2012 tuvo un efecto negativo y estadísticamente significativo sobre la duración del desempleo. Sin embargo, el aumento de la cuantía en 2023 generó un incremento de la duración moderado. Los efectos son heterogéneos en ambas reformas: los trabajadores con prestaciones más elevadas y con menor duración potencial reaccionan con mayor intensidad, mientras que para determinados subgrupos poblacionales los efectos son no significativos.

En conjunto, esta tesis proporciona evidencia causal robusta sobre reformas recientes y de gran relevancia del mercado laboral español y ofrece una evaluación integrada de cómo las instituciones influyen en los márgenes de entrada, permanencia y salida del empleo. Los resultados subrayan la importancia de diseñar políticas laborales sensibles al ciclo económico y a la heterogeneidad de los trabajadores, aportando implicaciones relevantes tanto para la investigación académica como para el diseño de políticas en economías con elevado desempleo.

Keywords

Minimum wage, Employment, Impact evaluation, Difference-in-differences, Gross employment flows, Unemployment benefits, Unemployment duration, Job search, Policy reform.

Palabras clave

Salario mínimo, Empleo, Evaluación de impacto, Diferencias en diferencias, Flujos brutos de empleo, Prestaciones por desempleo, Duración del desempleo, Búsqueda de empleo, Reforma de políticas.

Contents

List of Figures

List of Tables

Chapter 1

Introduction

In the past three decades, the empirical assessment of labour market institutions has been characterized by a sustained expansion of quasi-experimental methodologies. Research designs such as differences-in-differences, regression discontinuity, propensity score matching —to name but a few— went from playing a minor role to becoming the standard for causal identification in applied economics. This methodological shift, often referred to as the *credibility revolution* (Angrist and Pischke, 2010), has fundamentally reconfigured research questions, empirical validity standards, and publication criteria within the field.

This revolution has transformed three fundamental dimensions of research. First, there has been a shift towards the effects of treatment in specific contexts. Quasi-experimental designs sacrifice external validity in favour of internal validity: the estimated effects are quite credible at the local level, but their generalization requires the collection of evidence through replication in multiple contexts. Second, emphasis is placed on the heterogeneity of effects. Averages can hide substantial differences between subgroups, the identification of which is essential for understanding causal mechanisms and designing effective policies. Third, attention has been paid to multiple dimensions of adjustment. A full assessment of labour reform requires examining not only the stock of employment, but also gross hires and layoffs, job quality, income distribution, and overall well-being.

In labour economics, this revolution has had particularly deep consequences for two key institutions: the minimum wage and unemployment benefits. For decades, the debate surrounding the minimum wage was dominated by the competitive model, which predicts necessary reductions in employment in response to increases in the legal minimum. However, since the seminal study by Card and Krueger (1994) on New Jersey, rigorous empirical evidence has consistently challenged this prediction, leading to alternative theoretical frameworks that account for frictions, monopsony competition, and heterogeneity (Manning, 2003; Dube et al., 2010; Cengiz et al., 2019). These models suggest null or even a positive effect on aggregate employment, depending on the market conditions and the degree of frictions.

At the same time, the assessment of unemployment benefits has evolved from documentation of disincentive effects (Meyer, 1990; Katz and Meyer, 1990) to more detailed analyses that recognise that the magnitude of these effects varies significantly depending on the economic cycle, the availability of job vacancies, and the design of the system (Chetty, 2008; Schmieder and von Wachter, 2016). During recessions, when job scarcity is the limiting factor, disincentive effects tend to diminish, while the benefits of consumption smoothing and better job-matching become more pronounced.

The main lesson to be drawn from this literature is clear: the effects of labour institutions are fundamentally empirical questions, whose answers depend on the institutional context, scope of intervention, economic cycle phase, and nature of the agents affected.

1.1 The Spanish Case: Large-scale reforms in a singular institutional context

This doctoral thesis follows in the tradition of the credibility revolution through a rigorous empirical evaluation of recent reforms to the minimum wage and the unemployment benefit system in Spain. The Spanish labour market has experienced institutional transformations of exceptional magnitude over the past decade: the minimum wage increased by more than 30% in real terms starting in 2019, while unemployment benefits were substantially reformed in 2012 and 2023. These interventions are unparalleled in recent European history.

The uniqueness of the Spanish case lies in three elements. First, the marked macroeconomic contrast between reforms. The reduction of benefits in 2012 occurred when unemployment exceeded 25%, whereas the increase in the minimum wage in 2019 and the reform of benefits in 2023 took place during periods of economic expansion. This contrast enables us to examine how the economic cycle moderates the effects of labour market reforms—a central question in the recent literature that has rarely been addressed using within-country comparisons.

Second, three distinctive characteristics make Spain a particularly informative natural laboratory. High contractual dualism—with historical temporary employment rates exceeding 25%, well above the European average—implies that temporary and permanent workers face differentiated exposures to reforms and generates additional margins of adjustment for firms. Regional heterogeneity in wage structure and sectoral composition produces geographical variation in effective exposure to a uniform national minimum wage: in low-wage regions, a higher proportion of workers are directly affected. Finally, the high unemployment rate —systematically ranked among the highest in the European Union— and the significant cyclical volatility of job creation have fundamental implications for unemployment benefits: in contexts of scarce job vacancies, disincentive effects are mitigated by demand constraints; in times of expansion, these effects are intensified.

Third, the availability of administrative microdata with virtually universal coverage. The Continuous Sample of Working Lives (*Muestra Continua de Vidas Laborales*, MCVL) provides longitudinal individual information on employment records—wages, contract type, sector, firm size, sociodemographic characteristics—for 4% of the population affiliated with Social Security. The records of the State Public Employment Service (*Servicio Público de Empleo Estatal*, SEPE) contain detailed information on the universe of registered unemployment spells. Such a rich source of information enables the implementation of empirical strategies that combine difference-in-differences, duration models and weighting techniques to address issues of selection and heterogeneity.

A detailed analysis of the Spanish case is of twofold relevance: it serves as a basis for the design of national policies and contributes to a general understanding of how labour markets with high dualism and strong cyclical sensitivity respond to large-scale interventions.

1.2 Multidimensional evaluation

The minimum wage and unemployment benefits operate at different margins of the labour market, but their effects cannot be analysed in isolation. Both institutions influence the distribution of acceptable wages, incentives for job search, and employment and dismissal decisions. Consequently, their impact depends not only on their individual design, but also on the macroeconomic context and the institutional framework in which they operate.

The contribution of this doctoral thesis lies in its integrated analysis of how large-scale reforms to the minimum wage and unemployment benefits simultaneously alter gross hiring and separation flows, job retention and the duration of unemployment, as well as how these effects are modulated by the economic cycle and individual and regional characteristics. This approach enables a distinction of the short-term effects on labour transitions and the most persistent changes in labour market dynamics.

By combining temporal variation (recession versus expansion), territorial variation, and individual heterogeneity, the dissertation identifies not only the average effect of each reform but also the mechanisms through which labour institutions interact with a market characterized by high temporality and strong cyclical sensitivity. Thus, the analysis transcends the estimation of specific impacts and contributes to a more general understanding of the functioning of labour institutions in economies with high structural unemployment.

1.3 Objectives and structure

This dissertation comprises three research articles that share a common analytical thread: the causal evaluation of labour market institutions in Spain. One has been accepted for publication in a peer-reviewed journal; the remaining two are currently under review. The structure is organized around the following objectives:

Objective 1. Evaluate the impact of the increase in the minimum wage in January 2019 on employment retention and on the likelihood of remaining with the same employer among workers directly affected by the increase. This analysis focuses on determining whether the 22.3% increase generated involuntary (dismissals) or voluntary (resignations) separations among workers earning below the new minimum wage after its implementation and whether these effects vary according to employment characteristics (contract type and working hours) and geographic characteristics (regions with different average wage levels).

Objective 2. Analyse the effect of the minimum wage on gross employment flows—hiring and separations—and on net employment at the provincial level during the period 2015-2023, with particular attention to differences between full-time and part-time workers and between permanent and temporary contracts.

Objective 3. Evaluate the impact of the 2012 reform that reduced the replacement rate of unemployment benefits from 60% to 50% after six months of receipt, on the duration of unemployment episodes. This analysis is conducted in the context of the deep economic recession of 2008-2013, allowing examination of whether benefit generosity cuts during periods of weak labour demand significantly affect unemployment duration or whether, conversely, their effects are limited due to the scarcity of employment opportunities.

Objective 4. Evaluate the impact of the 2023 reform that increased the replacement rate from 50% to 60% after six months of receipt, on the duration of unemployment episodes. This analysis is conducted in the context of the economic expansion and strong employment growth of 2021-2023, allowing contrast of whether benefit generosity increases during periods of strong labour demand prolong unemployment duration more pronouncedly than during recessions.

Objective 5. Examine the heterogeneity of unemployment benefit reform effects according to beneficiary characteristics (benefit amount, potential benefit duration, age, number of children, sector and occupation of the last job episode) and according to the business cycle. This analysis allows identification of which groups of unemployed are most sensitive to changes in benefit generosity and under what macroeconomic conditions these effects are most relevant.

Objective 6. Integrate the findings from the analyses on minimum wage and unemployment benefits into a coherent conceptual framework that allows extraction of general lessons on the role of labour institutions in Spanish labour market performance and formulate evidence-based economic policy recommendations.

The structure of the dissertation is organized around these objectives. Following this introduction, Chapter 2 addresses the first objective, analysing the impact of the 2019 minimum wage increase on employment retention through difference-in-differences methodology with inverse probability weighting applied to individual data from the Continuous Sample of Working Lives. Chapter 3 focuses on the second objective, analysing minimum wage effects on gross employment flows through panel data techniques applied to aggregated provincial data. Chapter 4 addresses the third, fourth, and fifth objectives, analysing the effects of the 2012 and 2023 unemployment benefit reforms through causal survival models applied to SEPE administrative data. Finally, Chapter 5 integrates the findings from the empirical chapters, discusses their theoretical and policy implications, acknowledges the limitations of the analyses conducted, and proposes directions for future research.

1.4 Contributions

This dissertation makes significant contributions in three dimensions: theoretical, empirical, and economic policy.

From a theoretical perspective, the research contributes to the literature on labour institutions by providing empirical evidence that allows discrimination between predictions of alternative theoretical models. The findings of null or modestly positive effects of the 2019 minimum wage increase on employment retention (see Chapter 2 and Chapter 3) are inconsistent with the standard competitive model that predicts significant negative effects, but consistent with models incorporating monopsony power, adjustment costs, or efficiency wage effects (see Pissarides, 2000; Mortensen and Pissarides, 1994). Similarly, the findings that unemployment benefit effects on unemployment duration vary substantially according to the business cycle—being pronounced during the 2012 recession and weak during the 2023 expansion—provide empirical support for theoretical models that emphasize the cyclical dependence of labour institution effects (see Chapter 4).

From an empirical perspective, the research makes several methodological contributions. First, it refines the identification strategy for evaluating minimum wage effects on employment retention, defining treatment and control groups more precisely than previous studies and applying inverse probability weighting combined with difference-in-differences to improve group comparability (see Chapter 2). Second, it extends minimum wage effect analysis beyond aggregate employment to examine gross flows of hiring and separations, providing evidence on the specific mechanisms through which the minimum wage affects the labour market (see Chapter 3). Third, it applies causal survival models to evaluate unemployment benefit reform effects, a methodology that allows controlling for selection on observables more flexibly than traditional parametric approaches. Fourth, it exploits the existence of two opposite benefit reforms implemented under contrasting macroeconomic conditions to examine the cyclical dependence of effects, a research design uncommon in the literature (see Chapter 4).

From an economic policy perspective, the research provides relevant evidence for labour institution design in Spain and in other countries with similar institutional characteristics. The findings suggest that the Spanish labour market possesses greater capacity to absorb minimum wage increases than anticipated by some analysts, implying that moderate minimum wage increases can be a viable tool for improving low-wage worker incomes without generating significant adverse employment effects (see Chapter 2). However, the small but statistically significant effects on hiring and separation flows suggest that very large or repeated minimum wage increases could eventually generate cumulative employment effects that merit careful monitoring (see Chapter 3).

Regarding unemployment benefits, the findings reinforce the argument for countercyclical systems that adjust benefit generosity according to macroeconomic conditions. During deep recessions, when employment opportunities are scarce, benefit cuts have negative effects on unemployment duration, but impose significant costs on unemployed welfare. Conversely, during economic expansions, when vacancies are abundant, benefit increases can modestly prolong unemployment duration, suggesting that generosity should be carefully calibrated to balance social protection and job search incentives (see Chapter 4).

Additionally, the identification of significant heterogeneity in effects according to beneficiary characteristics suggests that differentiated benefit systems—that adjust generosity according to benefit amount, potential duration, or demographic characteristics—could improve system efficiency, concentrating protection on those groups that need it most and that are least sensitive to disincentive effects.

Chapter 2

Evaluating the impact of the large 2019 increase in the Spanish minimum wage on job retention and employment[1]

Abstract

This study investigates the impact of a substantial 22.3% increase in Spain's national minimum wage, implemented on January 1, 2019, on job retention and employment. Using individual administrative data and applying a difference-in-differences methodology with inverse probability weighting, the analysis finds no adverse effects of the minimum wage increase on job retention within the same firm or on overall employment permanence. Notably, the findings indicate modest positive effects on employment retention in low-wage regions and among full-time and temporary employees, though these effects are very short-lived. The findings align with theoretical frameworks suggesting that minimum wage increases may not necessarily lead to job losses, and they resonate with broader literature indicating neutral employment effects, especially for lower-income groups.

Keywords: minimum wage; job retention; employment; regions; impact evaluation; double differences.

JEL: C21, C54, J23, J38.

[1]The resulting paper is co-authored by José María Arranz and Carlos García Serrano. This manuscript has been accepted for publication in Humanities and Social Sciences Communications. DOI: https://doi.org/10.1057/s41599-026-07652-8

2.1 Introduction

Increases in the minimum wage (MW) can have impacts on net employment because they affect separations, hirings, or both. This is because the MW affects wages and can therefore influence the supply of and demand for labour, potentially altering the level and composition of employment. A large amount of economic (especially empirical) literature focuses on the effect that an increase in the MW can have on the aggregate employment rate. Research that examines the effects on the gross flows of workers or jobs is much smaller (Jiménez and Jiménez, 2021). However, knowing whether MW hikes affect the hires and/or separations of workers (the creation and/or destruction of jobs) is relevant for both theoretical and empirical reasons (see below).

Only a limited number of studies focus on gross employment flows. The present study follows this strain of the MW literature and contributes to fill the gap by examining the impact on job retention of the huge increase in the national MW established by the Spanish government on 1 January 2019, which raised it by 22.3% in nominal terms (21.6% in real terms). [2] This increase was part of the commitment adopted by the government to place the MW at a level equivalent to 60% of the average wage of the economy in 2023 to meet the objective recommended by the European Social Charter, since Spain was far from achieving that figure, around 45% (Eurostat, 2022)

Firstly, it focuses on a very large rise in the MW. This increase is unparalleled not only when considering its evolution in the last three decades in Spain but also when comparing with other MW increases in advanced economies. In fact, the 2019 increase is one that has rarely been seen in any other European Union Member State over the last decades, at least until the Covid-19 pandemic. [3] This provides an ideal setting for analysing the labour market effects of the MW. And the possibility that similar large hikes may happen in the future makes the present study interesting.

Secondly, the article uses high-quality individual-level administrative data from the Contin-

[2]The national minimum wage is called *Salario Mínimo Interprofesional* in Spanish and is set by the government on 1 January each year after consulting with the social partners (trade unions and employers' associations).

[3]See 'Monthly minimum wages - bi-annual data' of Eurostat. The trend in OECD countries in the last three decades has been to raise MW relative to the median wage. Over the period 1990–2020, the number of OECD countries with a minimum-to-median wage ratio (Kaitz index) exceeding 50% doubled (from nine in the 1990s to 18 in the early 2020s), particularly in the period after the 2008 financial crisis. This trend has been more pronounced in some Eastern European countries, which, having low MW, have increased them substantially above inflation in the late 2010s and early 2020s. For instance, in 2019, Bulgaria, Croatia, Czechia, Estonia, Romania, Slovakia, Hungary and Poland increased their MW in the range of 7–10% (Eurofound, 2019).

uous Sample of Working Lives (*Muestra Continua de Vidas Laborales*, MCVL, in Spanish), a data source that allows for the longitudinal tracking of individuals over time.

Thirdly, it uses the difference-in-differences (DID) methodology with inverse probability weighting (IPW) for repeated cross-sections (Blundell et al., 2004; Blundell and Costa Dias, 2009; Hernán and Robins, 2020; Sant'Anna and Zhao, 2020) to decipher the impact of the MW increase on job retention. Our analysis improves the methodology adopted previously by various authors (Stewart, 2004b,a; Portugal and Cardoso, 2006; Bryan et al., 2013; Dustmann et al., 2022), including those who focus on the Spanish 2019-MW hike (Barceló et al., 2021; Hijzen et al., 2025; Gorjón et al., 2024). [4] These studies use the standard DID technique without matching (exception for Gorjón et al. (2024), who use propensity score matching, PSM) with individual data defining the treatment and control groups depending on their position in the wage distribution at time t at the beginning of the employment transition (just before the MW rise takes place). Nevertheless, unlike most previous studies, which define the total number of employees who receive less than the MW of the following year as treated, we define a treatment group that earns just below the new MW (and thus clearly is affected by the rise), a control group that earns just above the new MW (and thus the rise has no impact on it), and comparable groups before the policy was implemented. Using close treatment and control groups in the wage distribution implies that the two are similar in terms of unobserved characteristics, allowing for the appropriate application of the DID technique and identification of the causal effect of raising the MW on employment/job retention. Furthermore, we implement the DID methodology by integrating IPW not only to ensure the initial equivalence of the control and treatment groups but also to sustain this parity across both pre-intervention and post-intervention periods. [5]

Other techniques could be used to evaluate the impact of minimum wage hikes, but they are not appropriate in our context. For example, if the treatment were truly staggered over time

[4] A recent study by Arnadillo et al. (2024) employs a synthetic control method with a pool of European countries to construct a counterfactual path for Spanish employment and focuses on macro-level employment outcomes. This makes it less suitable for examining the effects of the MW hike on worker retention in the same company or different labour market segments (such as those defined by contract type or working hours). Unlike this one, our study examines the effects of the MW increase on job retention probabilities, with particular emphasis on the implications of the dual labour market.

[5] Note that Gorjón et al. (2024) warn about the potential bias of their results due to their inability to control unobservable differences between the treatment and the control group. To overcome this, we use DID with IPW for repeated cross-sections. Additionally, by comparing with a period prior to the reform using this methodology, we control for potential seasonal effects that could influence contract termination—effects that may not be adequately captured by other studies employing PSM like Gorjón et al. (2024) —since we compare the probability of job loss in each specific month with that of the same month in the previous period.

among groups of workers (e.g., regions implemented changes at different times, or different subgroups were affected in different years), as it happens in the US context but not in most of the European countries, then it would be appropriate to use estimators designed for staggered DID (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Goodman-Bacon, 2021) . Dube and Lindner (2024) discuss these methodological advances in the context of the examination of local variations in the level of minimum wages, the research in the US and the criticisms to the Two-Way Fixed Effect (TWFE)-log(MW) model. However, as the reform was implemented simultaneously throughout the country (in January 2019 as the national cut-off date), the problem of TWFE estimators biased by treatment timing should not arise, and a two-period DID with IPW is sufficient (Dube and Lindner, 2024), as we show in the methodology section.

Beyond these favourable analytical conditions, the article makes a contribution to the literature related to the results we obtain. Overall, our analysis provides evidence of no adverse effects of the MW increase on overall employment permanence (contrary to the negative effects found by previous Spanish studies— Barceló et al. 2021; Hijzen et al. 2025; Gorjón et al. 2024) or on job retention within the same firm. It also uncovers modest positive effects on employment permanence in low-wage regions and among full-time and temporary employees, though these effects are very short-lived. Importantly, the granularity of our data allows us to distinguish these heterogeneous effects across different contract types and regional contexts, providing a more nuanced understanding of the impact of the MW hike.

We disaggregate results by contract type, which is particularly relevant when studying job retention. Workers affected by the MW increase exhibit a higher proportion of temporary contracts, which, by their very nature, have predefined end dates. In this context, an analysis that does not distinguish contract types — as is the case in Gorjón et al. (2024)— risks mistakenly attributing job separations to the MW hike when they may simply reflect the natural expiration of contracts. For this reason, our analysis distinguishes between two outcome variables: (1) the probability of remaining employed in any job, and (2) the probability of remaining employed with the same firm. The latter has rarely been used in empirical studies. However, this distinction is essential to evaluate whether employment termination, especially among temporary workers, is due to a decline in labour demand following the MW increase, or merely the natural expiration of contracts or dismissals for reasons unrelated to the MW.

Economic models generate distinct predictions for retention at the firm-level (remaining in the same job) versus overall employment continuity (remaining employed, possibly in a different job). In monopsonistic settings, a binding wage floor can increase worker retention within firms—firms facing an upward-sloping labour supply may respond to higher mandated wages by reducing hiring but retaining current employees, thereby lowering voluntary quits and hiring churn (Manning, 2003). By contrast, search-and-matching frameworks emphasize effects on job-finding and separation rates: an increase in the wage floor can reduce exits from employment, through higher reservation wages and improved match quality, or increase exits, if low-productivity jobs are destroyed and reallocation frictions impede rapid reemployment (Pissarides, 2000). As the Spanish labour market is normally considered a segmented market—mainly in terms of contract types— these mechanisms would plausibly lead to heterogeneous responses for job retention and employment retention.

Furthermore, a regional approach is undertaken with the aim of assessing whether the responsibilities of the MW should move towards a regional model, given its potential varying impact across regions based on the bite and fears that an increase in the national MW would have a particularly negative impact on areas with lower average wages. In this context, the heterogeneity analysis provides pertinent new evidence to inform potential economic policy decisions regarding the MW.

Our findings align with theoretical frameworks suggesting that MW increases may not necessarily lead to job losses and resonate with a growing body of literature pointing to neutral employment effects, especially for lower-income groups. The evidence of heterogeneity we provide offers valuable insights to inform economic policy decisions regarding the MW

The outline of the article is as follows. Section 2.2 reviews the literature on the effects of an MW hike on employment. Section 2.3 presents briefly the institutional setting, and the database and sample used for the empirical analysis. Section 2.4 sets out the DID impact assessment technique used in the research. Section 2.5 provides the results of the estimates. Subsequently, Section 2.6 concludes.

2.2 Literature review

The analysis of the effects of MW hikes on gross employment flows is relevant for two main reasons. First, it provides theoretical models with detailed information about the mechanisms driving employment effects—or the absence thereof—allowing a better understanding of how the labour market adjusts over time. MW increases may lead firms to reduce vacancy creation or maintain hiring levels, while simultaneously increasing job destruction or worker retention (Dube et al., 2016). Second, examining gross flows helps interpret the diverse empirical findings regarding net employment effects of MW hikes, which range from negative (Neumark and Shirley, 2022) to insignificant or even positive results, particularly in studies of the 'new minimum wage economy' (Card and Krueger, 1995; Brown, 1999; Dube, 2019a).

From a theoretical standpoint, MW hikes influence job creation and destruction as well as employment-to-unemployment transitions, producing net employment changes that may be negative, null, or positive depending largely on the prevailing labour market model. The neoclassical ('marginalist') framework predicts a clear-cut reduction in net employment following a binding MW increase, due to negative labour demand effects (Stigler, 1946). However, alternative perspectives challenge this view. The institutional–behavioural model, for instance, replaces the demand curve with a band and posits that employment is constrained by sales volume rather than the equality of wage and marginal revenue product (Lester, 1947; Kaufman, 2010). It also highlights that even sizeable MW hikes can be relatively modest compared to other routine cost increases, resulting in lower MW elasticities and adjustments spread across multiple channels, including product price increases (Hirsch et al., 2015). This model predicts smaller gross job flows but minimal net employment effects.

Monopsony and imperfect labour market models further enrich our understanding of MW impacts on hiring and separations. In dynamic monopsony settings, employers´ wage-setting power implies that MW hikes raise the attractiveness of lower-paying jobs, thereby reducing job-to-job separations and turnover (Manning, 2003, 2021; Munguía, 2020). Lower turnover can enhance productivity by reducing recruitment and training costs and fostering worker experience accumulation. Search and matching models, which incorporate frictions like search costs and limited labour mobility, generate wage-setting power via imperfect substitution across workplaces, analogous to price-setting in goods markets (Card et al., 2018). Thus, MW increases reduce transitions to non-employment when match quality is assessed after probation periods and vacancy posting costs vary heterogeneously (Brochu and Green, 2013). Job ladder models also suggest that higher MWs decrease job-to-job flows by reducing the arrival rate of better-paying offers (Dube et al., 2016).

Empirically, most MW impact studies focus on employment rates among low-wage workers, showing heterogeneous outcomes including small negative, null, or modestly positive effects. There are broad reviews (Belman and Wolfson, 2014; Neumark, 2019; Wolfson and Belman, 2019; Dube, 2019a) and also meta-analysis studies (Leonard et al., 2014; Jiménez and Jiménez, 2021). However, fewer studies analyse MW impacts on gross employment flows, generally indicating reduced or stable hires and separations following MW hikes. Regional or provincial-level analyses exploiting MW variation across geographic areas find decreased separation rates among less qualified workers in Canada (Brochu and Green, 2013) and the US (Dube et al., 2016), with persistence over several quarters post-hike. Sector- and firm-level studies often report insignificant employment effects and negative or null impacts on separations (Dube et al., 2007; Giuliano, 2013; Hirsch et al., 2015).

More granular analyses using individual-level data and DID techniques allow better identification of MW-affected workers and focus on employment transitions. UK studies on the 1999 MW introduction and subsequent increases generally find no adverse effects on employment retention (Stewart, 2004b,a; Bryan et al., 2013), except Dickens et al. (2015), who report negative effects for part-time women workers. Similar patterns emerge in Portugal (Portugal and Cardoso, 2006), the Netherlands (Bezooijen et al., 2024), and Germany (Dustmann et al., 2022), with negligible or positive employment and hours-worked effects among low-wage and low-skilled groups. For the latter country, Bossler et al. (2024) analyse the 22% increase in nominal terms in the German MW implemented in 2022 and neither identify an effect on the workers' job retention nor on the region-level employment of total jobs, regular jobs, or 'minijobs', although they find a negative effect on working hours of treated employees, mainly driven by 'minijobbers'.

Studies focusing on teens or young workers show negative effects on employment retention in some cases (Zavodny, 2000; Yuen, 2003; Campolieti et al., 2014) and null effects in others (Clemens and Strain, 2018). Recent advances incorporate heterogeneous treatment effects, labour market structure, and incomplete coverage. For example, Azar et al. (2024) find positive employment effects in highly concentrated US retail and fast-food labour markets, consistent with oligopsonistic competition models. Similarly, Caliendo et al. (2024) estimate small negative long-term employment effects in Germany, driven by reductions in marginal jobs, particularly in regions most affected by the policy. Blömer et al. (2024) find irregular, heterogeneous effects across regions, skills, and worker types, while Samutpradit (2024) reports differential impacts across covered and uncovered sectors in Thailand's dual economy.

In the case of Spain, recent studies focus on the 2019 MW hike, although the available evidence on gross employment flows is scarce. Barceló et al. (2021), following a DID methodology, find an increase in the individual probability of losing employment (12 months after the rise) of 2–3 percentage points (pp) among directly affected workers compared with their counterparts, implying elasticities of 0.10–0.15. [6] Using a similar setting, Hijzen et al. (2025) find a decline (0.6 pp) in the likelihood of remaining employed in the next year, which implies a very

[6]With aggregate data, Barceló et al. (2021) estimate a reduction in the net employment of 0.6–1.1 pp, which could imply lower job creation of more than 100,000 jobs. AiRef (2018) simulates the effects of the rise in the MW in 2019 using data from the MCVL-2017 and estimates a loss of total employment of only 0.15% in 2019 (24,000 fewer jobs). Hijzen et al. (2025), using their estimation on the probability of remaining employed and without taking account of hires, calculate that the decline in employment was about 7,400 jobs.

small own-wage labour demand elasticity of -0.1. Finally, Gorjón et al. (2024) employ a PSM technique and find no statistically significant effect on transitions from employment to unemployment until 4 months after the 2019-MW hike and a significantly positive, increasing impact afterwards, amounting to nearly 2 pp 9–11 months after the rise. Our study uses MCVL data like the previous ones but differs from them in that our econometric strategy overcomes some of their limitations, in addition to other differences previously mentioned in the introduction. [7] Moreover, Arnadillo et al. (2024) employs a synthetic control method and finds no effect of the MW increase on overall employment levels.

2.3 Institutional setting and data

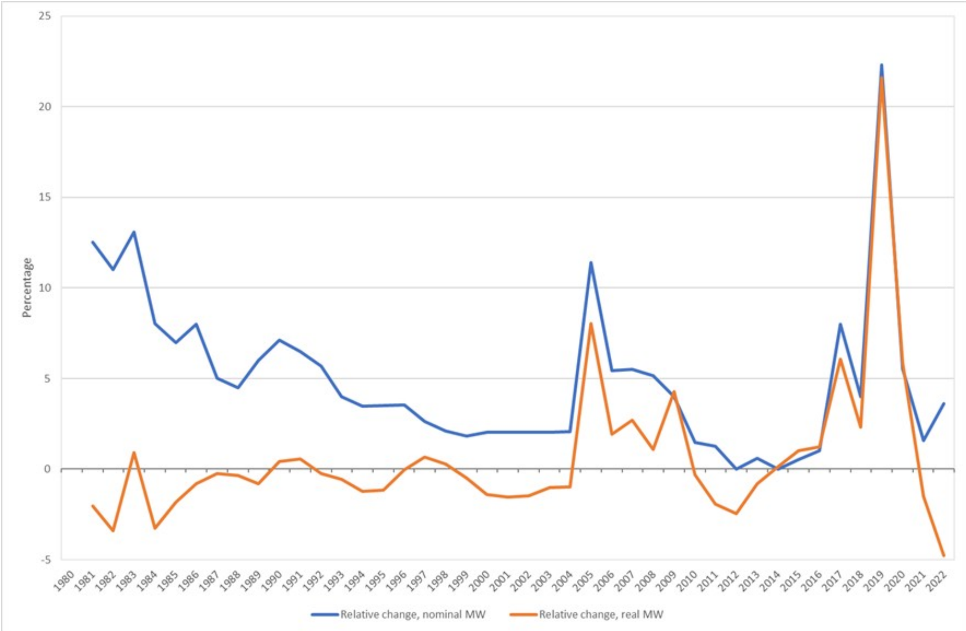
2.3.1 Institutional setting

In Spain, the government sets the MW annually (in December) by Royal Decree after consulting with major trade unions and employer organizations. Its amount is determined by considering factors such as economic conditions and inflation, aiming to ensure a basic standard of living for workers. Although the MW is established on a yearly basis, it is distributed in 14 payments: 12 regular monthly wages and two additional extraordinary payments. Figure 2.1 displays the evolution of the Spanish national MW in 1980-2022. It has experienced two large increases in real terms in the last 20 years: in 2005–2009 and in 2019–2022. They coincide with left governments coming to power with a clear commitment to increase the purchasing power of low-wage workers. Between these two periods, the MW remained practically unchanged. The huge increase of the MW in 2019 followed a long period during which the Kaitz index had been stable at around 40%.

Moreover, the MW is nationwide, meaning there are no differences based on types of worker —it is a single, uniform rate applicable to everyone across the country regardless of their age, occupation, sector, or region—. However, despite this uniformity, other wage floors exist. This is due to the collective bargaining system, which, as in other European countries, has several levels and is characterized by its high coverage (around 80%). Agreements can be at the sector, company or establishment-level, which are often negotiated at the regional/provincial level. Sector-province and sector-national agreements are the most common. They may specify a set

[7] The main difference between Barceló et al. (2021) and Hijzen et al. (2025) and ours is that we use (1) a control group closer to the treated group (for instance, the latter authors use as controls workers with high wages –between the median and 3,000 euros), and (2) IPW in order to balance the treatment and control groups before and after the MW hike by matching on covariates, thus taking account of the differences that exist among workers across the wage distribution.

Figure 2.1: Evolution of the national minimum wage in Spain (1980-2022)



Notes: Own elaboration based on data from the Ministry of Labour and Social Economy (minimum wage) and the National Institute of Statistics (consumer price index).

of wage floors (at least equal to the national minimum) for different occupations. As a result, while all workers are subject to the same national MW, effective wages of workers in low-wage occupational groups may vary depending on the industry and the region where these agreements take place.

Regarding other labour market institutions, employment protection legislation (EPL) is relatively strict in Spain. Its value of the OECD index of the stringency of individual dismissals of workers holding permanent contracts is above average. The same happens with the stringency of regulations on the use of temporary contracts. Despite this, the Spanish economy exhibits a high share of temporary employment (above 30% during the 1990s and 2000s, it was 25% compared to an average of 12% for the OECD in 2021, just before the labour market reform that helped reduce it below 20%). At the same time, the share of part-time employment (around 14%) is relatively low for European standards, with the particularity that half of part-time workers are so involuntarily.

2.3.2 The database

The database used to assess the impact of the increase in the MW on employment retention is the MCVL, an administrative dataset built upon the computerized records of the Spanish social security. The MCVL is very rich, and editions have been delivered every year since 2004. The reference population corresponds to workers who are registered with social security and recipients of pensions and unemployment benefits. [8] The information for each edition of the MCVL refers to 4% (based on simple random sampling) of the reference population for each year whose personal identification code contains the randomly selected figures in a given position. These figures are identical every year. This method guarantees that the same people are selected, as long as they continue to have a relationship with social security, and that the entries are representative of the separations in the population. [9]

Regarding the reference time frame for analysing individual situations, given that the MCVL is conducted based on a reference year, the selected population and their various records are only representative for that specific year. This means that a given edition of the MCVL, while capturing the affiliations of individuals in the past, does not include the affiliations of people who, for example, passed away or left the workforce without receiving a pension or unemployment benefits and were no longer recorded because they had no further relationships with the social security system. Therefore, the MCVL sample for each year (e.g., 2019) is representative of the population for that year (and not for previous years). This implies that to analyse a specific period (e.g., 2017-2019) it is necessary to link and organize the data from all the years in which the MCVL was conducted in order to capture not only individuals having any relationship with social security in 2019 (and all their past affiliation data) but also those who had a relationship in previous years (2017 or 2018) but no relation in 2019 (and again the corresponding affiliation information from prior years). In this study, all the files (including those corresponding to affiliations and the different contribution bases from all years) are used to construct a database of working lives for the period 2004-2019, following the procedure designed by Arranz et al. (2013), although only data from the 2017-2019 period are used.

The MCVL files can be linked by the anonymized identifier of the person belonging to the sample, which is the same for that person throughout the different files and the different years. This makes it possible to track individuals over time and know their employment status at any given time. In addition, as the database has information on the number of the compa-

[8]The database does not include the inactive, as well as workers who are working in the informal or underground economy or in certain marginal activities. Nevertheless, it does not suffer from lack of representativeness and its information is completely reliable. For instance, results are fully comparable to the ones obtained with other sources such as the Quarterly Labour Cost Survey (from the National Statistics Institute) in the case of wages and the labour statistics published by the Public Employment Service in the case of the amount of unemployment benefits and the number of recipients (Arranz and García-Serrano, 2011).

[9]The number of persons at national level for whom information is available each year has increased from 1,095,808 in 2004 to 1,322,948 in 2023. As the raising factor is 25, this means that the reference population consisted of 27,395,200 persons in 2004 and 33,073,700 persons in 2023 at national level. These figures are roughly equal to the number of people belonging to the working-age population according to the Spanish Labour Force Survey.

nies' contribution account (unique identifier), it is possible to know whether a person who works for a certain employer at a given time continues to work for that employer later. The database is worker-level and allows the follow up of workers' employment trajectories. While firm closures may lead to job losses unrelated to the policy change, the variable measuring continued employment also captures transitions to new jobs/unemployment and from other jobs/unemployment, allowing for an individual-level assessment of the MW's effects. Thus, in our analyses employment status will be measured in two ways: first, by assessing whether the worker remains employed in the same firm in subsequent months; and second, by examining whether the worker remains employed in any job (in either the same firm or any other firm) in successive months.

Finally, while the dataset contains precise information on monthly wages and social security affiliations, it does not report the exact number of hours worked. Instead, it includes a partiality coefficient, which indicates the proportion of a full-time schedule worked by part-time employees. Although this limits the precision of estimates related to working time or hourly wages, this is the only available measure of work intensity in Spanish administrative data and a standard constraint in research using the MCVL.

2.3.3 Selected sample

Based on the data from the MCVL, we select an appropriate sample of workers to evaluate the impact of the increase in the MW on the probability of remaining in employment. First, we select individuals who work (a) as an employee, that is, they are affiliated to the general Social Security scheme; and (b) for at least an entire month (October). [10] This process is performed for people who are employed in October 2018, who are followed in the subsequent months until October 2019, on the one hand, and for people who are employed in October 2017, who are followed in the subsequent months until October 2018, on the other hand. The first group of workers (employed in October 2018) provides information referring to the evolution of employment in the period after the rise in the MW (from January 2019, the *after* period), while the second group (employed in October 2017) offers information on a period similar to the previous one but prior to the large rise in the MW of 2019 (the *before* period).

