

Gorjón García, Lucía; Martínez de Lafuente, David; Romero, Gonzalo

Article

Employment effects of the minimum wage: Evidence from the Spanish 2019 reform

SERIEs - Journal of the Spanish Economic Association

Provided in Cooperation with:

Spanish Economic Association

Suggested Citation: Gorjón García, Lucía; Martínez de Lafuente, David; Romero, Gonzalo (2024) : Employment effects of the minimum wage: Evidence from the Spanish 2019 reform, SERIEs - Journal of the Spanish Economic Association, ISSN 1869-4195, Springer, Heidelberg, Vol. 15, Iss. 1, pp. 1-55, <https://doi.org/10.1007/s13209-023-00291-1>

This Version is available at:

<https://hdl.handle.net/10419/326949>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



<https://creativecommons.org/licenses/by/4.0/>



Employment effects of the minimum wage: evidence from the Spanish 2019 reform

Lucía Gorjón¹ · David Martínez de Lafuente¹ · Gonzalo Romero¹

Received: 4 October 2022 / Accepted: 2 October 2023 / Published online: 29 November 2023
© The Author(s) 2023

Abstract

We examine the employment effects of the 2019 minimum wage increase in Spain on individual probabilities of losing employment status (extensive margin) and lowering work intensity (intensive margin). To do so, we use variation of workers' exposure to the reform by comparing monthly employment transitions into unemployment and reductions in number of working hours between employees earning less than the minimum wage (treatment group) and those earning more and that should therefore be unaffected by the reform (control group). We find that the new minimum wage significantly increased the probability of experiencing unemployment (1.7 percentage points) and a reduction in work intensity (0.9 percentage points) for treated workers after one year. Our results suggest substantial heterogeneity by age, prior work intensity, economic sector and geographical region of employees affected by the reform.

Keywords Minimum wage · Employment effects · Matching · Work intensity

JEL Classification J23 · J38 · J68 · J88

The replication material for the study is available at <https://zenodo.org/record/8383749>.

✉ Gonzalo Romero
gonzalo.romero@iseak.eu

Lucía Gorjón
lucia.gorjon@iseak.eu

David Martínez de Lafuente
david.martinez@iseak.eu

¹ Fundación ISEAK, Bilbao, Spain

1 Introduction

In a context of rising inequality and job insecurity, minimum wages (MW) emerge as a popular tool to reduce in-work poverty and income inequality in the labor market.¹ Several political and distributive factors explain the growing social and political popularity of MW in the USA and many European countries. From a social standpoint, raises in minimum wages potentially increase income levels of workers located in the lower end of the wage distribution (e.g., teens and younger workers with lower education credentials) (Card 1992; Neumark et al. 2014; Manning 2021; Cengiz et al. 2021). From a political viewpoint, MW constitute a more attractive distributive policy than other monetary transfers because they achieve income pre-distribution without short-term fiscal costs (Barceló et al. 2021).

Despite their growing popularity, minimum wages remain a highly contentious policy from a policymaking and research standpoint. Most controversies over MW reforms stem, generally speaking, from its potential negative effects on employment. While opponents argue that MW entail certain risks for low-skilled workers by increasing unemployment or slowing down job creation (Neumark and Wascher 2010), advocates argue that they do not negatively affect employment (Card and Krueger 1995). Overall, several firm adjustment methods determine the net impact of MW reforms. In this sense, firms can respond to raises in the minimum wage by (a) reducing profit margins (Draca et al. 2011), (b) passing on labor costs to consumers (i.e., through price increases) (Harasztosi and Lindner 2019; Aaronson and French 2007) and (c) making labor adjustments at the extensive (i.e., workforce reductions) and intensive margins (i.e., reductions in contracted hours) (Manning 2021; Clemens 2021). Because local economic conditions (e.g., the degree of monopsony or monopolistic competition, the elasticity between capital and labor, etc.) determine the extent to which firms employ each adjustment path, the disagreement on whether MW deteriorate working conditions through more (less) job destruction (creation) or reductions in working hours largely remains.

In this article, we investigate the employment effects of MW by leveraging a substantial and persistent increase of the Spanish minimum wage. In 2019, Spanish authorities raised the MW by 22% (from €735.90 to €900). The apparent significance of this increase constitutes an interesting case study. First, the 2019 raise was the largest in recent Spanish history, well above the 5% nominal annual growth rate observed between 1981 and 2020. Second, this MW reform had substantial distributional implications by significantly increasing the MW-to-median annual wage income ratio (i.e., the Kaitz index rose from 49% in 2017 to 63% in 2019, according to Barceló et al. (2021)). Motivated by recent evidence of negative MW effects above 60% of median wage (Dube 2019), we leverage this reform to obtain new evidence of MW effects in an environment where firms had significant incentives to restructure their production processes.

¹ In a 2015 report, the OECD states that inequality in the majority of developed nations reached its highest point in the last three decades (OECD 2015). In contrast, according to the most recent data from the Spanish Labor Force Survey for the third quarter of 2021, 26% of the salaried population in Spain has a temporary contract. In terms of work intensity, the rate of part-time employment (13.5%) is not particularly high, but one out of every two individuals working in this capacity does so voluntarily.

To estimate the employment effects of the 2019 reform, we use panel data from the Continuous Sample of Working Lives (CSWL or MCVL for its acronym in Spanish). This administrative register records high-frequency data about wages and employment status of a random sample of workers from the Spanish Social Security. Our empirical strategy relies on comparing employment transitions between a group of workers who earned less than the newly established MW (treatment group) and a group of workers who earned more than the minimum wage threshold and that should therefore be unaffected by the reform (control group). While this approach somewhat resembles previous research in countries like the USA (Linneman 1982) and Germany (Dustmann et al. 2021), our analysis expands upon previous studies focusing on Spain (e.g., Galán and Puente 2012, 2015; Barceló et al. 2021) in several ways. First, we incorporate nearly the entire working population in our sample and thus do not restrict the analysis to full-time working population (e.g., Barceló et al. 2021). Our analytical sample therefore includes two important groups of workers that generally receive lower wages and might be more vulnerable to job loss after the reform: part-time workers and people who do not work every day of the month. Second, our empirical approach compares employment transitions on a monthly basis, therefore investigating the dynamic nature of employment adjustment and the possible differential effects of MW in the short and the medium term. Finally, our research design controls for observable differences between treatment and control groups using a matching strategy. While previous studies (e.g., Galán and Puente 2015) rely on the simple inclusion of covariates in a generalized difference-in-differences type of regression, our approach is based on a more careful application of the coarsened exact matching (CEM) technique (Iacus et al. 2012). Our research design thus allows to attain balance in observable characteristics while simultaneously relaxing the parametric specification used by these regression approaches.

We do not find immediate significant impacts on the probability of losing employment status or reducing the number of working hours following the MW reform. However, our dynamic estimates indicate statistically significant and sizable negative employment effects between five and twelve months after the reform. Altogether, the results suggest that, after one year, most of the negative employment effect happens through loss of employment status (64.9%) rather than a reduction in the number of working hours (35.1%). In particular, our baseline analysis suggests that, after twelve months, the 22% raise in minimum wage increased the probability of unemployment (reducing working hours) by 1.7 p.p. (0.92 p.p.) for workers affected by the reform. Taken together, the results imply an employment loss elasticity of -0.08, consistent with findings from Barceló et al. (2021) in Spain and median elasticity estimates from studies in the USA (Neumark and Shirley 2021).

To assess the presence of heterogeneous effects, we conduct separate analyses according to the gender, age, prior work intensity, location and prior economic sector of workers. Several findings emerge from this analysis. First, our findings indicate limited (statistically nonsignificant) differences between men and women workers in the probability of losing employment. In terms of work intensity, we observe a more immediate adjustment for men and a larger impact for women over the medium term. Second, results suggest significant heterogeneity by workers' age. Interestingly, we find that, while younger workers are more affected in terms of work intensity, older

workers suffer a larger employment loss effect. Third, we observe large differences between full-time and part-time workers, with the former expecting a larger work intensity adjustment. Fourth, we find considerable variation in the effect of the policy between the North and South of Spain. In particular, we find that the MW reform entailed more immediate and larger impacts for workers located in southern regions, where nominal salaries and market tightness are lower. Finally, we do find some evidence of more significant employment effects in the tertiary sector.

To validate the stability and reliability of our results, we perform a number of checks. In particular, we experiment with changing the baseline day of the week used for computing employment transitions and evaluating possible anticipation effects. Overall, our findings withstand these robustness checks. Additionally, we run a placebo test in a period with no MW increases. This placebo test yields null results, supporting the interpretation that our findings are not primarily driven by violations of the conditional independence assumption (CIA) through uncontrolled differences between treatment and control groups in the probability of harmful employment transitions. To go beyond these checks, we also produce impact estimates of the policy using an alternative identification approach based on a regression discontinuity design (RDD). Although we find that results vary significantly depending on the specification, the observed effects are consistent with an increased probability of unemployment in our preferred RDD approach. Finally, we complement our analysis using a macro-approach in the spirit of Harasztosi and Lindner (2019). To this end, we examine the difference between the pre- and post-treatment reform hourly wage frequency distributions. While suggestive, the analysis provides evidence of moderate employment loss in the economy in the year following the reform.

Our paper contributes to the vast empirical literature on the employment impact of minimum wages. Despite the growing number of microeconomic papers in the literature, the evidence on negative employment effects of MW remains mixed (e.g., see interesting surveys by Neumark and Wascher 2010; Card and Krueger 1995; Manning 2021). Ultimately, a large bulk of the literature finding negligible MW negative effects is criticized because they focus on small and temporary MW reforms (Sorkin 2015; Aaronson et al. 2018). Similar to Dustmann et al. (2021) and Harasztosi and Lindner (2019), this study addresses this issue directly by exploiting a large and permanent sharp increase in the minimum wage.

The paper is structured as follows. Section 2 reviews the literature, both nationally and internationally. Section 3 describes the institutional setting and data sources. Section 4 explains the identification strategy. Section 5 describes the results and Sect. 6 summarizes the robustness check. Finally, Sect. 7 discusses the limitations of the study and Sect. 8 concludes.

2 Literature review

Due to the mixed nature of the empirical evidence, the vast research literature on labor impacts of MW remains highly contentious (Manning 2021). For its part, empirical research has shown that an increase in the minimum wage does not necessarily have a negative effect on employment. Several papers stand out in this literature including

seminal contribution by Card and Krueger (1994) and, more recently, studies such as Cengiz et al. (2019, 2021) in the US case or Dustmann et al. (2021) in Germany. All in all, Neumark and Shirley (2021) show that most of MW articles find significant negative employment effects, particularly for low-skilled individuals.

The literature examining employment effects of successive MW in Spain can be deemed as large. Overall, research papers focusing on young workers (i.e., Dolado and Felgueroso 1997; Dolado et al. 1997; González et al. 2003; Galán and Puente 2015; Arellano and Jansen 2014) find negative employment effects. However, there are a few notable exceptions, including Cebrián et al. (2010), who find no effect on teenage employment, and Blázquez et al. (2009), who find a short-term positive effect of the MW on youth employment. In contrast, González et al. (2012) find a negative effect on immigrant worker employment that varies by gender and region of origin. More recently, Cebrián et al. (2020) examine the probability of maintaining employment of people affected by the MW for the 2017 increase, discovering a negative effect just before the reform's entry into force.

The number of papers focusing on the 2019 reform is scarcer. Using a micro approach, Lacuesta et al. (2019) estimate a 0.8% decrease in full-time salaried employment. The authors use microdata from the 2017 CSWL and rely on a projection of the analysis of the 2017 increase. Using a similar approach to ours, Barceló et al. (2021) use the CSWL database to examine the employment effects of the 2019 reform. The authors find a net employment aggregate loss of between 6 and 11% for workers directly affected by the measure. The estimated impact on individual employment loss, on the other hand, is between 2 and 3 p.p. for people working full-time for 30 days per month and up to 4 p.p. in terms of equivalent working hours for workers in the hospitality sector. One important limitation of these studies is that they exclude two particularly vulnerable groups of workers from the analysis: people working part-time and workers with contracts of less than a month. Using a more macro-approach, the AIREF estimates an employment loss of 0.13–0.23 p.p., which implies a drop of between 19,000 and 33,000 affiliated workers (AIREF 2020). The authors find evidence of uneven effects based on worker characteristics, with female and younger workers from less developed regions being more adversely affected. Using the Spanish Labor Force Survey (EPA), the Economic Cabinet of the workers' union *Comisiones Obreras* find that the decline in enrollment in the first quarter of 2019 was very similar to that of 2018, and the probability of maintaining employment for certain groups at risk due to the rise in the MW grows from 2018 to 2019. Based on this evidence, the authors conclude that the employment effects of the 2019 reform was close to zero (Comisiones Obreras 2019).

3 Institutional setting and data

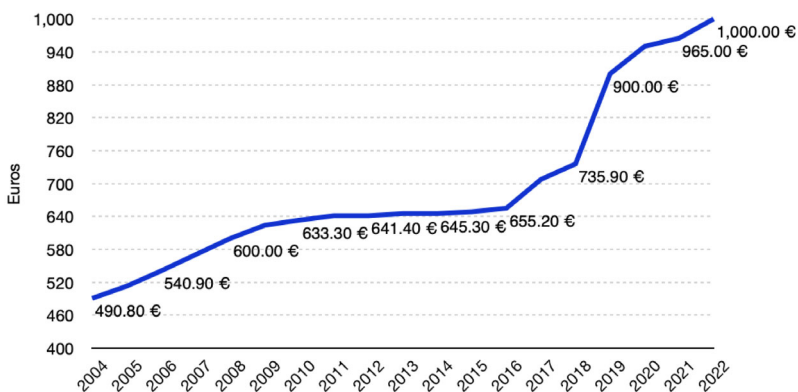
3.1 Institutional setting

In Spain, the central government oversees annually the setting of MW after negotiations with the most representative unions and employers' associations. Should the negotiations between the three parties fail, the central government can unilaterally

decide whether to adjust MW and by how much. Unlike in the USA, there are no territorial differences in the Spanish minimum wage since its application has a national scope. Since 1998, MW coverage extends to all workers, regardless of age or affiliation to a workers' union (Galán and Puente 2015). Annual MW updates typically consider a number of factors, including the Consumer Price Index (CPI), the national average productivity and the overall economic situation.

In recent years, the evolution of MW in Spain has been uneven (Fig. 1). Between 2004 and 2008, the MW grew steadily from €490.8 to €600 (a nominal increase of 22.3% or 5.6% per annum). As a consequence of the economic downturn, the MW grew at a slower rate during the Great Recession and early stages of recovery, reaching €655.2 in 2016. Although the annual growth can be deemed as small (1.2% per year on average), it ensured that the purchasing power of affected workers remained consistent with those stipulated in collective bargaining agreements (Barceló et al. 2021). After 2017, the MW began a faster growth. First, the center-right government of the Popular Party (PP) nominally increased minimum wages by 8% (from €655.2 to €707.7). Then, the center-left government of the Socialist Party (PSOE) introduced a sharp raise of the MW until €900 per month. Given that the Spanish MW in 2018 was €735.90, this decision represented a 22.3% year-on-year increase, which was unprecedented in recent decades. In subsequent years, further increases have occurred, albeit with a lesser magnitude than in 2019, until the MW reached €1000 in 2022.

The 2019 MW increase occurred in a context of economic expansion and job creation, albeit with signs of deceleration. According to the Spanish Statistical Office (INE for its Spanish acronym), the Spanish economy grew by 2% in 2019, 0.3 p.p. less than in 2018 and one p.p. less than in 2017. Similarly, data from the Spanish Labor Force Survey (EPA) reveal a 2.3% increase in the number of employed individuals in 2019, which is 0.3 percentage points less than the increase in 2018 and 2017.



Note: The chart shows monthly MW at 14 full time payments. For the year 2004, it shows the MW in effect as of July 1. For the year 2021, it shows the MW in effect as of September 1. Source: Ministry of Labour and Social Economy

Fig. 1 Evolution of MW in Spain (2004–2022)

3.2 Data and descriptive statistics

3.2.1 Description of the data source

Our primary data source is the 2019 Continuous Sample of Working Lives microdata (CSWL or MCVL for its acronym in Spanish). The CSWL employs administrative tax records from the Spanish Social Security (TGSS) and the Spanish counterpart of the IRS. For the specific purposes of this paper, we use the CSWL version without tax data.

The CSWL records a 4% representative random sample of the total workers affiliated with the Spanish Social Security. The 2019 CSWL data detail each worker's working history and earnings since their entry to the labor market until December 31, 2019. The CSWL has a panel data structure that allows to follow individual workers before and after the increase in the MW.

We observe individual personal characteristics of workers (e.g., gender and age), as well as registry dates of their employment and unemployment episodes of Social Security affiliation. For employment episodes, the CSWL includes, among other information, the type of contract and a part-time coefficient (hereafter, *coefpar*) that allows to impute number of working hours. Further, we observe the monthly contribution base for each individual and employment episode. In the analysis, we use this monthly contribution base as a *proxy* variable for wages.²

Based on the CSWL, we construct a monthly panel that follows workers over the January 2018 and November 2019 period. Using individual worker-level information, we use monthly employment status to record worker-specific individual labor trajectories. As we will discuss later, our main analysis relies on comparing employment transitions after the introduction of the 2019 reform between a treatment and a control group. To define these employment transitions, we designate November 2018 as the baseline period or *t0*. Although December is the period immediately preceding the increase in the MW, we instead use November to alleviate seasonality concerns. Further, we employ the second Tuesday of each month as the specific day to define employment transitions.³

To keep the panel tractable, we restrict our analysis to people working as employees and affiliated with the Social Security System on the second Tuesday of November, 2018. To enhance the validity of results, we incorporate workers in the Special System for Agricultural Workers and workers in the Special System for Household Employees in the sample.⁴ At the same time, we focus on people between age 16 and 60 as

² For a detailed explanation of this database, see Pérez (2008).

³ On this basis, we intend to reduce the variations in job creation and destruction caused by the calendar, which, in the case of Spain, have greater significance on the first and last days of each month, as well as on Mondays and Fridays (Conde-Ruiz et al. 2019).

⁴ In the cases of these two special contribution systems, it is not possible to include people working part-time in the analysis, since the variable indicating the length of the working day does not exist, which makes it impossible to infer the number of hours worked and, consequently, the wage per hour worked. This fact is an important limitation in the case of the Special System for Household Employees, since 61.8% of the people in this special system have a part-time contract in *t0*, while the incidence of part-time work is practically nonexistent in the case of the Special System for Agricultural Workers.

of January 2018 ($t - 10$) to exclude workers affected by potential transitions into retirement in the estimation period.

One data complication is that workers can display several employment spells in a given month. To define the monthly panel, we make the following decisions. In cases where an individual is both employed and unemployed in a given month, we keep the employment episode in the panel. Alternatively, we exclude workers that hold multiple jobs or that are simultaneously employed and self-employed. As a result, we drop 6.1% of observations in the sample (see Table 3 in Appendix for a brief descriptive analysis of sociodemographic characteristics of these observations).

Because information about specific creation or destruction of job positions is not available, an important limitation of the CSWL is that one cannot directly study job destruction. Thus, it is important to note that our paper focuses on the specific impact of MW on individual employment transitions rather than on the aggregate impact on job destruction.

3.2.2 Sample selection and calculation of hourly wages

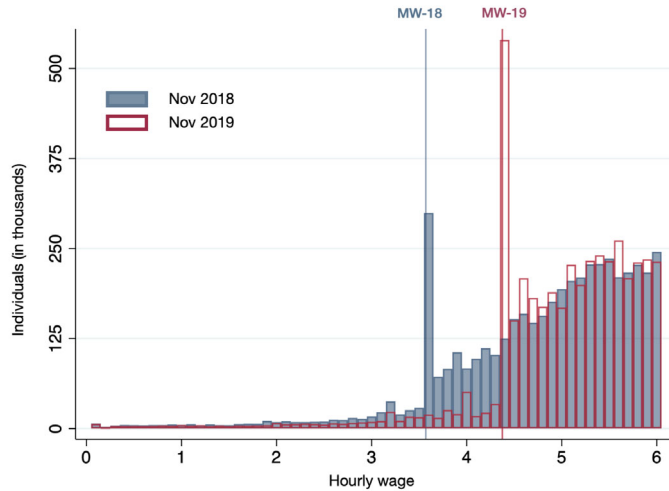
To maximize representativeness and sample size, our analysis expands beyond full-time workers employed every day of the month. In particular, our analysis includes full-time employees that do not work every day of the month and part-time workers, regardless of the number of days worked in a month. Previous work in Spain has typically excluded these types of workers that are arguably more vulnerable to potential job loss from MW raises.