[10] To calculate the wages of part-time workers, certain assumptions about working hours were made, as the database only provides a part-time percentage and does not specify actual hours worked. The agreed full-time working hours for 2018 were set at 1,744 hours annually. Based on this, the assumption is to treat these 1,744 hours as the standard for a full-time work schedule. Using the part-time percentage and the reference of the full-time hours, wage adjustments were then made accordingly. Figure A.1 of the Appendix displays the wage distribution for the analysed years, both unadjusted and adjusted for hours worked.

After making this selection, we have 12,655,025 employed persons in October 2017 and 13,227,275 in October 2018, of whom 421,100 receive the MW or less in October 2017 and 453,200 receive the MW or less in October 2018. The percentage of this group with respect to the total (the incidence rate of MW) is 3.3% in October 2017 and 3.4% in October 2018. Nevertheless, using the 2019 MW as a reference threshold, we find that 10.3% of workers earned below this level in October 2017, and this share decreased to 9.2% by October 2018, in line with the findings reported by Barceló et al. (2021) and Gorjón et al. (2024). When analyzing the distribution of employees according to various personal and occupational characteristics, and distinguishing the total number of employees from those earning a wage equal to or less than the minimum wage for each year, it is evident that young people and those with lower levels of education are over-represented among those earning the minimum wage (see Table A.1 of the Appendix). The jobs occupied by these workers, being at the bottom of the wage distribution, are on average more unstable than the rest: compared with the total, there is a higher proportion of workers in jobs belonging to manual occupations, branches of activity such as agriculture, trade/hospitality, and other service activities, and working part-time and with fixed-term contracts. This picture of the incidence of employment paid the MW or less is like that offered by other previous studies.

Looking at the monthly wage distribution in the ex-ante and ex-post periods shows that the rise in 2018–2019 (bottom panel) was large, while that in 2017–2018 (top panel) was very small (see Figure A.1 of the Appendix). In fact, the latter was similar to the increase in the average wage, so the real MW remained practically unchanged, in relative terms as well, in this period. The comparison of the two periods allows us to verify that the small increase in the MW in January 2018 did not cause a shift in the wage distribution from 2017 to 2018, supporting our choice of 2017–2018 as an appropriate pre-intervention period.

Based on this information referring to the periods before and after the rise in the MW of 2019, it is possible to define, on the one hand, a treatment group (formed by those workers who are directly affected by the rise in the MW) and a control group (formed by those workers who, being similar to the previous ones, are not affected by the increase) in the ex-post period, referring to the years 2018–2019, and, on the other hand, two groups (treatment and control) in the ex-ante period, referring to the years 2017–2018.

In the ex-post period, the treatment group includes those workers who, in October 2018, have a wage equal to or higher than the MW of 2018 but lower than the new MW of 2019. This criterion assumes that these workers are the most likely to be affected by the rise in the MW. The control group is made up of those who, in October 2018, have a wage that is equal to the

MW of 2019 or is above a threshold that is 10% higher than this MW. This control group will be modified later to check the robustness of the results obtained.

In the ex-ante period, treated and control groups are constructed in a similar way. On the one hand, the group of 'treated' individuals (prior to the intervention) is made up of workers employed in October 2017 have a wage equal to or higher than the MW of 2017 but lower than the new MW of 2019. On the other hand, the control group (prior to the intervention) is made up of those who have a wage in October 2017 that coincides with the MW of 2019 or is up to 10% more than this MW.

The resulting sample provides information on 2,833,250 workers, of whom 1,479,250 are employed in October 2017 (877,825 being treated and 601,425 untreated) and 1,354,000 are employed in October 2018 (755,600 being treated and 598,400 untreated). Table A.2 of the Appendix shows the distribution of the treatment and control groups for both years according to various personal and work attributes. The predominant characteristics of salaried workers in the sample of treatment and control groups are similar to those of workers earning the MW or less: people of middle age, with studies equal to/below the compulsory level of education, who work in manual jobs and in companies belonging to the tertiary sector. In relation to the variables of job retention, we provide the information for some, not all, of the subsequent months. The treatment group exhibits proportions of workers who remain employed (in general and, above all, in the same company) after the MW hike that are lower than those of the group of controls, being these proportions similar in 2018 and 2019 in both groups.

2.4 Methodology: DID treatment effects

To evaluate the impact of the increase in the Spanish MW established by the government on 1 January 2019 on job and employment retention, we estimate the following equation using the DID technique for repeated cross-section data:

$$Y_i^{(m)} = \beta_1^{(m)} + \beta_2^{(m)}\, t_i + \beta_3^{(m)}\, Z_i^{\text{Oct}} + \beta_4^{(m)}\, (Z_i^{\text{Oct}} \cdot t_i) + \beta_5^{(m)}\, X_i^{\text{Oct}} + u_i^{(m)} \tag{2.1}$$

The dependent variable (Y) is the likelihood that worker i remains in employment ($Y = 1$) versus it does not ($Y = 0$) m months later. The model is estimated using a discrete choice model (logit). The $u_i^{(m)}$ is the error term. The explanatory variables (X) refer to personal (gender, nationality, age, level of education and region) and labour information (occupation, seniority, type of contract, working time and industry).

As we use data for 2017-2019, three variables are included in the previous model to implement the DID technique. The first one is a dichotomous variable (Z_i^{Oct}) that takes the value 1 if the worker is treated, 0 otherwise. The second one a dichotomous temporal variable (t_i) that takes the value 1 if the worker is employed in October 2018 and observed until October 2019 (ex-post period), the value 0 if the individual is employed in October 2017 and observed until October 2018 (ex-ante period). In the ex-ante and ex-post periods, the treatment and control groups are constructed similarly, as explained above. The treatment variable is defined exogenously. Unlike studies that use aggregate data from geographical areas such as states or regions, we know which workers receive wages equal to or less than the MW thanks to the availability of individual data. Moreover, we do not need to use information on the characteristics of individuals to impute whether or not a worker actually earns the MW or less. Furthermore, we use pre-treatment MW level status as treatment identifier. Finally, the third variable is the interaction of both variables ($Z_i^{\text{Oct}} \cdot t_i$) and reflects the impact of the MW hike in 2019. The DID effect of the policy change is captured by coefficient β_4. This parameter measures the change in the evolution of the probability of remaining employed for the workers affected by the rise in the MW (treatment group) compared with the workers not affected (control group) after the intervention (the increase in the MW in January 2019) and compared with the evolution of the probability of remaining employed of treated and control workers before the intervention.

Before discussing the comparability assumption, it is important to clarify the structure of the data. Our analysis relies on a repeated cross-section DID design, in which different individuals may be observed in the ex-ante and ex-post periods, and each wave constitutes a random draw from the population in the corresponding period rather than a panel following the same individuals over time. This approach is widely used in the literature when panel data are not available (Card and Krueger, 1994; Heckman et al., 1997, 1998; Blundell et al., 2004). The key identifying assumption is that observations are randomly selected within each period. Since this assumption may not hold perfectly in practice, we complement the DID strategy with matching and IPW techniques to enhance the comparability between treatment and control groups.

Although either the treatment and control groups share the same composition in terms of their characteristics in both periods or this composition has changed in the same direction between the two periods Table A.2 of the Appendix, some double differences in characteristics between the control and the treatment group, both before and after the MW-2019 increase, prove to be statistically significant (mainly occupation, seniority, type of contract and region).

Consequently, when this happens, it is convenient to apply either matching through PSM (e.g. kernel) or IPW techniques with DID to alleviate this problem (see, among others, Blundell et al., 2004; Abadie, 2005; Blundell and Costa Dias, 2009; Villa, 2016; Dube and Lindner, 2024; Sant'Anna and Zhao, 2020) and to ensure that individuals are identical, irrespective of their affiliation with the treatment or the control group, as well as to determine whether they are in the pre-treatment or the post-treatment period.

The DID matching estimator combines DID with matching techniques. It follows the standard DID framework, but constructs the control group using matching in order to improve comparability with the treated group. PSM pairs treated and control individuals with similar observable characteristics prior to applying the DID estimator, thereby reducing bias due to initial differences, albeit at the cost of discarding unmatched observations and reducing the effective sample size. In contrast, IPW reweights observations based on their probability of treatment and improves covariate balance without excluding data. In our quasi-experimental setting, both approaches are implemented to identify the average treatment effect on the treated (ATT), which is the parameter of interest. While IPW is often used to estimate the average treatment effect (ATE), it can also be employed to recover the ATT by appropriately reweighting the control group (see Allan et al., 2020). [11]

For that purpose, Equation 2.1 will be estimated using a complementary method to the DID treatment effect through the incorporation of inverse probability weighting (IPW), as described by Hernán and Robins (2020). The IPW technique consists in estimating the treatment effect on the outcome variable by weighting the available observations with the inverse probability of receiving treatment. The weights are given by:

$$w_i = \begin{cases} \dfrac{\widehat{\Pr}[Z_i = 1]}{\widehat{\Pr}[Z_i = 1 \mid X_i]} & \text{for the treatment group,} \\[2ex] \dfrac{1 - \widehat{\Pr}[Z_i = 1]}{1 - \widehat{\Pr}[Z_i = 1 \mid X_i]} & \text{for the control group.} \end{cases} \tag{2.2}$$

Following this procedure, a *pseudo-population* is created, consisting of a series of w_i that are replicas of each individual in the database. Workers who are less represented in the allocation to receive the intervention (i.e., those with a low probability of participating in the treatment)

[11] IPW estimates ATE because it weights all individuals in the sample to represent the entire population, adjusting for differences in covariates between treated and untreated groups. PSM tends to estimate ATT because it matches treated individuals with similar untreated individuals, allowing the impact of treatment on those who received it to be assessed. In theory, PSM can also estimate ATE if it adjusts correctly and matches all individuals, not just treated individuals. However, in practice, PSM is more commonly used to estimate ATT.

exhibit proportionally higher weights, whereas workers who are highly represented to receive treatment have proportionally lower weights. This enables the attainment of a *balanced* population that is comparable in terms of stability, time, and changing confounding factors over time across treatment assignment levels (see Hernán et al., 2004).

After applying the inverse probability weights, the resulting pseudo-population achieves a high degree of comparability between treated and control individuals. In particular, the double differences in observed covariates are effectively reduced to near zero (none of them are statistically significant), as shown in Table A.3 of the Appendix. This indicates that the weighted groups are balanced, and any remaining differences in outcomes can be more confidently attributed to the treatment effect rather than pre-existing differences between groups.

The fundamental identification assumption of the DID approach is that the difference in average outcomes between the comparison groups before the substantial MW uprating—when the MW either did not increase or increased very little—would have evolved in parallel in the absence of the reform. In other words, the difference between treatment and control groups in the pre-treatment period should have been maintained over time. The validity of this *common trends* assumption is crucial for the correct identification of the model and the DID estimator (β_4).

Ideally, this assumption could be verified by examining how the outcome variables evolved for the same individuals across multiple pre-treatment periods. However, the way in which we construct our groups (employed in October) prevents following the same individuals across years using the same outcome variable. Nevertheless, we assess the plausibility of parallel trends through several complementary strategies. First, the bottom of Table A.2 of the Appendix reports employment outcomes variables in November (the month preceding the reform) showing no statistically significant differences between treatment and control groups. Second, Figure A.2 of the Appendix presents the evolution of aggregate employment for treated and control workers, illustrating similar trends prior to the MW increase. Third, a placebo test has been carried out by estimating DID with a *false* treatment group, i.e., workers not affected by the MW increase (Gertler et al., 2016). Workers whose wages were 10–20% above the MW in the year following their observation are classified as *falsely* treated and compared with controls earning up to 10% above the new MW. The coefficient of the interaction in this test is expected to be non-significant, providing further evidence that the model correctly captures treatment effects. The results confirm this hypothesis (see Figure A.3 of the Appendix). Taken together, these checks support the plausibility of the parallel trends assumption in our setting.

However, in our design, the strict demonstration of parallel trends is less critical than in standard DID applications. By construction, the PSM and IPW procedures create treated and control groups that are highly comparable in terms of observed characteristics before and after the intervention, as shown by the near-zero double differences reported in Table A.3 of the Appendix. In this context, the main role of the DID estimator is to capture the treatment effect conditional on already balanced groups, while controlling for any residual unobserved heterogeneity, rather than to fully eliminate pre-existing differences.

2.5 Results

This section provides the results of DID treatment effects using the IPW model presented in the methodological section. We offer results by type of contract and working time schedule, and by region, to observe potential differences in outcomes. The reason is that, on the one hand, firms can react to the MW hike by altering their behaviour with respect to the permanence in employment of workers with different labour relationships and working hours; and, on the other hand, the bite of the MW may differ according to the average wage, and therefore the incidence of employment paying the minimum, in each region. This differentiated analysis contributes additional insights to the literature beyond those offered in previous studies.

Although we have also carried out our analysis using other approaches (DID with kernel PSM and standard DID), we focus exclusively on elaborating upon the results obtained through the DID with IPW model, because it is the approach that most effectively minimizes the differences between the treatment and the control group both before and after the intervention. Nonetheless, the results across all methodologies are quite similar (see Figure A.4 of the Appendix). In addition, given that the policy shock was implemented nationally, standard errors should account for within-group correlation. Following Bertrand et al. (2004), we have estimated our models using clustered standard errors at the sector and region levels. Furthermore, an analysis to check the robustness of the results is performed, including estimates with alternative control groups and estimates restricting the sample to certain jobs.

2.5.1 Benchmark results

As we mentioned above, we distinguish between remaining employed in the same company and continuing to be employed in general. This distinction is important. When it comes to measuring the impact of an increase in the MW on job retention, the relevant point is whether companies, as a result of rising labour costs, lay off some workers, that is, whether all or part of firms' workforce continues to be employed by them. However, most studies do not take this distinction into account, either because they use aggregated data and focus on aggregate employment rates or because they do not have information on the employer identifier in the database. One of the contributions of our study is precisely to provide estimates on the probability of remaining employed by the same company, which is the variable of greatest interest when conducting a study on the impact of an MW increase on employment using individual data.

The impact of the MW hike on job permanence is reflected in the interaction between the treatment variable and the time variable ($Z_{im} \cdot t_{im}$) with the parameter β_4 in Equation 2.1. Figure 2.2 shows the marginal effects of this double difference related to the change in the MW on the probability of remaining employed, either in the same company (*Panel (a)*) or broadly in any job (*Panel (b)*), after being observed as an employee. The results are presented for all months following the individual's observation after the increase in the MW to assess the potential effects.

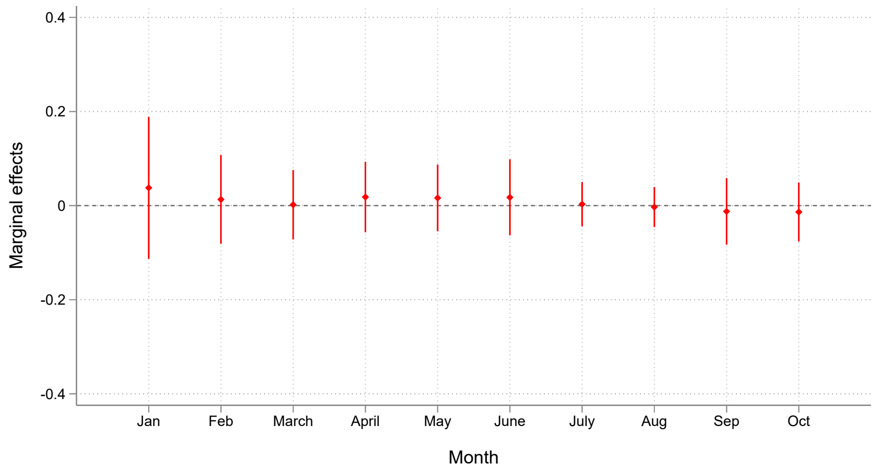
The results of the estimates indicate that the MW did not produce adverse effects on the probability that the affected workers remained employed in the same company following the rise in the MW in 2019 when compared with both similar unaffected workers and similar workers from an earlier period in which the MW barely changed. Moreover, we did not find any negative effects on the overall probability of remaining employed. [12] The variable of permanence in employment (in any job) is commonly used as an endogenous variable in previous studies on the impact of the MW on employment using the DID technique, while remaining employed in the same company is a stricter variable of job retention and best reflects the most immediate and direct impact of the rise in the MW since companies have to decide whether to continue with the employment relationship once the MW has risen. Therefore, the results suggest that the MW hike in 2019 did not produce adverse effects on the permanence in the same job/company and on the employment of the affected workers.

One might question whether an anticipation effect of the policy could lead to changes in the dependent variables in the months preceding the MW hike. We think that the likelihood of significant anticipation effects is minimal given the timing and context of the policy change.

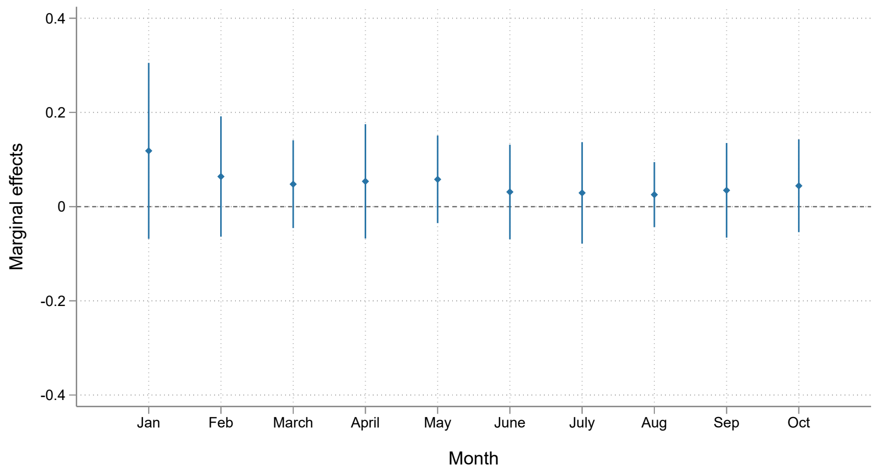
[12] The 95% confidence interval (CI) excludes effects of more than ±0.02 probability points on the probability of remaining in employment or in the same firm. Note that we estimate models using tens of thousands of observations. The baseline means—i.e., the probabilities of remaining employed in November, before the MW increase—as well as the sample sizes are reported in Table A.2 of the Appendix. Furthermore, for illustrative purposes, we focus on months 6 and 12 after treatment. Using Stata's power command, we estimate that the minimum detectable effect (MDE) at 80% power and α = 0.05 is approximately 0.005 (0.5 pp) for employment retention and 0.008 (0.8 pp) for same-firm retention. This confirms that our study could have detected even modest declines in both outcomes, and the observed effects near zero allow us to confidently rule out economically meaningful negative effects.

Figure 2.2: Estimate results from DID with IPW

(a) Probability of remaining employed by the same company in 2019



(b) Probability of remaining employed in any job in 2019



Notes: Estimate results from DID with IPW logit models of the probability of remaining employed, either by the same company or broadly in any job, after the 2019–MW increase. Regressor estimates correspond to marginal effects (β_4) from DID models estimated using IPW logit. Vertical lines represent 95% confidence intervals. Source: MCVL 2017–2019

The increase in the MW was officially announced on December 21, 2018, merely seven days before its implementation, providing limited opportunity for firms or workers to adjust their behaviour in advance (this is usual practice: the new MW is always announced in late December, with no prior knowledge of the exact increase). While the government had previously signalled its intention to raise the MW—partly in alignment with the European Social Charter—the magnitude of the nominal increase (22.3%) was largely unexpected. Social partners did not anticipate such a sharp adjustment, particularly given that, although the nominal MW had risen by 7% in 2017, the subsequent increase in 2018 was a much more modest 2%. Nevertheless, to account for this potential anticipation effect, we selected workers in October of the year prior to the increase, allowing us to examine whether such an effect emerges in November and December. The results indicate no significant effects on the dependent variables, confirming the absence of an anticipation effect (see also the bottom of Table A.2 of the Appendix, where the November figures are provided descriptively).

2.5.2 Heterogeneity

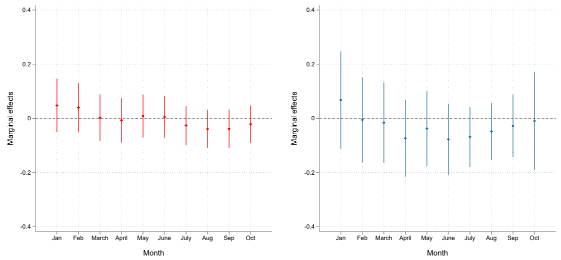
It is crucial to investigate the impact of the MW on both the likelihood of remaining employed and the likelihood of staying in the same job position as this approach can provide insights into the implications for broad, national policy applications. However, the key to informing potential policymaker decisions lies in examining individuals who might be adversely affected by the increase. Even if the aggregate effect is null, it may happen that the increase in the MW has had a negative/positive effect on a specific group of workers. This is more likely for those for whom the MW bite is more likely because their wage is low. Here we focus on three dimensions: type of contract, working time and region. On the one hand, temporary and part-time workers command lower wages than permanent and full-time workers and make up a large portion of workers earning the MW. On the other hand, regions exhibit significant wage disparities and differ in their incidence of MW employment. According to conventional wisdom, the notion is that the bite of the increase in the MW could be greater for those groups of workers whose employment can be more easily adjusted and those regions where the average wage is lower.

First, we analyse two key worker characteristics separately: (i) contract type—distinguishing between permanent and temporary contracts and (ii) working time—comparing full-time and part-time workers. The results, shown in Figure 2.3, correspond to the estimated marginal effects of the interaction term ($Z_{im} \cdot t_{im}$), which captures the differential impact of the MW increase over time for affected workers. The estimation has been carried out by using sample split.

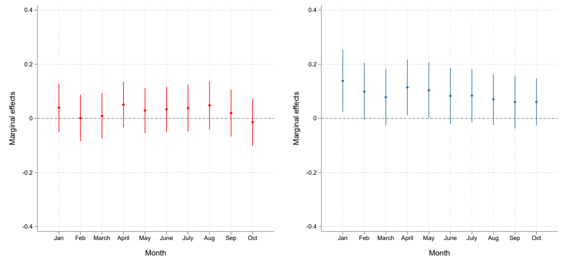
The estimates show that the increase in the MW did not lead to a reduction in the probability of remaining employed in the same job or company for any of the groups considered. However, when looking at overall employment (regardless of staying in the same firm), some differences emerge. Specifically, temporary and full-time workers experienced a small but statistically significant increase in their likelihood of remaining employed during the first few months after the MW increase. In contrast, permanent and part-time workers showed slight negative effects, but these were not statistically significant, suggesting no clear adverse impact for these groups. A plausible interpretation is that these workers, being most directly exposed to the MW increase, are temporarily retained by employers seeking to avoid turnover costs during the initial adjustment period.

Figure 2.3: Estimate results from DID with IPW by type of contracts and working time schedule

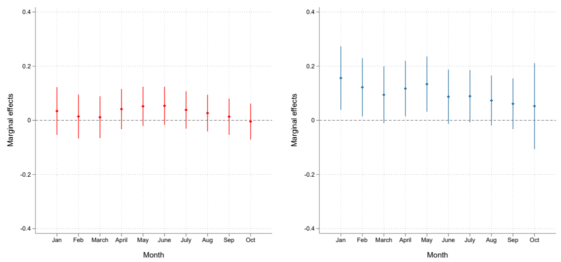
(a) Results for permanent workers in 2019



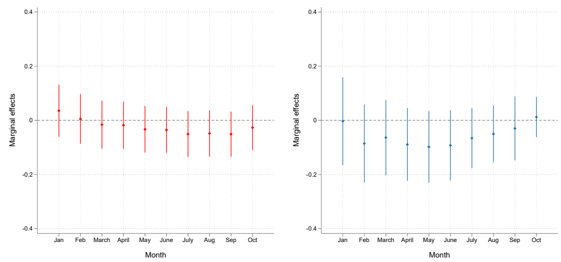
(b) Results for temporary workers in 2019



(c) Results for full-time workers in 2019



(d) Results for part-time workers in 2019



Notes: Estimate results from DID with IPW logit models on the probability of remaining employed, either with the same employer (left panel) or in any job (right panel), after the 2019 MW increase, by type of contracts and working time schedule. Regressor estimates correspond to marginal effects (β_4) from DID models estimated using IPW logit. Vertical lines represent 95% confidence intervals. Source: MCVL 2017–2019

Second, we examine whether the effects of the MW increase differ across regional groups, given that average wage levels and the incidence of MW employment vary substantially across regions. To this end, we divide regions into three categories (low-wage, mid-wage, and high-wage) based on their average wage levels. Figure 2.4 shows the estimated marginal effects of the interaction variable by regional group, both for the full sample and separately by type of contract (results by working schedule are not shown but are available upon request). As previously, the estimation has been carried out by splitting the sample. In this analysis, we focus in particular on the distinction between open-ended and temporary contracts, as we consider contract type to be especially relevant. This is because many temporary contracts terminate regardless of MW adjustments, and affected workers are disproportionately represented among those with temporary contracts. Therefore, distinguishing by contract type allows for a clearer identification of the potential impact of the MW increase on employment dynamics.

The results show no adverse effects of the MW increase in any specification for mid- and high-wage regions. In low-wage regions, affected workers experience a small but positive differential increase in the probability of remaining employed overall, compared to unaffected workers. This effect emerges five months after the MW hike and is mainly driven by permanent workers (and to a lesser extent, full-time workers). The reason could be that in regions where pre-reform wages were closer to the new minimum a larger share of workers experiences a wage increase, resulting in more pronounced short-term effects on employment retention. As for the probability of staying employed in the same firm, no significant differences are observed—except for a slight positive effect for permanent workers two months after the MW increase.

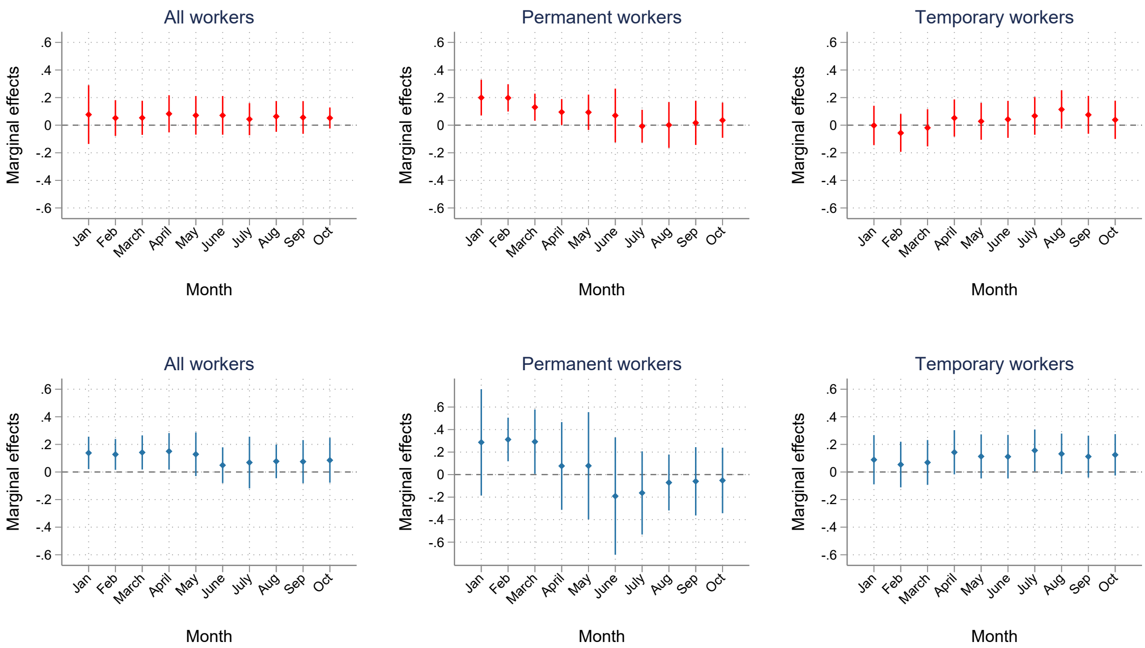
2.5.3 Robustness analysis

Having presented the results obtained with the main sample, a series of tests is carried out in this subsection with the objective of examining the robustness of the results and their sensitivity to changes in the sample and its composition to show the validity of the analysis. All the robustness tests are performed on the two endogenous variables defined above. We present the outcomes of the robustness tests using the IPW DID model approach.

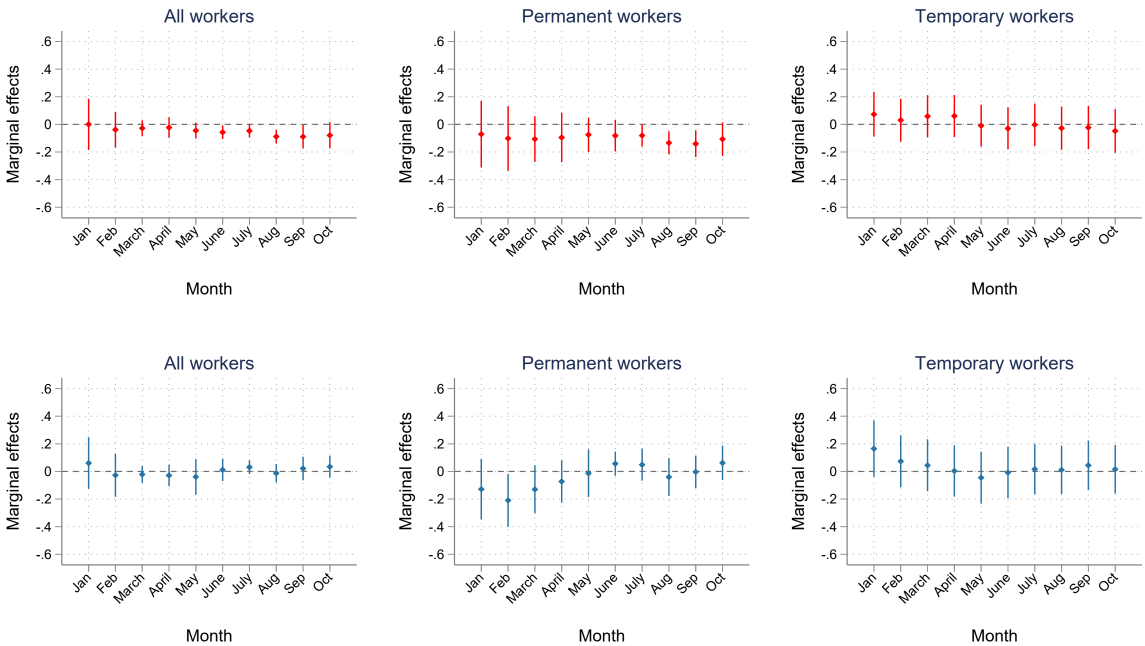
In the first block, the main analysis is replicated but the criteria used to build the control group are modified. The starting point is the control group used in the basic analysis (*Control 1*). Subsequently, a second control group will be created, consisting of the same workers from *Control 1*, but excluding those whose wages are above the 2019 MW wage yet do not exceed that MW by more than 2%. This new selection is called *Control 2*. This approach is intended to remove potential spillover effects between the treatment and control groups from the analysis. Then, the selection criterion of the control group is relaxed, expanding this group to individuals observed as employees, either in October 2017 for those in the ex-ante period or in October 2018 for those in the ex-post period, who have a wage exceeding the 2019 MW by up to 20%.

Figure 2.4: Estimate results from DID with IPW by region and type of contract

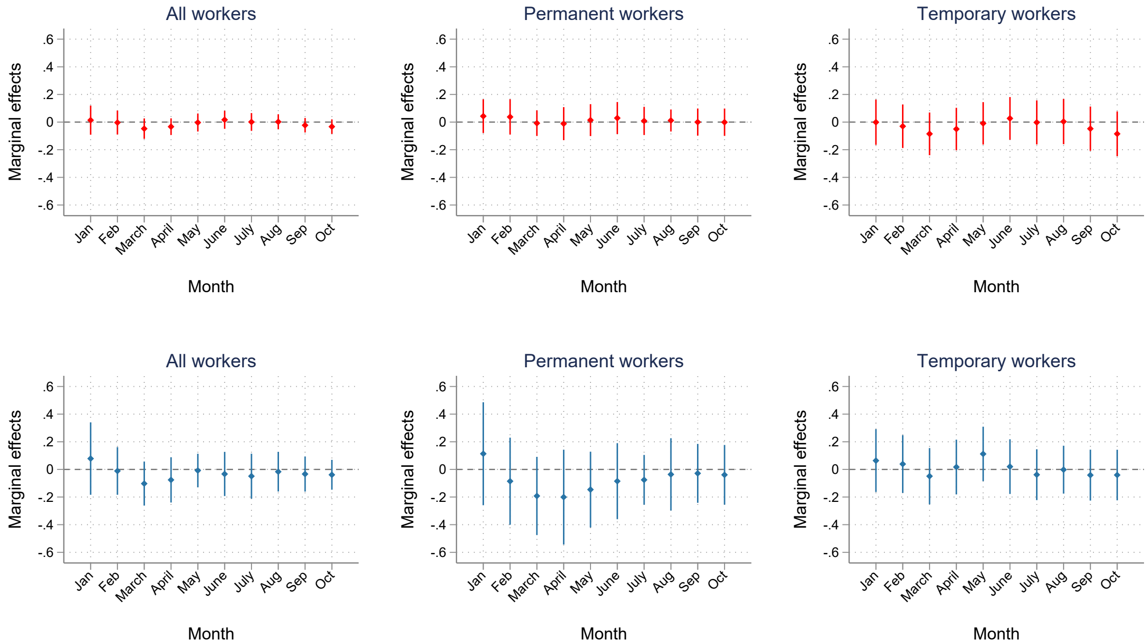
(a) Results for low-wage regions in 2019



(b) Results for mid-wage regions in 2019



(c) Results for high-wage regions in 2019



Notes: Estimate results from DID with IPW logit models of the probability of remaining employed, either by the same company (in red) or broadly in any job (in blue), by region and type of contract. Regressor estimates correspond to marginal effects (β_4) from DID models estimated using IPW logit. Vertical lines represent 95% confidence intervals. Source: MCVL 2017–2019

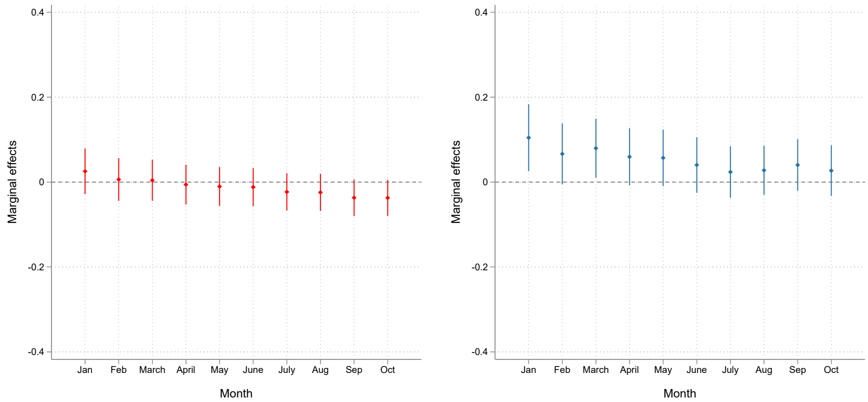
Consequently, we extend the threshold of the untreated individuals from 10% above to 20% above the new MW. This new selection is called *Control 3*. Previous studies also pursue a strategy of expanding the control group (Stewart, 2004a; Portugal and Cardoso, 2006)

Figure 2.5 illustrates the trajectory of the MW increase's impact on the probability of remaining employed, either within the same company or generally, over the subsequent months following the rise, using both alternative definitions of the control group. As in the main analysis, most of the estimated effects of the increase in the MW on the probability of remaining employed either in the same company or in any job are not statistically significant.