As a result of including these workers in the analysis, one needs to impute hourly wages to determine workers' position relative to the MW in the reference period. That is, we will directly assign individuals to treatment and control group based on whether their hourly wage in $t0$ falls below or above the post-reform hourly MW. To determine the pre-treatment hourly wage, we first determine how many days each person worked during each time period. The number of daily hours worked in the reference period is then imputed using the work intensity coefficient *coefpar*. This is an important drawback of the data that do not report actual working hours but a coefficient reflecting work intensity (ranging from 0 to 1000 for full-time workers). Also, because specific wage earnings data are missing in the CSWL, we proxy workers' wages through data of Social Security monthly contribution bases in the establishment each individual works. As a result, the formula used to calculate each worker's hourly wage proxy is as follows:

$$\frac{\text{Monthly contribution basis}}{\text{Days worked in reference month}} / \text{Hours worked observed in period } t$$

To impute numbers of hours worked from the part-time coefficient, full working day is set at 8 h. As a result, we set to four the number of hours worked per day for, say, a person working half-time (i.e., with a work intensity coefficient of 500).⁵

⁵ Given that the full working day is not equivalent to 8 h in all agreements, there is a small measurement error in this variable. The results should not be affected if this error is randomly distributed among the groups covered in this study.



Source: Own calculations using CSWL 2018 and 2019.

Fig. 2 Employee distribution (November 2018 and November 2019) *Source:* Own calculations using CSWL 2018 and 2019

To determine treatment status, we compare each worker's hourly wage with the post-reform hourly MW. That is, we consider as treated those workers who earned less than the newly established hourly MW. To compute the 2019 hourly minimum wage, we follow several steps. First, because the MW is based on 14 payments and contribution bases are defined monthly, we prorate the €950 MW into 12 payments, thus resulting in a monthly MW of €1050. Second, because treatment status is assigned according to observed hourly wages, we re-express the post-reform MW into hourly wages. This approximation yields a MW threshold of €4.375 per hour.⁶

3.2.3 The hourly wage distribution before and after the 2019 reform

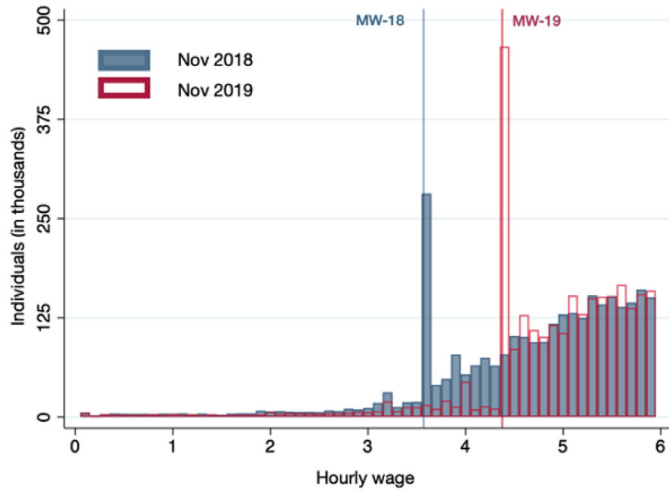
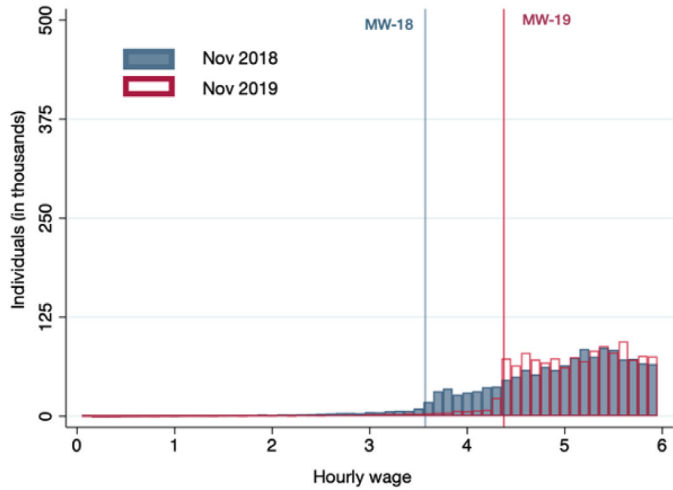
Figure 2 depicts the hourly wage distribution prior to the new MW's implementation (November 2018) and one year after (November 2019). As can be seen, we observe a shift in wage distribution from the 2018 MW⁷ to the 2019 MW and a decrease in the number of wage earners below this threshold.⁸ Such pattern can be seen also in the period immediately after the new MW came into force, suggesting that the policy change generated rapid adjustments in the labor market (see Fig. 12 for a comparison of hourly wage distribution between November 2018 and January 2019).

The data suggest a significant shift in the wage distribution for both full-time and part-time workers (see Fig. 3). At the same time, we observe clear bunching of workers

⁶ This reference is calculated by dividing the 12-month prorated MW (1050) by 30 days and then by 8 h.

⁷ For 2018, the MW in terms of wage/hour is €3.57.

⁸ It is possible to find unusually low contribution bases, below the legal minimums, which may be due to errors or irregular situations.

a) *Full-time*b) *Part-time*

Source: Own calculations using CSWL 2018 and 2019.

Fig. 3 Employee distribution, by work day (November 2018 and November 2019)

around the post-reform €4.375 hourly minimum wage threshold for both types of workers. Altogether, this suggests that our approach toward computing hourly wages provides a fairly reliable identification of potentially treated workers, including part-time workers.

3.2.4 Who are minimum wage workers?

Table 1 provides descriptive information about the general characteristics of minimum wage workers. While column 1 describes the proportion of workers affected by the reform (i.e., those earning less than €4.375 per hour in November 2018), columns (2) and (3) depict the incidence of the legally binding MW in 2018 and 2019. Focusing on results from column 1—that describes MW incidence of our potential treatment group—we find that approximately 9% of the entire working population is affected by the MW increase. However, there exists significant heterogeneity based on workers' characteristics. In particular, we find that the raise in MW disproportionately affects women (10.6%), young people—notably those under the age of 26 (24.1%)—and workers in firms with fewer than 10 employees (17.7%). We find sizable sectorial disparities of MW. The primary sector shows the highest incidence of the new minimum wage, with about 1 in 2 individuals affected (44.3%). The service sector has a relatively high prevalence, including hotels and restaurants (9.7%) and commerce (8.9%), as well as home, artistic, recreational and social activities (27.3%).

4 Methodology and identification strategy

4.1 Evaluation approach

When establishing causality, it is desirable that individual (un)observed features between treatment and control groups differ as little as possible. Randomization achieves balance between two groups but rarely occurs in policymaking. The 2019 MW reform is no exception in this regard. In addition, because minimum wage in Spain is negotiated at the national level, a control group of strictly comparable workers is unavailable (i.e., individuals who receive a wage below the new MW and who are not affected by the reform). These two elements hinder our ability to address selectivity into treatment using quasi-random (regional) variation (e.g., Cengiz et al. 2019, 2021).

To tackle these limitations and reliably assess the impact of the MW increase, our approach fundamentally relies on comparisons of employment transitions between a group of workers affected by the reform (treatment group) and a set of workers unlikely impacted by the raise (control group). We define treatment assignment statically using workers' November 2018 (t_0) hourly wage position relative to the 2019 MW threshold (€4.375 per hour). This empirical approximation presumes that (1) individuals earning less than the 2019 MW in t_0 are directly affected by the reform and (2) workers with wages above this threshold are not impacted by the change.⁹ Further, our approach crucially relies on a high comparability between workers in the treatment and in the control groups. That is, employment transitions of members in the control group need to provide a good approximation of the counterfactual transitions of treated workers, in the absence of the MW increase.

⁹ A possible effect that is not possible to capture in this analysis is that the increase in the MW entails indirect effects whereby workers with wages above the MW would be negatively affected by the reform. If so, the results of the impact estimation could contain downward biases. This and other limitations of the impact study are discussed in detail in the corresponding section.

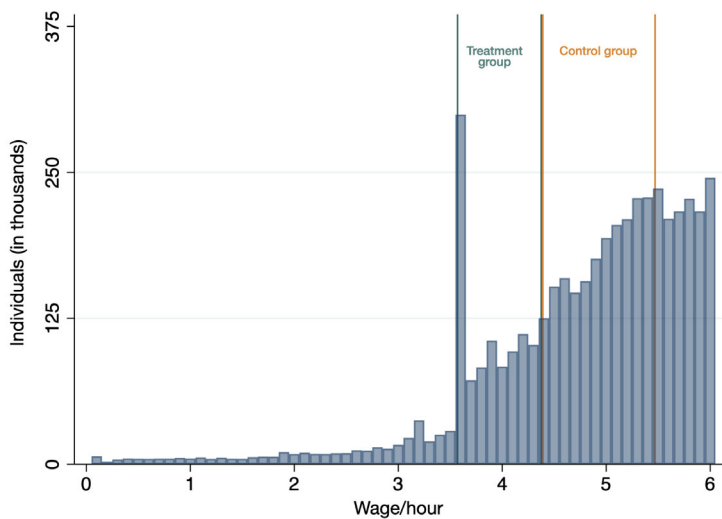
Table 1 Incidence of MW in November 2018 and November 2019 (%) *Source:* Own calculations using CSWL