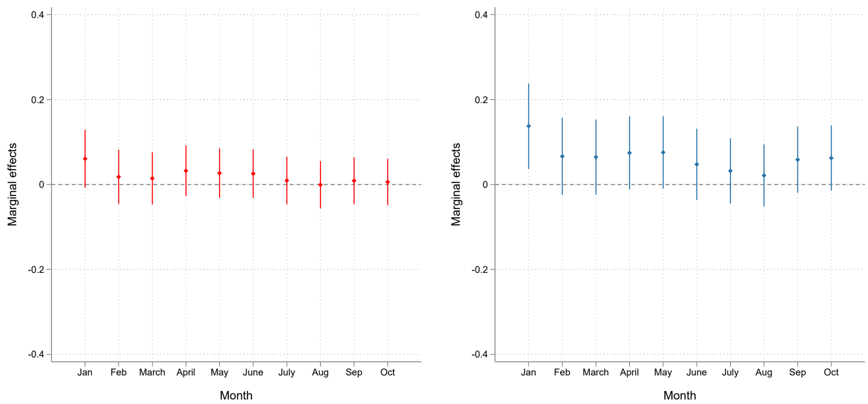
In the second block of tests, one restricted sample is created: people who are classified within the most qualified occupational groups are excluded. The occupational categories are proxied by the *contribution group* or *job category*, a classification established by the social security that groups employees according to the functions they perform in their job and used to establish the contribution bases (10 categories). White-collar high-skilled occupations correspond to the first three contribution groups. The reason for the elimination of these groups is to check what changes in the results obtained when the most qualified jobs (the MW of which corresponds to different and higher contribution bases than those of the rest of the groups) are not considered. These three occupational groups account for just under 15% of the treatment and control groups

Figure 2.5: Estimate results from DID with IPW: indicators *Control 2* and *Control 3*

(a) Alternative control group (*Control 2*)



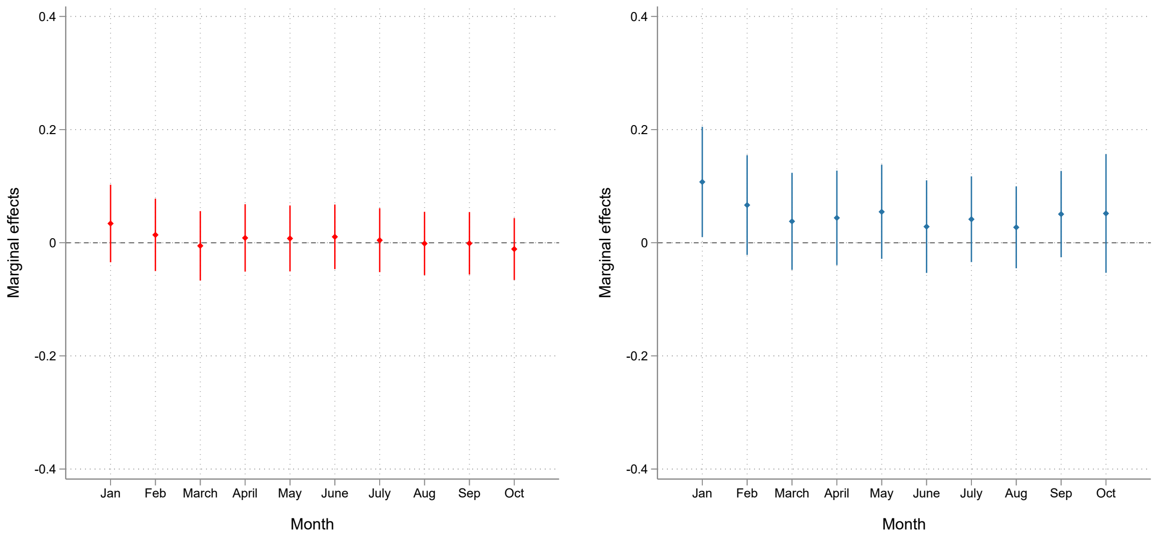
(b) Alternative control group (*Control 3*)



Notes: Estimate results from DID with IPW logit models of the probability of remaining employed, either by the same company (left panel) or broadly in any job (right panel), after the 2019–MW increase: indicators *Control 2* and *Control 3*. Regressor estimates correspond to marginal effects (β_4) from DID models estimated using IPW logit. Vertical lines represent 95% confidence intervals. Source: MCVL 2017–2019 in the initial sample. Nevertheless, note that, despite being skilled jobs, the companies employ workers who earn the MW.

Figure 2.6 displays the results when using the sample that excludes the most qualified occupations. As can be seen, the results are practically identical to those obtained with the 'full' sample. In this case, the coefficients of the interaction variable of treatment and time are not statistically significant in almost any month. Therefore, the conclusions are essentially the same as those obtained with the complete initial sample.

Figure 2.6: Estimate results from DID with IPW: Sample of workers in medium- and low-skilled occupations (contribution groups 4 to 7)



Notes: Estimate results from DID with IPW logit models of the probability of remaining employed, either by the same company (left panel) or broadly in any job (right panel), after the 2019–MW increase. Sample of workers in medium- and low-skilled occupations (contribution groups 4 to 7). Regressor estimates correspond to marginal effects (β_4) from DID models estimated using IPW logit. Vertical lines represent 95% confidence intervals. Source: MCVL 2017–2019

Finally, one methodological consideration in our study is the use of a DID design estimated with non-linear models, such as logistic regression (Athey and Imbens, 2006). To address this issue and assess the robustness of our findings, we additionally estimated the probability of remaining employed—both within the same firm and in any other job—using a linear probability model. The results are very similar to those obtained from the main specifications reported previously (see Figure A.5 of Appendix), providing reassurance regarding the robustness of our conclusions.

2.6 Discussion and conclusions

This article examines the effects on employment/job retention of the huge rise in the MW that occurred in Spain in 2019 using individual data. The study applies repeated cross-section DID techniques with IPW models, as proposed by Hernán and Robins (2020), to mitigate selection biases and confounding elements in the impact assessment of the MW. The analysis defines treatment and control groups based on a sample of salaried workers made up of those who are most likely to be affected by the increase in the MW (those who, before the increase, had a wage that was between the previous MW and the new MW) and those who are close to the previous ones in the wage distribution (those who, before the increase, earned between the new MW and up to 10% above the new MW). Two variables of interest are considered, i.e. continuing to be employed in the same company and continuing to be employed in general (by being employed in the same company or in another company). By focusing on job retention within the same firm, this study contributes to the literature by providing a detailed perspective on how MW increases impact direct job continuity. Alternative definitions of the control group, as well as one restricted sample, are used to check the robustness of the results obtained.

Our key finding is that the increase in the MW does not negatively affect the probability of remaining employed either within the same company or in any job of workers who are directly affected by the hike in any month of the year following the rise. These findings of no significant impact persist across robustness tests, such as extending the control groups and restricting the sample to workers in medium/low-skilled positions. Although we detect very small positive effects on the probability of employment in some of the initial months of the year for full-time and temporary workers, this effect is largely non-significant until the end of the year. For part-time and permanent workers, the effects are largely null. Furthermore, our results also reveal that the null employment impact of the 2019 MW increase hardly varies based on region. In areas with lower average wages where the bite is potentially greater, the MW hike appears to have created some positive incentives for job search and retention, thereby raising the likelihood that affected workers remain employed, although this effect seems to be very short-lived. Simultaneously, higher-wage regions exhibit statistically non-significant effects on employment retention. In sum, the overall picture is one of non-negative impacts of the MW hike on job and employment stability of affected workers in different regions.

While these results provide a clear picture of the immediate effects of the 2019 MW increase, it is important to acknowledge that identifying medium- and long-term effects is challenging because the COVID-19 pandemic strongly affected labour markets across Europe starting in early 2020. Employment dynamics observed after 2019 may thus reflect a combination of the policy impact and pandemic-related shocks, making it difficult to isolate the former. Consequently, the results presented here should be interpreted as reflecting the immediate consequences of the MW hike, and further research will be needed to assess how these effects evolved over time under more stable conditions.

The findings obtained in this work complement previous research (Card and Krueger, 1994, and subsequent authors), which suggests that MW increases do not necessarily lead to reductions in employment, and align with empirical evidence (Portugal and Cardoso, 2006; Brochu and Green, 2013; Dustmann et al., 2022) that points to a null/negative impact of MW hikes on separations (layoffs and quits), even when the MW increase is large (Bossler et al., 2024). Those authors argue that a higher MW can promote job retention, particularly within lower-income segments and in low-wage contexts. Even in these contexts, the existence of frictions affecting the decisions of employers and workers in the labour market can lead firms, faced with a rise in the MW, to absorb the increase in labour costs in other ways: through increases in productivity, a reduction of some non-wage components of labour costs, price increases or a reduction of business margins (even without passing on the rise in wages to prices).

Empirical evidence (including survey answers of managers about their reactions to the policy) points to these business response pathways (Hirsch et al., 2015; Riley and Bondibene, 2017; Harasztosi and Lindner, 2019; Dube and Lindner, 2024). Other authors refer to monopsony power to reconcile the absence of negative employment effects of MW hikes with the economic theory and also find evidence that the degree of monopsonistic competition in the labour market affects the effect that wage increases produce on employment, being less negative or even positive in the most concentrated markets (Munguía, 2020; Azar et al., 2024). In the European context, Popp (2024) performs a comprehensive test of this argument by using labour market concentration as a proxy for monopsony power in Germany and finds that sectoral MWs lead to negative employment effects in slightly concentrated/more competitive labour markets, whereas this effect weakens with increasing concentration and becomes positive in highly concentrated/monopsonistic markets. This would imply that there is scope to increase wages in high-concentration areas where wages tend to be below marginal productivity [13].

[13]The effects of MW hikes can go beyond its potential impact on employment, which is the dimension that economists have studied the most. In fact, they can extend to various variables, such as working conditions, migration, the reallocation of workers between firms, consumption, wage inequality and poverty. Examining these variables is beyond the scope of this article. For the sake of brevity, we will only mention, for example, that the literature suggests that increases in the MW reduce wage inequality and poverty (Dube, 2019b), something that has been observed in the Spanish case (Arranz and García-Serrano, 2025).

Understanding possible non-linearities in the employment effect from MW hikes that are both large and at high levels is interesting due to increasing experimentation by governments on the MW policy. Evidence on this issue is limited and inconclusive, ranging from small effects (Sturn, 2018; Harasztosi and Lindner, 2019; Giupponi et al., 2024; Clemens and Strain, 2025) to larger negative effects (Clemens and Strain, 2018). Some authors point to 60% as the threshold above which additional increases in the MW could bring about more negative impacts, depending on institutional attributes such as a stricter EPL (Égert et al., 2024). The challenge is to consider the dimensions of what a large MW means (ending level, size/pace of change) and that researchers provide estimates along those dimensions to avoid weak inference when searching for heterogeneity by thresholds (Dube and Lindner, 2024). In this vein, the Spanish evidence would suggest that countries where the MW is low relative to the average/median wage and a gap between productivity and wages exists have room to increase the MW without damaging employment [14].

Therefore, there are several avenues of research related to the effects of the MW on employment that are still relatively unexplored and that require more empirical evidence, so that policymakers have more and better information to make appropriate decisions. Among them, we can highlight not only the non-linearities, but also the possible medium- and long-run effects, the impact of continuous (cumulative) increases in the MW in contrast to sporadic increases or the effects on hiring (from other jobs and unemployment) to better estimate the influence of MW hikes on total net employment. Another avenue that remains largely unexplored in the Spanish context is the potential spillover effect of MW increases on collectively bargained wage floors. In settings where collective agreements play a key role in wage setting, as in Spain, higher statutory MW could indirectly push up the lower bounds of sectoral wage negotiations, reinforcing some of the job retention dynamics explored in this paper. Evidence from France, for instance, shows that statutory MW changes influence wage rigidity in industry-level agreements, leading to upward adjustments beyond the legal minimum (Fougère et al., 2018). Understanding how these institutional complementarities operate could further enrich the analysis of MW impacts on employment and wage structures.

[14] Note that in the case of Spain the difference between the level of productivity and wages was relatively high (at least before 2019) compared with other European countries. The productivity of the Spanish economy was almost equal to the productivity of European countries that have a national MW (it represented 97% in 2019), while the Spanish MW was much lower than the minimum of these countries, weighted by the size of their economies (it only represented 75%).

Chapter 3

The impact of the minimum wage on gross employment flows: An analysis at province level in Spain[1]

Abstract

This study investigates the effect of the minimum wage on gross employment flows, focusing on hiring and job separations, as well as net employment outcomes. Using aggregate data from an administrative data source covering Spanish provinces from 2015 to 2023, we find a significant negative effect of the minimum wage on hires and separations in provinces where a larger share of minimum wage earners exists compared to other provinces. However, the net employment effect, although positive, is close to zero, indicating that the size of the reduction in hires and separations is similar. Furthermore, a rise in the incidence of the minimum wage leads to an increase in hiring and separations among part-time workers, while it reduces them among full-time workers. The effects, although statistically significant, are small and economically negligible.

Keywords: minimum wage, hiring, separations, impact, administrative data.

JEL: J23, J38, J63.

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[1]The resulting paper is co-authored by José María Arranz and Carlos García Serrano. This manuscript has been accepted for publication in the *International Labour Review*. The article will be published under a Creative Commons Attribution License (CC BY 4.0).DOI (10.16995/ilr.23444)

3.1 Introduction

The minimum wage (MW) has been the subject of theoretical and empirical analysis for decades, but there is no full consensus on its effect on the labour market. In general, empirical studies have examined the effect of increases in the MW on net employment, which is defined as the difference between the number of jobs created and the number of jobs destroyed over a given period (Dube, 2019a; Neumark and Shirley, 2022). Departing from this traditional approach, the present paper aims to analyse the effect of an increase in the MW on hires and separations as well as on net employment. Our case study focuses on Spain, a country characterised by a highly regulated labour market and where the MW is set by the government and is nationwide —uniform for the whole country—. For this purpose, we use data on hires and separations at the provincial level over the period 2015-2023. These units are analogous to counties in the United States.

There are three main contributions of this article to the literature. First, it investigates the causal effect of increases in the MW on employment flows, an area where evidence is scarce. The rationale for focusing on employment hires and separations lies in their potential to shed light on how labour market actors, including both employers and workers, react to increases in the real MW (i.e. the statutory minimum wage adjusted for inflation). From an employer's perspective, MW hikes raise labour costs, which may prompt firms to reduce hiring or increase layoffs (Neumark et al., 2014). However, either in the presence of adjustment costs from hiring and firing or when firms have market (monopsonistic) power, employers may exhibit limited responsiveness to changes in the MW, choosing to address the increase in labour costs by reducing profit margins rather than cutting worker headcount (Dube et al., 2016). Therefore, variations at the province level in the labour market structure may affect firms' decisions regarding hiring and firing differently. From the perspective of workers, an increase in the MW could improve job attachment and reduce labour mobility, as incentives to seek alternative opportunities diminish (Portugal and Cardoso, 2006). Moreover, a MW rise may act as an incentive to re-enter the labour market after a period of unemployment, which could lead to an increase in the supply of labour (Bryan et al., 2013).

The second main contribution is that the focus is on Spain, a country with strong employment protection and labour market institutions and where the impact of MW hikes remains relatively unexamined. For instance, the collective bargaining system, as in other European countries, has several levels and is characterized by its high coverage (around 80%). Moreover, the employment protection legislation is relatively high, as reflected in the OECD's Employment Protection Legislation index for individual and collective dismissals of regular contracts. [2]. The Spanish economy also exhibits a high share of temporary employment. [3] At the same time, the share of part-time employment (around 13%) is relatively low for European standards (18% on average) and is mainly involuntary, with around half of part-time workers reporting to hold such employment arrangement involuntarily, compared to an average of nearly 20% in the European Union. [4] Thus, an important motivation for this paper is the scarcity of empirical works that examine the impact of increases in the MW on gross labour flows in Spain. Existing studies have focussed on assessing the effect of increases in the MW on aggregate employment or the probability of job retention (Hijzen et al., 2025; Arnadillo et al., 2024; Gorjón et al., 2024) or on household income and poverty (Arranz and García-Serrano, 2025), with no studies assessing its effects on job inflows or outflows (see the Section 3.2 for literature review).

The third main contribution is on the methodology and type of data used to investigate the impact of MW hikes on employment flows. Province-level data for the period 2015-2023 is used because it was a period during which significant nominal increases occurred from 2017 onwards (an increase of 8% in 2017 and 22.8% in 2019), substantially raising the real MW (see Figure 3.1 below). The subsequent analysis is based on panel data techniques that capture variation across provinces over time; this is, to our knowledge, the first attempt in the literature to employ this approach to analyse the research question at hand for the case of Spain. This research evaluates the impact of the incidence of the MW (share of workers who earn wages at or below the MW) on hires and separations of workers using an econometric framework in which the identification is based on variation in the effective incidence of the MW across province-level labour markets. [5] Province-level indicators are constructed from individual-level data drawn from an administrative database. This aggregation allows us to explore the effects of MW changes on local labour markets while relying on high-quality administrative microdata.

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[2]Spain scores 2.43 on a 0–6 scale, above the OECD average of 2.23. These are the most recent available values (2019). Source: OECD Employment Protection Legislation (EPL) index, OECD Data Explorer (Version 4, 2013–2019), available at: OECD Data Explorer.

[3]After lying above 30% during the 1990s and 2000s, it reached 25% in 2021, compared to an average of 12% for the OECD as a whole. The labour market reform of December 2021 helped reduce it below 20%.

[4]These data refer to 2023 (Eurostat Database: Persons in part-time employment by age – % of total employment [lfsa_eppga] – and Persons in involuntary part-time employment – % of total part-time employment [lfsa_eppgai]).

[5]Some reported wages may appear below the mandatory MW due mainly to specific exemptions for certain types of employment (e.g., training contracts).

This paper is organized as follows. Section 3.2 provides a brief review of the literature on the effects of MW on gross employment flows. Section 3.3 describes the MW in Spain and its evolution in recent years. Section 3.4 presents the database and the selected sample, along with a descriptive analysis of the data. Section 3.5 outlines the panel data method used for the analysis. Section 3.6 presents the results of the estimations. Finally, the Section 3.7 summarizes the findings.

3.2 Literature review

From a theoretical perspective, the economic literature has sustained an extensive debate regarding the nature of low-wage labour markets and the potential effects of an increase in the MW. On the one hand, it is argued that in markets characterised by a high level of competition and an overall absence of major market failures, the introduction of a MW would tend to reduce employment (Stigler, 1946). On the other hand, whenever there is a deviation from the assumptions of perfect competition, the adverse effects of the MW on employment might be more muted (Lester, 1960). Therefore, in the presence of labour market frictions (Acemoglu, 2001) or in monopsonistic labour markets (Manning, 2003), increases in the MW may have a small or null impact on employment levels.

From the perspective of employment flows, theoretical predictions differ across frameworks. Both the canonical search model of Mortensen and Pissarides (1994) with endogenous separations and off-the-job search and the variant in Pissarides (2000) that incorporates uncertainty about match quality predict that increases in the MW lead to higher dismissal rates and lower accession rates —defined, respectively, as the number of layoffs and hires relative to total employment—, since fewer matches are profitable, meaning that unemployment rises. This, in turn, makes it easier for firms to fill vacancies, as the initial increase in unemployment expands the pool of job seekers and improves matching efficiency in the presence of heterogeneity in match quality and pre-existing mismatches, as highlighted in search-and-matching models (Pissarides, 2000; Pinoli, 2010). As a result, accession rates gradually recover after their initial decline following the increase in the MW. Once hiring recovers, labour turnover may rise above its initial level (Pinoli, 2010).

Other matching models with on-the-job search provide different predictions regarding separations, predicting that either quits or layoffs may decline after MW increases. In 'job ladder' models —both based on wage posting (Burdett and Mortensen, 1998) and on bargained wages (Pissarides, 2000)—, the arrival of better offers shapes worker's incentives to remain in their current job. Therefore, changes in the offer wage distribution can affect the rate at which workers leave their jobs for better ones. These types of models predict fewer quits by lowering the arrival rate of better-paying job offers in response to an increase in the MW, so that it reduces job-to-job flows. In matching models with heterogeneous vacancy costs — Brochu and Green (2013)—, match quality is initially uncertain, but over time, as more information about the match value is revealed, employers and workers decide whether to continue or dissolve the employment relationship. When the MW increases and becomes binding, existing matches are re-evaluated. While low-productivity matches may be terminated, high-quality matches remain profitable even after the wage increase. For these matches, higher replacement and hiring costs following the increase in the MW may strengthen incentives for retention, potentially reducing transitions to non-employment. Therefore, lower separations may be accompanied by fewer hires, reflecting reduced labour turnover rather than weaker labour demand.

Finally, monopsony and institutional-behavioural models also suggest that an increase in MW can reduce hiring and separations without necessarily harming net employment. In institutional models, employment is limited by sales rather than by the equality between wages and marginal revenue product (Lester, 1947; Kaufman, 2010), so that even sizeable MW hikes may represent a modest cost shock. Firms can adjust through several channels, reducing gross job flows while keeping net employment changes small (Hirsch et al., 2015). In monopsony labour markets, higher MWs reduce separation rates, especially quits, by increasing workers' incentives to stay (workers are more satisfied). With fewer separations, replacement needs fall, and higher wages also attract more applicants, potentially supporting additional vacancies. As a result, hiring does not necessarily decline, retention rises, and job to job transitions and turnover fall (Manning, 2003, 2021; Munguía, 2020). [6] This implies that an increase in the MW might not affect net employment and could even contribute to increasing it.

From an empirical viewpoint, a sizeable body of literature has assessed the impact of MW hikes on aggregate employment (Card and Krueger, 1994; Neumark et al., 2014; Caliendo et al.,

[6]This occurs because the existence of frictions in the labour market implies that there are rents to jobs, which gives employers some market power over their workers, so that they can set wages below the marginal productivity of workers. Heterogeneous preferences, imperfect information and mobility costs are the main potential sources of frictions (Manning, 2003). The consequence of these frictions is an increase in the MW raises the pay of previously underpaid low-wage jobs, making them more attractive to workers and reducing the incentive to leave for other positions.

2018; Holtemöller and Pohle, 2020). [7].

Many studies adopt a regional approach, especially in the United States, using panel or time series models at the state or county level and exploiting differences in binding federal and state MW, with employment rates as the main outcome (Neumark et al., 2014; Neumark and Wascher, 2004; Allegretto et al., 2011). This strategy has also been adopted in studies focusing on other countries, such as Germany and the United Kingdom, where the MW does not vary geographically (Stewart, 2002; Dolton et al., 2012; Bruttel, 2019; Dustmann et al., 2022; Bossler and Schank, 2023).

However, fewer studies have examined the effects of MW hikes on hiring and/or separations at an aggregate level. Among these contributions, some rely on explicit theoretical assumptions, while others adopt a more agnostic reduced form approach (Belman and Wolfson, 2014).

On the one hand, several studies rely on models of imperfectly competitive labour markets. For instance, Pinoli (2010) and Hirsch et al. (2015) appeal to the search model and to the institutional model, respectively, while Portugal and Cardoso (2006), Brochu and Green (2013) and Dube et al. (2016) rely on the matching model with on-the-job search to explain their results. Portugal and Cardoso (2006), the first empirical work to focus on both hiring and separations, study the impact of the increase in the MW in Portugal that affected young workers (aged 15–20) in 1987. They find that the share of young workers declined in both accessions and separations, with a larger drop in separations. [8] Brochu and Green (2013), using Canadian data from 1979-2008, report reductions in hires, quits and layoffs among low-skilled teens following MW increases. Similarly, Dube et al. (2016) find comparable results for the US from 2000-2011, showing that the separation, hiring and turnover rates decrease significantly among adolescent and workers in the hospitality sector. Contrary to the previous studies, Hirsch et al. (2015) find null effects on separations and accessions in their study of quick-service restaurants in Georgia and Alabama, while Pinoli (2010), using data from Spain for youth workers on the period 2000-2006, finds higher separations rates and unchanged/lower accession rates.

[7] Many reviews focus on either the US (Neumark, 2019; Wolfson and Belman, 2019; Neumark and Shirley, 2022) or other developed economies (Neumark and Wascher, 2008; Belman and Wolfson, 2014; Dube, 2019a). There are also studies that carry out meta-analyses (Leonard et al., 2014; Jiménez and Jiménez, 2021). Note that the empirical evidence reviewed in this section only refers to developed economies; results can be different in developing economy contexts due to the prevalence of informal employment and the 'lighthouse effect' (Boeri et al., 2011)

[8] These authors also use worker-level data and compare the results of a treatment group and a control group before and after the increase to examine the permanence on employment. Their results confirm that workers who earned below the MW were more likely to continue working with (less likely to separate from) the same employer than those who had not been affected by the measure.

On the other hand, some empirical studies of the effects of MW hikes on gross labour flows do not rely on a specific theoretical labour market model. Instead, they focus on reduced-form empirical relationships using regional or aggregate data. Several works that use regional data find negative effects of increases of MW on accessions and separations for Canada and the US (Liu et al., 2016; Gittings and Schmutte, 2016). Liu et al. (2016), using data from US counties between 2000 and 2009, find that a higher MW is associated with higher earnings, but also with lower employment and less labour turnover (fewer separations and new hires), for young people aged 14–18. For the 19-21 and 22-24 age groups there is a decrease in entry flows, separations and overall turnover. Gittings and Schmutte (2016) analyse US counties and find that the flow of young workers into and out of jobs is reduced by MW increases. Similar results are found for some European countries, especially for hiring (Bryan et al., 2013; Bossler and Gerner, 2020).

Some studies point out heterogenous effects of MW hikes on the hiring/firing rate by industry and market structure. For instance, Gopalan et al. (2021) find reduced low-wage hiring concentrated on the tradable sector, while Gittings and Schmutte (2016) find negative employment effects in low-turnover markets and where job leavers have short non-employment spells, but positive effects in high-turnover markets and where non-employment spells are long. Additional evidence suggests that MW increases may shift employment toward higher-wage or larger firms (Aaronson et al., 2018; Dustmann et al., 2022). Other studies examining labour market flows in specific contexts—such as firms, sectors, or cities—report negative or null effects on separations and no significant effects on employment (Dube et al., 2007; Giuliano, 2013).

In sum, the extant body of empirical evidence tends to find that MW increases reduce both hirings (accessions) and separations (quits, layoffs). These findings contradict perfect competition models and support imperfect competition models. Different imperfect competition models explain these effects through frictions that make hiring and separation costly. This can lead employers to lay off fewer workers and workers to quit less, while at the same time employers hire fewer new workers and fewer workers move to other jobs. As a result, overall gross flows of jobs and workers either remain unchanged or decline slightly.

In the case of Spain, recent studies focus on the 2019 MW hike, with limited evidence on gross flows. Barceló et al. (2021)find an increase in the individual probability of losing employment (12 months after the rise) among directly affected workers compared to their counterparts. [9] Using a similar differences-in-difference setting, Hijzen et al. (2025)find a decline in the likelihood of remaining employed in the next year. Moreover, Gorjón et al. (2024) find no statistically significant effect on transitions from employment to unemployment within 4 months after the 2019-MW hike and a significantly positive 9–11 months after the rise. Moreover, Arnadillo et al. (2024) employ a synthetic control method finding no effect of the MW increase on overall employment levels.

While these studies provide valuable evidence on the short-term effects of individual MW hikes on job separations or employment probabilities, they generally do not analyse hiring, do not exploit variation across provinces, and focus on a single MW increase (i.e., the 2019 policy change). In contrast, our study examines a longer period (2015–2023) with relative heterogeneity in MW changes, allowing us to evaluate how changes in the incidence of the MW over time affect both hiring and separations. In addition, we extend the analysis to consider potential heterogeneity across job characteristics. Thus, our study provides a more comprehensive examination of the effect of changes in the MW on labour market dynamics in Spain and hence complements the existing literature.

3.3 The minimum wage in Spain

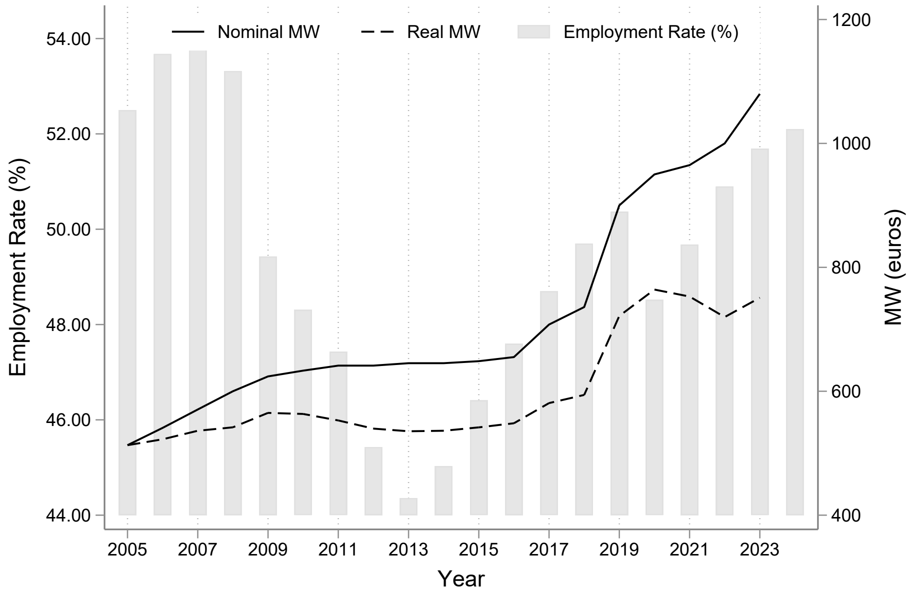
In Spain, the MW is set annually by the government, following consultations with trade unions and business organizations. Its level is determined by considering factors such as economic growth, employment trends, the consumer price index (CPI), and productivity. The MW is defined on an annual basis, though it is distributed across 12 regular monthly payments and 2 additional extraordinary payments. Furthermore, the MW is uniform nationwide, with no distinctions by sector, age, or region.

Figure 3.1 presents the evolution of the MW, both in nominal and real terms, alongside the share of employed individuals over total adult population (aged 16 and over) in Spain between 2005 and 2023. The nominal MW shows consistent growth over time, except between 2011 and 2016, and accelerates from 2017 onwards. This increase is particularly pronounced between 2019 and 2023, coinciding with government policies aimed at improving workers' incomes. In contrast, the real MW (deflated using the CPI with a base year of 2005) exhibits a more

[9] With aggregate data, Barceló et al. (2021) estimate a reduction in net employment of 0.6–1.1 pp, which could imply lower job creation of more than 100,000 jobs. AiRef (2018) simulates the effects of the rise in the MW in 2019 using data from the MCVL-2017 and estimates a loss of total employment of only 0.15% in 2019 (24,000 fewer jobs). Hijzen et al. (2025), using their estimation on the probability of remaining employed and without taking account of hires, calculate that the decline in employment was about 7,400 jobs.

irregular pattern: it rises between 2005 and 2009; falls from 2011 to 2016 as inflation exceeded nominal increases; and shows an upward trend starting in 2017, interrupted by the COVID-19 pandemic.

Figure 3.1: Evolution of the MW and the employment rate in Spain



Notes: Own elaboration based on data from the Ministry of Labour and Social Economy and the National Statistics Institute (INE).

The employment rate fluctuated significantly in this period. Up until 2008, employment shares (and levels) increased and remained high, but from that year until 2013 declined due to the global economic crisis. Since 2014, employment has shown a progressive recovery, except in 2020. In general, periods of greater increase in the MW coincide with years of economic expansion; conversely, in periods of recession, the MW remains stagnant.

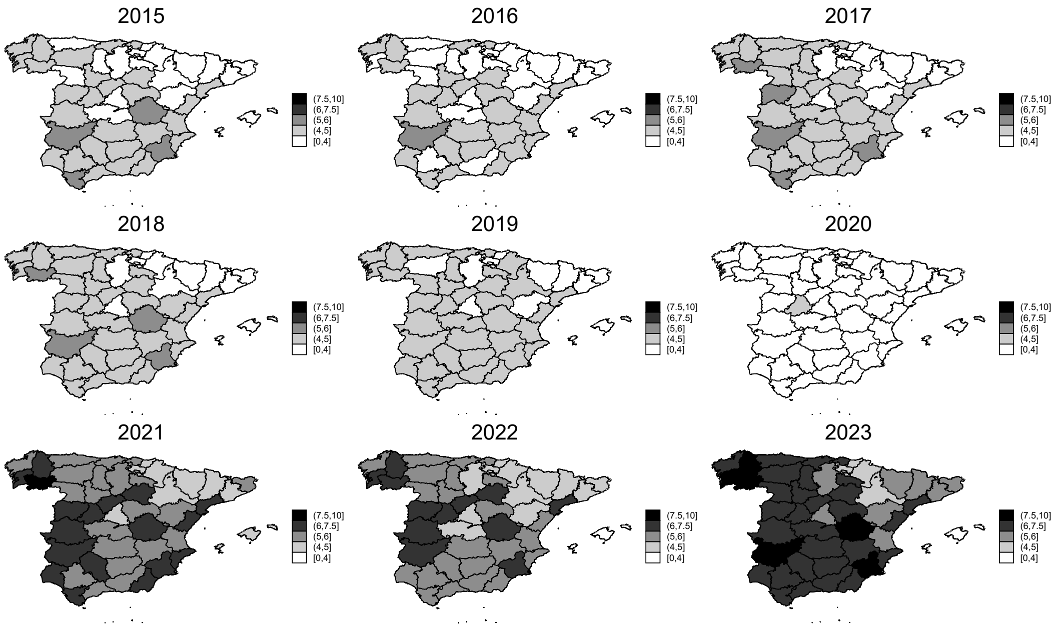
The MW is set nationally, but the composition of paid employment and the average wage level differ between provinces, meaning these regions are not equally affected by the level of the MW and its fluctuations. Figure 3.2 shows the proportion of workers earning the MW or less in Spanish provinces between 2015 and 2023. [10]

Between 2015 and 2019, the national share was approximately 4.2%. However, this proportion varied by region: it was higher in the northwest, central, and southern regions, and lower in the north-eastern regions. In 2019, despite a 22% increase that brought the MW closer to the target of 60% of the median wage, the proportion of workers earning the MW or less remained

[10]We only consider wage and salary workers, because MW does not apply to the self-employed. Thus, the share is calculated dividing the number of workers earning the MW or less and the total number of paid employees.

constant across the country, although regional differences narrowed. The most notable change occurred after the pandemic, when the percentage of individuals earning the MW or less rose to 5.3% of salaried employment. During these years, the MW got closer to the national median wage, resulting in two key observations: a reduction in wage dispersion variance during the year with the largest MW increases, and an increase in incidence from 2021 to 2023, with 2022 being the year in which the MW reached 62% of the median wage. [11]

Figure 3.2: Evolution of the percentage (%) of wage and salary workers earning the MW or less between 2015 and 2023 by province in Spain.



Notes: Own elaboration based on MCVL 2015-2023 data.Values are expressed as percentage (0–100).

3.4 Data

The database used to analyse the impact of the MW on gross worker flows is the Continuous Sample of Working Lives (Muestra Continua de Vidas Laborales, MCVL), an official annual statistical source compiled by the Spanish Ministry of Inclusion, Social Security, and Migration. Each edition of the MCVL contains information on 4% of the reference population (individuals affiliated with the Social Security system and recipients of unemployment benefits and pensions), drawn through simple random sampling. The number of individuals for whom information is available each year ranges between 1.1 and 1.2 million; for instance, in 2023, this

[11]The median wage is calculated using data from the Annual Survey of Wage Structure (ASWS), provided by Eurostat.

corresponds to a reference population of approximately 31 million people.

For each worker, the MCVL provides both personal information (including date of birth, sex, nationality, province of birth, province of first Social Security registration, place of residence, country of birth, and educational attainment) and employment-related details (such as contribution group, start and end dates of employment episodes, contract type, part-time work coefficient, type of employment relationship, wage earnings, and the industry of the employer). The availability of monthly wage and non-wage earnings, along with precise entry and exit dates for each employment spell, allows the exact identification of the starting and ending dates of work for each job, as well as the calculation of job tenure for employees. These variables facilitate the computation of daily aggregate job hires and separations throughout the year.

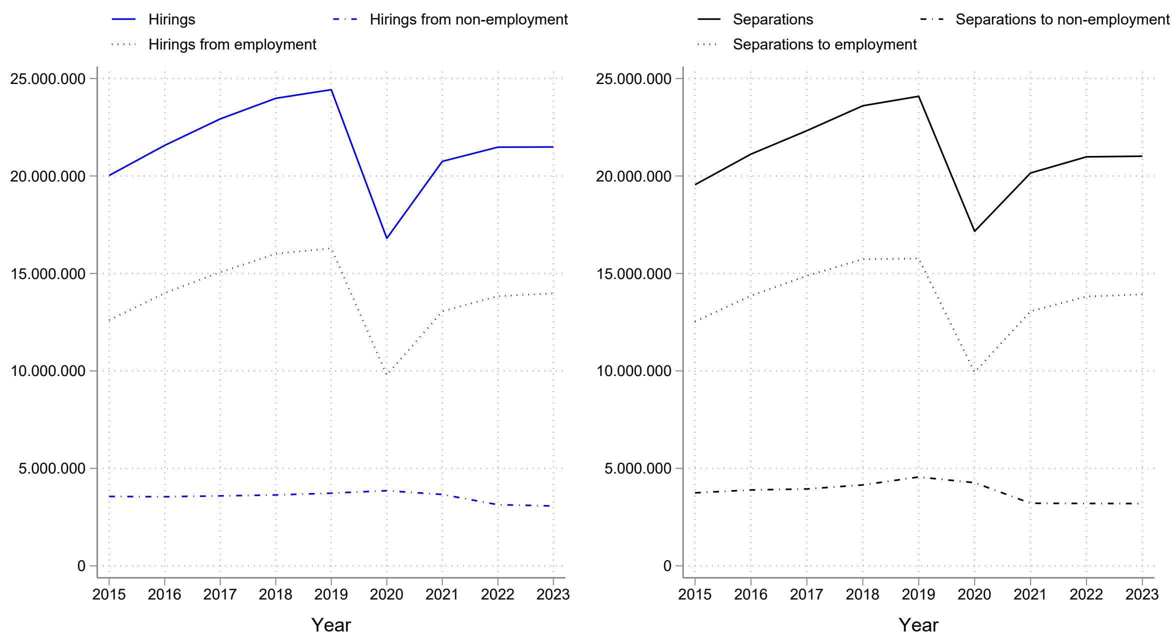
Furthermore, the original microdata and the linking of individual information thanks to the availability of a personal identification code allow the identification of both the origin of job entries and the destination of job exits, in terms of corresponding labour market status. On one hand, job hires can originate from employment, unemployment with benefits, unemployment without benefits, or inactivity. On the other hand, separations between employees and employers can lead to different post-employment situations, such as employment in another firm, unemployment with benefits, unemployment without benefits, or inactivity. In the empirical analysis, non-employment includes both unemployment and inactivity.