Group	November 2018 Incidence of 2019 MW (%)	November 2018 Incidence of 2018 MW (%)	November 2019 Incidence of 2019 MW (%)
Total	9.0	4.6	6.2
<i>Gender</i>			
Men	7.5	4.3	5.7
Women	10.6	5.0	6.8
<i>Age</i>			
16–25 years	24.1	13.1	15.4
36–34 years	10.6	5.0	6.7
36–44 years	6.8	3.3	4.9
45–54 years	6.7	3.6	5.1
55 years and older	6.0	3.7	5.2
<i>Establishment size</i>			
Less than 10 employees	17.7	10.2	13.9
10–49 employees	7.7	2.7	4.2
50–249 employees	6.3	2.3	3.4
250 or more employees	3.0	1.0	1.8
<i>Economic activity (sector)</i>			
<i>Primary</i>	44.3	26.5	35.7
Mining and quarrying, manufacturing, supplies	3.6	1.5	2.3
Construction	2.9	1.2	2.0
Retail	8.9	2.9	5.1
Transportation and storage	4.3	1.5	2.7
Accommodation and food service	9.7	4.7	4.3
Information and communication	5.3	2.3	3.7
Financial, insurance and real estate	5.8	1.9	4.0
Professional, scientific and technical	8.5	3.9	5.6
Administrative and support services	12.7	4.3	6.8
Public administration	6.3	3.5	4.5
Education	8.0	4.1	4.5
Health and social work	5.1	1.8	2.8
Household, arts, entertainment and recreation	27.3	13.0	17.7

Our main outcome variable identifies monthly employment transitions experienced by workers relative to $t0$ (when all of them were employed). To examine the dynamic nature of the reform's impact, we define monthly transitions between November 2018 and 2019. The key idea is to assess the impact, from a worker's perspective, on the extensive margin (i.e., probability of transitioning to unemployment) and on the intensive margin (i.e., probability of a reduction in number of working hours). To do so, we define three possible individual transitions: (1) the affected person remains employed and their work intensity does not decrease, (2) the affected person remains employed but their work intensity is lower than at $t0$, and (3) the affected person transitions to unemployment (understood as non-employment).

4.2 Implementation of identification strategy

We begin by comparing workers located on opposite sides of the 2019 MW cutoff in the hourly wage distribution. To mitigate possible productivity differences between groups, we define two relatively narrow hourly wage intervals to construct treatment and control groups (see Fig. 4). More specifically, we assign workers with hourly wages between the 2018 MW (€3.57 per hour worked) and the 2019 MW (€4.375) to the treatment group. Alternatively, we assign individuals with wages between 102 and 125% of the new MW (or equivalently, between €4.39 and €5.47 per hour worked) to the control group. We discard individuals with hourly wages below 2018 MW threshold due to potential lack measurement error in the CSWL (i.e., firms could not pay wages below €3.57 per hour in $t0$). Workers with salaries in the 100–102% MW range are



Source: Own calculations using CSWL.

Fig. 4 Treatment and control groups

also excluded to minimize the risk of incorrectly assigning treated individuals to the control group (see hourly wage imputation process described in Sect. 3.2).

One unlikely possibility is that falling into the treatment or the control group (or equivalently, to the left or the right of the 2019 MW threshold) is “as good as random.” While this assumption cannot be directly tested, we evaluate the balancing of several observed characteristics between the treatment and the control group. Table 2 shows that there statistically significant ($p < 0.01$) and quantitatively large differences between these two groups. Ultimately, this suggests that one should pursue additional strategies to address selectivity into treatment and mitigate bias in the estimation process.

To enhance balance between treated and control units, we rely on a matching strategy. In particular, we apply a *coarsened exact matching* (CEM) approach to match treated workers in a 1:1 ratio to controls using pre-treatment characteristics at t_0 . More specifically, our matching procedure performs exact matching between workers below and above the 2019 threshold on the following criteria: gender, nationality, establishment size intervals, contribution group, contribution regime, industry and type of contract. We do almost exact matching after coarsening the following continuous variables into intervals: age groups and work intensity coefficient.¹⁰ Compared to exact matching or PS matching, our approach enhances the overlap between treatment and control groups, while simultaneously avoiding practical drawbacks like the “curse of dimensionality” or the incorrect specification of the propensity score. Workers left unmatched by the algorithm are excluded from the analysis. In order to maximize sample size and enhance comparability, we use matching with replacement that allows control units to be paired with multiple treated units. Applying this matching strategy, we are able to find one control unit for 31,238 workers (out of a total 35,144, 88.9%).¹¹ Table 2 describes the quality of the matching procedure. By design, we find that differences in observable characteristics between the treatment and control groups are not statistically significant.

Our matching approach relies on a selection-on-observables type of identification, which requires that employment transitions of control units provide a reliable approximation of the counterfactual situation of treated units, in the absence of the reform. That is, to identify the effect of the 2019 MW raise, we need that, conditioning on observed covariates, treatment assignment is independent of potential employment transitions in the case of no treatment. We believe that our matching approach somewhat improves traditional regression modeling strategies. While “controlling” for covariates through regression is (in principle) a good strategy, it has some limitations. First, regression modeling is not transparent about the distribution of covariates between treatment and control groups. Second, regression heavily relies on model specification through functional forms (i.e., extrapolation), unless there exists significant overlap between treatment and control groups.

¹⁰ Since it is possible to find multiple partners for the same individual, one is selected at random. Thus, all individuals in the treatment group are matched with the same number of individuals. As a result, when this technique is applied, the distributions of the treatment and control groups are identical, as well as the number of observations in both groups.

¹¹ Once matching is carried out, the sample of treated individuals accounts for 65% of all individuals who had a wage below the new MW at t_0 .

Table 2 Differences in means between treatment and control groups, before and after matching

Group	Pre-matching			Matching			
	Treatment (1) (%)	Control (2) (%)	Test value) (3)	(p- Treatment (4) (%)	Control (5) (%)	Test value) (6)	(p-
Gender							
Men	43.4	42.4	0.001***	42.3	42.3	1.000	
Women	56.6	57.6	0.001***	57.7	57.7	1.000	
Age							
16–25 years	23.2	13.3	0.000***	19.1	19.1	1.000	
36–34 years	28.2	27.7	0.1444	28.8	28.8	1.000	
36–44 years	23.4	29.1	0.000***	25.2	25.2	1.000	
45–54 years	19.0	22.5	0.000***	20.4	20.4	1.000	
55 years and older	6.3	7.4	0.000***	6.5	6.5	1.000	
Nationality							
Native	81.0	85.5	0.000***	81.6	81.6	1.000	
Immigrant	19.0	14.5	0.000***	18.4	18.4	1.000	
Type of contract							
Permanent full-time	35.6	48.3	0.000***	39.3	39.3	1.000	
Permanent Part-Time	15.9	18.7	0.000***	16.4	16.4	1.000	
Temporary Full-Time	27.9	19.8	0.000***	30.6	30.6	1.000	
Temporary Part-Time	11.7	12.6	0.000***	12.0	12.0	1.000	
Establishment size							
Less than 10 employees	55.5	42.8	0.000***	54.8	54.8	1.000	
10–49 employees	19.8	24.5	0.000***	20.4	20.4	1.000	
50–249 employees	14.7	16.6	0.000***	14.6	14.6	1.000	
250 or more employees	9.9	16.1	0.000***	10.2	10.2	1.000	
Work intensity							
Coefpar = 1000	71.6	66.1	0.000***	70.9	70.9	1.000	
Coefpar [750–1000)	8.8	12.1	0.000***	9.0	9.0	1.000	

Table 2 (continued)

Group	Pre-matching			Matching			
	Treatment (1) (%)	Control (2) (%)	Test value) (3)	(p- Treatment (4) (%)	Control (5) (%)	Test value) (6)	(p-
Coefpar [500–750)	12.4	14.3	0.000***	13.1	13.1	1.000	
Coefpar [250–500)	5	5.2	0.190	4.9	4.9	1.000	
Coefpar < 250	2.2	2.3	0.441	2.0	2.0	1.000	
<i>Observations</i>	<i>35,144</i>	<i>77,404</i>		<i>31,238</i>	<i>31,238</i>		

We report the differences in observed characteristics between treatment and control groups before (columns (1) and (2)) and after (columns (4) and (5)) the matching procedure. Columns (3) and (6) report the p-values for tests for statistically significant differences in observed characteristics. In these columns, ***, ** and * indicate significance at the 1, 5 and 10% critical level. Source: Own calculations using CSWL

An important drawback of our empirical strategy is that the conditional independence assumption (CIA) is highly restrictive. In this sense, we are aware that difference-in-differences (DID) methods constitute, in principle, a more attractive empirical approximation. For this reason, we first experimented with adopting an event–study design on the matched sample for causal identification. Ultimately, we discarded this possibility because significant pre-trend differences were found between treated and control units (see Fig. 13 in Appendix). Thus, our final decision reflects preferences toward pursuing a more transparent design that, despite its flaws, provides a better intuition about the target causal parameter being estimated, in our opinion.

4.3 Regression specification

We estimate employment effects of MW by comparing monthly employment transitions of treatment and control units relative to baseline period $t0$ (i.e., November 2018). To do so, we use multinomial logit regression in the matched sample. Again, our dependent variable reflects three different monthly transitions: (1) the person remains employed and their work intensity does not decrease, (2) the person remains employed but their work intensity is lower than at $t0$, and (3) the affected person transitions to unemployment (understood as non-employment).¹² Formally, we propose the following log-odds linear specification for each transition $j = \{1, 2, 3\}$:

$$\rho_{ijt} = \log \frac{\pi_{ijt}}{\pi_{i1t}} = \beta_{jt} MW_i + X_i' \delta_{jt},$$

¹² Our specification is implicitly geared toward estimation of monthly transitions that could differ substantially from cumulative impacts on employment loss (as measured, for example, by duration in unemployment until a given month). Our approach does not therefore consider potential negative effects of the reform on the duration of unemployment spells for the treatment group.

With the following multinomial probabilities:

$$\pi_{ijk} = \frac{\exp(\rho_{ijk})}{\sum_k \exp(\rho_{ijk})} \text{ for } k = \{1, 2, 3\},$$

where $t = \{1, \dots, 12\}$ and $\rho_{i1t} = 0$ following convention. Here, α_{jt} is a transition-specific constant and δ_{jt} captures the set of regression coefficients for covariate vector X_{it}' of individual baseline characteristics: sociodemographic variables (sex, age and nationality) and labor variables (type of contract, type of working day, sector of activity, establishment size and contribution group). The decision to add covariates in the regression stem from efficiency concerns. Because we do not incorporate autonomous regions as a variable in the matching procedure, we include them as a set of fixed effects in the regression. The inherent loss of observations due to the curse of dimensionality motivated our decision to not incorporate autonomous region as a set of additional variables in the matching procedure. The variable MW_i is our dummy variable of interest that takes value 0 if unit i belongs to control group (i.e., earns an hourly wage above the 2019 MW) and 1 if she belongs to the treatment group. We estimate the model for each time period $t = \{1, \dots, 12\}$ individually. Thus, we obtain monthly estimates of parameters α , β and δ . For interpretation purposes, we transform coefficients into average marginal effects. For the sake of brevity, we report the corresponding average marginal effect of β_{jt} that is our main parameter of interest.

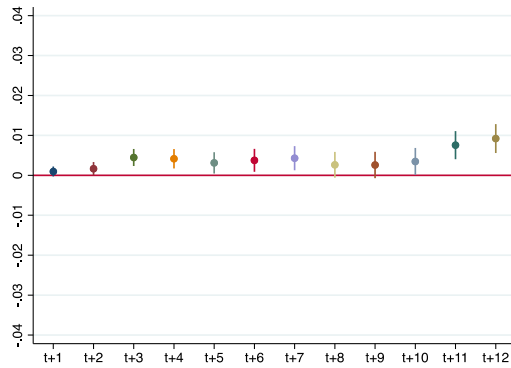
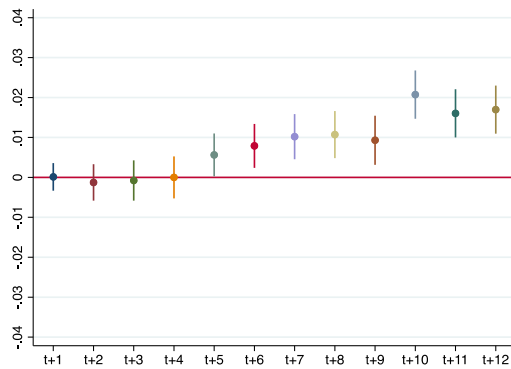
5 Results

5.1 Main results

Figure 5 details regression estimates of the impact of the reform (relative to t_0) for two outcomes: the probability of decreasing number of hours worked (shown in Panel A) and the probability of transitioning to unemployment (Panel B). More specifically, Fig. 5 plots the average marginal effect of our variable of interest MW_i from the above multinomial regression. Detailed results from regression analyses are presented in Appendix Tables 3 through 14.

Estimates from Panel A suggest moderate, although statistically significant effects, on the intensive margin of adjustment (reduction in working hours). Panel A shows that workers affected by the reform face a higher probability of reducing job intensity than the control group. We find that the monthly effects become statistically significant at $p < 0.05$ two months after the reform ($t + 3$). Overall, the results indicate that effect sizes remain relatively stable and close to zero between $t + 2$ (0.17 p.p.) and $t + 10$ (0.35 p.p.) Estimates are, however, relatively larger in the last two periods of analysis. By $t + 12$, we find that the minimum wage reform increases the probability of reducing working hours by about 0.92 p.p. for treated individuals.

Employment loss estimates from Panel B indicate no immediate sizable effects for workers earning less than the MW in the baseline period. In particular, we find no statistically significant effects on the probability of job loss until $t + 5$ (0.56 p.p.). Yet, results indicate substantial time-varying heterogeneity. In particular, we find that,

a) *Employment with work intensity reduction*b) *Unemployment*

Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.

Source: Own calculations using CSWL.

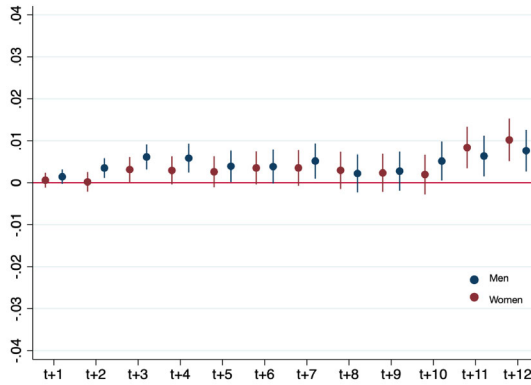
Fig. 5 Impact of MW raise on employment

relative to $t + 5$, the employment loss effect nearly doubles by $t + 7$ (1 p.p.) and triples by $t + 12$ (1.7 p.p.). Ultimately, this suggests (non-monotonically) increasing impacts on employment loss in the medium term.

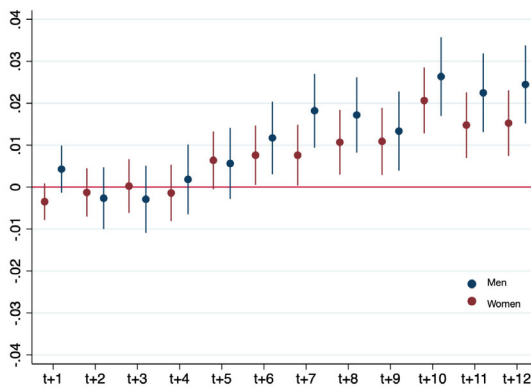
Altogether, comparisons between both panels suggest that most of the negative effect of the MW increase is captured through the extensive margin (employment loss). For instance, one year following the MW increase ($t + 12$), approximately two-thirds of the negative impact observed is attributable to the loss of employment (1.7 p.p.), while the remaining third is attributable to the adjustment in work intensity (0.92 p.p.).¹³ This qualitative pattern is comparable to that found by Barceló et al. (2021), who also observe a greater effect on unemployment than on working time adjustment. Considering the 22.3% increase in the MW, these results indicate that the elasticity of

¹³ Appendix II contains the tables with the estimations results.

a) *Employment with work intensity reduction*



b) *Unemployment*



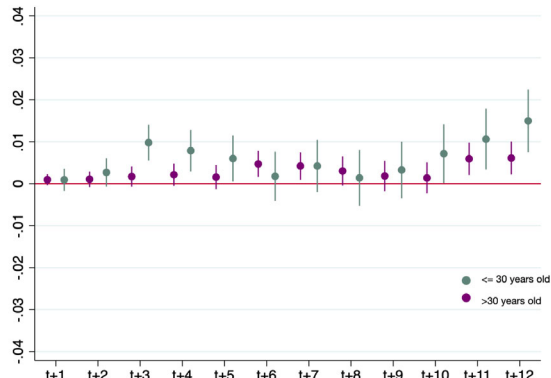
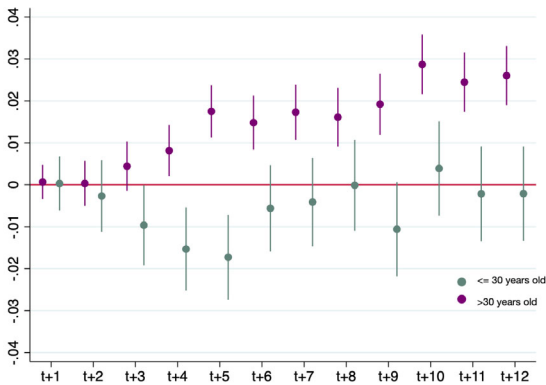
Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B) for men (blue) and women (red). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

Fig. 6 Impact of MW raise on employment, by gender

employment loss to the MW at time $t + 12$ is -0.08 , which is close to the median elasticity (-0.112) obtained by Neumark and Shirley (2021).

5.2 Heterogeneity by group

Thus far, the evidence suggests that the 2019 MW increase had no immediate impact on employment (during the first four months), but gradually increased over time. In addition, results indicate that this adjustment primarily occurs through a greater loss of employment (extensive margin) than through reductions in work intensity (intensive margin). Yet, it is possible that these results hide significant heterogeneity based on workers' characteristics. Thus, we analyze possible impact heterogeneity for several groups based on workers' gender, age, prior labor intensity, geographical region and sector of activity.

a) *Employment with work intensity reduction*b) *Unemployment*

Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B) for younger (grey) and older workers (purple). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.

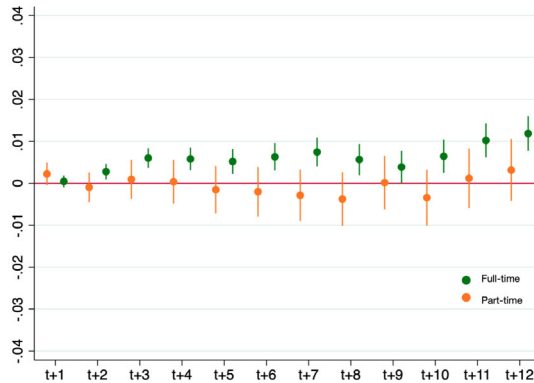
Source: Own calculations using CSWL.

Fig. 7 Impact of MW raise on employment, by age group

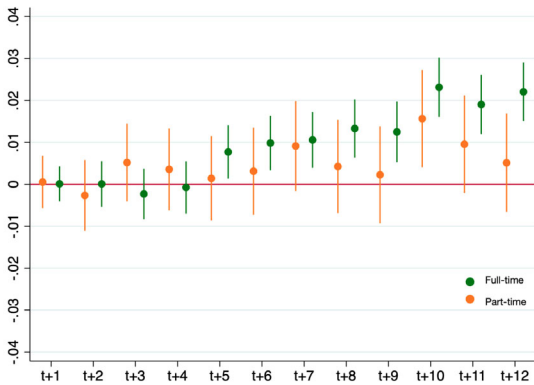
Separate results for men and women are shown in Fig. 6. Panel A indicates that the adjustment in work intensity immediately after the minimum wage reform primarily affected men (approximately 0.35 p.p. in $t + 2$ and 0.61 p.p. in $t + 3$). These significant effects, despite being close to zero, persists throughout most of the analysis horizon. Women, on the other hand, do not appear to be affected by work intensity adjustment until $t + 11$ (see Tables 6 and 7 in Appendix for additional details). Figure 6 panel B reveals that the effect on job loss follows a similar trend for men and women. Although the effect is larger for men than for women in most periods, the large overlap of standard errors between two type of workers prevents us from concluding that the increase had a greater impact on men.