Once the information for all workers has been compiled, the analysis of job entries and exits is conducted over the period 2015–2023. A database is constructed in which the cross-sectional unit of observation is the province and the time unit corresponds to the year. This approach is feasible because the MCVL provides information on the location of each employment spell at the level of the province. Since the objective is to examine the relationship between the incidence of the MW and the number of job entries and exits, an incidence indicator is constructed as the ratio between the number of MW earners and the total number of employees each year in each province. As a result, the final database includes information for 52 provinces over the years 2015–2023, yielding a total of 364 observations.

Figure 3.3 displays the national trends in job hires by origin and job separations by destination over the period 2015–2023, while Figure 3.4 depicts the evolution of total hires and separations disaggregated by work schedule (full-time or part-time) and contract type (permanent or temporary). [12]

The evolution in job hires and separations appears quite similar. This is typical in aggregate labour market flows: when there are no major shocks affecting total employment, inflows and outflows tend to move in parallel, resulting in stable or slowly growing employment levels. These stable dynamics were interrupted in 2020, when the COVID-19 pandemic occurred, while a substantial decline in temporary contract hires and separations, along with an increase in permanent contracts, was observed in 2022. This shift is attributable to the effect of the December 2021 labour market reform, which restricted firms' issuance of fixed-term contracts to specific tasks or projects.

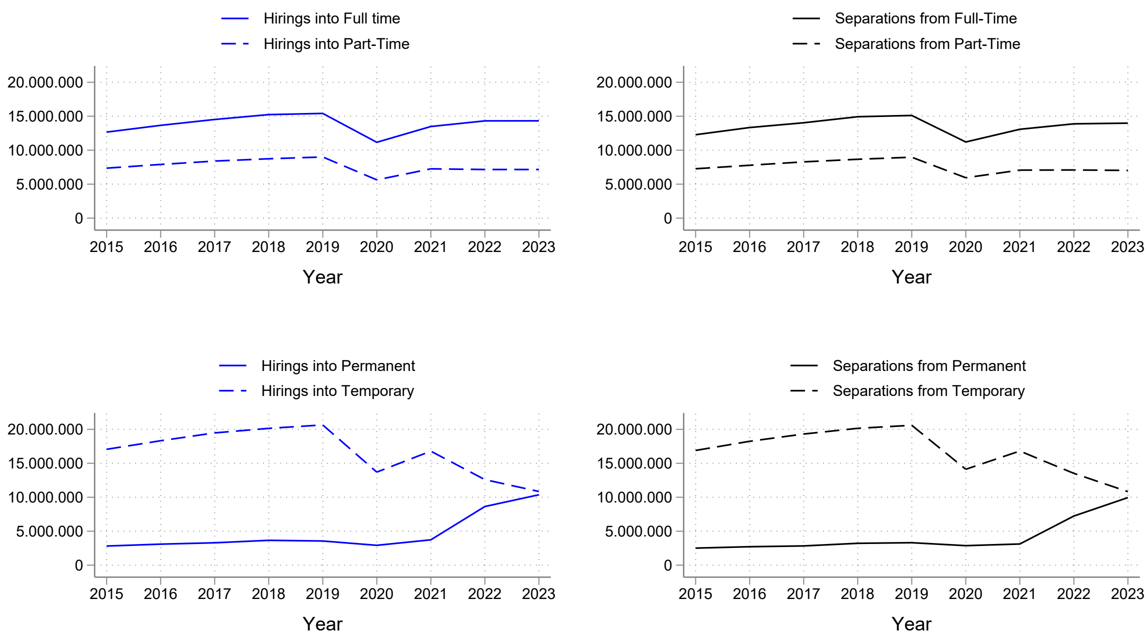
Figure 3.3: Evolution of spells of hires and separations in Spain



Notes: Own elaboration based on MCVL 2015-2023 data.

[12]It is important to note that these figures provide information on the gross flows of employment over the entire year at the national level, including all individual episodes of hiring and separations. Thus, the chart represents spells, not workers. Many of the employment relationships associated with these entries and exits are short-term, so that a single worker may contribute several entries and exits throughout the year, which contributes to the high number of recorded hires and separations. Moreover, it should be noted that in 2015-2021 the share of temporary employment—defined as the share of salaried workers without an open-ended (permanent) contract— in Spain was around 25%, and that during that period around 90% of newly signed contracts were temporary. In absolute terms, the annual number of temporary contracts signed was 20 million in 2016, 22.5 million in 2019, and 19.4 million in 2021, for example (data from the Contract Statistics published by the Public Employment Service: https://www.sepe.es/HomeSepe/es/que-es-el-sepe/estadisticas/datos-avance/contratos.html

Figure 3.4: Evolution of spells of hires and separations by work schedule and contract type in Spain



Notes: Own elaboration based on MCVL 2015-2023 data.

3.5 Methodology

This section outlines the methodology used to analyse how the MW affected employment flows (hires and separations) across different geographic areas (provinces) from 2015 to 2023. We use a panel data framework to examine whether provinces with greater exposure to the MW experienced different effects compared to those with lower exposure. To this end, we construct a province-level measure of MW incidence to capture heterogeneous effects of the MW on employment flows, depending on the province-specific exposure. We then link this continuous incidence measure to provincial labour market outcomes.

Most studies of changes in the national MW exploit this variation across geographic areas. The key idea is to compare labour market outcomes in more exposed ("treated") areas with those in less exposed ("control") areas. We estimate a first differences panel data model that controls for local conditions and exploits within-province variation in the incidence of the MW on employment flows (see Neumark and Wascher, 2004; Neumark et al., 2014; Allegretto et al., 2011; Dolton et al., 2012; Dustmann et al., 2022; Bossler and Schank, 2023). [13]

A potential concern with estimating the model in levels is that provinces with higher turnover (measured as hires plus separations per employee) may systematically differ in labour market efficiency. Such difference could be correlated with MW incidence and thus bias the estimate results. Estimating the model in first differences helps mitigates this potential endogeneity. Identification relies on within-province variation in MW incidence over time, which removes time-invariant unobserved heterogeneity and reduces bias arising from persistent differences in turnover or other structural features of local labour markets. Formally, the first-differenced model is specified as:

$$\Delta Y_{pt} = \alpha \Delta X_{pt} + \beta \Delta I_{pt} + \gamma \Delta \log(GDP_{pt}) + \Delta \varepsilon_{pt} \tag{3.1}$$

where ΔY_{pt} is the dependent variable of interest , defined alternatively as the change in the hiring rate or in the separation rate—each measured as the number of hires or separations relative to total salaried employment—in province p in year t (each outcome is analysed in a separate specification); ΔX_{pt} is a vector of time-varying provincial-level controls, including averages or proportions of worker characteristics (sex, educational level, job seniority, and sectors of economic activity) for each province p in year t; $\Delta \log(GDP_{pt})$ captures changes in provincial GDP (in logarithms); and ε_{pt} is the error term of the model. The key variable is ΔI_{pt}, the change in the incidence of the MW measured as the percentage of individuals in province p earning the MW or less at the beginning of year t. [14] All employment flow variables are expressed as proportions ranging from 0 to 1, whereas MW incidence is expressed as a percentage from 0 to 100.

Under standard assumptions, fixed-effects estimation would identify the causal effects of MW based on province-level variation provided that unobserved heterogeneity across areas (e.g., local institutions, productive structure, labour culture) remains constant over time. This concern is addressed in the first-differences specification in (1), which removes time invariant

[13] Using geographical areas as the unit of observation may raise concerns if MW decisions in one area are influenced by policies in neighbouring areas, as might occur in the United States. However, this concern is absent in Spain and most European countries, where the MW is set nationally.

[14] We have included the average turnover rate over the last ten years to partially control for potential endogeneity arising from persistent differences in local labour market dynamics. Additionally, as the pandemic years (2020 and 2021) are included in the analysis, we have added a pandemic dummy variable in addition to provincial GDP as a control for the business cycle to account for the extraordinary labour market conditions during those years and their potential impact on employment dynamics. These adjustments allow us to better isolate the impact of MW changes from both long-term turnover patterns and the effects of the COVID-19 shock.

province specific factors. In addition, time-varying local variables, such as local GDP growth, capture changes in economic conditions within provinces over time. Accordingly, identification in the model relies on within-province changes in the effective MW incidence across local labour markets, while controlling for time-varying characteristics across provinces (Dube and Lindner, 2024). Thus, the coefficient β in Equation 3.1 measures the change in the proportion of employment flows (hires or separations) caused by a one-percentage-point increase in MW incidence. [15]

Finally, we focus on the contemporaneous incidence of the MW within each year. The reason is that in Spain the statutory MW is set on January 1st, and our data capture hires and separations over the entire year. The assumption is that employers can react relatively quickly to increases in the MW and do so uniformly across provinces. This is reasonable in the case of hiring, but it would also need to apply to separations (layoffs), especially of temporary workers. Nevertheless, although the effects of an MW change may not materialize instantaneously, our objective is to measure the short-term, within-year impact. Therefore, the observed impact on employment flows should be interpreted as reflecting the within-year response to the MW change, while acknowledging that lagged effects may exist.

3.6 Empirical results

3.6.1 Main results

This section presents the results obtained from the estimation of Equation 3.1. [16] The findings are reported for total hires and separations, then disaggregated by schedule and contract type. Only the coefficient of the MW incidence on flows is provided. [17]

Figure 3.5 presents the results of the first difference panel data model for total hiring and separation rates and for transitions by origin and destination, which can be identified from the microdata (see the data section). The results indicate that the MW has significant effects on gross labour market flows. When the incidence of MW increases by 10 percentage point (pp), the hiring rate decreases by 0.063 pp and the separation rate by 0.071 pp. Thus, provinces with higher incidence exhibit lower hires and separations than provinces with lower incidence.

[15] In other words, a 1 percentage point increase in the incidence of the MW leads to a $\beta \times 100$ percentage point change in the proportion of hires or separations within the province.

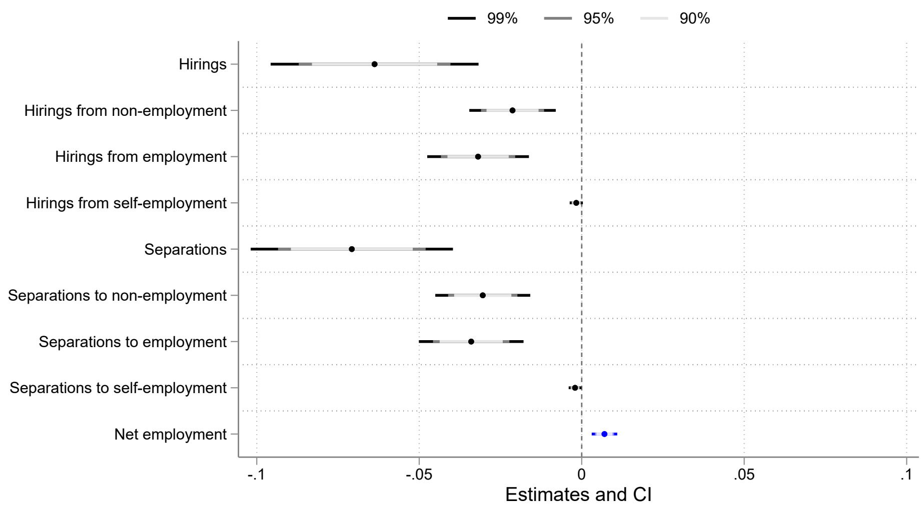
[16] Descriptive statistics are provided in Table B.1 of the Appendix.

[17] Full estimates are not shown, but available upon request.

When analysing the origin of hires, the negative effect of higher MW incidence is observed for employment-to-employment transitions and for hires from non-employment (either unemployment or inactivity), while there is no effect on hires from self-employment. Likewise, in the case of the destination of separations, the negative effect applies mainly to job-to-job and employment-to-non-employment flows.

These results are consistent with findings from previous studies (Portugal and Cardoso, 2006; Gittings and Schmutte, 2016; Liu et al., 2016) and can be interpreted through the lenses of several theoretical models. In a 'job ladder' model, higher wage offers affects workers' incentives to remain in their current positions, so that MW hikes can reduce quits (Dube et al., 2016). Similarly, in matching models with heterogeneous vacancy costs, when high quality matches are achieved after an initial period, increases in the MW reduce layoffs, because dismissing and subsequently re-hiring workers is costly (Brochu and Green, 2013). In monopsony models of the labour market (Manning, 2003), employers possess wage-setting power, implying that higher MWs can increase the relative value of retaining workers rather than laying them off. [18] Institutional-behavioural models (Hirsch et al., 2015) can similarly accommodate these results, highlighting that MW increases may reduce hiring and separations.

Figure 3.5: Results of the panel data model in first differences estimated on different dependent variables



Notes: coefficient β from Equation 3.1 — point estimate and confidence intervals.

Furthermore, the net effect of a higher MW on employment is also analysed. The result

[18]Intuitively, if firing a worker entails losing the investment in training and experience, it can be more efficient for firms to retain employees at a higher wage than to incur the costs of separation and subsequent recruitment.

shows that this effect is positive and statistically significant, but small in magnitude, indicating that the reduction in hires is similar, although slightly higher than, the reduction in separations. Therefore, when the incidence increases by 10 pp, net employment increases by 0.007 pp in provinces with higher MW incidence compared to provinces with lower MW incidence. This result of a nearly null employment effect is in line with the one obtained in much of the empirical literature (Dube et al., 2010; Allegretto et al., 2011, 2017; Dolton et al., 2012, 2015; Leonard et al., 2014; Cengiz et al., 2019; Manning, 2021).

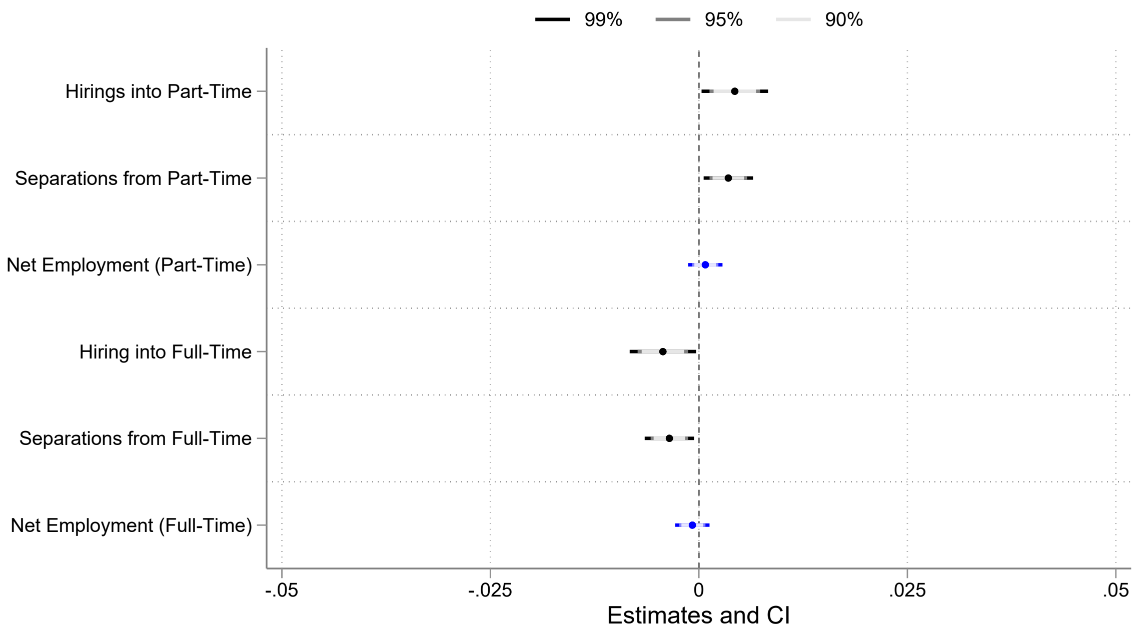
Although the aggregate results indicate that hires and separations decrease in provinces with higher MWs, it is also important to consider other dimensions related to the quality and composition of employment which may be affected by increases in the MW. For instance, firms in provinces with a higher incidence of low-wage employment may respond to increases in the MW by adjusting hires and separations based on working hours or modifying employment contracts to better adapt to rising labour costs. To examine this issue, we analyse hires and separations based on working hour schedule and contract duration. The results are presented in Figure 3.6 and Figure 3.7, respectively.

On the one hand, the results indicate that higher MW incidence increases hiring and separation rates for part-time workers, while reducing hiring and separations for full-time workers in provinces with higher MW exposure, although the effects are small. Specifically, a 10 pp increase in MW incidence is associated with a 0.005 pp increase in part-time inflows, whereas full-time inflows decline by a similar amount. Changes in separations follow the same pattern: an increase of 0.004 pp for part-time workers and a comparable decrease for full-time workers. Overall, these effects are economically negligible, and net employment does not differ significantly for either full-time or part-time workers. This suggests that MW incidence does not substantially alter employment flows across work schedules.

The available empirical evidence in this regard revolves around working hours instead of work schedule. Nevertheless, it is limited and mixed, and, in general, most of it does not suggest large adjustments to hours of work conditional on employment. For instance, Cengiz et al. (2019) and Gopalan et al. (2021) do not find any significant reduction in hours in the US due to hikes in the MW, and Hampton and Trotty (2023) find a small negative effect for affected incumbent workers. In the case of European countries, Stewart and Swaffield (2008) find a reduction of hours, while Connolly and Gregory (2002) find no effect.

On the other hand, regarding the effects by contract duration, the coefficients are not statistically significant in any specification. This suggests that an increase in the incidence of MW does not significantly affect the hires or separations of workers with either permanent or temporary contracts. Likewise, the net effect is zero for both types of employment.

Figure 3.6: Results of the panel data model in first differences estimated on different dependent variables by full-time or part-time status

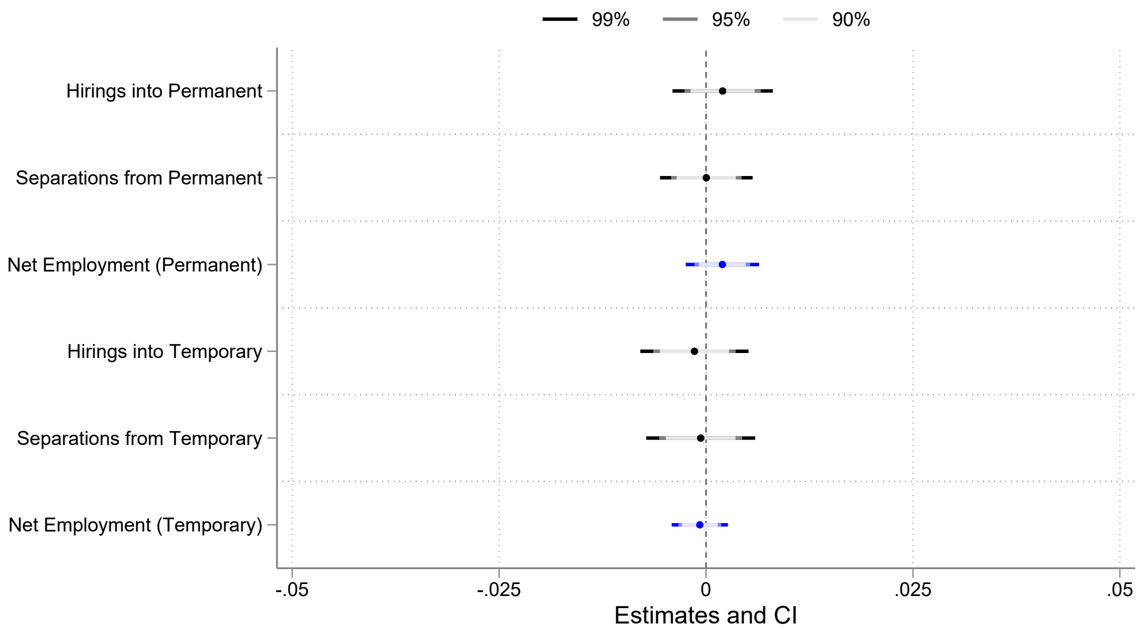


Notes: coefficient β from Equation 3.1 — point estimate and confidence intervals.

Finally, the impact of the MW on employment quality is examined by combining contract type and work schedule, resulting in four labour categories: full-time permanent employment, part-time permanent employment, full-time temporary employment, and part-time temporary employment. Working hours and contract type can be important determinants of work-life balance. In Spain, much of these types of employment are involuntary, as many workers who would otherwise like to do so are unable to access full-time positions or permanent contracts. According to the Spanish Labour Force Survey, almost half of employees in part-time employment work report lacking full-time employment opportunities (Instituto Nacional de Estadística (INE), 2025).

Figure 3.8 presents the estimates. As shown, the incidence of the MW does not have statistically significant effects on gross employment flows in almost any of the categories considered. A positive and statistically significant effect is detected for full-time temporary workers only. When net effects are estimated, no significant effects are found for any of the groups.

Figure 3.7: Results of the panel data model in first differences estimated on different dependent variables by contract type



Notes: coefficient β from Equation 3.1 — point estimate and confidence intervals. The years 2022 and 2023 are excluded from the analysis due to the potential impact of the labour reform of December 28, 2021.

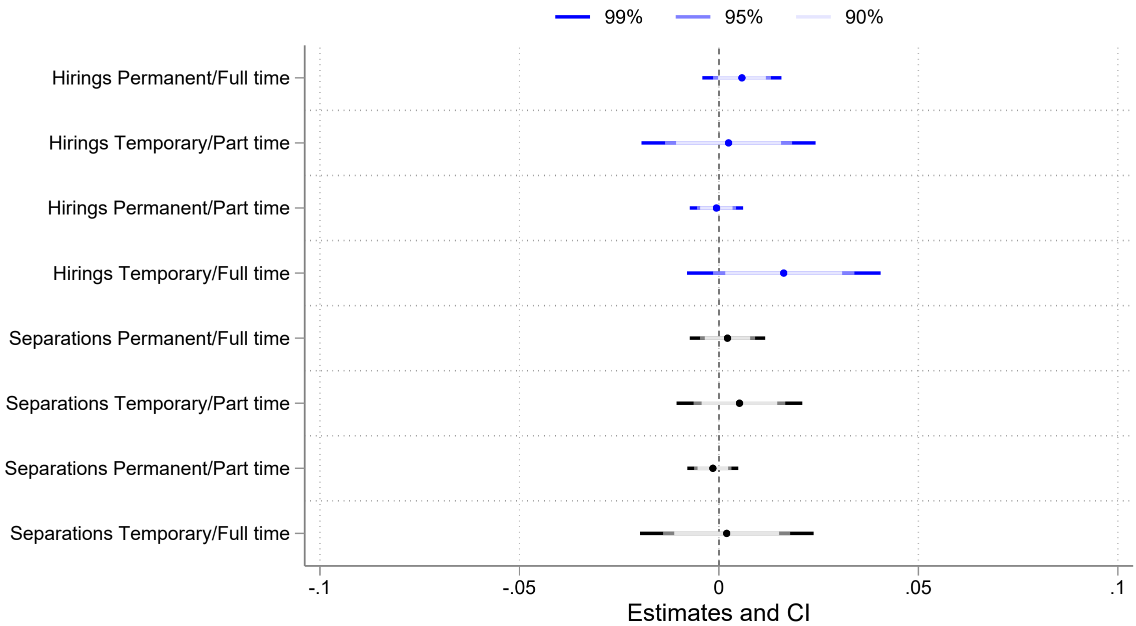
[19] Therefore, increasing the MW does not appear to significantly affect these dimensions of employment.

In summary, the results suggest that increases in the incidence of MW brings about reductions in hires and separations that are more pronounced in provinces with higher incidences. Moreover, the MW appears to not affect gross employment flows based on either work schedule or contract duration. These findings suggest that the MW may not induce large changes in the composition of employment, at least regarding labour market entries and exits, dissimilarly affecting workers differently depending on their work schedule and contract type. [20]

[19] Results not reported, but available upon request.

[20] As a robustness check, we repeated the analysis separately for women and men. The results remain stable across genders, with estimated effects of similar magnitude and statistical significance to those obtained in the main analysis. This suggests that the findings are not driven by gender-specific dynamics.

Figure 3.8: Results of the panel data model in first differences estimated on different dependent variables based on the combination of contract type and work schedule



Notes: coefficient β from Equation 3.1 — point estimate and confidence intervals. The years 2022 and 2023 are excluded from the analysis due to the potential impact of the labour reform of December 28, 2021.

3.6.2 Heterogeneous effects across provinces by MW exposure

While the baseline results capture the average effect of changes in MW incidence across provinces, an important question is whether these effects differ depending on the degree of provincial exposure to the MW. Provinces in Spain display heterogeneity in the share of workers initially affected by MW changes, leading to different employment responses.

We begin by computing the average incidence of the MW for each province over the period 2015–2023 and classify provinces into three groups—low, medium, and high exposure—based on terciles of this distribution. Table B.2 of the Appendix reports descriptive statistics for each group, including average hiring rates, separation rates, and net employment changes.

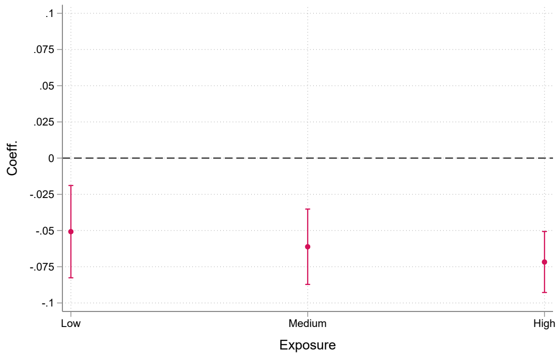
The figures show differences in the level of employment flows across provinces with different degrees of exposure to the MW. Provinces in the low-exposure group display higher levels of hiring, separations, and net employment compared to provinces with medium and high exposure, which exhibit more similar labour market dynamics. These differences likely reflect structural characteristics of local labour markets, such as sectoral specialization, firm size distribution, and the prevalence of low-wage employment. These descriptive patterns suggest that provinces differ not only in their exposure to the MW, but also in their baseline employment dynamics, providing a strong motivation to explore whether changes in MW incidence have heterogeneous effects across regions.

To assess whether the impact of MW incidence differs by exposure level, we estimate an extension of Equation 3.1 by interacting changes in MW incidence with indicator variables for high-, medium-, and low-exposure provinces. This approach allows the marginal effect of MW incidence on employment flows to vary systematically across exposure groups, while maintaining the same identification strategy based on within-province variation over time.

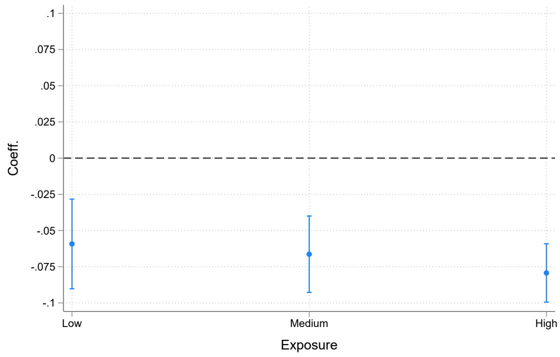
Figure 3.9 reports the estimated effects of changes in MW incidence on hiring and separation rates by provincial exposure group. Across all groups, increases in MW incidence are associated with reductions in hiring and separation rates. Moreover, these negative effects tend to be larger in provinces with higher average exposure to the MW over the period. However, tests indicate that the differences in the coefficients across exposure groups are not statistically significant. This implies that the response of employment flows to changes in MW incidence does not differ across provinces with different exposure levels. Overall, the results suggest that the negative effects on gross employment flows are broadly similar across regions, even in provinces with higher average exposure to the MW.

Figure 3.9: Effects of MW incidence on outcome variables (Y) when provinces are grouped according to their average incidence over the entire period

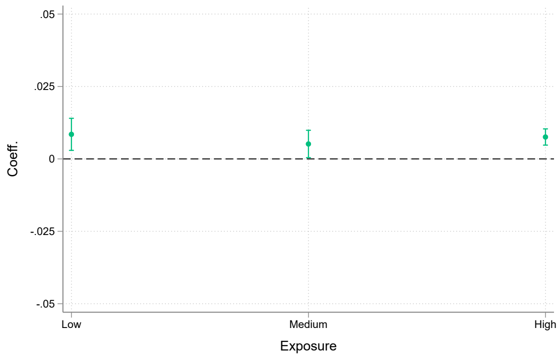
(a) Effect of MW Incidence on hirings by exposure



(b) Effect of MW Incidence on separations by exposure



(c) Effect of MW Incidence on net employment by exposure



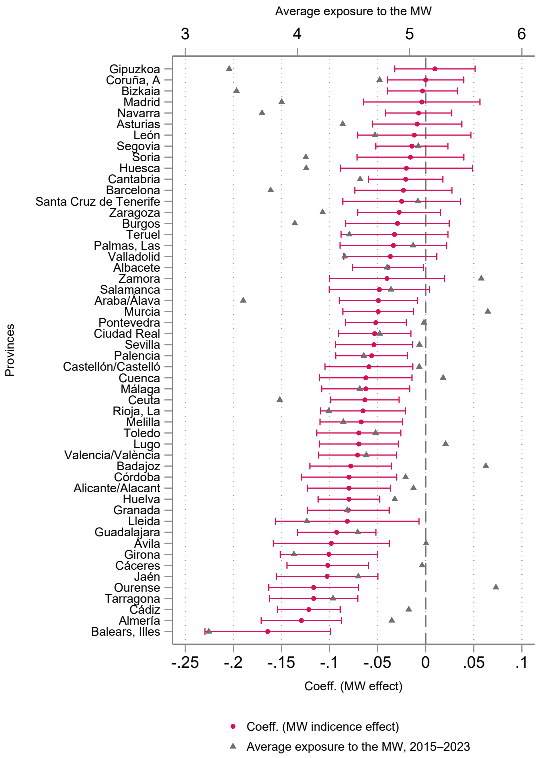
Notes: coefficient β from Equation 3.1 — point estimate and confidence intervals at the 95% level. See also the note to Table B.2.

Finally, to further explore potential heterogeneity beyond broad exposure categories, we estimate province-specific responses by exploiting province-level differences in MW incidence. This exercise provides a ranking of provinces according to the estimated impact of MW incidence on hiring, separations, and net employment. Given the limited number of observations per province and the exploratory nature of this analysis, the results should not be interpreted causally. Instead, they serve to illustrate the distribution of estimated effects across provinces and to identify whether a small number of local labour markets drive the aggregate results. The full set of province-level estimates is reported in Figure 3.10, together with average MW exposure over the entire period.

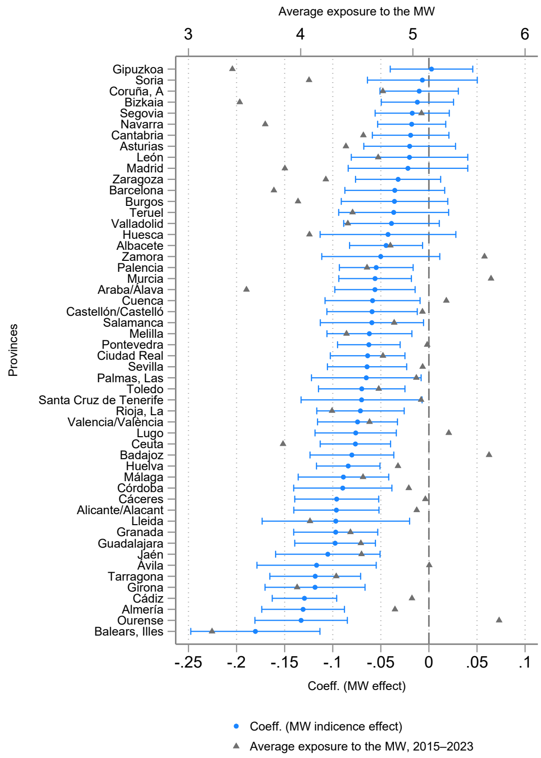
This analysis shows that there are provinces that exhibit similar patterns of employment inflows and outflows and net employment, regardless of their MW incidence. At the bottom of the ranking, provinces with relatively low and medium (the Balearic Islands, Lleida, Girona) as well as high (Ourense, Almería, Cádiz, Ávila, Cáceres) average levels of exposure to the MW show relatively large and similar negative effects on hiring and separation rates from MW exposure, contributing to a net zero employment effect. A similar pattern is observed at the opposite end of the ranking by size of the estimated impact, with provinces with either low and medium (Gipuzkoa, Bizkaia, Madrid, Navarra, Soria. Huesca, Barcelona) or high (A Coruña, León, Tenerife, Las Palmas) exposure to the MW showing zero effects on hiring and separations, also resulting in small or null employment net effects. These findings indicate that while net employment impacts are limited, turnover effects vary substantially across provinces; in some provinces hiring and separation respond strongly to MW changes but cancel out, whereas in others both effects are minimal. In summary, the MW appears to be a mechanism that reduces labour turnover, especially in markets with a high prevalence of low-wage jobs, without generating net job losses.

Figure 3.10: Effects of MW incidence on outcome variables (Y) by provinces

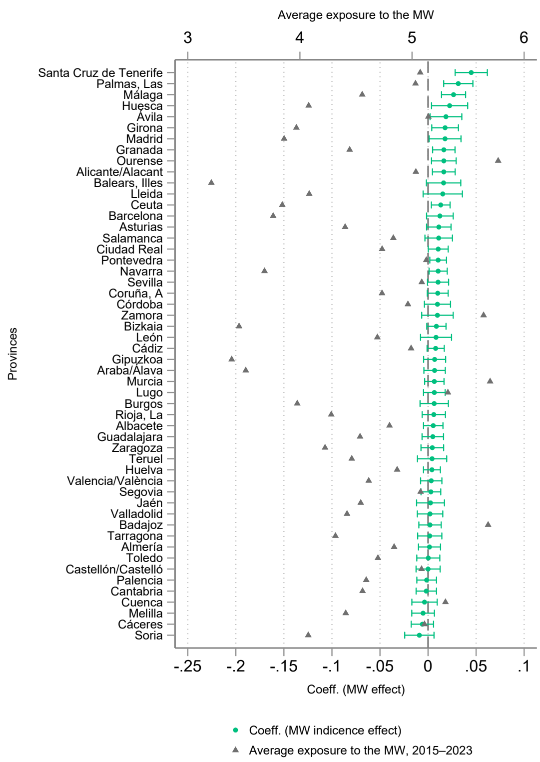
(a) Effect of MW Incidence on hirings by provinces



(b) Effect of MW Incidence on separations by provinces



(c) Effect of MW Incidence on net employment by provinces



Notes: coefficient β from Equation 3.1 — point estimate and confidence intervals at the 95% level

3.7 Discussion and conclusions

This article has examined the effect of increases in the Spanish MW on labour market dynamics, specifically on gross employment flows (job entries and exits) during the period 2015–2023. To this end, a panel data model in first differences at the province level has been estimated, considering each province's exposure to changes in the MW. The main findings are as follows.

First, the results indicate that the impact of the MW on gross employment flows has been modest over the period analysed, implying that increases in the MW did not lead to significant changes in net employment levels. Thus, a higher incidence of the MW translates into a lower volume of total job inflows and outflows, suggesting modest retention and job creation effects. Therefore, the impact on net employment is limited, although positive, reflecting that the reduction in separations is only slightly larger than the reduction in hires.

Second, as regards the composition of hires and separations, in provinces with a higher effective incidence of the MW, we find that the increase in the latter leads to a rise (a decline) in part-time (full-time) hires and separations although the estimated effects are close to zero. The fact that the net employment effect is not statistically significant indicate that we do not find a reduction in full-time contracts to offset the higher costs associated with MW increases. Furthermore, the analysis by contract duration shows that MW hikes do not have a significant effect on the hiring or separation of temporary and permanent workers. In addition, more disaggregated results that combine four employment categories do not appear to show significant effects on either job entries or exits.

The findings of this study are consistent with a large body of empirical research that finds no effects of MW hikes on overall employment or on the employment levels of the groups directly affected (Dube, 2019a). They also are consistent with a more limited, but growing, body of literature examining gross employment flows that finds negative/null impacts of increases in the MW on hires and separations, particularly in Europe (Portugal and Cardoso, 2006; Bryan et al., 2013; Dustmann et al., 2022), but also in North America (Brochu and Green, 2013; Dube et al., 2016; Liu et al., 2016; Gopalan et al., 2021).

The results in this paper are well-aligned with several aspects of the MW adjustment process highlighted in models of imperfect competition in the labour market, where market frictions—such as imperfect information, mobility costs, heterogeneous preferences—affect employer-employee matching and decision-making. In 'job ladder' models, increases in the MW affect the offer wage distribution and the rate at which workers quit their jobs. In matching models with heterogeneous vacancy costs, quality matches are achieved after an initial probation period, so that an increase in the MW affects the decisions of employers on layoffs and hires. In these types of models, in response to a MW hike, employers may reduce hiring rates as they become more selective and create fewer new positions, and at the same time workers are less likely to quit or be laid off, especially when higher wages improve job retention. The result is that labour mobility decreases, i.e. there are fewer job-to-job and employment-to-non-employment transitions. Similarly, institutional-behavioural models also predict that MW hikes can reduce accessions and separations, since employers may use other channels to adjust to increasing labour costs. The same is true for monopsony models, in which the employment level is a function of the degree of competition in the market, so that, upon a rise in the MW, employers pay more - although still below marginal productivity - and incumbent workers remain within the same firm, resulting in higher job retention and lower job-to-job separations and worker turnover decline.