Figure 7 depicts the same analysis focusing instead on two age groups (i.e., below and above 30). In this instance, both panels reveal substantial differences between the two types of workers. Overall, people older than 30 show, on average, a larger impact on the probability of losing a job, whereas younger workers show a quantitatively greater

a) *Employment with work intensity reduction*



b) *Unemployment*

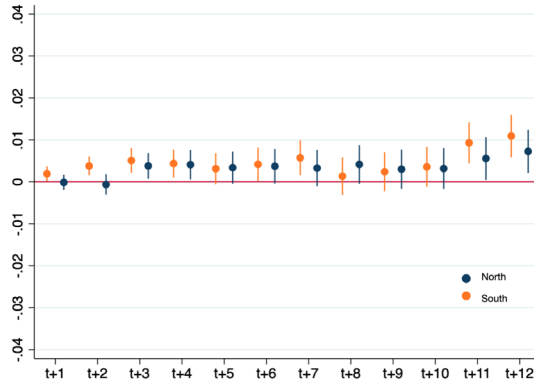
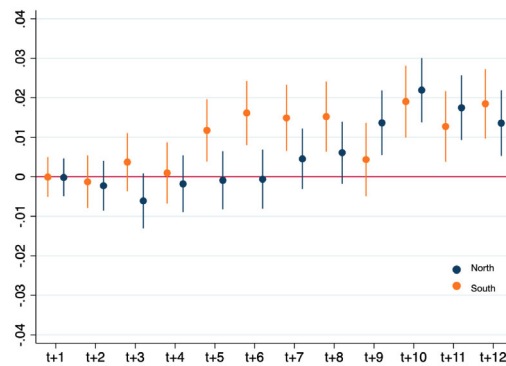


Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B) for full-time (green) and part-time (yellow) workers. The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors. Source: Own calculations using CSWL.

Fig. 8 Impact of MW raise on employment, by workday

impact on the probability of reducing number of working hours. Interestingly, we find a negative unemployment effect for young people between $t + 3$ and $t + 5$. That is, we find that young workers affected by the reform have a positive significant employment effect that banishes after $t + 8$. Panel A reveals, however, that the reduction in working hours is greater on workers under the age of 30 years. This effect is significant in the short term (approximately 1 percentage point in $t + 3$) and becomes more pronounced one year after the increase (1.5 p.p.).

Next, our findings suggest significant impact heterogeneity of MW reform depending on work intensity of employees in t_0 (Fig. 8). Results indicate that the reform implies a quantitatively larger impact on full-time workers than on part-time workers. Despite differences between part-time and full-time workers being small at first, we find that full-time workers experience a greater increase in the job loss probability over time. In this regard, results indicate statistically significant differences in the probability of unemployment by $t + 12$ (2.2 p.p. for full-time workers as opposed to 0.5

a) *Employment with work intensity reduction*b) *Unemployment*

Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B) for North regions (blue) and South regions (orange) workers. The regions that are included in *North* category are: Galicia, Asturias, Cantabria, Basque Country, Navarre, Aragon, Catalonia, Balearic Islands, La Rioja, Castile and Leon and Madrid. The regions that are included in *South* category are: Extremadura, Castilla-La Mancha, Comunidad Valenciana, Andalusia, Canary Islands, Ceuta and Melilla. The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.

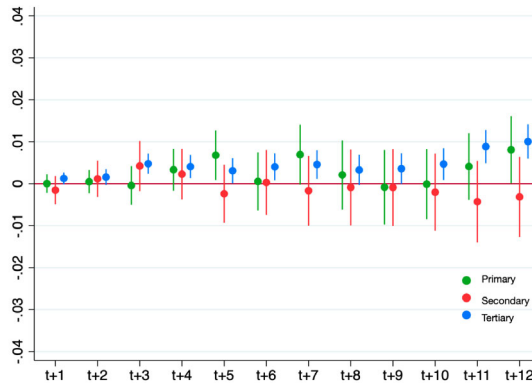
Source: Own calculations using CSWL.

Fig. 9 Impact of MW raise on employment, North vs South of Spain

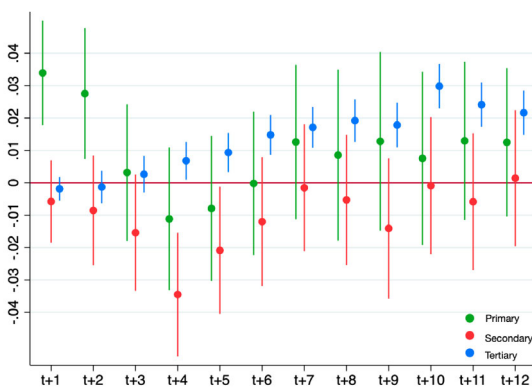
p.p. for part-time workers). Similarly, we find that full-time workers also experience a larger adjustment in work intensity than part-time workers. Results reveal an overall increasing impact on the intensive margin for full-time workers (i.e., the impact is 0.7 p.p. in $t + 5$ but grows until 1.18 p.p. in $t + 12$). Part-time workers, on the other hand, face little adjustment in work intensity, as point estimates remain relatively stable in the period of analysis.

We further explore the possibility that employment effects differ between the northern and southern geographical regions in Fig. 9. Because Spain displays important differences in the geographical distribution of wages and labor market tightness, one could expect to find a marked difference in the impact of the reform between the North and South of the country, with the latter region being more affected. Results from Panel A indicate small and statistically nonsignificant differences between the two regions in the probability of reducing labor intensity (e.g., 0.7 p.p. in the North vs 1.1 p.p. in

c) *Employment with work intensity reduction*



d) *Unemployment*



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B) for primary sector (green), secondary sector (red) and tertiary sector (blue) workers. To construct these three activity groups, we follow the National Classification of Economic Activities (CNAE). The primary sector includes all Group A activities. The secondary sector includes the activities of Groups B - F. The tertiary sector includes the activities of Groups G - U. The points represent regression point estimates from the multinomial logit model specified in section 4. We however do not incorporate autonomous region fixed effects in the regressions due to convergence problems of maximum likelihood estimation procedure. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

Fig. 10 Impact of MW raise on employment, by economic sector

the South in $t + 12$). Panel B, on the other hand, reveals some interesting geographical patterns between the two regions in the probability of losing employment. While we observe substantial and statistically significant effects in both the North and South, the latter seems to have been affected from an earlier stage. In particular, we find that statistically significant effects starting at $t + 5$ in the South and at $t + 9$ in the North. Altogether, these effects seem to relatively stabilize by November 2019. Yet, we find slightly more employment loss estimates in the South than in the North (i.e., 1.85 p.p. versus 1.36 p.p. in $t + 12$).

Finally, we highlight the differences in the observed employment effects across economic sectors in Fig. 10. Panel A indicates that the labor intensity adjustment is more pronounced in the tertiary sector than in the primary or secondary sector (e.g.,

1 p.p. versus 0.08 p.p. and -0.03 p.p. in $t + 12$). We do not find statistically significant effects on the probability of reducing labor intensity for workers employed in the secondary sector throughout year following the reform. Panel B shows a relatively similar pattern whereby the workers in the tertiary sector are more affected also in the probability of losing employment (e.g., 2.2 p.p. versus 0.1 p.p. and 1.3 p.p. in the secondary and primary sectors at $t + 12$). It must be noted, however, that estimated impacts for employees in the primary and secondary sectors are not statistically significant at conventional levels in most of the analyzed periods. An interesting observation in this regard is the significant and sizable immediate effects found for workers of the primary sector in $t + 1$ (3.3 p.p.) and $t + 2$ (2.8 p.p.), suggesting a quicker labor market adjustment in this sector.

6 Robustness checks

To assess whether our findings are robust, we examine the stability of the results with several checks. We report specific details in Appendix, but summarize the main results here.

6.1 Placebo test

In our setting, the plausibility of CIA is a strong condition that can be easily argued against theoretically. Thus, we perform a placebo test using the same procedure described in Sect. 4. Our aim is to empirically argue that our findings do not primarily reflect innate differences between treated and control units in the probability of losing employment or reducing work intensity. To do so, we replicate our baseline analysis by evaluating a fictitious MW raise in May 2018. If significance of results from Sect. 5 were primarily driven by baseline differences of detrimental employment transition, we would expect negative significant effects also under this placebo test. In contrast to our main analysis, we do not find significant differences between treated and control subjects (see Appendix 4 for details). This finding reinforces our results and suggests that differences between treated and control workers are not the main driver behind our findings.

6.2 Additional sensitivity analyses

We perform two additional sensitivity checks to assess robustness to changes in the specification. On the one hand, we experiment with using the second Saturday (instead of the second Tuesday) of each month as our reference period to define employment transitions. In doing so, we include individuals who work only on weekends that are excluded from our original analysis. Results of this check yield highly comparable estimates relative to the main analysis (see Appendix 4). On the other hand, we rerun the analysis using September 2018 as our $t0$ baseline reference to rule out potential

anticipation effects (Cebrián et al. 2020).¹⁴ The results of this robustness test do not clearly suggest that such an effect occurred for the 2019 increase (see Appendix 5).

6.3 Alternative identification strategy: regression discontinuity design

The analysis thus far is subject to a couple of important limitations. First, matching strategies pose certain identification concerns. Despite best efforts to convincingly provide evidence in favor of our empirical approximation, we are aware that CIA-based methods imply a number of valid criticisms and generate skepticism. Second, our main analytical approach crucially relies on studying employment transitions for workers that are employed in November 2018. Insofar as there exists historically high seasonality in the Spanish labor market, our results may not capture potentially important MW effects for employees in sectors like tourism or agriculture, where the minimum wage incidence is particularly significant.

To address these drawbacks, we complement our previous analysis by adopting a regression discontinuity design (RDD). We believe that RDD is a suitable empirical approximation to evaluate the impact of the 2019 reform due to the presence of a clear running variable (i.e., the pre-reform wages of workers), a cutoff (i.e., the newly established MW) and a discontinuous sharp treatment assignment rule (i.e., workers with wages above the cutoff should not be affected by the reform). By exploiting this alternative design, we can partially address the main identification concern regarding the comparability of treatment and control units. The intuition is that, because treatment and control workers at or near the cutoff should be comparable, observing a discontinuous change in employment transitions for workers earning pre-reform wages just below the newly established MW should constitute evidence of significant employment effects of the reform.

To assess the impact of the MW increase through RDD, we adopt two complementary approaches. In both instances, we expand our analysis beyond employees working in November 2018 and separately estimate employment effects for workers employed in each month of year 2018. This approximation allows us to examine the impact on employees working in months other than November. Our first empirical approach is based on a parametric (global) strategy that uses the following regression:

$$y_{i,t+12} = \alpha + \beta MW_{it} + \delta_1 f(W_{it}) + \delta_2 f(W_{it}) MW_{it} + \varepsilon_{it},$$

where $y_{i,t+12}$ is a dummy variable indicating whether worker i transitions to unemployment in month $t + 12$. The decision to now consider the situation 12 months later is motivated by seasonality concerns that were not clearly addressed in Sect. 4.

¹⁴ Because our analysis uses the CSWL-2019, this robustness check is limited in that it does not capture potential anticipation effects through non-renewals of certain temporary contracts ending in October, November and December of 2019. The reason being that workers that are not entitled to unemployment benefits and that did not work throughout 2019 are absent from the CSWL-2019. Ultimately, this implies that the employment loss consequences of the reform might be downward biased because there exist a number of employees potentially impacted by the reform that are not explicitly incorporated in our analysis. Ultimately, we believe this possibility is rather limited given affiliation trends by wage groups shown in Barceló et al. (2021).

MW_{it} is again an indicator that takes value 1 if unit i earns an hourly wage below the 2019 MW in time t and 0 otherwise. Here, function $f(\cdot)$ represents a polynomial on the running variable W_{it} . Hourly wage W_{it} is normalized to 0 in the MW cutoff (€4.375 per hour). To assess the sensitivity of results, we vary the order of polynomials. For comparability purposes, this regression analysis incorporates workers with hourly wages located in the same bandwidth from Sect. 4 (i.e., €3.57 and €5.47 per hour). The main coefficient of interest is β and captures the discontinuous jump on the probability of losing employment in the MW cutoff.

Alternatively, our second approach relies on a nonparametric (local) strategy that limits the analysis to observations that lie within a limited bandwidth around the cutoff. This approximation is motivated by concerns that the above polynomials can introduce bias based on how accurately they approximate the conditional expectation functions of the job loss probability. To mitigate this concern, we first compare the expected (mean) probability of losing employment for treated and control units in a very narrow bandwidth of $\pm \text{€}0.125$ hourly wages around the cutoff. Because the size of the bandwidth can drastically alter RDD results, we also produce impact estimates using the nonparametric procedure proposed by Calonico et al. (2017) that computes optimal bandwidths.

The main results are shown in Appendix Table 17. While the three initial panels display regression results using different order of polynomials, the last two panels highlight the nonparametric RDD estimations. The different columns show separate estimates for subsamples of workers employed in each month of 2018. Results indicate significant variability based on the RDD specification. In particular, we find generally negative (close to zero) estimates in the linear (cubic) specification. Alternatively, we find positive impacts (i.e., increased probability of job loss) in the quadratic and nonparametric specifications, in line with our previous findings. Yet, this highly sensitive and non-robust pattern indicates that our RDD results should be interpreted with significant caution.

Despite the absence of robust findings, we gravitate toward the nonparametric approach from Calonico et al. (2017) as our preferred RDD specification (last panel of Table 17). Several reasons motivate this choice. First, polynomial regressions do not provide a neat approximation of the conditional expectation functions. Figure 17 provides visual illustration of the identification design by plotting the local sample means of small wage bins alongside different polynomial fits for the November 2018 subsample. Unfortunately, none of these figures provide a compelling picture in favor of either specification. Consequently, we lean toward the approach that provides a more localized approximation. Second, the results using the optimal bandwidth reasonably align with findings from Sect. 5. For instance, the estimated employment loss in November 2019 is 1.58 p.p. (compared to 1.7 p.p. in Sect. 5). Third, we observe significant discontinuities in certain covariates at the cutoff, except when we use the optimal bandwidth (see Appendix Table 18). These significant disparities might render control and treatment units not directly comparable, thus casting further doubts about the identifying assumption in the remaining specifications.

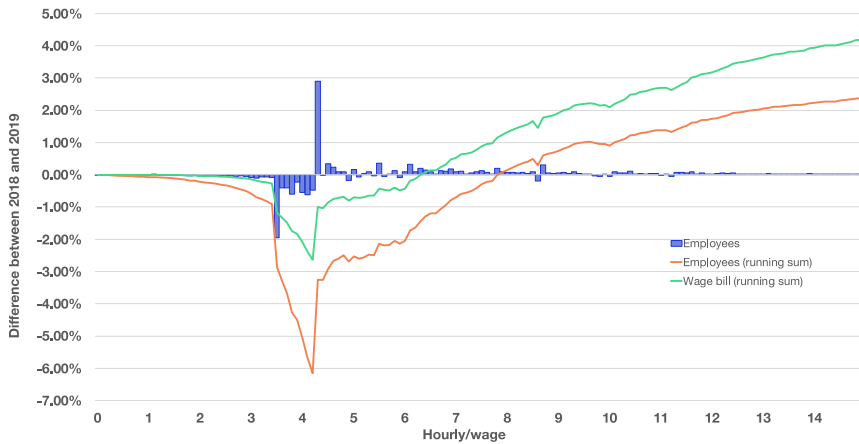
Altogether, we feel it is important to acknowledge that the credibility of these RDD estimates falls short of ideal standards. Further, we believe there are two additional considerations that need to be raised. First, our results may hint at the presence

of manipulation through sorting around the cutoff. In particular, we reject the null hypothesis of the McCrary density test in 8 out of 12 monthly subsamples. Because these types of density tests are high-powered and require a significant number of observations at the cutoff, we cannot confirm with absolute certainty whether results reflect a signal of manipulation or noise. Second, it should be noted that results from RDD are not directly comparable with the ones displayed in Sect. 5. The reason being that RDD identifies the Local Average Treatment Effect (LATE) for workers around the cutoff. Insofar as there exists significant treatment effect heterogeneity based on pre-reform wages, the LATE and the ATT can differ substantially.

6.4 Bunching

Finally, we perform an additional check using aggregated data to complement our previous results. To this end, we follow a similar bunching approach to that of Harasztsi and Lindner (2019). The main goal of this approach is to examine differences between the pre- and post-treatment reform hourly wage frequency distributions and evaluate whether excess workers above the MW between 2019 and 2018 compensate missing workers below this threshold. For this purpose, we use wage distributions from Fig. 2 to calculate the difference in workers between November 2018 and 2019 for each hourly wage bin. To normalize worker counts, we express these differences relative to total employment in November 2018. Blue bars thus show the relative change in number of employees in a given wage bin relative to November 2018. To represent the cumulative employment loss or gain up to a given wage range, we draw a line (orange) that captures the running sum of cumulative changes up to each hourly wage bin. To represent the analogous loss or gain in the wage bill, we also plot the cumulative change in the wage bill using a green line.

The findings in Fig. 11 highlight two main conclusions. First, there is a clear decrease in the number of employees below the new MW (€4.375). At the same time, there is a significant increase around the new MW and, to a lesser extent, in some higher-wage ranges. Second, the negative cumulative sum until relatively high-wage bins indicates a significant number of missing employees between 2018 and 2019, suggesting negative employment effects of the reform. The running sum drops to a sizable negative number (around 6% of pre-reform employment) below the minimum wage. It then recovers and relatively stabilizes in the 2% range, before increasing again at a faster rate around the 6€ hourly wage bin. While it is true that we observe a total positive change in cumulative employment, results indicate that the decrease in the number of employees is not compensated until the €8 per hourly wage bin. As far as the wage bill is concerned, we find a significant drop and subsequent recovery between the ranges of the old and new MW. However, the observed difference is 1% and the cumulative loss is compensated in a wage range closer to the MW (€6.4 per hour).



Note: The figure depicts the difference between the two wage distributions shown in Figure 2. The orange (green) line shows the running sum of employment (wage bill) changes up to each wage bin.
Source: Own calculations using CSWL-2018 and CSWL-2019.

Fig. 11 Difference in employee distribution between 2018 and 2019, by wage bins

7 Limitations of the study

The results of the paper are subject to a number of caveats. First, we use contribution bases to determine whether a person belongs to the treatment or the control group. Due to the presence of bonuses and other extraordinary payments, there exists an uncertain number of workers whose contribution base is greater than the MW despite receiving a lower base wage. Because the data do not allow to deduct these allowances to compute base salaries, the use of contribution bases results in an incorrect assignment into the control group of a number of workers directly affected by the reform. Ultimately, the presence of potentially treated units in the control group introduces bias in the estimation. Intuitively, the size of the bias depends on the degree to which these individuals face a lower risk of job loss than the control group. That is, if misassigned workers have a lower risk of job loss than the control group, the actual impact of the minimum wage increase would be overestimated. On the contrary, if this was not true, estimates can be biased downward (as the real differences between the two groups would be artificially diminished).

Second, due to the inclusion of part-time workers in the analysis, our findings are also susceptible to misclassification of these type of workers into the treatment and control groups. In our approach, a person belongs to either group based on which side of the hourly wage threshold they fall on. Due to the use of the imperfect work intensity coefficient *coefpar* and assumptions about the number of hours worked, our methodology is subject to risks of measurement error. Ultimately, this limitation constitutes an additional factor for potential misclassification of units in the treatment and control groups.

Third, inasmuch there exist unobservable differences between treatment and control groups, our approach suffers risks of CIA violations. Thus, the results of the study

should be interpreted with caution. Since treatment assignment is based on pre-reform hourly wages, there exists potential productivity differences between workers in the treatment group and those in the control group, resulting in upward bias of the reform. Also, if the length or type of temporary contracts between treatment and control units varies significantly, our results might also reflect these inherent differences. While we have performed a placebo and a RDD analysis to assess the extent of this bias, we cannot conclude with absolute certainty that the estimates partially reflect these differences.