Several studies have provided empirical support for some of these mechanisms. Some authors (Riley and Bondibene, 2017; Mayneris et al., 2018; Harasztosi and Lindner, 2019; Drucker et al., 2021; Dube and Lindner, 2024) have found that firms may absorb rising labour costs through alternative channels, which would reduce their gross employment flows: productivity gains, increased working hours, reductions in non-wage labour costs, price increases, or lower profit margins (without necessarily passing the wage increase onto prices). Others provide evidence that the degree of monopsonistic competition in the labour market influences the effects of wage increases on employment, with the effect being less negative—or even positive—in more concentrated labour markets (Azar et al., 2024; Munguía, 2020). This implies that there is more scope to increase wages in high-concentration regions and industries where wages tend to be below marginal productivity.

There are several avenues of future research regarding the effects of the MW on employment that remain unexplored. One such avenue is the medium- and long-run effects of cumulative MW hikes relative to one-off increases. Another is nonlinearities in the employment effect from MW hikes that are relatively large and that depart from high levels. This is relevant because governments have been increasingly experimenting with the MW policy after the Great Recession. Evidence is limited and inconclusive, ranging from small effects (Sturn, 2018; Harasztosi and Lindner, 2019; Giupponi et al., 2024) to larger negative effects (Clemens and Strain, 2018).

[21] Finally, it would also be interesting to examine the potential spillover effect of MW increases on collectively bargained wage floors and their effects on employment and wage structures in the European context.

[21]The Spanish evidence gathered in this paper would suggest that countries where the MW is low relative to the average/median wage and a gap between productivity and wages exists have more room to increase the MW without damaging employment.

Chapter 4

Unemployment benefits and job finding: Evidence from Spain's policy reforms across the business cycle[1]

Abstract

This paper examines how unemployment insurance (UI) generosity affects unemployment duration by exploiting two reforms implemented in Spain under contrasting macroeconomic conditions. In 2012, the replacement rate was reduced after six months of benefit receipt, whereas in 2023 it was increased. Using universe-level administrative data from the Spanish Public Employment Service, we estimate causal survival models to identify the impact of changes in benefit generosity on unemployment duration and to explore heterogeneity by potential benefit duration and benefit level. The results show that the 2012 reform reduced aggregate benefit duration by 25.6%, while the 2023 increase led to a modest 6.5% extension, highlighting the asymmetric sensitivity of job search behaviour to benefit cuts versus increases across different phases of the business cycle. Responses are heterogeneous: individuals with medium-to-high benefits and shorter entitlement durations react more strongly to changes in generosity.

Keywords: unemployment benefits, job finding, policy reform, business cycle, duration data models, impact evaluation, administrative data

JEL: C52, J64, J65.

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[1]The resulting paper is co-authored by José María Arranz and Carlos García Serrano. The authors acknowledge financial support received by Institute for Fiscal Studies (IEF). This manuscript is currently under second-round review at the International Journal of Manpower.

4.1 Introduction

The aim of unemployment insurance (UI) systems is to smooth consumption for households facing job losses while balancing the generosity of benefits against potential disincentives to search for work. Striking this balance is especially relevant in periods of macroeconomic turbulence, when both demand for income support and concerns about labour market efficiency are heightened. This paper examines the effects on the duration of unemployment of two opposite reforms to the amount of UI benefits in Spain. These reforms were implemented under markedly different macroeconomic conditions, providing a unique opportunity to study how economic cycles shape behavioural responses to changes in benefit generosity. To do so, we use rich administrative data and an empirical strategy based on causal survival models (Hernán and Robins, 2020).

In 2012, amid a deep recession and fiscal crisis, the Spanish economy faced a critical situation, in which the unemployment rate reached a peak of 25%, compared to 11% in the EU-28. In response to growing concerns about fiscal sustainability and labour market performance, the conservative government reduced the benefit replacement rate (RR) from 60% to 50% after the sixth month of receiving UI, while keeping the initial RR (before the sixth month of receiving UI) unchanged at 70%. The reform was explicitly aimed at reducing public spending and encouraging searching for jobs in a context of austerity and depressed labour demand.

A decade later, following a sustained recovery interrupted only by the COVID-19 pandemic, the Spanish labour market remained fragile, but macroeconomic conditions had improved. In December 2022, with unemployment still high (13.2%, the highest in the EU), the government enacted a reform that reversed the 2012 change, increasing the RR back to 60% after the sixth month (keeping the 70% RR for the first six months unchanged). Unlike the 2012 reform, this policy emphasized income support over incentives to search for jobs, reflecting a more stable economic environment and the availability of fiscal space therein.

This contrast in policy is not only relevant in itself but also allows for a comparative evaluation of labour market behaviour during different phases of the business cycle. The theoretical and empirical literature has long emphasized the importance of benefit duration and generosity for spells of unemployment (Katz and Meyer, 1990; Lavoie and Seccareccia, 2004; Tatsiramos and van Ours, 2014; Card et al., 2015); however, the magnitude of these effects is likely to be context dependent. During economic recessions, when job vacancies and hiring opportunities are scarce, the disincentive effects of extended or more generous unemployment benefits on job search behaviour tend to be limited, because the primary constraint for unemployed individuals is the lack of available jobs rather than the level or duration of benefits. Conversely, during periods of economic expansion characterized by a stronger labour demand, the generosity of unemployment benefits can exert a more direct influence on the intensity and duration of job searches, as workers may have greater incentives to prolong their searches or be more selective in light of the economic support provided. Yet, the empirical literature has devoted limited attention to how cyclical conditions mediate the effects of UI generosity, particularly in using within-country comparisons under a stable institutional framework (Bell et al., 2024).

This distinction is particularly relevant given certain theoretical contributions (Landais et al., 2010; Tatsiramos and van Ours, 2014), which argue that the effects of UI generosity are not constant over the business cycle. In recessions, when job rationing dominates and search externalities arise, the disincentive effects of more generous UI tend to be limited, while the value of consumption smoothing increases. Conversely, during expansions, job search incentives may become more responsive to benefit generosity. This framework provides a compelling rationale for analysing how behavioural responses to UI reforms differ across macroeconomic environments, which is a central objective of this paper.

Furthermore, these effects may also differ depending on the potential duration of the UI benefit, which is not the same for all recipients. This is the case in the unemployment benefit system of most developed countries, where the entitlement period varies depending on each worker's prior contributions. It might be thought that changing the amount of the UI benefit could have a greater impact on the behaviour of recipients with shorter entitlements than on that of those with longer ones, since the change in the generosity of the first parameter (the amount) would be quantitatively more relevant for those who enjoy less generous values of the second parameter (the potential duration). This interaction between changes in the benefit amount and the potential benefit duration (PBD) has scarcely been studied empirically (Jessen et al., 2025).

The main contribution of this article is twofold. First, it analyses how changes on the level of UI benefits interact with PBD. To do so, it focuses on Spain, a country with persistently high unemployment and a segmented labour market, which offers a relevant case for understanding UI reforms in fragile labour market contexts, and exploits detailed administrative data from the Spanish Public Employment Service (PES); this enables precise identification of benefit amounts, potential durations and claimant characteristics, allowing for accurate classification of treatment and control groups based on observable entitlement characteristics, thereby improving estimation precision. Second, the article takes advantage of a unique opportunity to compare two contrasting policy reforms implemented under very different economic conditions within the same country. Spain's persistently high unemployment makes it an ideal setting to observe how the labour market responds to opposite policy changes, providing insights into the interaction between UI policy and macroeconomic context without introducing cross-country variation. By highlighting the interaction between UI policy and the business cycle, this paper contributes to the broader literature on UI and labour market dynamics. The findings have implications for the design of countercyclical policies that adjust to macroeconomic conditions, especially in countries facing structural labour market challenges.

The paper is structured as follows. Section 4.2 reviews the literature on the effects of UI benefit levels on the duration of unemployment, while Section 4.3 outlines the institutional context of the Spanish UI system and provides a detailed description of the two reforms under study. Section 4.4 presents the empirical strategy, Section 4.5 describes the administrative data set and sample construction and offers descriptive statistics of the key variables. Section 4.6 reports the estimation results and Section 4.7 provides the corresponding robustness analysis. Finally, Section 4.8 concludes by summarizing the main findings and discussing their implications for policy and future research.

4.2 Literature review

Job search models have long provided a theoretical foundation for studying the impact of UI on the duration of unemployment (Mortensen, 1977; Lippman and McCall, 1979; Van den Berg, 1990). These frameworks assume that unemployed individuals receive wage offers and decide whether to accept them based on a reservation wage – the minimum acceptable wage that balances the expected benefits of continued search against the costs of remaining unemployed.

These models yield clear predictions regarding the two main policy parameters of UI systems: the benefit level and the PBD. Higher benefit amounts tend to increase reservation wages and, in some cases, reduce the intensity of searches, thereby lowering the likelihood of accepting jobs and prolonging unemployment spells – a mechanism often interpreted as a moral hazard effect. In contrast, time-limited benefits introduce a dynamic component to search behaviour: as the entitlement period nears its end, the value of remaining unemployed diminishes, leading to declining reservation wages and greater search efforts. Consequently, the likelihood of job acceptance typically rises over the course of the spell.

The literature thus emphasizes that UI generosity shapes job search behaviour in different ways across the unemployment spell. PBD has a stronger influence toward benefit exhaustion, while benefit levels exert their main effect early in the unemployment episode.[2] Overall, more generous UI tends to extend the duration of unemployment, although the magnitude and timing of this effect depend on institutional rules and worker characteristics.

Moving beyond individual-level, partial equilibrium models, general equilibrium search models incorporate both firm and worker behaviour (Pissarides, 2000). In these settings, wages are determined endogenously through bargaining, based on the surplus generated by a match. Here, more generous UI raises the value of unemployment to workers, strengthening their bargaining position and pushing up wages. This, in turn, lowers firms' expected returns from hiring, leading to reduced vacancy creation and higher overall unemployment. These models also suggest that increased generosity may affect inflows into unemployment, as when benefits are relatively attractive, some low-productivity workers may voluntarily leave employment (Mortensen and Pissarides, 1994).

In this theoretical context, accounting for the business cycle becomes crucial when analysing UI effects on unemployment duration and reemployment probabilities. Understanding whether policies that alter the generosity of unemployment benefits should exhibit procyclical or countercyclical characteristics requires the consideration of the relationship between UI design and macroeconomic conditions. Several theoretical contributions (Landais et al., 2010; Andersen and Svarer, 2011; Landais et al., 2018) argue that UB should be designed in a countercyclical manner. In economic downturns, the scope for moral hazard is reduced due to limited job availability, while the consumption-smoothing role of UI becomes more salient. Consequently, the disincentive effects associated with higher replacement rates during recessions are substantially smaller than during economic expansions. By contrast, Mitman and Rabinovich (2015) suggest that optimal UI benefits may be either countercyclical or procyclical, depending on the budget.

The empirical literature has extensively documented how UI design affects job search behaviour and unemployment duration, focusing primarily on potential duration and benefit levels – findings that align broadly with theoretical predictions.

Most studies assessing changes in PBD adopt difference-in-differences (DD) techniques with

[2]Weekly administrative data from the United States show sharp increases in exit hazards as benefit exhaustion nears: Katz and Meyer (1990) estimate a 94% rise from six to two weeks before expiration, with an additional 78% increase in the final week. Similar patterns are observed in Europe: in Austria, Card et al. (2007) report that the exit hazard is 2.4 times higher in the week of benefit exhaustion than more than twelve weeks before. Lalive et al. (2006) further show that extending benefits shifts this hazard peak to the new exhaustion date. These results highlight that job search intensifies as workers approach the end of their entitlements, consistent with Mortensen (1977) job search model, and underscore the importance of PBD in shaping unemployment dynamics.

duration models to estimate causal effects. These studies generally find that extended benefit durations reduce unemployment exit rates, although estimates vary. For instance, Lalive and Zweimüller (2004) show that a 1991 reform in Austria extending UI entitlement significantly reduced job finding rates by 17%, and in a similar context, Lalive et al. (2011) evaluate a reform that increased PBD by nine weeks for workers aged 40–49 and 22 weeks for those over 50, finding unemployment increases of 26% and 56%, respectively. In Portugal, Novo and Silva (2017) study a 1999 extension of benefit duration and estimate an increase in the length of unemployment of 42 days for younger workers and 110 days for those aged 40–44. Evidence from the U.S. also indicates that there is a positive relationship between extended benefit duration and unemployment duration (Katz and Meyer, 1990; Card and Levine, 2000). Conversely, reductions in benefit duration appear to strengthen search incentives and hasten re-employment, with van Ours and Vodopivec (2006) providing such evidence from a reform in Slovenia.

Studies on UI benefit generosity yield similar results. Carling et al. (2001) examine a cut in the RR from 80% to 75% in Sweden in 1996 and find, using duration models, that the reform raised the unemployment exit rate by 10%. Similarly, Doris et al. (2020) analyse a benefit cut affecting young workers during the Great Recession in Ireland and show that it shortened the duration of unemployment. In the U.S., Card et al. (2015) use regression discontinuity (RD) designs to show that benefit reductions significantly shorten the duration of unemployment, with elasticities being particularly large during recessions. Similarly, Meyer and Mok (2014) examine a 1989 benefit cut in New York and estimate a benefit–duration elasticity of 0.1–0.2. In contrast, Uusitalo and Verho (2010) document that a benefit increase in Finland in 2003 reduced job-finding rates by 17%.

In regard to UI design and economic conditions, empirical evidence supports the view that the disincentive effects of UI extensions are weaker during recessions. Studies for the United States and Germany show that extending benefit duration in periods of slack economic activity can be welfare enhancing, even when the direct effects on unemployment exits are modest (Kroft and Notowidigdo, 2016; Rothstein, 2011; Schmieder et al., 2012). Taken together, the theoretical and empirical contributions on this area suggest that the behavioural effects of UI are not constant over the business cycle but instead vary with macroeconomic conditions, providing a rationale for countercyclical adjustments in both benefit levels and durations. Nevertheless, evidence on the cyclical sensitivity of UI effects within countries operating under stable institutional frameworks remains limited.

While most evidence on UI design and unemployment duration comes from formal labour markets, the role of informal employment merits consideration. In this area, evidence from countries where informal work is widespread provides some insights. Gerard and Gonzaga (2021) show that in Brazil behavioural responses to UI are largely driven by transitions from informal to formal employment: after UI exhaustion, formal reemployment rises not because more workers leave non-employment, but because some move from informal to formal jobs. Similarly, Filiz (2017) finds in Turkey that longer UI entitlements reduce the probability of "cheating", defined as receiving UI benefits while simultaneously holding a wage-earning job, which often involves informal work. These findings highlight that UI design and generosity may affect reemployment differently depending on the prevalence of informal work, suggesting that studies relying solely on formal sector data—like most Spanish and European studies—may underestimate certain behavioural channels present in high-informal contexts.

In the Spanish context, the 2012 reform was previously analysed by Rebollo-Sanz and Rodríguez-Planas (2020). Using DD and RD techniques applied to duration models, with a control group of workers with fewer than 181 days of UI entitlement, they find that the reform reduced the average unemployment duration by 14%, or approximately 5.7 weeks. While valuable, the data set they use (the Continuous Sample of Working Lives) requires imputing entitlement duration based on workers labour histories and does not include information about the level of benefits, which may introduce measurement error and limit subgroup analyses. More generally, Bover et al. (2002) analyse the effects of unemployment benefit duration and business cycle conditions on unemployment duration using longitudinal data on Spanish men from 1987–1994, emphasizing the role of cyclical conditions. By contrast, our analysis examines whether changes in UI generosity affect unemployment duration differently across phases of the business cycle.

Some reforms have simultaneously changed both PBD and benefit levels, but only a few studies have analysed these joint effects. Lalive et al. (2006) evaluate a 1989 Austrian reform that raised both entitlement duration and RRs, finding that both parameters reduce unemployment exit rates but operate at different stages, with benefit levels mattering most early in the spell, and duration mattering more near exhaustion. In Spain, Arranz et al. (2009) study a 1992 reform that reduced both benefit amounts and entitlement duration, plus stricter eligibility rules, and find that a 10% reduction in benefit levels increased exit rates by 5%, while cutting benefit duration raised them by 2%. Relatedly, Jessen et al. (2025) analyse the interaction between changes in benefit levels and PBD. Using Polish data, they find that a 10% increase in benefit levels raises the duration of unemployment modestly by 0.06 months for workers with short entitlements (6 months), while the same benefit rise increases unemployment duration by 0.24 months for workers with longer entitlements (12 months).

4.3 Institutional background

As in most OECD countries, Spain provides unemployment benefits through two distinct programmes, namely contributory (UI) benefits and non-contributory, unemployment assistance (UA) benefits. The former targets individuals who have involuntarily lost their jobs – that is, those dismissed or whose fixed-term contracts have ended – and who have contributed to social security for at least 12 months within the last six years. Individuals receiving full disability benefits, those aged over 65 and those who voluntarily left their jobs are not eligible for UI.

The potential duration of UI depends on the number of months for which individuals have contributed (see Table 4.1). Starting from a minimum of four months, the benefit duration increases in two-month increments up to a maximum of two years. For example, individuals who have contributed for between 12 and 18 months are entitled to four months of benefits, while those with 19 to 24 months of contributions qualify for six months, and so on, up to a maximum of two years.

Table 4.1: Duration of UI benefits in Spain

Months of Contributions	Benefit Entitlement Period (days)	UI Entitlement Period (months)
12–17	120	4
18–23	180	6
24–29	240	8
30–35	300	10
36–41	360	12
42–47	420	14
48–53	480	16
54–59	540	18
60–65	600	20
66–71	660	22
>72	720	24

The amount of UI is determined by the *gross RR*, which represents the percentage of the regulatory base (the average salary on which contributions were made in the six months prior to job loss) received as unemployment benefit. The RR varies with the duration of the benefit period (see Table 4.2). Until 2012, the RR was set at 70% of the regulatory base for the first 180 days and dropped to 60% from day 181 onwards; however, this RR after day 181 has undergone two major modifications: in 2012 and 2023.

Prior to July 15, 2012, monthly UI benefits were 70% of the regulatory base during the first 180 days; they then decreased to 60% from day 181 until benefit exhaustion or re-employment. After the 2012 reform, the RR remained at 70% for the first 180 days but dropped to 50% from

Table 4.2: Changes in UI benefit replacement rates after the 2012 and 2023 reforms in Spain

April 1992 – June 2012		July 2012 – December 2022		Since January 2023	
RR	**UI period**	**RR**	**UI period**	**RR**	**UI period**
70%	1–6 months	70%	1–6 months	70%	1–6 months
60%	7–24 months	50%	7–24 months	60%	7–24 months

Notes: UI period refers to the entitlement period for unemployment insurance benefits.

day 181 onwards, representing a reduction of 16.66% in the RR beyond six months of receiving benefit. This change reinforced the negative relationship between benefit levels and unemployment duration, a relationship supported by various academic studies and international organizations aiming to reduce the disincentive effects of extended benefits (Cahuc and Lehmann, 2000).

More than a decade later, following successive reductions in unemployment and improvements in the economic environment, the government implemented a new reform effective January 1, 2023, which reversed the 2012 change. The RR was increased from 50% back to 60% after day 181; therefore, the benefit amount was set at 70% of the regulatory base for the first 180 days (unchanged from previous policy reform) and 60% from day 181 until benefit exhaustion.

Two key institutional features must be taken into consideration to correctly interpret the following analysis. First, both the 2012 and 2023 reforms exclusively affected unemployed individuals entitled to more than 180 days of UI. According to Table 4.1, this group includes workers with at least 24 months of contributions prior to losing their jobs. Consequently, individuals who entered the unemployment protection system after the reforms but did not meet the minimum contribution requirements for over 180 days of UI benefits were unaffected by these alterations, and no behavioural changes would be expected from them. It is important to note the existence of minimum and maximum benefit amounts in the Spanish UI system. These thresholds are not fixed, as they vary depending on several factors, including the number of dependent children and, importantly, the number of working hours prior to job loss. As a result, each individual faces personalized minimum and maximum benefit levels. A significant proportion of recipients receives UI benefits close to, or at, the minimum threshold, and these individuals are unlikely to be affected by the reforms under analysis, since the changes in RRs typically apply to UI benefits above the minimum.

Second, and critically for the empirical strategy, Spanish unemployment protection legislation prohibits retroactive application of reforms. Each individual is subject to the regulations in force at the start of their unemployment spell, regardless of subsequent regulatory changes during the benefit period. This institutional feature allows for a clear identification of treated and control groups based on the timing of the implementation of the reforms. Specifically, it enables comparisons among individuals entering unemployment just days apart but subject to different benefit regimes due to the regulatory change. This institutional setting forms the foundation of the identification strategy employed in this study.

4.4 Empirical strategy

This section presents the econometric model used to evaluate the impact of the reduction (increase) in UI benefits implemented in July 2012 (January 2023) on the duration of unemployment while receiving UI. Exit rates from the contributory scheme are analysed using a mixed proportional hazard (MPH) duration model. The hazard (or exit) rate in the MPH model is represented as follows:

$$h_i\left(\frac{t_i}{X}, \theta\right) = \lambda_{0i}(t_i)\, \phi_i(X'\beta_i)\, \varphi_i(\theta), \quad i = 1, 2, \ldots, N \tag{4.1}$$

where ti denotes the current duration of the unemployment spell under the contributory scheme for individual i. In this model, the hazard rate of exiting UI benefits and transitioning into employment is influenced by three components. First, the function $\lambda_{0i}(t_i)$, referred to as the 'baseline hazard', captures the effect of elapsed time in UI benefits on the probability of finding a job, holding all other factors constant. Second, the function $\phi_i(X'\beta_i)$ represents the effect of observed explanatory variables on the hazard rate of exiting UI benefits. Third, $\varphi_i(\theta)$ accounts for unobserved heterogeneity, such as individual ability, motivation, skills or other latent characteristics that influence the probability of re-employment. To ensure that the hazard rate in Equation 4.1 remains non-negative, each of these components is modelled using an exponential specification, which guarantees the positivity of the overall hazard function.

To capture the effect of the explanatory variables on the hazard rate of exiting UI benefits, the following specification is used:[3]

$$\phi_i(X'\beta) = \exp\left(\beta_0 + \beta_1 Z_i + \beta_2 T_i\right) \tag{4.2}$$

[3] Exits from UI benefits are defined as: (a) transition to self-employment or (b) transition to paid employment. Non-exits are defined as: (a) benefit exhaustion, (b) transition to UA benefits or (c) other causes (e.g. administrative reasons).

The vector of explanatory variables X_i can be decomposed into several components. Specifically, Z_i denotes a vector of exogenous control variables – including personal, demographic and job characteristics. The variable T_i is a binary indicator for the policy change implemented in either 2012 or 2023, taking the value 0 for individuals who entered unemployment before the reform (control group) and 1 for those who entered after it (treatment group).

This study employs an MPH model with a piecewise constant exponential (PCE) baseline hazard, which is among the most widely used approaches for modelling continuous-time duration data (see Jenkins, 2005). This specification allows the hazard rate to remain constant within predefined time intervals while varying across them, offering flexibility to model time dependence without imposing a restrictive parametric functional form on duration. In our analysis, time is divided into 24 monthly intervals, which enables a detailed characterization of the time pattern of unemployment exit.

To efficiently account for unobserved heterogeneity across individuals, a frailty term following a gamma distribution is included – an approach commonly adopted in the literature. Additionally, to address potential biases arising from differences in observable characteristics between the treatment and control groups, we apply inverse probability weighting (IPW) following the methodological framework proposed by Hernán and Robins (2020).

4.5 Data and sample selection

4.5.1 Data and description of treatment and control groups

The data set used in this study was provided by the Spanish PES (*Servicio Público de Empleo Estatal*, SEPE), the public institution responsible for managing unemployment benefits in Spain. These administrative data offer an exceptional level of detail, providing individual-level information for the entire universe of UI recipients. The records include socio-demographic variables (gender, age, number of children, nationality, province and autonomous region of residence), characteristics from the most recent job (sector, occupation, contribution history and reason for job separation) and detailed information on the benefit received (start date, amount, potential duration and days already consumed).

Compared to other data sets commonly used for labour market analysis in Spain – such as the MCVL – this data set has the advantage of not requiring assumptions or imputations regarding key parameters of the unemployment benefit system (such as benefit bases and amounts, or entitlement periods), which substantially enhances the accuracy and robustness of the results. Furthermore, as the data set includes the entire population of recipients of both UI and UA benefits, it contains many observations, which enables disaggregated and subgroup analyses without compromising representativeness or statistical power.

Based on this data set, we construct two independent samples to evaluate the effects of the 2012 and 2023 reforms. To analyse the 2012 reform, which came into effect on July 16, we define the control group as individuals who entered UI benefits between July 10 and 13, and the treatment group as those who entered between July 17 and 20. Similarly, for the 2023 reform, implemented on January 1, the control group includes individuals who became unemployed under UI benefits between December 20 and 23, 2022, and the treatment group those who did so between January 4 and 7, 2023.

These time windows are selected based on a key identification criterion: in both cases, the control group consists of individuals who became unemployed before the reform was publicly announced (July 13, 2012 and December 24, 2022, respectively), which helps in minimizing potential anticipation effects and ensures that entering UI benefits is not influenced by the policy. Importantly, the individuals assigned to the treatment and control groups are selected within a very narrow time frame, which helps ensure that they are not differentially affected by seasonal fluctuations or short-term variation in labour market conditions. This design supports the assumption that treatment assignment – entering UI benefits before or after the reform – is quasi-random around the cut-off date (see the note to Figure 1). We exclude from the analysis the exact dates of reform implementation (July 16, 2012, and January 1, 2023), as they show abnormally high volumes of entry into UI benefits, potentially compromising group balance. [4]

Importantly, for both reforms, we restrict the analysis to individuals with an entitlement of more than 180 days of UI benefits. Those with shorter entitlements are excluded from both treatment and control groups, as they are not comparable to those with longer entitlements, particularly with respect to previous employment characteristics. This restriction ensures that the treatment and control groups are more homogeneous and supports the validity of the causal comparison. We also restrict the sample to workers under the age of 60, as the behaviour of older workers—who may face very different job search incentives and retirement pathways when dismissed—could otherwise bias the estimated results. Finally, we focus only on ex-

[4]We exclude the exact reform dates to ensure comparability across weekdays and avoid distortions driven by calendar effects. July 16 fell on Monday, and January 1 coincided with both the start of the month and a public holiday, dates that show unusually high inflows into unemployment benefits. Omitting these dates preserves balance between treated and control groups without affecting the interpretation of our main results. To address potential concerns, we conducted additional robustness checks using alternative windows around the reform dates. These exercises confirm that the estimated effects remain very similar, indicating that our results are not driven by the exclusion of the exact reform dates.

its from UI to employment (both paid and self-employment), excluding transitions through specific employment programmes (such as UI benefit capitalization) whose characteristics and requirements for participants have changed over time. This approach provides a clearer behavioural interpretation of job-search incentives and avoids the counterintuitive patterns that arise when including these exits.

Figure 4.1 shows the number of new UI spells within these selected windows. As can be seen, entries spike significantly on the implementation date – especially on January 1, which is also the first day of the month, a point at which unemployment entries tend to be systematically higher.

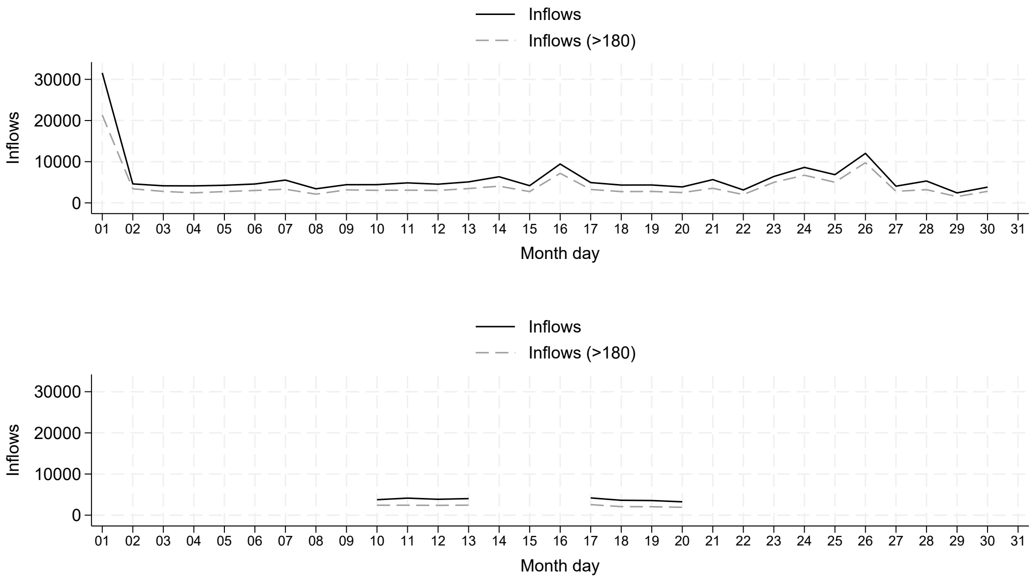
A key requirement for the identification strategy proposed in this paper is that the observable characteristics of control and treatment groups must be comparable – that is, average covariates should not differ significantly between the two groups. Meeting this condition allows us to isolate the causal effect of the reform on the duration of UI benefits, under the assumption that policy timing is exogenous.

In our samples, treatment and control groups exhibit broadly similar compositions; however, some statistically significant differences emerge in characteristics such as gender, potential UI benefit duration and occupation. To ensure comparability, we construct a pseudo-population using IPW, following (Hernán and Robins, 2020). In other words, the use of IPW allows us to approximate a randomized experimental setting by weighting observations by the inverse of the probability of treatment assignment, a method that is widely used in observational studies to adjust for covariate imbalances. Table 4.3 presents the covariate balance test after applying IPW (for the sake of comparison, Table C.1 of the Appendix provides the characteristics of the whole sample, including individuals with an entitlement above 180 days). No statistically significant differences remain among groups, supporting the interpretation of differences in UI benefit duration as causal effects of the 2012 and 2023 reforms.

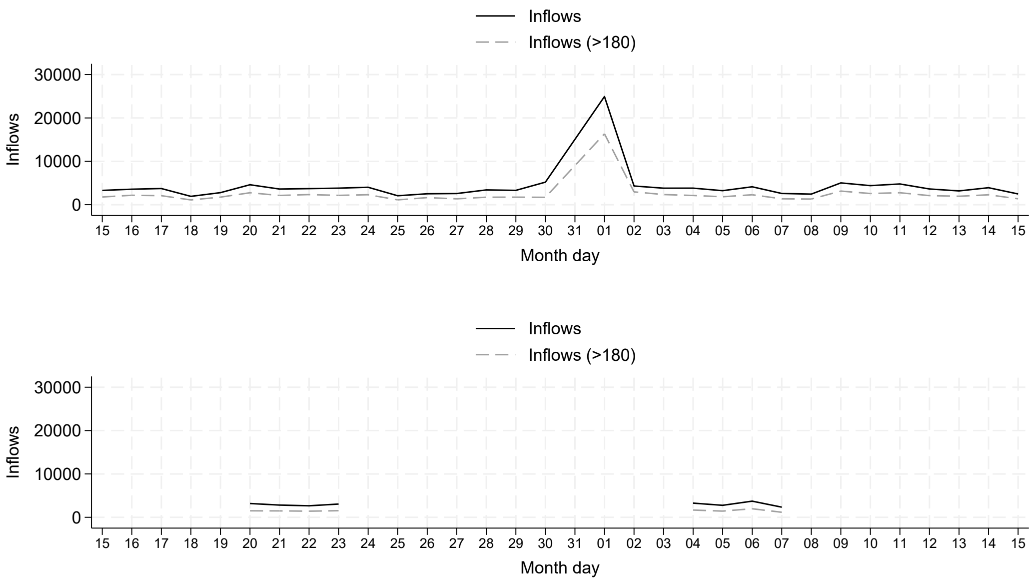
Moreover, the predominant characteristics of unemployed individuals in both samples (2012 and 2023) are very similar. However, in regard to exit behaviour from UI – the dependent variable in our econometric models – there is a substantial difference in the re-employment rate: in 2012, only 30% of individuals found a job, whereas in 2023 this figure rises to nearly 50%. This difference is consistent with the macroeconomic conditions prevailing in each period. In 2012, Spain was experiencing a deep recession with historically high unemployment rates, while in 2023, the country was undergoing an economic expansion, marked by a notable improvement in labour market conditions.

Figure 4.1: Total UI benefit inflows and inflows of individuals entitled to more than 180 days of benefits

(a) UI benefits inflows around the 2012 reform. Top: Daily UI inflows (full sample). Bottom: Selected comparison window



(b) UI benefits inflows around the 2023 reform. Top: Daily UI inflows (full sample). Bottom: Selected comparison window



Notes: The comparison windows are defined to (i) capture behaviour prior to the official announcement and (ii) exclude Mondays and the exact implementation day, which typically display atypical reporting patterns. To validate the identifying assumption, we perform a two-sample Kolmogorov–Smirnov test comparing the distribution of UI inflows in the pre- and post-reform periods within the selected windows. The test fails to reject the null of distributional equality at conventional significance levels. This supports the interpretation of inflows as an exogenous component. K-S for the 2012 reform sample: 0.750 (p-value of 0.221). K-S for the 2023 reform sample: 0.500 (p-value of 0.699). SEPE microdata.

Table 4.3: Characteristics of workers eligible for more than 180 days of UI benefits, before and after the 2012 and 2023 reforms

Characteristics	2012 Reform					2023 Reform				
	Before	After	Diff.	t	Sig.	Before	After	Diff.	t	Sig.
Gender										
Female	0.421	0.424	-0.003	0.35		0.460	0.459	0.001	-0.12	
Male	0.579	0.576	0.003	-0.35		0.540	0.541	-0.001	0.12	
Nationality										
Foreign-born	0.103	0.106	-0.003	0.63		0.136	0.138	-0.001	1.00	
Age group										
18–29 years	0.185	0.191	-0.005	0.83		0.198	0.200	-0.003	0.35	
30–39 years	0.380	0.380	0.000	0.05		0.313	0.311	0.002	-0.28	
40–49 years	0.268	0.265	0.003	-0.47		0.292	0.294	-0.002	0.23	
50–59 years	0.166	0.164	0.002	-0.37		0.197	0.195	0.002	-0.28	
Reason for job separation										
Dismissal	0.684	0.681	0.003	1.08		0.593	0.594	-0.002	0.17	
End of contract	0.316	0.319	-0.003	-1.08		0.407	0.406	0.002	-0.17	
Number of children										
No children	0.535	0.539	-0.004	0.55		0.560	0.565	-0.004	0.46	
One child	0.236	0.235	0.001	-0.18		0.231	0.226	0.005	-0.67	
Two or more children	0.229	0.226	0.003	-0.48		0.208	0.209	-0.001	0.14	
Sector										
Agriculture and fishing	0.034	0.035	-0.001	0.40		0.031	0.031	0.000	0.01	
Industry	0.163	0.160	0.003	-0.51		0.099	0.098	0.001	-0.16	
Construction	0.131	0.131	0.000	-0.02		0.093	0.093	0.000	0.05	
Services	0.670	0.672	-0.002	0.26		0.717	0.717	0.000	0.00	
Occupation										
High-skill occupations	0.175	0.171	0.005	-0.77		0.123	0.123	0.000	0.01	
Associate professionals	0.112	0.108	0.004	-0.72		0.084	0.084	0.000	0.08	
Clerical support workers	0.146	0.146	0.000	0.09		0.164	0.163	0.000	-0.06	
Service and sales workers	0.164	0.169	-0.005	0.81		0.235	0.234	0.001	-0.13	
Craft and trades workers	0.160	0.161	-0.001	0.21		0.130	0.130	0.000	0.00	
Plant and machine operators	0.078	0.079	-0.001	0.17		0.073	0.073	0.000	-0.03	
Elementary occupations	0.165	0.166	-0.001	0.21		0.191	0.192	-0.001	0.15	
UI potential benefit duration										
Group 0 (120–180 days)	–	–	–	–		–	–	–	–	
Group 1 (240–360 days)	0.288	0.295	-0.007	0.97		0.417	0.418	-0.001	0.16	
Group 2 (420–540 days)	0.147	0.148	-0.001	0.24		0.209	0.207	0.001	-0.19	
Group 3 (600–720 days)	0.564	0.556	0.008	-1.05		0.374	0.374	0.000	-0.01	
UI amount benefit quintile										
Quintile 1	0.217	0.198	0.019	-2.87	*	0.221	0.223	-0.002	0.31	
Quintile 2	0.205	0.211	-0.005	0.84		0.186	0.188	-0.001	0.19	
Quintile 3	0.190	0.191	-0.001	0.17		0.181	0.183	-0.001	0.17	
Quintile 4	0.265	0.277	-0.012	1.70		0.211	0.201	0.010	-1.32	
Quintile 5	0.123	0.124	-0.001	0.21		0.200	0.206	-0.005	0.69	
Unemployment exit										
Remain UI benefit	0.700	0.709	-0.008	1.08		0.502	0.518	-0.016	1.67	
Exit UI benefit	0.300	0.291	0.008	-1.08		0.498	0.482	0.016	-1.67	
Average UI benefit duration (days)	413.429	407.486				258.704	268.212			
Number of observations	9,589	8,504				6,181	6,467			

Notes: The selected entry windows correspond to individuals who became UI benefit immediately before and after the implementation dates of the 2012 and 2023 UI reforms in Spain. This comparison allows for the identification of comparable groups around the policy cutoffs, minimizing potential confounding due to seasonal or cyclical labour market variation.

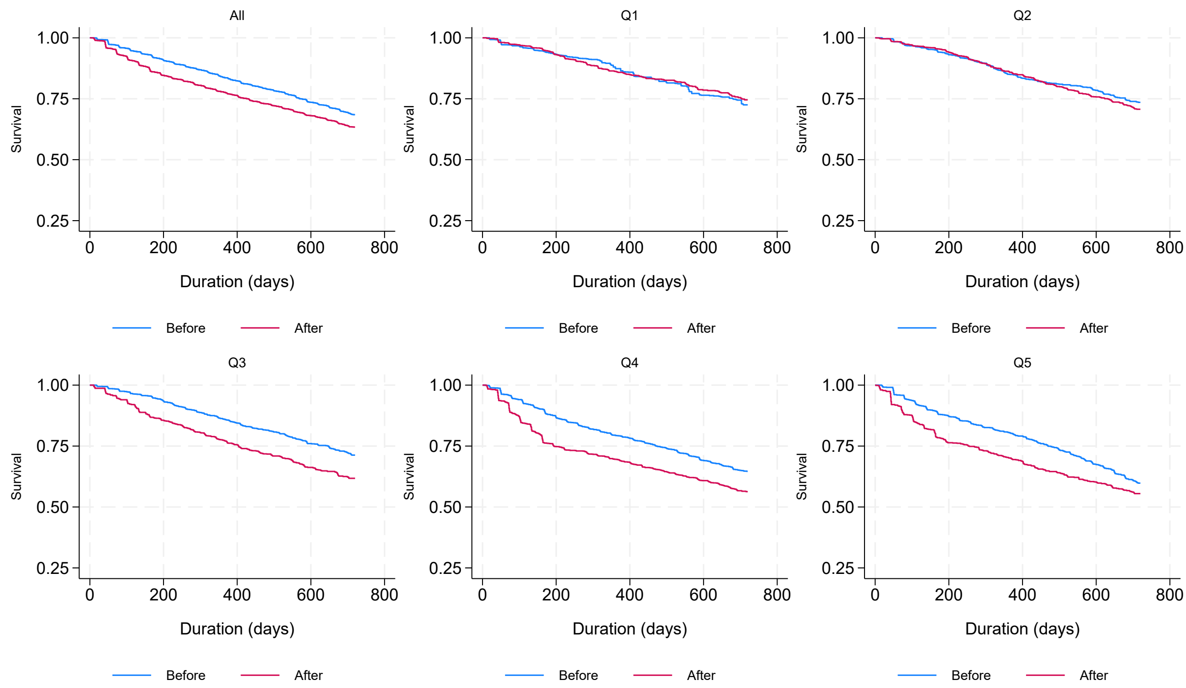
4.5.2 Non-parametric survival analysis

To conclude the descriptive analysis, we present non-parametric Kaplan-Meier survival curves comparing UI benefit duration between the affected (treated) and unaffected (control) groups in each reform. Note that this analysis excludes individuals with entitlement periods shorter than 180 days, as they are not comparable to those with longer entitlements. These curves are constructed separately for different subgroups, segmenting individuals by both the quintiles of the initial benefit amount and the PBD at the time of UI benefit entry. This double disaggregation facilitates a preliminary exploration of whether the impact of the reform on UI benefit duration varies according to the generosity of the UI system, in line with the hypotheses outlined in the theoretical framework. Results are presented in Figure 4.2 for the 2012 reform and in Figure 4.3 for the 2023 one.

The survival curves reveal distinct patterns among groups, anticipating some of the results that will later be quantified through duration models and a quasi-experimental strategy. Specifically, in the case of the 2012 reform, we observe a faster exit from UI among individuals receiving higher benefit amounts following the benefit cut, whereas the opposite pattern, i.e. a longer duration receiving UI benefit, is observed after the benefit increase in 2023. These differences suggest that the reforms may have altered job search incentives in a heterogeneous manner, depending on the level of protection received by the individual measured by the benefit level. However, when examining survival curves by PBD for both years, no clear differences emerge in the descriptive patterns.

Figure 4.2: Survival curves for the treatment group (post-reform) and control group (pre-reform): the 2012 reform

(a) By quintiles of UI amount



(b) By UI potential benefit duration

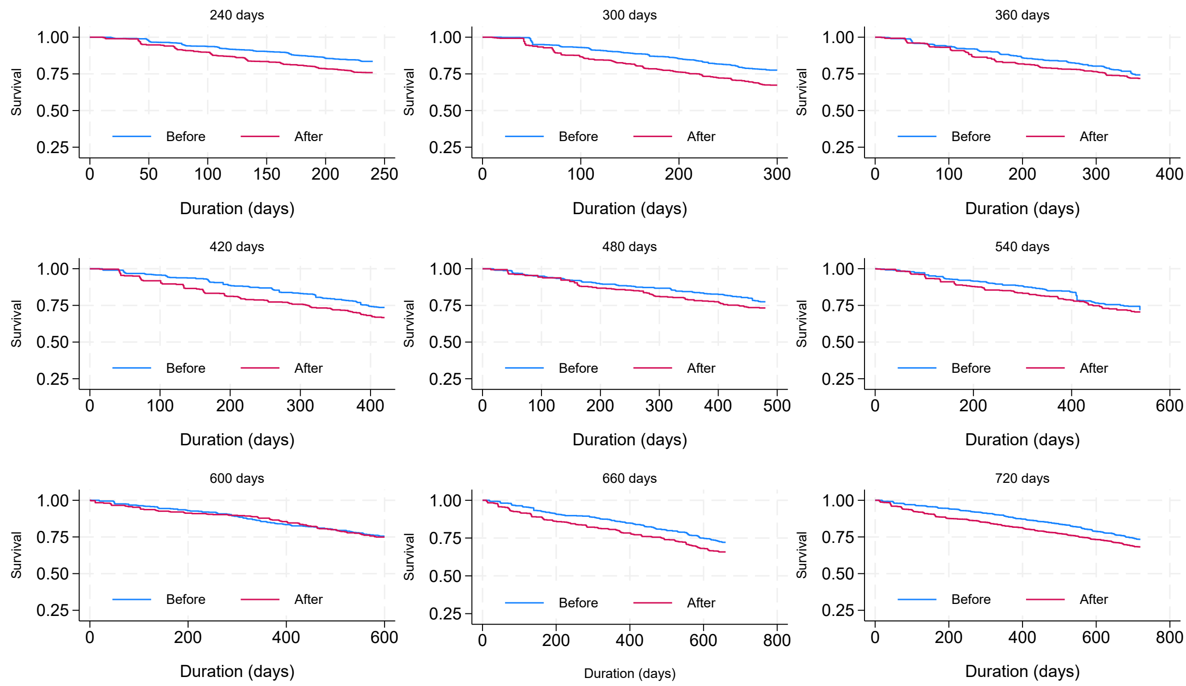
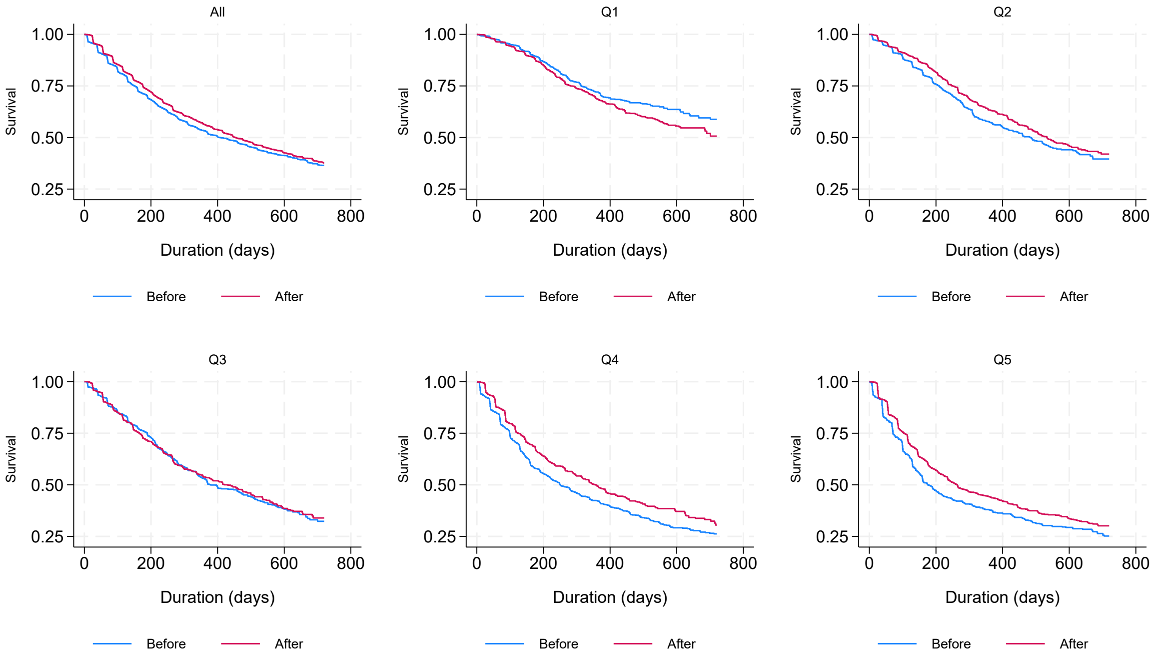
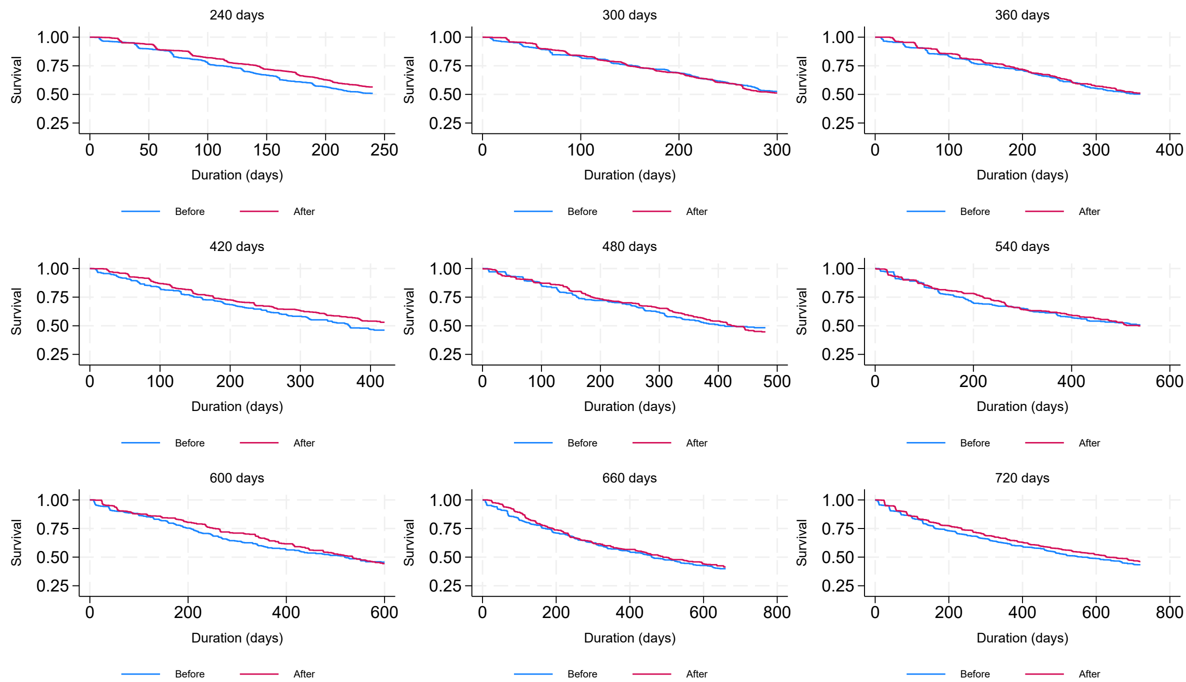


Figure 4.3: Survival curves for the treatment group (post-reform) and control group (pre-reform): the 2023 reform

(a) By quintiles of UI amount



(b) By UI potential benefit duration



4.6 Results

This section presents the estimation results that quantify the impact of changes in UI benefit generosity on UI unemployment duration. The analysis is restricted to workers entitled to more than 180 days of benefits – that is, those who accumulated at least 24 months of contributions – according to the eligibility criteria described in Table 4.1. As outlined in the institutional framework section, both reforms exclusively affected recipients with entitlements beyond 180 days, since the benefit reduction/increase was only applied after that threshold.

Results are disaggregated by benefit amount quintiles and PBD intervals. Two key elements significantly shape the duration of unemployment among recipients of UI benefits: the benefit amount and the potential duration of entitlement. These factors not only influence job search incentives directly but are also correlated with both observable and unobservable individual characteristics. Therefore, to adequately capture heterogeneity in the response to the reform, we report disaggregated results across both parameters of the UI system. We begin by analysing the 2012 reform, followed by the results for the 2023 one. The results are presented in Figure 4.4, *Panel (a)*, for the 2012 reform and in *Panel (b)* for the 2023 reform. More detailed information on the estimates can be found in Table C.2 and Table C.3 in the Appendix.

4.6.1 Impact of the 2012 reform

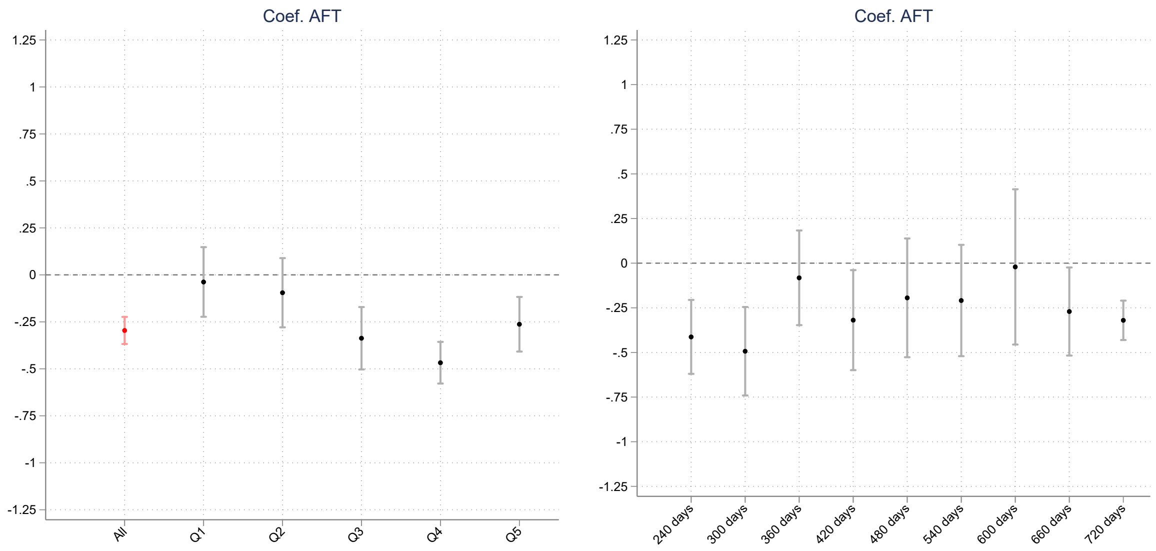
Figure 4.4, *Panel (a)*, presents the estimated coefficients (β_2) from the semi-parametric PCE model, disaggregated by benefit amount quintiles and by PBD groups. The results shown correspond to the coefficients from the accelerated failure time (AFT) specification.[5] The first key finding is that the estimated coefficient for the overall sample indicates that the reform had a negative and statistically significant impact on the UI duration. In particular, the cut of the UI benefit implemented in 2012 reduced the unemployment duration in 25.6%. However, there is substantial heterogeneity across the benefit amount distribution and, to a lesser extent, across groups defined by PBD.

With regard to benefit levels, statistically significant effects are observed in the third, fourth and fifth quintiles. For these groups, the reform had a significant impact, implying a reduction in UI benefit duration and therefore an increase in the exit rate from UI benefit, following the

[5] In AFT models, the dependent variable is the logarithm of the duration time, so that the coefficients are interpreted as changes in the speed at which the event occurs: a positive effect 'accelerates' the time to failure (shorter expected duration), while a negative effect 'slows it down' (longer expected duration). In proportional hazards (PH) models, the dependent variable is the instantaneous risk of failure, and the covariates are interpreted as multiplicative factors on that hazard: a positive coefficient implies a proportional and constant increase in risk over the entire time horizon.

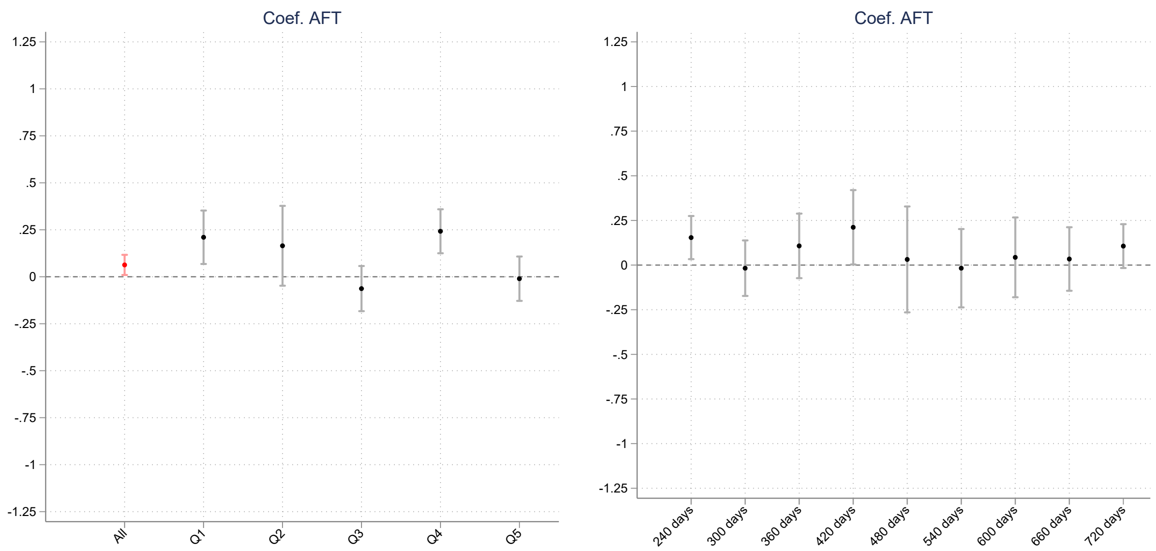
Figure 4.4: Estimated results from the IPW semiparametric PCE model

(a) IPW approach. Results from the 2012 reform (reduction in the gross RR) by quintiles of UI benefit amount (left) and by potential benefit duration (right)



Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification. The number of individuals (unique IDs) included in each model is as follows—Left panel: All: 17,365; Q1: 3,153; Q2: 3,725; Q3: 3,229; Q4: 4,983; Q5: 2,275. Right panel: 240 days: 2,458; 300 days: 1,584; 360 days: 1,168; 420 days: 917; 480 days: 838; 540 days: 834; 600 days: 862; 660 days: 1,270; 720 days: 7,434. The total number of person-period observations for each specification is reported in Table C.2 in the Appendix. SEPE microdata.

(b) IPW approach. Results from the 2023 reform (increase in the gross RR) by quintiles of UI benefit amount (left) and by potential benefit duration (right)



Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification. The number of individuals (unique IDs) included in each model is as follows—Left panel: All: 12,199; Q1: 2,656; Q2: 2,208; Q3: 2,159; Q4: 2,557; Q5: 2,619. Right panel: 240 days: 2,475; 300 days: 1,541; 360 days: 1,173; 420 days: 947; 480 days: 788; 540 days: 820; 600 days: 780; 660 days: 1,096; 720 days: 2,619. The total number of person-period observations for each specification is reported in Table C.3 in the Appendix. SEPE microdata.

implementation of the new benefit scheme. Specifically, individuals located in the third quintile experienced a 28.6% reduction in UI benefit duration, while those located in the fourth quintile experienced a 37.3% reduction and those in the fifth quintile a 23.1% reduction, compared to similar individuals who were not affected by the reform (i.e. those who entered the UI system just before the reform took effect).

A plausible explanation for this heterogeneity is that a substantial proportion of workers in the lower quintiles was already receiving the minimum benefit amount, or an amount close to the minimum, from the beginning of their UI benefit spell. In such cases, the reduction in the RR applied after day 180 had little to no impact on the actual amount received, which could explain the absence of significant effects among these groups. This pattern suggests that the impact of the reform was mostly concentrated in the upper part of the UI benefit distribution.

Regarding the PBD results, some groups exhibit significant effects on unemployment duration. For individuals entitled to 240 days of benefits, a 33.8% reduction in benefit duration is estimated, while for those entitled to 300 days, the reduction is 38.9%. Among groups with extended entitlement, a significant decrease is also observed: those entitled to 660 days show a 23.7% reduction, and those with 720 days, a 27.4% reduction. For groups with longer entitlements, these effects can be explained by the extended window during which they can be affected. Conversely, groups with limited entitlement, such as 240 days, experience a reduction despite their shorter coverage, which may reflect precarious labour market conditions, sufficient to be impacted by the reform.

In summary, the 2012 reform significantly reduced UI benefit duration on average, but the effects were concentrated among specific subgroups. Statistically significant reductions were observed for workers in the upper benefit quintiles and for specific groups defined by PBD, especially workers with shorter and larger entitlements, while effects for adjacent duration classes were null. This pattern suggests that reductions in UI benefit generosity primarily affect job search behaviour when they imply a meaningful loss of income. Furthermore, our overall results are broadly consistent with Rebollo-Sanz and Rodríguez-Planas (2020), who also report a reduction in unemployment duration, (25.6% compared to their 26%). The present study expands on their analysis by leveraging detailed information from SEPE administrative records on the parameters (amount and entitlement duration) of the UI system and a larger sample size, allowing us to document important heterogeneity in the effects of the reform.

4.6.2 Impact of the 2023 reform

Figure 4.4, *Panel (b)*, presents the estimated coefficients (β_2) from the semi-parametric model disaggregated by benefit amount quintiles and PBD, corresponding, as before, to the coefficients from the AFT specification. The aggregate results for the 2023 reform suggest a modest positive and statistically significant effect of the increase in the gross RR on UI benefit duration, with affected recipients, i.e. those who entered UI benefit after the increase in the RR, experiencing a 6.5% increase in UI benefit duration relative to comparable non-affected recipients, indicating a lower rate of exit from UI benefit. When disaggregating by benefit amount, this effect is concentrated exclusively in the fourth quintile, where affected individuals exhibited a 27.4% longer UI benefit duration than non-affected individuals. A plausible explanation for this, which is similar to that offered for the 2012 reform, is that individuals in this segment are not constrained by the lower or upper bounds of the benefit schedule, meaning that the increase in benefit generosity had a salient impact on their incentives. This pattern partially aligns with the 2012 findings, although no similar behaviour is observed in other middle- or high-income quintiles.

In terms of PBD, the estimation results indicate a statistically significant increase in the UI benefit duration only for workers entitled to 240 days of UI benefits. Specifically, affected individuals in this group experienced a 16.6% increase in UI benefit duration compared to their non-affected counterparts with the same entitlement. In summary, the results suggest that the 2023 reform led to a slight increase in UI benefit duration, though the effect was limited in terms of both magnitude and population coverage.

Comparing the effects of the 2012 and 2023 reforms reveals common patterns regarding worker sensitivity to changes in UI benefit levels. In both cases, the effects are concentrated among specific segments of the UI population, mainly those in the upper quintiles of the UI benefit distribution and those with shorter entitlement durations. While the 2012 reform, which was focused on a reduction in UI benefit generosity after day 180, resulted in significant decreases in UI benefit duration for these groups (around 25% in the third and fifth quintiles, up to 37% in the fourth quintile, and 30-40% for those with 240-300 days of entitlement), the 2023 reform, which was aimed at expanding UI benefit generosity, produced the opposite effect (a 27.4% increase in UI duration in the fourth quintile and a 16.6% increase among individuals with 240 days of entitlement). In the case of the 2012 reform, individuals with the longest entitlements were also affected. These findings suggest that both reductions and increases in UI benefit generosity only significantly alter job search behaviour when they imply a substantial variation in the recipient's net income.

Notably, workers in the lowest benefit quintile also show a significant positive response, a pattern that contrasts with the null effect observed for this group in the 2012 reform. This asymmetry is consistent with the institutional design of the Spanish UI system: in 2012, recipients in the lowest quintile were already receiving benefits at or near the statutory minimum, meaning that the reduction in the replacement rate had no binding effect on their actual benefit amount. Conversely, the 2023 increase in the replacement rate was sufficient to push their calculated benefit above the minimum threshold, generating a meaningful income gain that extended job search duration for this group. The opposite logic applies to the highest quintile: while these workers were strongly affected by the 2012 cut, as their benefits were well above the minimum and thus fully exposed to the reduction, in 2023 many of them may be constrained by the maximum benefit cap, limiting the effective gain from the increase in the replacement rate and explaining the absence of a significant response in this group. These findings underscore that the distributional effects of UI reforms are shaped by the benefit schedule's floors and ceilings: cuts fail to reach those already at the minimum, while increases fail to reach those already at the maximum.

4.6.3 Individual heterogeneous treatment effects

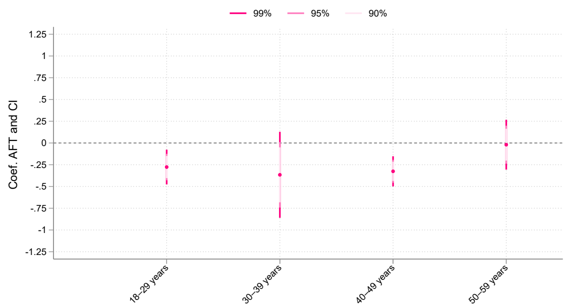
We examine the heterogeneous effects of the reforms across multiple subpopulations, including age group, number of children, industry and occupation, to assess whether average treatment effects mask variation across groups.[6] The estimates are obtained by fitting separate models for each. The results are presented in Figure 4.5 for the 2012 reform and Figure 4.6 for the 2023 reform.

For the 2012 reform, effects are widespread. The negative impact is quite similar regardless of age, family size, industry or occupation. Only older workers and employees in the industrial sector appear largely unaffected. The strongest negative effects are observed among prime-age workers, individuals with larger families and those employed in the construction sector and in white-collar and low-skilled occupations. In contrast, the 2023 reform shows fewer statistically significant effects. Nevertheless, the largest increase in unemployment duration is concentrated among prime-age workers—while younger and older persons are not affected—, as well as among employees in the industry and the ones working in white-collar, high- and

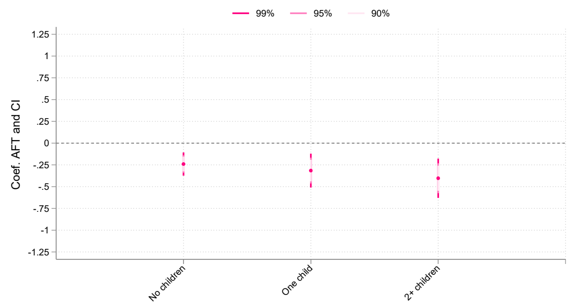
[6] A spatial heterogeneity analysis was also carried out to examine how the effects of the reforms vary across Autonomous Communities, highlighting differences in responses between regions with higher and lower unemployment rates (see Section Appendix C.A).

Figure 4.5: Heterogeneity in the impact of the 2012 reform

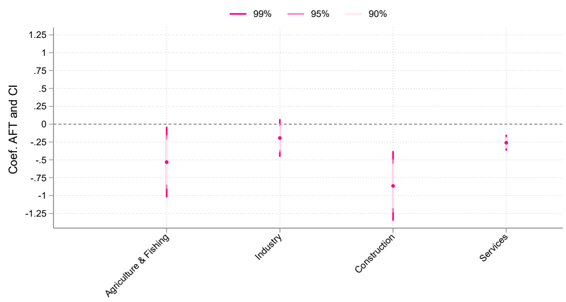
(a) Heterogeneity analysis by age



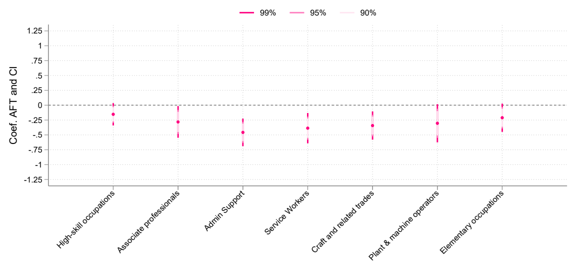
(b) Heterogeneity analysis by number of children



(c) Heterogeneity analysis by sector



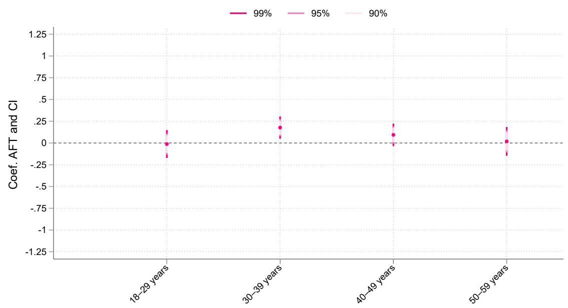
(d) Heterogeneity analysis by occupation



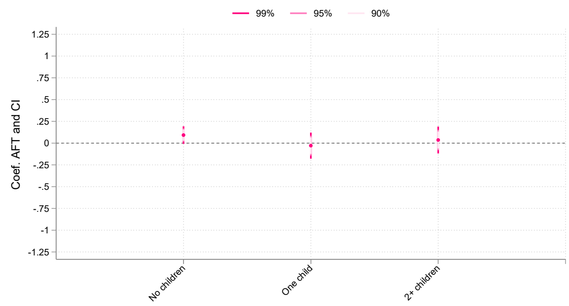
Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification.

Figure 4.6: Heterogeneity in the impact of the 2023 reform

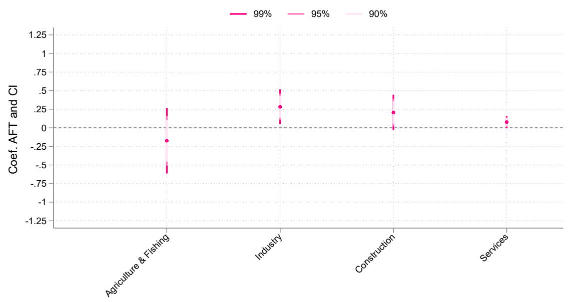
(a) Heterogeneity analysis by age



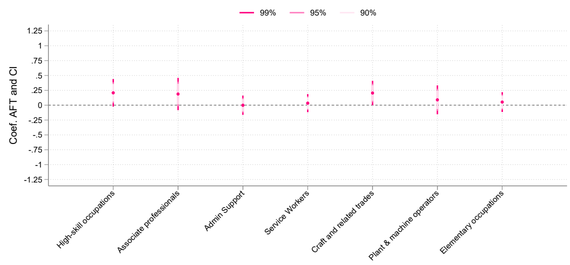
(b) Heterogeneity analysis by number of children



(c) Heterogeneity analysis by sector



(d) Heterogeneity analysis by occupation



Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification.

medium-skilled jobs.

Thus, while the 2012 reform affected prime-age workers across most sectors, the 2023 reform shows more selective effects, concentrated among prime-age workers and those in more skilled jobs. Overall, these findings indicate that the impact of the reforms is not uniform across the population.

4.7 Robustness check

4.7.1 Placebo test

To strengthen the internal validity of the research design and support the causal interpretation of the reform timing, we implement placebo tests for both reforms as a robustness check. The results are reported in Figure C.2 (*Panel (a)* for the 2012 reform and *Panel (b)* for the 2023 reform). Specifically, we replicate the baseline specification, using the same assignment criteria for treatment and control groups as the main analysis, but shifting the timing one month prior to each reform date—to periods without any actual intervention. This procedure helps verify that no time-specific shocks (such as seasonality, anticipatory behaviour, or concurrent policy changes) are driving the estimated results.

For the 2012 reform, this design simulates a fictitious policy change in a period with no actual intervention, allowing us to assess whether the selection criteria alone could generate spurious treatment effects. Under this placebo scenario, the estimated effects are expected to be statistically insignificant. Consistent with this expectation, we find no significant differences between the placebo treatment and control groups across all specifications. We implement an analogous placebo test for the 2023 reform, again shifting the timing one month earlier while keeping the assignment rules unchanged. As in the 2012 placebo exercise, the estimated effects are not statistically significant in any specification suggesting that the estimated effects are not driven by seasonality, anticipation or other time specific shocks.

4.7.2 DiD

As a second robustness check, we estimate a difference-in-differences (DiD) model as an alternative to the main analysis, which relied on simple differences weighted using IPW. This approach assesses whether the results remain stable when using a comparison group composed of individuals unaffected by the reforms.

While the main analysis defines treatment and control groups solely based on the timing of unemployment entry relative to the reform date, the DID approach introduces an additional identification dimension based on the potential entitlement to UI benefits. Individuals entitled to more than 180 days of UI benefits are considered treated, as they are the ones affected by changes in the RR from day 180 onward. This change involved a reduction under the July 2012 reform and an increase under the January 2023 one. Conversely, individuals entitled to fewer than 180 days of UI benefits are unaffected by these reforms, since the modified RR only applies beyond day 180. Thus, the causal effect of the reform is identified through the interaction between a time variable – indicating the pre- and post-reform periods – and a treatment variable, which distinguishes between affected and unaffected individuals based on their potential entitlement. To mitigate potential bias from unobserved factors that may vary over time and affect the groups differently, we include time-varying explanatory variables, such as unemployment benefit amounts and individual characteristics. Additionally, we implement a robustness check by interacting the numeric period variable with regions in the MPH duration model to capture region-specific time-varying effects. These specifications do not materially change the results.

To assess the validity of the parallel trends assumption in the DiD estimation, we have plotted the evolution of average unemployment duration for treated and control groups around each reform (see Figure C.3 in the Appendix). Pre-reform trajectories evolve in a highly similar manner, supporting the plausibility of the DiD design. Moreover, to formally assess the parallel trends assumption in the pre-reform period, we estimate an OLS regression in which the dependent variable is UI benefit duration and the independent variables include the treatment indicator, a daily linear time trend and their interaction. The estimation uses only entries into unemployment before the 16 July 2012 reform (from 1 July 2012) and before the 1 January 2023 reform (from 15 December 2022). The coefficient on the interaction between treatment and time trend is small and statistically insignificant, suggesting that pre-reform parallel trends in unemployment duration were similar between the treatment and control groups (see Table C.5 in the Appendix).

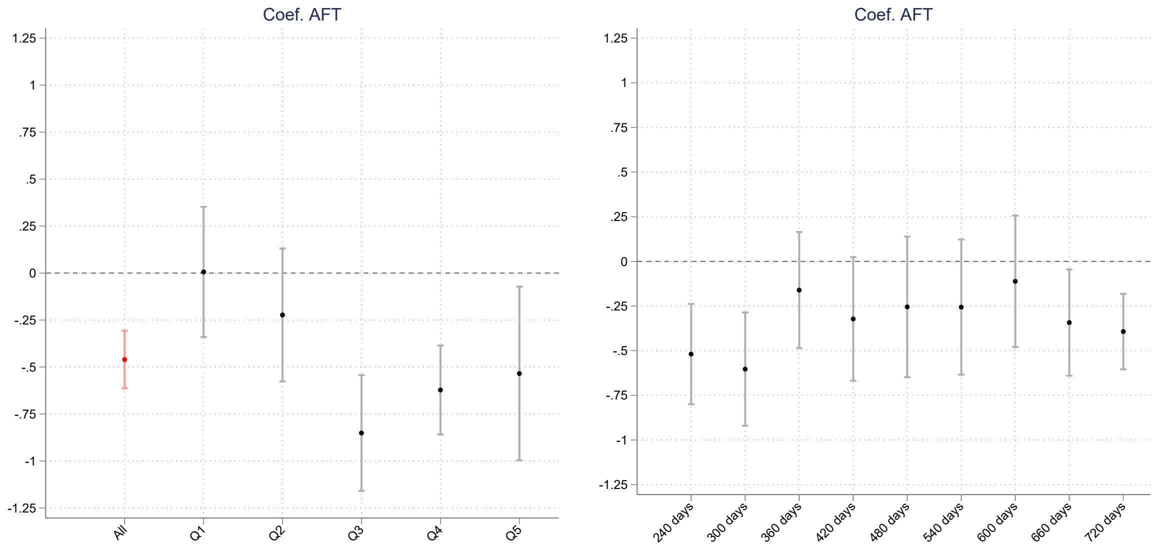
The results of the DiD approach are provided in Figure 4.7. They confirm the main findings. For the 2012 reform, we observe a negative and statistically significant effect on UI duration at the aggregate level (*Panel (a)*), alongside heterogeneity patterns consistent with the main analysis: individuals in the middle and upper quintiles of the earnings distribution (particularly quintiles 3, 4 and 5) show a reduction in UI benefit duration in response to the lower RR. Similarly, when analysing heterogeneity by potential duration, we find that the effect is concentrated among individuals entitled to 240-300 days and 720 of UI benefits, for whom the duration of unemployment decreases in a manner consistent with the main findings. A similar pattern emerges for the 2023 reform (*Panel (b)*). The DiD estimates show an increase in UI duration among affected individuals, particularly among those receiving above-average benefit amounts and among recipients with shorter entitlement duration (240 days).

The consistency of results across both specifications indicates that the behaviour of individuals not affected by the reforms (i.e. those entitled to fewer than 180 days of UI benefits) remained unchanged. This supports, the absence of other macroeconomic shocks that could have altered unemployment dynamics around the reform dates, and indicates limited spillover effects, reinforcing the causal interpretation of the main estimates.

However, it is important to note that the DiD strategy may introduce selection bias, as individuals with more, or fewer, than 180 days of entitlement may differ systematically in unobserved and observed characteristics related to employment history or unemployment risk. In contrast, the main analysis defines groups based solely on the date of unemployment entry (similarly to a natural experiment), using the reform implementation date as a cut-off, which can be considered exogenous. This classification allows for comparisons among individuals with similar entitlements but subject to different policies, thereby enhancing the validity of the design. In this sense, the main approach aligns with the methodological framework proposed by Hernán and Robins (2020), based on the emulation of randomized trials, and provides a more transparent and comparable strategy than a DiD model founded on structurally different groups.

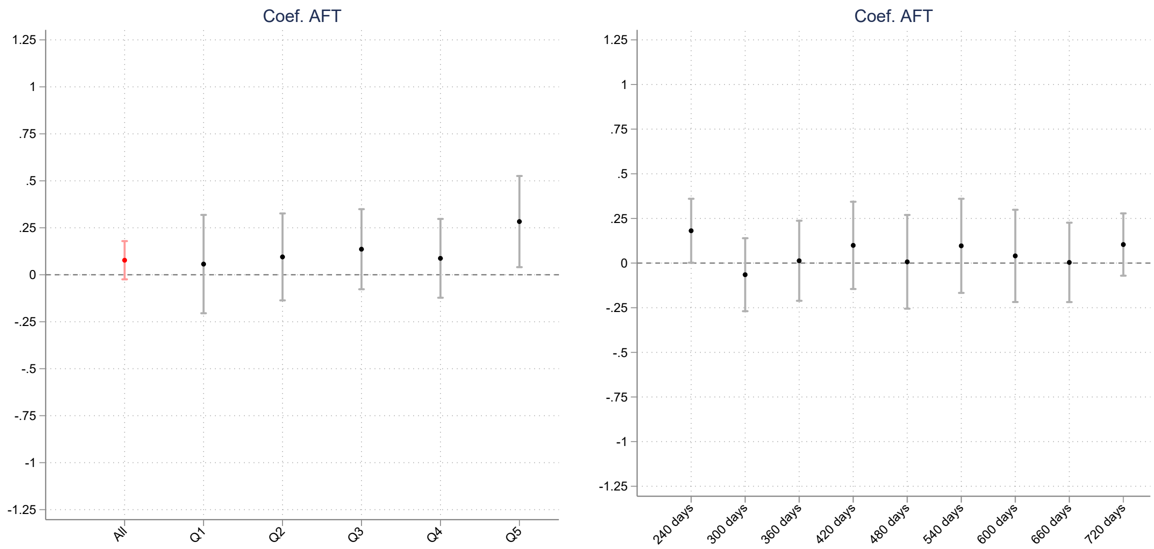
Figure 4.7: Estimated results from the DiD semiparametric PCE model

(a) Results from the 2012 reform by quintiles of UI benefit amount (left) and by potential benefit duration (right)



Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification. The number of individuals (unique IDs) included in each model is as follows —Left panel: All: 29,445; Q1: 5,898; Q2: 5,889; Q3: 5,884; Q4: 8,672; Q5: 3,102. Right panel: 240 days: 10,519; 300 days: 9,645; 360 days: 9,229; 420 days: 8,978; 480 days: 8,899; 540 days: 8,895; 600 days: 8,923; 660 days: 9,331; 720 days: 15,495.

(b) Results from the 2023 reform by quintiles of UI benefit amount (left) and by potential benefit duration (right)



Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification. The number of individuals (unique IDs) included in each model is as follows —Left panel: All: 23,834; Q1: 4,771; Q2: 4,764; Q3: 4,770; Q4: 5,129; Q5: 4,400. Right panel: 240 days: 10,200; 300 days: 9,266; 360 days: 8,898; 420 days: 8,672; 480 days: 8,513; 540 days: 8,545; 600 days: 8,505; 660 days: 8,821; 720 days: 10,304.

4.8 Discussion and conclusions

This paper analyses the impact of two contrasting reforms that altered the amount of UI benefits in Spain, implemented in very different economic contexts. The 2012 reform, enacted amid the economic crisis and austerity policies, reduced the gross RR starting from the seventh month of receiving benefit, with the aim of containing public expenditure and strengthening incentives to return to work. Conversely, the 2023 reform, which was approved in a context of economic growth and strong job creation, increased the RR to improve coverage and protection for the unemployed.

Our results indicate that both reforms have modest and heterogeneous effects on UI duration. At the aggregate level, the 2012 benefit reduction led to a 25.6% decrease in unemployment duration, whereas the 2023 increase resulted in a 6.5% rise. In both cases, responses were concentrated among specific groups, who adjusted their exit rates to employment in reaction to changes in benefit generosity. These groups include recipients of medium and high benefit levels and those with shorter potential durations (and those with the longest entitlements in 2012). The study provides evidence that changes in benefit generosity influenced the behaviour of specific subpopulations of unemployed individuals depending on their initial benefit level and entitlement, especially in the case of the 2012 reform.

Our analysis of heterogeneous treatment effects shows that impacts of the reforms are not uniform across the population. For both the 2012 and 2023 reforms, certain subgroups responded more strongly to changes in benefit generosity. In 2012, the effects are concentrated among prime-age workers, individuals with dependants, and employees in the construction sector and in white-collar, low-skilled occupations. In 2023, prime-age workers, employees in the industrial sector and those working in white-collar and higher-skilled jobs are among the most affected. Taken together, these patterns indicate that age, family status, occupation and industry mediate the response to UI generosity, highlighting the importance of considering heterogeneous effects in policy evaluation. These findings reinforce our main conclusion that evaluating UI reforms based solely on average effects may misrepresent their true impact and underscore the value of tailoring policy design to the characteristics of the affected population.

These findings are broadly consistent with theoretical and empirical research suggesting that the impact of benefit generosity unemployment duration may vary over the business cycle (Bover et al., 2002; Tatsiramos and van Ours, 2014), although the estimated effects remain heterogeneous and not uniform across subpopulations. During recessions, moral hazard associated with generous UI benefits may be lower due to the scarcity of vacancies and employment opportunities, which can limit the effect of reducing RRs on job search behaviour. In expansion phases, however, incentives derived from benefit levels become more relevant, exerting a stronger influence on unemployed workers' decision-making. This hypothesis is often interpreted as supporting the idea that the optimal design of UI benefits should be countercyclical – that is, adjusted to economic conditions: lower benefits in expansions to avoid discouraging searching for jobs in a tight labour market, and higher generosity in downturns to ensure protection and social stability (see Andersen and Svarer, 2011; Landais et al., 2010). However, our results show that even a modest increase in benefits during an expansion (the 2023 reform) produced statistically significant, albeit small, increases in unemployment duration, indicating that some recipients may adjust their search behaviour even under favourable labour market conditions. Conversely, the 2012 reduction in benefits during a recession generated substantial declines in UI duration. While this might appear to reflect a moral hazard effect, the driving force might instead be income constraints: the cuts directly reduced resources available to recipients, particularly those in higher benefit quintiles, compelling them to exit UI more quickly rather than altering their underlying job search incentives.

Beyond this explanation of unemployed workers' response to benefit generosity, other aspects related to the Spanish labour market influence the importance of moral hazard in unemployment benefits. In 2012, high unemployment and fiscal constraints likely offset the effects of the cut in benefits by limiting alternative job opportunities, especially for less qualified and less experienced workers. In 2023, during the expansion phase, one would have expected higher moral hazard due to greater job opportunities; however, other factors may have partially mitigated the disincentive effects of the RR increase, such as labour market improvements and complementary measures, including successive minimum wage increases and the 2022 labour reform that eliminated certain temporary contracts and induced a shift from temporary to permanent employment.

This study contributes to the international debate on the optimal design of benefit systems by providing empirical evidence from one of the EU countries with the highest structural unemployment and strong cyclical sensitivity. Overall, the findings highlight the importance of adapting unemployment protection design to the business cycle, balancing efficiency and equity. Reforms aimed at reducing benefit generosity may accelerate unemployment exits – but at the cost of weakening protection when it is most needed. Conversely, benefit increases may be socially desirable during recoveries or specific shocks, provided they are accompanied by active policies that limit adverse incentive effects. The findings suggest that reforms should be evaluated not only according to their direction but also based on their timing and fit with the institutional and macroeconomic environment.

While this study provides some evidence on the impact of benefit levels on unemployment duration, it has several limitations from an equity and social welfare perspective. In particular, it does not examine effects on household income or economic stability due to the lack of household-level information in the administrative data from the PES. Given the central role of UI benefits in protecting living standards, future research should address their distributional impact. Furthermore, examining the quality of post-unemployment employment is essential. Beyond the speed of re-employment, it is important to assess whether workers obtain jobs comparable to their previous ones in terms of pay, stability and working conditions. This is particularly relevant in the Spanish context, where high contractual duality means that re-employment often implies a transition from permanent to temporary employment, potentially undermining the long-term effectiveness of unemployment protection.

Chapter 5

Conclusion

The three empirical chapters provide the evidential basis for the conclusions that follow. Chapter 2 evaluates the impact of the 22.3% minimum wage increase implemented in January 2019 on job retention and employment permanence, finding no adverse effects and modest short-lived positive effects among full-time and temporary workers in low-wage regions. Chapter 3 extends this analysis to gross employment flows at the provincial level over 2015–2023, showing that higher minimum wage exposure reduces both hiring and separations — attenuating labour turnover rather than destroying net employment, whose effect is close to zero — with heterogeneous responses by working hours. Chapter 4 evaluates two reforms to the unemployment benefit replacement rate implemented under contrasting macroeconomic conditions: the 2012 cut reduced average unemployment duration by 25.6%, while the 2023 increase led to a modest 6.5% extension, with effects concentrated among recipients with medium-to-high benefits and shorter entitlement durations. Together, these findings point to three overarching conclusions.

The first is cyclical: the effects of institutions depend fundamentally on the phase of the cycle in which they are applied. The same policy can generate different responses depending on the strength of demand and the availability of job vacancies. In periods of expansion, increases in the minimum wage are absorbed mainly through reduced turnover and job reallocation, rather than through net job destruction. However, in contractionary environments, variations in benefit generosity selectively affect the duration of unemployment, conditioned by income constraints and scarcity of opportunities. Therefore, the timing of the reform is as important as its content.

The second consideration is structural. The institutional framework of the Spanish labour market shapes the mechanisms of transmission. In a context of contractual dualism and extended collective bargaining, adjustments do not occur primarily through staff reductions, but rather through retention, turnover and selection during transitions. Similarly, changes in the generosity of benefits do not uniformly shift job search behaviour, but rather concentrate their effects on specific groups, depending on their financial margins and productive environment. This evidence is difficult to reconcile with a strictly competitive interpretation and fits better with frameworks of imperfect competition and frictions.

The third idea is systemic: institutions interact with each other. The combined impact of the minimum wage, benefits and hiring rules is not cumulative. Changes in one dimension alter incentives and constraints in others, leading to simultaneous changes in various labour market mechanisms. As a result, some aggregate outcomes may appear stable—such as net employment—while significant adjustments in flows, durations, and profiles occur beneath the surface. Evaluating each policy in isolation can obscure these cross-effects.

From a theoretical perspective, the contribution of the thesis is not to confirm or refute a canonical result, but rather to delimit the scope of validity of different predictions. The conclusions regarding the minimum wage are incompatible with a mechanical reading of the competitive model, but are consistent with frameworks that incorporate monopsony, adjustment costs or efficiency wages, under two conditions: a relatively low starting point and an expansionary context. In the area of unemployment, the results are consistent with models in which incentives are more important when job opportunities exist, while in times of recession, liquidity constraints and a shortage of job vacancies limit behavioural responses. In both cases, the thesis demonstrates that the initial context and the timing of the reform are as decisive as its magnitude.

The policy implications follow directly from this overview of mechanisms. With regard to minimum wages, the lesson is not that any increase is harmless, but rather that predictable and gradual increases, based on observable benchmarks such as productivity or average wages, can improve income and stability without significant adverse effects when the starting point is low and demand is favourable. This requires continuous monitoring of incidence, flows, and composition to detect possible thresholds or non-linearities. With regard to unemployment, the guiding principle is countercyclicality: avoiding widespread cuts during recessions, when their impact on behaviour is limited and their cost in terms of welfare is high, and calibrating expansions during periods of growth, backing them up with active policies that maintain the intensity of job search and improve matching. In both areas, the design must be differentiated, as the documented heterogeneity justifies adjusting generosity and requirements to profiles with different sensitivities and risks.

The territorial dimension reinforces this conclusion. In a country with marked differences in productivity and sectoral structure, the effects of the same measure vary according to the local fabric. Incorporating territorial sensitivity into labour market reforms helps to avoid reinforcing asymmetries.

This conclusion must also acknowledge its limits. The validity of the conclusions depends on the identification assumptions adopted. In the analysis of the minimum wage the retention analysis is limited to the short term due to the onset of the pandemic, while the study of provincial flows, although covering a broader horizon, operates at an aggregation level that prevents the identification of detailed individual trajectories.

In the case of unemployment benefits, the analysis focuses on the duration of unemployment and does not examine other relevant dimensions such as the quality of subsequent employment or household well-being, a limitation stemming from the information available in administrative records. Ultimately, the results pertain to specific reforms implemented within a particular historical and institutional context. Extrapolation to other environments or to changes of different magnitudes should be approached with caution.

The underlying conclusion is that, in markets characterized by high structural unemployment and significant cyclical sensitivity, institutions exert less influence through their ability to immediately alter net employment and more through their effect on the mechanisms that organize employment: who remains employed, who transitions, how long unemployment lasts, and how job matching is configured. A well-calibrated minimum wage and a counter-cyclical, differentiated unemployment insurance can, together, protect incomes, reduce unnecessary turnover, and improve allocation, without sacrificing efficiency when the economy presents opportunities. Designing policies guided by these principles (context, structure, and institutional interaction) is the pathway to a more stable, inclusive, and productive labour market

Conclusiones

Los tres capítulos empíricos proporcionan la base sobre la que se asientan las conclusiones que siguen. El Capítulo 2 evalúa el impacto del aumento del 22,3% del salario mínimo en enero de 2019 sobre la retención del empleo, sin encontrar efectos adversos y con efectos positivos modestos y de corta duración entre trabajadores a tiempo completo y temporales en regiones de bajos salarios. El Capítulo 3 extiende el análisis a los flujos brutos de empleo a nivel provincial entre 2015 y 2023, mostrando que una mayor exposición al salario mínimo reduce tanto las contrataciones como las separaciones — atenuando la rotación laboral sin destruir empleo neto, cuyo efecto es cercano a cero — con respuestas heterogéneas según la jornada laboral. El Capítulo 4 evalúa dos reformas de la tasa de sustitución de las prestaciones por desempleo bajo condiciones macroeconómicas contrastantes: el recorte de 2012 redujo la duración media del desempleo en un 25,6%, mientras que el aumento de 2023 generó una modesta extensión del 6,5%, con efectos concentrados en perceptores con prestaciones medias-altas y menor duración potencial. En conjunto, estos hallazgos apuntan a tres conclusiones generales.

La primera es temporal: los efectos de las instituciones dependen de la fase del ciclo económico. La misma política puede tener distintos resultados según la demanda y la disponibilidad de vacantes. En periodos de expansión, los aumentos del salario mínimo se absorben principalmente mediante la reducción de la rotación de trabajadores y la reasignación de empleos, más que por pérdida neta de puestos de trabajo. En periodos de recesión, los cambios en la generosidad de las prestaciones afectan principalmente la duración del desempleo, dependiendo de los ingresos de las personas y la falta de oportunidades. Por eso, el momento de la reforma es tan importante como lo que se cambie.

La segunda conclusión es estructural. El marco laboral de España influye en cómo se transmiten estos cambios. En un contexto con contratos muy distintos y negociación colectiva extendida, los ajustes no se producen principalmente mediante despidos, sino a través de la retención de empleados, la rotación y la selección durante las transiciones. De forma similar, los cambios en las prestaciones no afectan por igual a todos, sino que tienen mayor efecto en grupos específicos según sus ingresos y entorno productivo. Estos resultados no encajan con una visión estrictamente competitiva, pero sí con modelos que incluyen competencia imperfecta o fricciones en el mercado.

La tercera idea es que las instituciones interactúan entre sí. El efecto combinado del salario mínimo, las prestaciones y las reglas de contratación no se suma de forma simple. Reformar una institución modifica los incentivos y restricciones en otras, generando cambios en varios mecanismos al mismo tiempo. Por eso, algunos resultados generales, como el empleo neto, pueden parecer estables, mientras que por debajo ocurren cambios importantes en flujos, duraciones y perfiles. Evaluar cada política por separado puede ocultar estos efectos cruzados.

Desde el punto de vista teórico, la tesis no busca confirmar o refutar un resultado específico, sino mostrar en qué contextos las distintas predicciones funcionan. Los resultados sobre el salario mínimo no se ajustan a una visión competitiva estricta, pero sí a modelos que incluyen monopsonio, costos de ajuste o salarios de eficiencia, siempre que el salario inicial sea bajo y el contexto expansivo. En el caso del desempleo, los resultados coinciden con modelos en los que los incentivos son más importantes cuando hay oportunidades de empleo, mientras que en recesión las restricciones de liquidez y la falta de vacantes limitan la reacción de las personas. En ambos casos, el contexto inicial y el momento de la reforma son tan importantes como la magnitud de los cambios.

Las implicaciones de política se derivan de estos hallazgos. Sobre los salarios mínimos, no se dice que cualquier aumento sea seguro, sino que aumentos previsibles y graduales, basados en referencias como productividad o salarios promedio, pueden mejorar los ingresos y la estabilidad sin efectos negativos importantes, siempre que el punto de partida sea bajo y la demanda favorable. Esto requiere seguimiento constante de los flujos y la composición de los trabajadores para detectar posibles límites o efectos no lineales. En cuanto al desempleo, la regla principal es contracíclica: evitar recortes masivos durante las recesiones, cuando su efecto es limitado y su costo en bienestar alto, y ampliar las prestaciones en periodos de crecimiento, apoyadas con políticas activas que mantengan la búsqueda de empleo y mejoren el emparejamiento laboral. En ambos casos, las políticas deben adaptarse a los distintos perfiles de las personas según su sensibilidad y riesgo.

La dimensión territorial refuerza esta idea. En un país con diferencias marcadas en productividad y estructura sectorial, los efectos de la misma medida varían según la región. Incluir esta sensibilidad territorial ayuda a no aumentar las desigualdades.

También hay que reconocer los límites del estudio. La validez de las conclusiones depende de los supuestos de identificación que se usaron. En el análisis del salario mínimo, la retención solo se estudia a corto plazo debido a la pandemia, y el análisis de flujos provinciales, aunque más amplio, no permite ver trayectorias individuales con detalle.

En el caso de las prestaciones por desempleo, el análisis se centra en la duración del desempleo y no en otras dimensiones importantes como la calidad del empleo posterior o el bienestar de los hogares, limitaciones que surgen de la información disponible en registros administrativos. En definitiva, los resultados se aplican a reformas específicas dentro de un contexto histórico e institucional concreto. Aplicarlos a otros contextos o a cambios muy distintos debe hacerse con precaución.

La conclusión principal es que, en mercados con alto desempleo estructural y alta sensibilidad cíclica, las instituciones afectan menos al empleo neto inmediato y más a los mecanismos que organizan el empleo: quién se mantiene trabajando, quién transita, cuánto dura el desempleo y cómo se produce el emparejamiento laboral. Un salario mínimo bien ajustado y un seguro de desempleo contracíclico y diferenciado pueden proteger los ingresos, reducir la rotación innecesaria y mejorar la asignación de empleo, sin perder eficiencia cuando hay oportunidades. Diseñar políticas considerando contexto, estructura e interacción institucional es el camino hacia un mercado laboral más estable, inclusivo y productivo.

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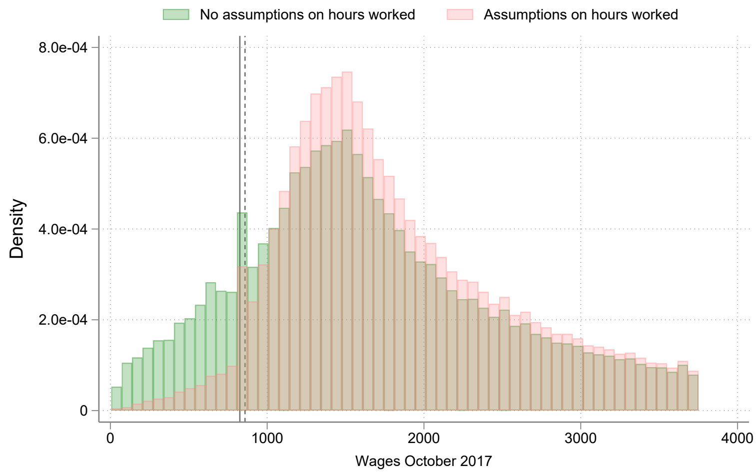
Appendices

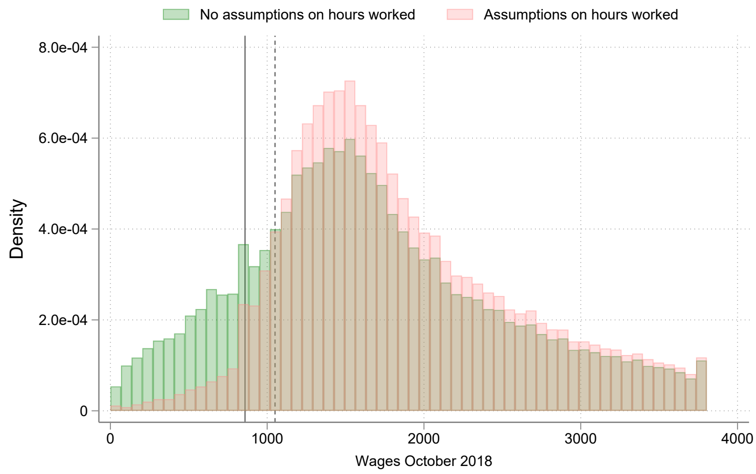
Appendix Chapter 2

Figure A.1: Wage distribution: October 2017 and October 2018

(a) October 2017

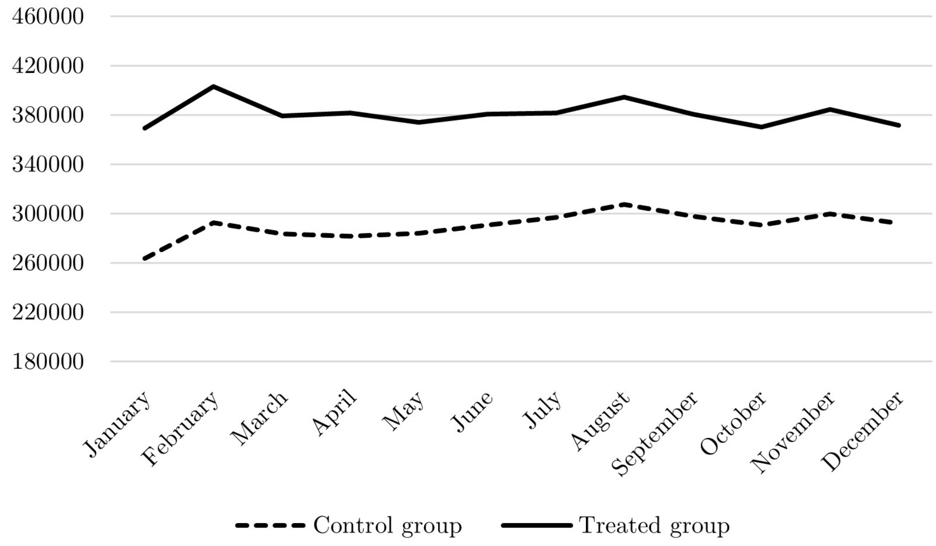


(b) October 2018



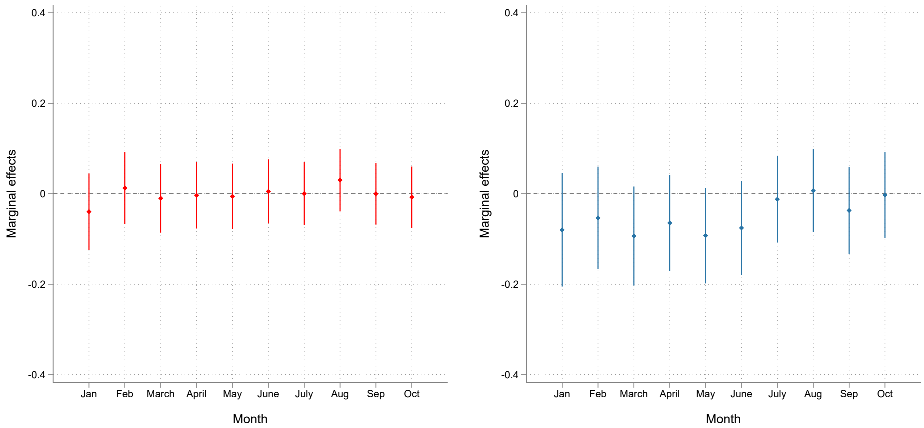
Notes: The solid vertical line represents the MW for the current year. The dashed vertical line shows the new MW for the following year, illustrating the updated wage limit affecting workers whose wage falls between the current year's MW and the projected one. The wages are capped at €3,751 and €3,803, the maximum contributory amount in 2017 and 2018, respectively. Source: MCVL 2017–2019.

Figure A.2: Parallel trends in the evolution of employment in the period prior to the MW hike (year 2018)



Notes: Own elaboration based on MCVL 2017–2019.

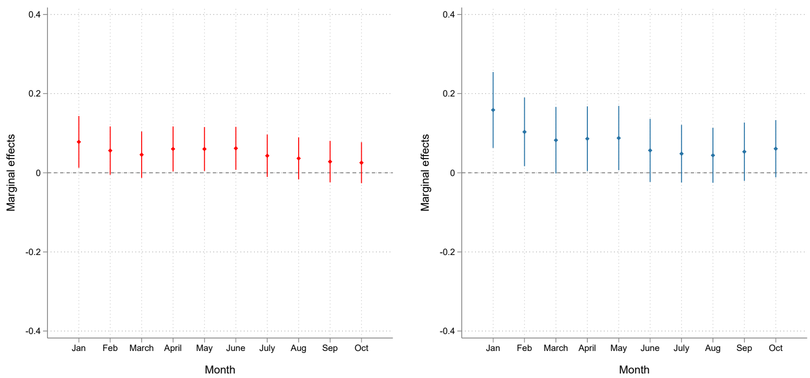
Figure A.3: Estimate results from DID placebo test



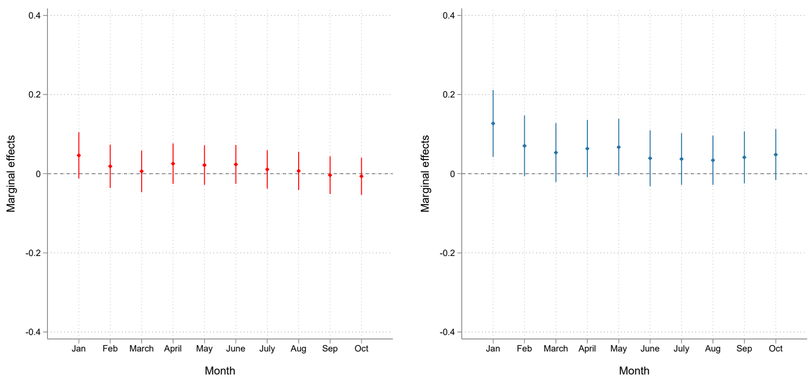
Notes: Results from DID with logit models of the probability of remaining employed, either by the same company (left panel) or broadly in any job (right panel). Regressor estimates correspond to marginal effects (β_4) from DID models estimated using IPW logit. Vertical lines represent 95% confidence intervals. MCVL 2017–2019

Figure A.4: Estimate results from DID: alternative approaches

(a) Standard DID



(b) PSM-DID



Notes: Results from DID with logit models of the probability of remaining employed, either by the same company (left panel) or broadly in any job (right panel), after the 2019–MW increase. Regressor estimates correspond to marginal effects (β_4) from DID models. Vertical lines represent 95% confidence intervals. MCVL 2017–2019

Table A.1: Workforce characteristics by wage level (all employees vs. employees earning ≤ MW), 2017–2018 (October 2017 and October 2018)

	All employees		MW earners	
	2017	2018	2017	2018
Gender				
Man	0.525	0.524	0.397	0.393
Age				
18–30	0.160	0.164	0.351	0.332
31–45	0.444	0.432	0.354	0.350
46–64	0.396	0.405	0.295	0.318
Nationality				
Spaniard	0.927	0.922	0.883	0.878
Level of education				
Missing value	0.021	0.020	0.027	0.026
Equal/less than mandatory	0.361	0.360	0.467	0.456
Higher baccalaureate	0.180	0.178	0.148	0.152
Vocational training	0.166	0.167	0.163	0.158
University studies	0.273	0.275	0.195	0.208
Contribution group (occupation)				
Groups 1–2 (non-manual + qualified)	0.192	0.193	0.115	0.122
Groups 3–7 (non-manual - qualified)	0.403	0.401	0.263	0.285
Groups 8–10 (manual occupations)	0.405	0.406	0.622	0.592
Industry				
Agriculture and industry	0.154	0.152	0.073	0.069
Construction, transport and storage	0.106	0.110	0.060	0.054
Trade and hospitality	0.258	0.256	0.325	0.297
Information and professional services	0.177	0.179	0.229	0.228
Administration, education and health	0.238	0.237	0.215	0.245
Other service activities	0.067	0.066	0.099	0.107
Region				
Northwest	0.087	0.087	0.089	0.090
Northeast	0.106	0.105	0.071	0.078
Madrid	0.165	0.166	0.125	0.130
Centre	0.109	0.108	0.130	0.133
East	0.316	0.316	0.299	0.291
South	0.170	0.172	0.235	0.221
Canary Islands	0.047	0.047	0.051	0.057
Seniority				
Less than 6 months	0.221	0.233	0.355	0.413
More than 5 months but less than a year	0.102	0.102	0.161	0.152
1 year or more but less than 2 years	0.116	0.119	0.164	0.142
2 years or more but less than 5 years	0.183	0.184	0.198	0.169
5 years or more but less than 10 years	0.169	0.153	0.082	0.082
10 years or more	0.210	0.210	0.041	0.043
Type of contract				
Permanent	0.756	0.755	0.469	0.457
Temporary	0.244	0.245	0.531	0.543
Working time schedule				
Full time	0.723	0.729	0.648	0.663
Part time	0.277	0.271	0.352	0.337
Observations (unweighted)	506,201	529,091	20,437	22,053
Individuals (weighted)	12,655,025	13,227,275	510,925	551,325

Notes: The grouping of the Spanish regions (Autonomous Communities) into seven groups refers to level 1 of the Nomenclature des Unités Territoriales Statistiques (NUTS1): 'Northwest' (Galicia, Asturias and Cantabria); 'Northeast' (Basque Country, Navarre, Rioja and Aragon); 'Madrid' (Community of Madrid); 'Centre' (Castilla y León, Castilla-La Mancha and Extremadura); 'East' (Catalonia, Valencian Community and Balearic Islands); 'South' (Andalusia, Murcia, Ceuta and Melilla); and 'Canary Islands' (Canary Islands). Own work based on the data of the MCVL.

Table A.2: Sample composition: distribution of treated and untreated workers according to personal and job characteristics (October 2017 and October 2018)

	2017		2018				
	Treated	Control	Treated	Control	DID	χ^2	Sig.
Gender							
Man	0.420	0.399	0.415	0.391	-0.003	0.270	
Age							
18–30	0.351	0.266	0.345	0.272	0.012	4.860	**
31–45	0.380	0.434	0.368	0.413	-0.009	2.280	
46–64	0.269	0.300	0.287	0.315	-0.003	0.350	
Nationality							
Spaniard	0.883	0.886	0.875	0.884	-0.006	2.500	
Level of education							
Missing value	0.025	0.025	0.025	0.023	-0.001	0.620	
Equal/less than mandatory	0.473	0.477	0.471	0.477	0.002	0.160	
Higher baccalaureate	0.149	0.160	0.152	0.159	-0.005	1.140	
Vocational training	0.175	0.178	0.168	0.181	0.009	4.040	**
University studies	0.179	0.161	0.184	0.161	-0.006	1.490	
Contribution group (occupation)							
Groups 1–2 (non-manual + qualified)	0.101	0.069	0.106	0.072	-0.001	4.570	**
Groups 3–7 (non-manual - qualified)	0.335	0.379	0.342	0.374	-0.012	5.280	**
Groups 8–10 (manual occupations)	0.564	0.552	0.552	0.554	0.014	0.370	
Industry							
Agriculture and industry	0.078	0.093	0.076	0.089	-0.002	0.200	
Construction, transport and storage	0.060	0.079	0.059	0.076	-0.001	1.690	
Trade and hospitality	0.304	0.344	0.297	0.330	-0.007	1.550	
Information and professional services	0.240	0.217	0.246	0.216	-0.006	0.870	
Administration, education and health	0.209	0.167	0.210	0.173	0.004	11.310	***
Other service activities	0.110	0.100	0.112	0.115	0.013	0.120	
Region							
Northwest	0.093	0.108	0.091	0.104	-0.001	0.000	
Northeast	0.061	0.062	0.061	0.062	0.000	1.350	
Madrid	0.144	0.148	0.149	0.148	-0.005	0.150	
Centre	0.123	0.116	0.120	0.115	0.002	0.150	
East	0.276	0.285	0.284	0.277	-0.016	8.470	***
South	0.220	0.216	0.212	0.221	0.012	6.310	**
Canary Islands	0.083	0.063	0.084	0.073	0.009	7.430	***
Seniority							
Less than 6 months	0.379	0.349	0.400	0.363	-0.008	2.000	
More than 5 months but less than a year	0.152	0.139	0.151	0.136	-0.002	0.290	
1 year or more but less than 2 years	0.152	0.146	0.144	0.147	0.008	3.530	
2 years or more but less than 5 years	0.173	0.177	0.167	0.183	0.013	7.550	***
5 years or more but less than 10 years	0.090	0.113	0.082	0.097	-0.009	6.380	**
10 years or more	0.055	0.076	0.055	0.075	-0.001	0.150	
Type of contract							
Permanent	0.612	0.718	0.637	0.723	-0.021	14.360	***
Temporary	0.387	0.280	0.360	0.276	0.023	16.690	***
Working time schedule							
Full time	0.662	0.643	0.650	0.637	-0.007	0.314	
Part time	0.338	0.357	0.350	0.362	-0.007	0.314	
Probability of remaining employed in any job							
November	0.988	0.989	0.987	0.989	0.001	0.020	
January	0.916	0.939	0.924	0.936	-0.011	11.91	***
May	0.881	0.914	0.884	0.909	-0.008	4.92	**
October	0.852	0.888	0.850	0.879	-0.007	2.26	
Probability of remaining employed in the same firm							
November	0.927	0.939	0.924	0.934	-0.002	0.62	
January	0.916	0.939	0.924	0.936	-0.011	4.13	**
May	0.704	0.747	0.706	0.740	-0.009	3.27	
October	0.605	0.655	0.604	0.649	-0.005	0.54	
Observations (unweighted)	35,113	24,057	30,224	23,936			
Individuals (weighted)	877,825	601,425	755,600	598,400			

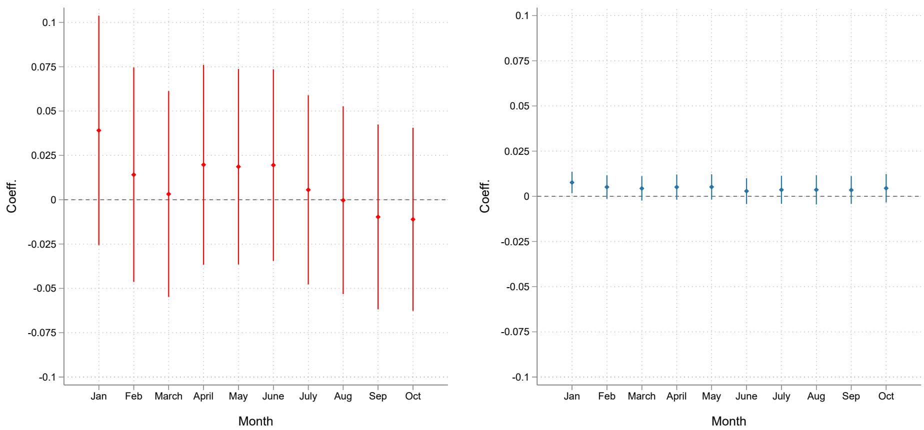
Notes: Statistical significance is indicated as ** if $p < 0.05$ and *** if $p < 0.01$. χ^2 reports the test of the DID for each category. The grouping of the Spanish regions (Autonomous Communities) into seven groups refers to level 1 of NUTS1: 'Northwest' (Galicia, Asturias and Cantabria); 'Northeast' (Basque Country, Navarre, Rioja and Aragon); 'Madrid' (Community of Madrid); 'Centre' (Castilla y León, Castilla-La Mancha and Extremadura); 'East' (Catalonia, Valencian Community and Balearic Islands); 'South' (Andalusia, Murcia, Ceuta and Melilla); and 'Canary Islands' (Canary Islands). Own work based on the data of the MCVL.

Table A.3: Sample composition: distribution of treated and untreated workers according to personal and job characteristics after IPW (October 2017 and October 2018)

	2017		2018				
	Treated	Control	Treated	Control	DID	χ^2	Sig.
Gender							
Man	0.420	0.416	0.415	0.411	0.000	0.000	
Age							
18–30	0.348	0.341	0.345	0.334	-0.004	0.440	
31–45	0.380	0.387	0.368	0.374	0.000	0.000	
46–64	0.271	0.272	0.287	0.292	0.004	0.460	
Nationality							
Spaniard	0.883	0.882	0.875	0.873	0.002	0.180	
Level of education							
Missing value	0.025	0.026	0.025	0.025	-0.001	0.070	
Equal/less than mandatory	0.476	0.472	0.471	0.475	0.009	2.020	
Higher baccalaureate	0.148	0.149	0.152	0.153	0.000	0.000	
Vocational training	0.175	0.176	0.168	0.169	-0.001	0.020	
University studies	0.175	0.177	0.184	0.178	-0.008	2.610	
Contribution group (occupation)							
Groups 1–2 (non-manual + qualified)	0.097	0.097	0.106	0.098	-0.008	4.270	
Groups 3–7 (non-manual - qualified)	0.333	0.343	0.342	0.348	-0.003	0.330	
Groups 8–10 (manual occupations)	0.570	0.560	0.552	0.554	0.012	3.570	
Industry							
Agriculture and industry	0.078	0.079	0.076	0.077	0.000	0.000	
Construction, transport and storage	0.057	0.060	0.059	0.058	-0.003	1.550	
Trade and hospitality	0.304	0.306	0.297	0.302	0.003	0.370	
Information and professional services	0.243	0.245	0.246	0.247	-0.002	0.080	
Administration. education and health	0.207	0.201	0.210	0.202	-0.002	0.130	
Other service activities	0.111	0.110	0.112	0.114	0.003	0.700	
Region							
Northwest	0.092	0.094	0.091	0.090	-0.002	0.430	
Northeast	0.061	0.063	0.061	0.061	-0.001	0.250	
Madrid	0.144	0.146	0.149	0.151	0.000	0.010	
Centre	0.124	0.119	0.120	0.117	0.002	0.250	
East	0.277	0.280	0.284	0.284	-0.003	0.220	
South	0.221	0.217	0.212	0.213	0.005	0.990	
Canary Islands	0.082	0.082	0.084	0.083	0.000	0.010	
Seniority							
Less than 6 months	0.380	0.381	0.400	0.398	-0.004	0.410	
More than 5 months but less than a year	0.152	0.151	0.151	0.149	-0.001	0.100	
1 year or more but less than 2 years	0.152	0.150	0.144	0.146	0.004	0.810	
2 years or more but less than 5 years	0.175	0.171	0.167	0.169	0.006	1.770	
5 years or more but less than 10 years	0.089	0.092	0.082	0.084	-0.001	0.050	
10 years or more	0.053	0.055	0.055	0.054	-0.004	2.400	
Type of contract							
Permanent	0.656	0.658	0.676	0.677	-0.048	1.080	
Temporary	0.344	0.342	0.324	0.323	0.048	1.080	
Working time schedule							
Full time	0.659	0.635	0.648	0.630	0.053	0.060	
Part time	0.341	0.365	0.352	0.370	-0.053	0.060	
Observations (unweighted)	35,113	24,057	30,224	23,936			
Individuals (weighted)	877,825	601,425	755,600	598,400			

Notes: Statistical significance is indicated as ** if $p < 0.05$ and *** if $p < 0.01$. χ^2 reports the test of the DID for each category. The grouping of the Spanish regions (Autonomous Communities) into seven groups refers to level 1 of the Nomenclature des Unités Territoriales Statistiques (NUTS1): 'Northwest' (Galicia, Asturias and Cantabria); 'Northeast' (Basque Country, Navarre, Rioja and Aragon); 'Madrid' (Community of Madrid); 'Centre' (Castilla y León, Castilla-La Mancha and Extremadura); 'East' (Catalonia, Valencian Community and Balearic Islands); 'South' (Andalusia, Murcia, Ceuta and Melilla); and 'Canary Islands' (Canary Islands). Own work based on the data of the MCVL.

Figure A.5: Results from DID with IPW: linear probability models



Notes: Results from DID-IPW with linear probability models on the probability of remaining employed, either with the same employer (left panel) or in any job (right panel), after the 2019–MW increase. Regressor estimates correspond to Coefficients (β_4). Vertical lines represent 95% confidence intervals. Source: MCVL 2017–2019

Appendix Chapter 3

Table B.1: Descriptive statistics (percentages %)

	2015	2016	2017	2018	2019	2020	2021	2022	2023
Sex									
Male	50.90	50.40	50.90	50.60	50.50	49.70	50.20	49.70	49.70
Female	49.10	49.60	49.10	49.40	49.50	50.30	49.80	50.30	50.30
Age									
Under 25	10.40	11.10	11.90	13.30	13.22	8.00	12.00	13.50	16.60
Between 25 and 50	69.70	68.40	67.00	64.20	64.22	64.10	63.60	61.70	60.10
Over 50	19.90	20.60	21.10	22.50	22.54	28.00	24.50	24.80	23.30
Education									
Missing data	2.40	2.40	2.20	2.20	2.20	1.60	2.10	2.30	2.50
Education $\leq$ mandatory	43.50	43.30	43.20	42.70	42.50	40.50	41.70	41.10	40.50
Upper secondary school	15.10	15.10	15.40	15.60	15.90	16.40	16.40	17.10	17.50
Vocational training	17.20	17.50	17.50	17.70	17.90	18.20	17.90	17.70	17.50
University studies	21.90	21.80	21.60	21.80	21.50	23.30	22.00	21.80	22.00
Job seniority									
Less than 6 months	53.80	54.80	55.50	55.40	55.80	43.80	50.80	55.70	61.80
6 months to 1 year	8.90	8.90	9.10	9.10	8.70	8.80	9.00	11.00	9.10
1 year or more	37.30	36.40	35.40	35.50	35.50	47.40	40.20	33.20	29.20
Industry affiliation									
Agriculture & livestock	1.00	1.00	1.00	1.00	1.00	1.00	1.20	1.10	1.10
Manufactures	10.00	9.90	9.50	9.40	9.30	11.00	9.90	9.70	9.60
Construction, transport & storage	10.30	10.00	10.00	9.90	9.80	9.80	10.00	9.60	9.50
Commerce & hospitality	29.00	29.50	29.00	28.50	28.40	25.70	24.40	25.80	25.50
Information & professional services	19.90	20.10	20.70	21.10	21.50	19.30	23.10	21.40	21.80
Administration, education & healthcare	23.80	23.30	22.60	22.70	22.40	26.40	25.40	25.20	24.80
Other service activities	6.10	6.20	7.20	7.40	7.50	7.00	6.10	7.20	7.60
Contract type									
Permanent contract	63.10	63.00	62.90	64.10	64.20	67.80	67.50	79.30	80.40
Temporary contract	36.90	37.00	37.10	35.90	35.80	32.20	32.50	20.70	19.60
Working hours									
Part-time	33.10	33.00	32.70	32.20	32.40	29.00	30.00	29.50	29.20
Full-time	66.90	67.00	67.30	67.80	67.60	71.00	70.00	70.50	70.80
Contract type + working hours									
Permanent full-time	45.20	45.30	45.40	46.30	46.60	49.20	49.00	55.60	56.80
Temporary part-time	13.20	13.10	12.80	12.20	12.20	10.00	10.60	5.70	5.30
Permanent part-time	13.50	13.60	13.70	14.00	13.90	14.40	14.40	18.60	18.90
Temporary full-time	21.10	21.40	21.90	21.50	21.60	20.30	20.20	14.30	13.20

Notes: Descriptive statistics refer to employment spells rather than total employment. The sample consists of continuous employment histories, providing detailed information on each individual employment episode. Values are expressed as percentages.

Table B.2: Descriptive statistics of labour market flows by provincial exposure to MW changes

Provincial exposure	Mean MW incidence (%)	Hires	Separations	Net employment	N provinces
Low exposure	3.92	22,266.57	21,852.46	414.11	18
Medium exposure	4.64	12,830.12	12,608.69	221.43	17
High exposure	5.24	14,175.31	13,930.87	244.43	17

Notes: Provinces are classified according to their average minimum wage (MW) incidence over 2015–2023. Low exposure provinces: Balears (Illes), Gipuzkoa, Bizkaia, Araba/Álava, Navarra, Barcelona, Ceuta, Madrid, Girona, Burgos, Soria, Huesca, Lleida, Zaragoza, La Rioja, Tarragona, Asturias, and Melilla. Medium exposure provinces: Valladolid, Granada, Teruel, Guadalajara, Jaén, Málaga, Cantabria, Palencia, Valencia/València, León, Toledo, A Coruña, Ciudad Real, Albacete, Salamanca, Almería, and Huelva. High exposure provinces: Córdoba, Cádiz, Las Palmas, Alicante/Alacant, Santa Cruz de Tenerife, Segovia, Castellón/Castelló, Sevilla, Cáceres, Pontevedra, Ávila, Cuenca, Lugo, Zamora, Badajoz, Murcia, and Ourense. Hires, separations, and net employment correspond to average annual labour market flows per province.

Appendix Chapter 4

Table C.1: Characteristics of workers entering UI benefit before and after the 2012 and 2023 Reforms: July 10–13 vs. July 17–20, 2012, and December 20–23, 2022, vs. January 4–7, 2023

Characteristics	**2012 Reform**					**2023 Reform**				
	Before	**After**	**Diff.**	**t**	**Sig.**	**Before**	**After**	**Diff.**	**t**	**Sig.**
Gender										
Female	0.436	0.437	-0.001	0.130		0.473	0.471	0.002	-0.280	
Male	0.564	0.563	0.001	-0.130		0.527	0.529	-0.002	0.280	
Nationality										
Foreign-born	0.152	0.152	-0.001	0.140		0.162	0.162	0.000	1.380	
Age group										
18–29 years	0.247	0.248	-0.001	0.220		0.250	0.253	-0.003	0.510	
30–39 years	0.371	0.371	0.000	-0.040		0.284	0.282	0.002	-0.330	
40–49 years	0.247	0.246	0.000	-0.080		0.274	0.273	0.002	-0.270	
50–59 years	0.135	0.135	0.001	-0.130		0.193	0.193	-0.001	0.140	
Reason for job separation										
Dismissal	0.465	0.463	0.002	-0.270		0.457	0.457	0.000	-0.050	
End of contract	0.535	0.537	-0.002	0.270		0.543	0.543	0.000	0.050	
Number of children										
No children	0.572	0.573	-0.001	0.150		0.588	0.594	-0.006	0.970	
One child	0.219	0.219	0.000	-0.100		0.220	0.214	0.005	-0.890	
Two or more children	0.209	0.208	0.000	-0.080		0.193	0.191	0.001	-0.260	
Sector										
Agriculture and fishing	0.075	0.075	0.000	0.160		0.046	0.046	0.000	-0.150	
Industry	0.129	0.128	0.001	-0.270		0.086	0.085	0.001	-0.160	
Construction	0.129	0.129	0.000	-0.050		0.090	0.091	-0.001	0.280	
Services	0.665	0.666	-0.001	0.150		0.726	0.725	0.000	-0.060	
Occupation										
High-skill occupations	0.160	0.160	0.000	-0.080		0.102	0.102	0.000	0.060	
Associate professionals	0.093	0.092	0.000	-0.130		0.077	0.077	0.000	-0.040	
Clerical support workers	0.122	0.122	0.000	0.020		0.146	0.146	0.001	-0.110	
Service and sales workers	0.180	0.180	-0.001	0.160		0.248	0.247	0.001	-0.220	
Craft and trades workers	0.174	0.174	0.000	0.000		0.130	0.132	-0.001	0.250	
Plant and machine operators	0.071	0.071	0.000	-0.010		0.063	0.063	0.000	-0.010	
Elementary occupations	0.201	0.201	0.000	0.000		0.234	0.235	-0.001	0.110	
UI potential benefit duration										
Group 0 (120–180 days)	0.408	0.409	-0.001	0.160		0.480	0.480	0.000	0.040	
Group 1 (240–360 days)	0.172	0.174	-0.001	0.260		0.216	0.217	-0.001	0.150	
Group 2 (420–540 days)	0.088	0.088	0.000	0.020		0.109	0.108	0.001	-0.260	
Group 3 (600–720 days)	0.331	0.329	0.002	-0.390		0.195	0.195	0.000	0.000	
UI amount benefit quintile										
Quintile 1	0.197	0.194	0.003	-0.650		0.197	0.197	0.000	-0.010	
Quintile 2	0.202	0.204	-0.001	0.320		0.200	0.200	0.000	0.080	
Quintile 3	0.221	0.221	0.000	0.060		0.207	0.207	-0.001	0.100	
Quintile 4	0.278	0.279	-0.001	0.260		0.222	0.215	0.006	-1.140	
Quintile 5	0.102	0.102	0.000	-0.030		0.174	0.180	-0.006	1.050	
Unemployment exit										
Remain UI benefit	0.785	0.793	-0.008	1.690	*	0.614	0.613	0.001	-0.160	
Exit UI benefit	0.215	0.207	0.008	-1.690	*	0.386	0.387	-0.001	0.160	
Average UI benefit duration (Days)	299	297	-1.991	-0.730		188	193	-4.708	2.170	**
Number of observations	15,706	14,493				11,983	12,340			

Notes: The selected entry windows correspond to individuals who became UI benefit immediately before and after the implementation dates of the 2012 and 2023 UI reforms in Spain. This comparison allows for the identification of comparable groups around the policy cut-offs, minimizing potential confounding due to seasonal or cyclical labour market variation.SEPE microdata.

Table C.2: Estimated results from the IPW semiparametric PCE model by quintiles of UI benefit amount and by potential benefit duration. 2012 reform

Panel A. By quintiles of UI benefit amount

	All	Q1	Q2	Q3	Q4	Q5
Treatment	-0.296***	-0.038	-0.095	-0.337***	-0.468***	-0.263***
	(0.037)	(0.095)	(0.094)	(0.085)	(0.057)	(0.074)
$\ln(\theta)$	-17.55***	-13.57***	-15.95***	-16.91***	-14.05***	-71.01***
	(0.104)	(0.962)	(1.082)	(0.278)	(0.263)	(0.710)
Constant	4.752***	-2.386***	21.67***	1.656***	4.376***	166.7***
	(0.534)	(0.822)	(0.459)	(0.344)	(1.065)	(1.094)
Observations	253,769	41,462	55,890	50,024	72,600	33,793

Panel B. By days of UI potential benefit duration

	240	300	360	420	480	540	600	660	720
Treatment	-0.413***	-0.493***	-0.082	-0.319**	-0.194	-0.209	-0.021	-0.271**	-0.320***
	(0.106)	(0.126)	(0.135)	(0.143)	(0.170)	(0.159)	(0.222)	(0.126)	(0.0562)
$\ln(\theta)$	-15.60***	-13.89***	-252.1***	-13.81***	-100.9***	-62.86***	0.668	-13.01***	-14.47***
	(0.228)	(0.302)	(0.516)	(0.881)	(0.591)	(1.171)	(1.843)	(0.632)	(1.070)
Constant	4.365***	5.174***	805,494***	32.32***	35.37***	33.52***	5.959***	33.02***	3.740***
	(0.718)	(1.264)	(0.551)	(0.581)	(0.739)	(0.756)	(1.385)	(0.657)	(0.598)
Observations	17,289	13,264	11,532	10,458	10,903	12,196	13,938	22,116	142,073

Notes: For clarity of presentation, all independent variables have been omitted except the treatment variable. The full model in Panel A includes controls for gender, citizenship, age group, number of children, region, occupation, sector, and days of potential benefit duration. The full model in Panel B includes controls for gender, citizenship, age group, number of children, region, occupation, sector, and quintiles of benefits. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. SEPE microdata.

Table C.3: Estimated results from the IPW semiparametric PCE model by quintiles of UI benefit amount and by potential benefit duration. 2023 reform

Panel A. By quintiles of UI benefit amount

	All	Q1	Q2	Q3	Q4	Q5
Treatment	0.063**	0.210***	0.165	-0.063	0.242***	-0.010
	(0.027)	(0.073)	(0.108)	(0.061)	(0.060)	(0.060)
$\ln(\theta)$	-18.13***	-14.65***	0.369***	-15.27***	-15.67***	-17.39***
	(0.068)	(0.179)	(0.079)	(0.155)	(0.161)	(0.102)
Constant	2.988***	-3.276***	2.764***	3.024***	1.783***	1.429***
	(0.108)	(0.285)	(0.489)	(0.265)	(0.213)	(0.224)
Observations	114,323	28,833	23,234	20,231	20,875	21,150

Panel B. By days of UI potential benefit duration

	240	300	360	420	480	540	600	660	720
Treatment	0.154**	-0.017	0.107	0.212*	0.0316	-0.018	0.043	0.034	0.106*
	(0.062)	(0.079)	(0.092)	(0.106)	(0.151)	(0.112)	(0.114)	(0.091)	(0.063)
$\ln(\theta)$	-17.98***	-14.21***	-13.35***	-12.34***	-0.309	-15.30***	-13.89***	-14.21***	-1.680***
	(0.112)	(0.367)	(0.311)	(0.721)	(1.211)	(0.358)	(1.076)	(0.301)	(0.406)
Constant	3.652***	3.459***	3.987***	3.428***	4.406***	3.703***	3.800***	3.193***	3.209***
	(0.266)	(0.321)	(0.366)	(0.416)	(1.078)	(0.447)	(0.538)	(0.362)	(0.298)
Observations	14,258	10,694	9,310	8,303	7,871	8,824	9,341	12,539	33,183

Notes: For clarity of presentation, all independent variables have been omitted except the treatment variable. The full model in Panel A includes controls for gender, citizenship, age group, number of children, region, occupation, sector, and days of potential benefit duration. The full model in Panel B includes controls for gender, citizenship, age group, number of children, region, occupation, sector, and quintiles of benefits. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. SEPE microdata.

Appendix C.A: Spatial Heterogeneous Treatment Effects

This appendix analyses how the effects of the 2012 and 2023 reforms vary across Spain's regions. To do so, the models from Equation 4.1 are estimated separately for each Autonomous Community. The idea is to take advantage of the large differences in unemployment across regions to show that communities with higher unemployment respond differently from those with lower unemployment. For this purpose, we plot the effects against the unemployment rates of these regions in 2012 and 2023, respectively (see Figure C.1).

Regarding the 2012 Reform, the reduction in UI benefit generosity implemented in July 2012 shortened unemployment duration in most regions, consistent with the aggregate results. There is substantial variation across regions. Castile and Leon, Extremadura, Andalusia, and Galicia experienced the largest negative effects, indicating a strong reduction in UI duration following the reform. Other regions, including Valencia, the Basque Country, Navarra, La Rioja and Catalonia, also show moderate reductions. In contrast, some regions—including Asturias, the Canary and Balearic Islands, Cantabria, Castilla–La Mancha, Madrid and Murcia—exhibited little or no effect. These muted responses may reflect structural or institutional factors, such as a higher share of recipients already receiving minimum benefits, lower exposure to the policy change, or regional labour market conditions that dampened the reform's impact.

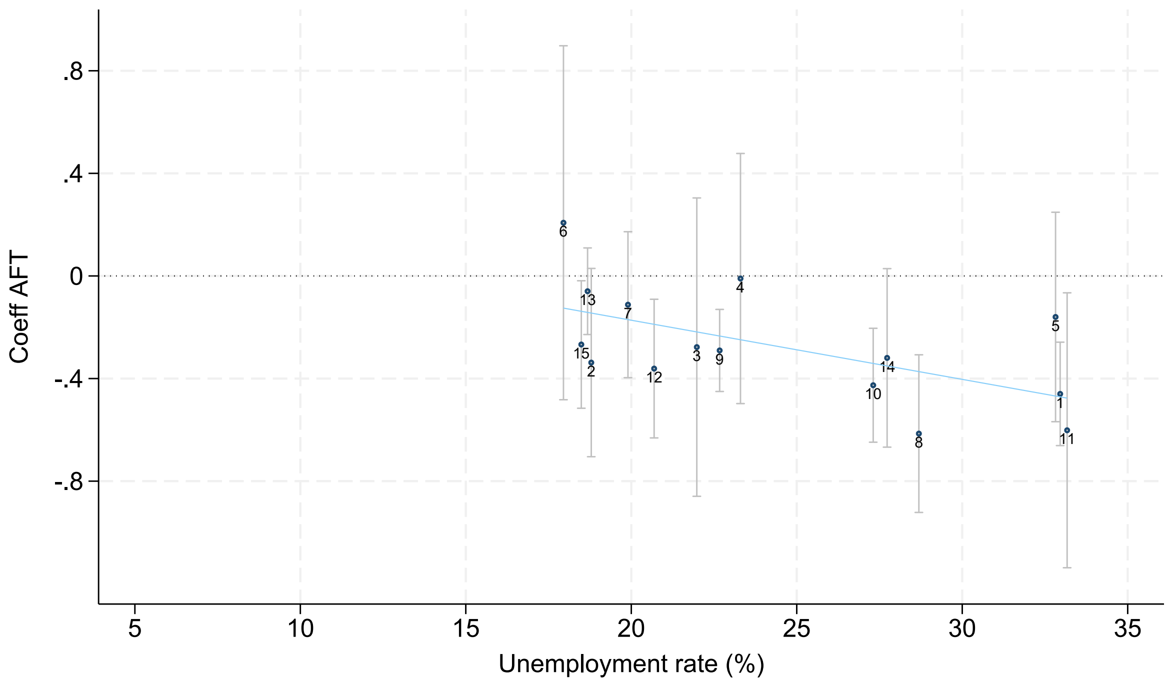
Overall, the 2012 reform displays marked regional heterogeneity, with the strongest reductions concentrated in specific high unemployment regions (Andalusia, Extremadura), while other regions exhibited comparatively minor effects.

By contrast, the 2023 reform, which increased UI generosity, produced mostly non-significant effects on unemployment duration across regions. Only Andalusia, Galicia, and the Basque Country showed statistically significant positive effects, corresponding to a modest increase in unemployment duration among affected recipients. The limited impact in other regions suggests that the benefit increase was generally insufficient to meaningfully alter job search behaviour, except in contexts where the relative income gain was more salient.

Taken together, the regional analysis highlights that the effectiveness of UI reforms depends strongly on local labour market conditions and the magnitude of the policy change relative to recipients' prior entitlements. While the 2012 reduction accelerated exits from UI in several regions, the 2023 increase had a more selective and modest impact, primarily affecting regions where recipients could meaningfully benefit from the higher entitlement.

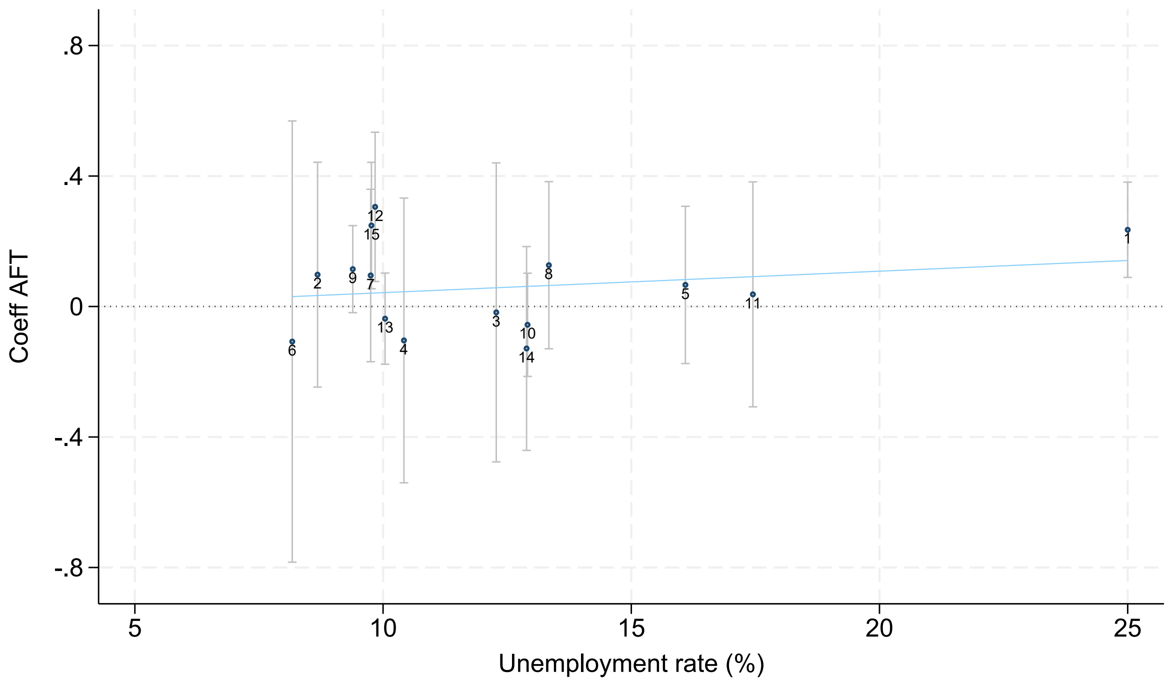
Figure C.1: Heterogeneous Regional Impacts of the 2012 and 2023 UI Reforms

(a) Results from the 2012 reform



Notes: The numeric codes used in the text and figures refer to this table (see Table C.4).

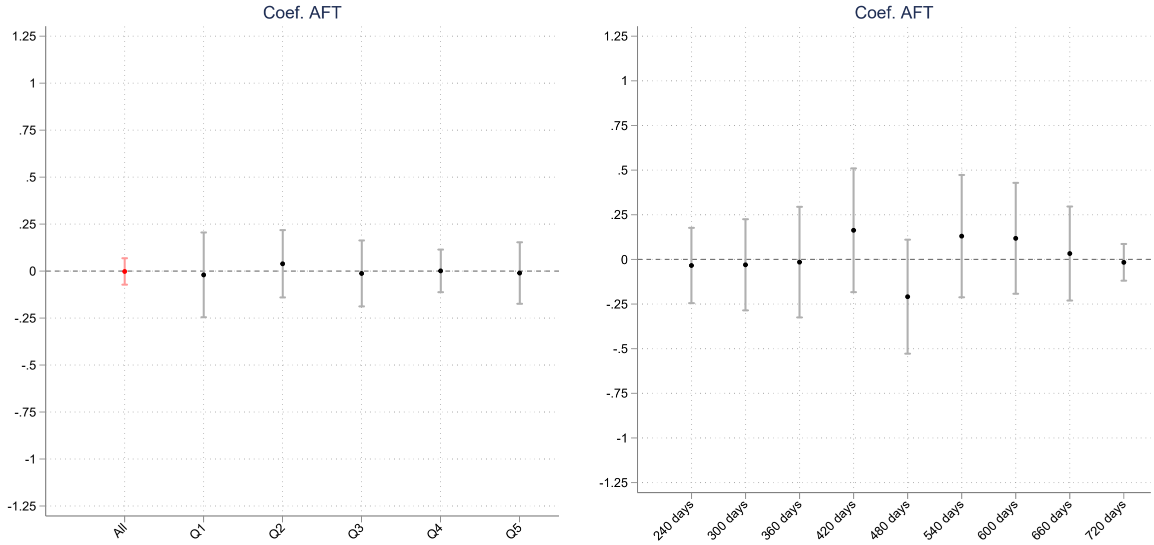
(b) Results from the 2023 reform



Notes: The numeric codes used in the text and figures refer to this table (see Table C.4).

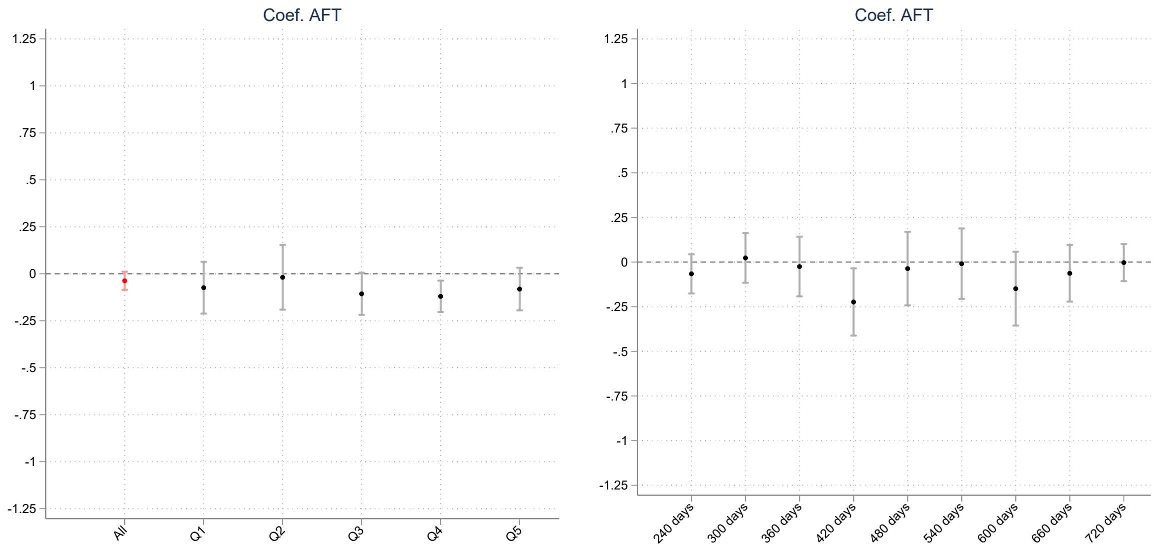
Figure C.2: Placebo tests: Estimated results from the IPW semiparametric PCE model

(a) Results from the 2012 reform by quintiles of UI benefit amount (left) and by days of potential benefit duration (right)



Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification. The number of observations in each model is as follows —Left panel: All: 13,895; Q1: 2,780; Q2: 2,733; Q3: 2,383; Q4: 4,219; Q5: 1,780. Right panel: 240 days: 1,932; 300 days: 1,281; 360 days: 992; 420 days: 734; 480 days: 697; 540 days: 715; 600 days:771; 660 days: 978; 720 days: 5,795. Coefficient (β_2) associated with the treatment variable and CI.

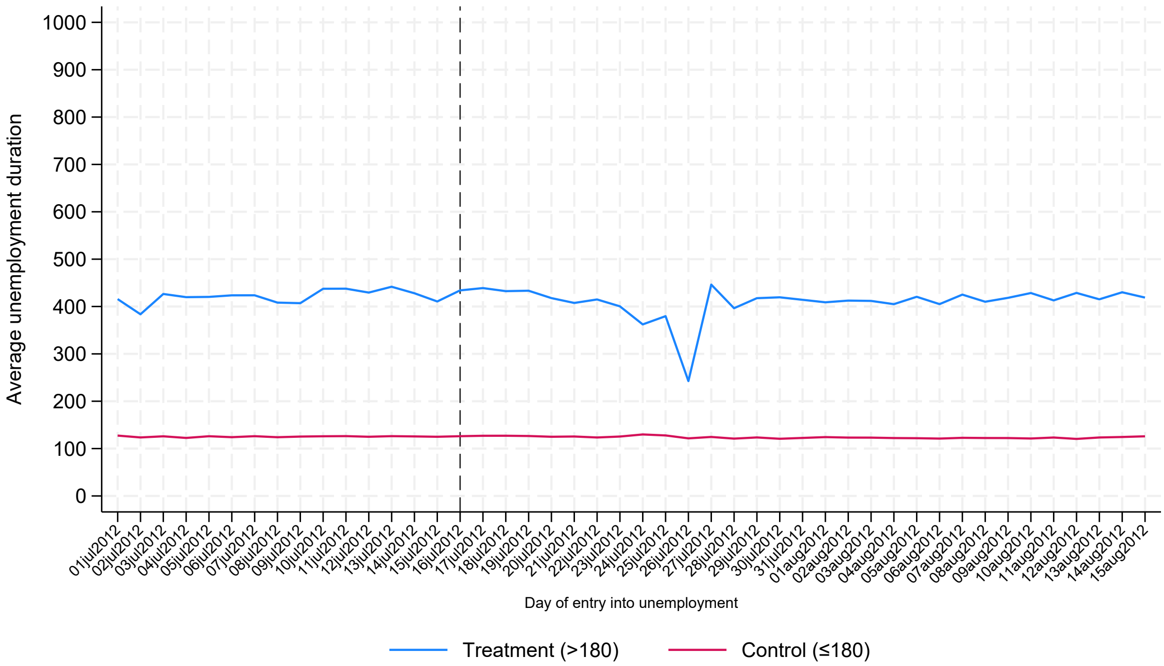
(b) Results from the 2023 reform by quintiles of UI benefit amount (left) and by days of potential benefit duration (right)



Notes: The figure displays the estimated AFT coefficients (β_2) associated with the treatment variable for each specification. The number of observations in each model is as follows —Left panel: All: 13,439; Q1: 3,069; Q2: 2,585; Q3: 2,385; Q4: 3,569; Q5: 1,831. Right panel: 240 days: 2,500; 300 days: 1,658; 360 days: 1,276; 420 days: 987; 480 days: 864; 540 days: 897; 600 days: 860; 660 days: 1,246; 720 days: 2,929.). Coefficient (β_2) associated with the treatment variable and CI.

Figure C.3: Pre-policy trends in unemployment duration: treatment and control groups

(a) 2012 reform



(b) 2023 reform

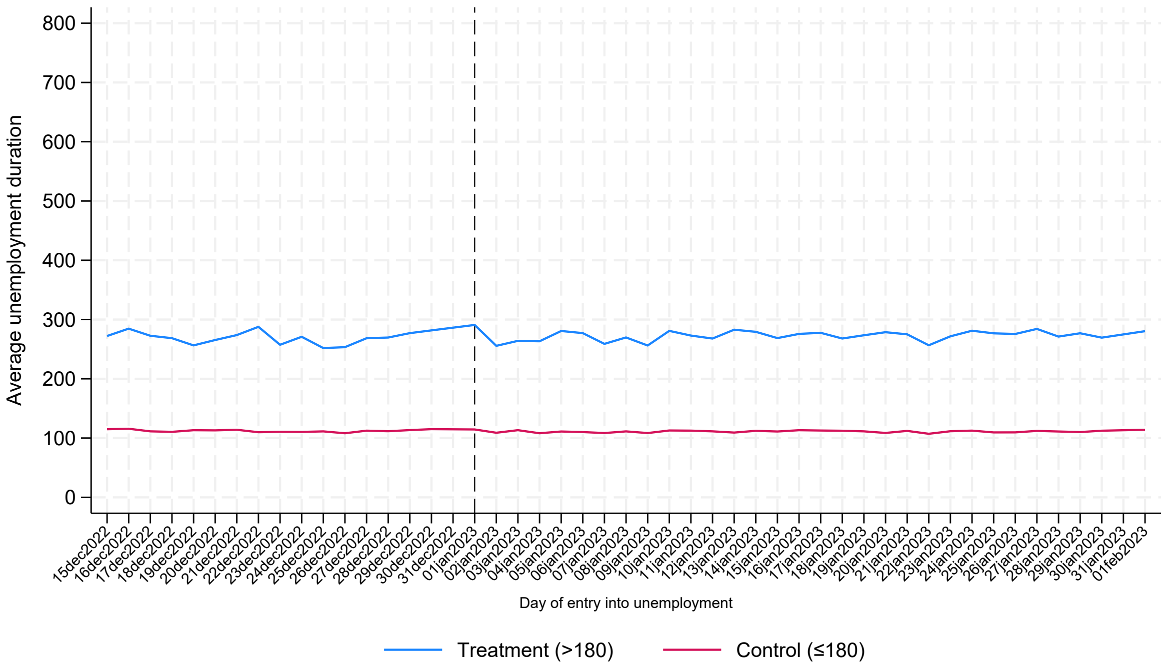


Table C.4: Codes for Spanish Autonomous Communities

Code	**Autonomous Community**
1	Andalucía, Ceuta and Melilla
2	Aragón
3	Asturias
4	Balearic Islands
5	Canary Islands
6	Cantabria
7	Castilla-La Mancha
8	Castilla y León
9	Catalonia
10	Valencian Community
11	Extremadura
12	Galicia
13	Madrid
14	Murcia
15	Basque Country, Navarra and La Rioja

Notes: Ceuta and Melilla are grouped together with Andalusia due to the very small number of observations available for these two cities when considered separately, which improves the stability and precision of the estimations. Similarly, Basque Country, Navarra and La Rioja are grouped together for the same reason.

Table C.5: Pre-reform parallel trends –treatment, daily trend, and interaction

Variable	**2012**	**2023**
Treated	296.1***	159.5***
	(3.519)	(2.937)
Trend	0.0902	-0.0046
	(0.333)	(0.202)
Treated * Trend	0.548	-0.157
	(0.433)	(0.296)
Constant	124.7***	112.6***
	(2.692)	(2.094)
Observations	49,938	43,996
R-squared	0.404	0.222

Notes: The dependent variable is UI benefit duration (in days). The table reports OLS estimates of the treatment indicator, the daily linear pre-reform trend and their interaction, for the 2012 and 2023 reforms. The coefficient on the interaction term is small and not statistically significant, confirming the parallel trends assumption. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. SEPE microdata.