Fourth, it should be noted that the findings of the study should not be interpreted as direct evidence of job destruction or a slowdown in job creation. The reason being that the analysis focuses on job loss or reduced work intensity from the worker's perspective. In this sense, our approximation cannot conclude whether a firm or establishment has decided to reduce its workforce or freeze new hires in response to the rise in the MW. For this reason, we want to acknowledge that there exist other methodological alternatives that allow to evaluate this type of important effects. Among these approaches, we find particularly interesting the possibility of comparing the employment dynamics of regions/cells with different pre-reform exposure to minimum wage as in Dustmann et al. (2021). By contrasting regions or cells with different MW exposure, this approximation allows the assessment of certain employment effects that are not driven solely by workers employed when the MW was raised. Despite these advantages, we decided to focus on pursuing a more micro individual approach that enables a more detailed heterogeneity analysis based on individual worker characteristics, including gender or age. While it would be interesting to expand our analysis and provide complementary evidence using this alternative methodology, we believe it goes somewhat beyond the scope of this paper.

Finally, we want to conclude by highlighting that there could be other significant labor market consequences of the reform, like the potential reallocation of workers from smaller to larger firms (Dustmann et al. 2021). This type of effects and some others (e.g., the distinction between job destruction due to dismissals versus temporary layoffs) could well be studied using a firm–worker approach that explores the evolution of firm–worker-specific matches after the reform. We believe these are interesting considerations that deserve further scrutiny in future research. This study is, however, intended at analyzing the more quantitative (rather than qualitative) consequences of the reform. We therefore decided to abstain from studying these considerations in the current paper.

8 Concluding remarks

This article provides complementary evidence of the employment effects of a large raise in the minimum wage in Spain. To this end, our analysis uses administrative Social Security records from the 2019 CSWL. Our identification strategy compares employment transitions between a group of workers who earned less than the newly established MW prior to the reform and workers who earned more than the minimum wage threshold. Although this approach is not new, this article extends previous literature of MW in Spain in several ways. First, we incorporate two relevant worker types

traditionally excluded from the analysis: part-time workers and employees working less than the entire month. Second, we explore the impact of MW raises on both the extensive margin (probability of employment loss) and the intensive margin (probability of work intensity reduction). Third, we study impact on monthly transitions, therefore assessing the effect of the reform both in the short and in the medium term.

We find that the reform had no effect on employment in the period immediately following the increase (up to five months after the increase). However, a significant negative effect emerges thereafter, primarily through the extensive margin. In the 12 months following the reform, we find a negative effect of 1.7 p.p. increase in the probability to unemployment transition. Taking into account the nominal increase of 22.3% in the MW, this result indicates an elasticity of -0.08 between the loss of employment and the MW. As with work intensity through a reduction in working hours, we find a relatively small effect that also grows over time. Quantitatively speaking, the effect size is significantly smaller than that observed to job loss (0.92 p.p.).

From the heterogeneity analysis, we can draw several conclusions. First, our results show that there are few differences between male and female workers in terms of the probability of employment loss. However, we do find differences in labor intensity, with a greater immediate impact on men and a greater medium-term adjustment for women. Second, we observe some differences depending on the age of employees affected by the increase in MW. Specifically, our results indicate that older workers suffer to a greater extent from job loss, while in younger workers the effect is concentrated in labor intensity. Third, we find notable differences depending on the type of working day of the affected workers, with a greater adjustment in labor intensity for those who work full-time. Fourth, significant differences in the impact of the policy between the North and South of Spain are observed, with the MW reform showing more immediate and larger effects on workers in southern regions characterized by lower nominal salaries and market tightness. Finally, there is evidence of greater employment effects of the policy in the tertiary sector.

Acknowledgements We thank the editor Caterina Calsamiglia and two anonymous referees for very useful suggestions. We gratefully acknowledge contribution of ISEAK's team and, especially, that of Sara de la Rica for their invaluable continuous guidance throughout the project. We thank Aitor Lacuesta, Ernesto Villanueva, Inmaculada Cebrián and Carlos Martín for helpful comments.

Funding This work was supported by the Ministry of Labour and Social Economy.

Data availability The authors do not have permission to make the data used in this study available.

Declarations

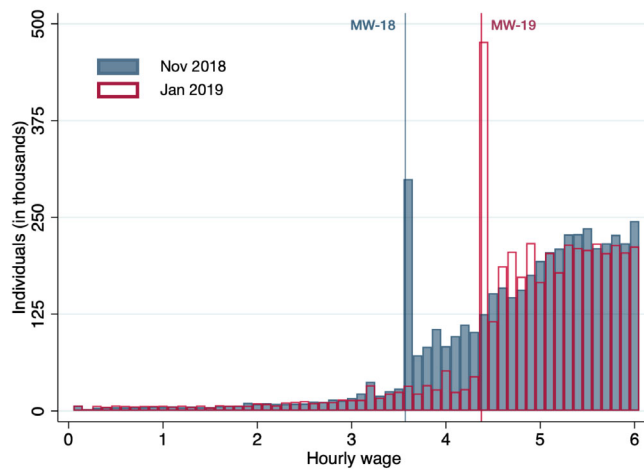
Conflict of interest The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Open Access This article is licensed under a Creative Commons Attribution 4.0 International License, which permits use, sharing, adaptation, distribution and reproduction in any medium or format, as long as you give appropriate credit to the original author(s) and the source, provide a link to the Creative Commons licence, and indicate if changes were made. The images or other third party material in this article are included in the article's Creative Commons licence, unless indicated otherwise in a credit line to the material. If material is not included in the article's Creative Commons licence and your intended use is not permitted by statutory regulation or exceeds the permitted use, you will need to obtain permission

directly from the copyright holder. To view a copy of this licence, visit <http://creativecommons.org/licenses/by/4.0/>.

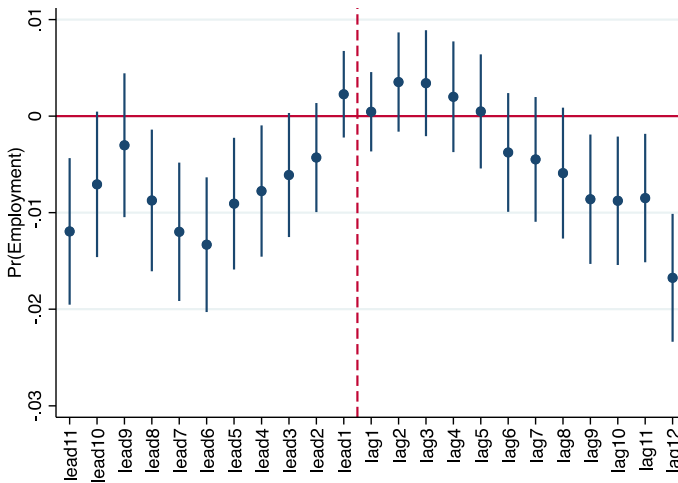
Appendix 1: Additional descriptive statistics

See Figs. 12 and 13 and Tables 3 and 4.



Source: Own calculations using CSWL 2018 and 2019.

Fig. 12 Hourly wage distribution (November 2018 and January 2019)



Note: The figure shows estimates of the effect of MW increase on the employment status using monthly leads and lags in an event-study design. The outcome variable is a dummy that takes value =1 if the worker is employed and 0 otherwise in a given month. The treatment group is constituted by workers earning between €3.57 and €4.375 per hour as of December 2018. The control group is constructed through a matched sample using workers earning between €4.39 and €5.47 per hour as of December 2018. The matched sample is constructed through Coarsened Exact Matching using the following variables: gender, nationality, establishment size intervals, contribution group, contribution regime, industry, type of contract, and age groups. Unmatched treated units are discarded from analysis as in Section 4. We show the 95% confidence interval based on robust standard errors.

Source: Own calculations using CSWL.

Fig. 13 Absence of parallel pre-trends between treatment and control groups. Event–study design

Table 3 Differences in means between the treatment group, the control group and the group of workers holding multiple jobs

Group	Treatment (1) (%)	Control (2) (%)	Workers holding multiple jobs (3) (%)	Test (<i>p</i> -value) (1)-(3) (5)	Test (<i>p</i> -value) (2)-(3) (5)
Gender					
Men	43.4	42.4	46.6	0.000***	0.000***
Women	56.6	57.6	53.4	0.000***	0.000***
Age					
16–25 years	23.2	13.3	6.9	0.000***	0.000***
36–34 years	28.2	27.7	21.7	0.000***	0.000***
36–44 years	23.4	29.1	30.2	0.000***	0.000***
45–54 years	19.0	22.5	29.1	0.000***	0.000***
55 years and older	6.3	7.4	12.0	0.000***	0.000***

Table 3 (continued)

Group	Treatment (1) (%)	Control (2) (%)	Workers holding multiple jobs (3) (%)	Test (<i>p</i> -value) (1)-(3) (5)	Test (<i>p</i> -value) (2)-(3) (5)
Nationality					
Native	81.0	85.5	85.3	0.000***	0.484
Immigrant	19.0	14.5	14.7	0.000***	0.468
Observations	35,144	77,404	34,804		

Note: We report the differences in observed sociodemographic characteristics between treatment group (column 1), control group (column 2) and the group of workers holding multiple jobs (column 3), which are dropped from the analysis. Columns (4) and (5) report the *p*-values for tests for statistically significant differences in observed characteristics. In these columns, ***, ** and * indicate significance at the 1, 5 and 10 percent critical level. *Source*: Own calculations using CSWL

Table 4 Average number of hours worked by contract type
Source: Own calculations using CSWL

Type of contract	Treatment	Control
Permanent full-time	7.86	7.79
Temporary full-time	7.99	7.99
Permanent part-time	4.85	5.04
Temporary part-time	4.35	4.48

We report the average number of hours worked by individuals of each group (treatment and control) in *t0*. In the case of employees working full-time, it is possible that the value of *coefpar* is not exactly 1,000. Consequently, and despite the standardization carried out in the analysis, the average number of hours worked in these cases is not exactly 8, although it is very close

Appendix 2: Estimation results

See Tables 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15 and 16.

Table 5 Impact of MW on employment *Source:* Own calculations using CSWL

	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	$t + 6$	$t + 7$	$t + 8$	$t + 9$	$t + 10$	$t + 11$	$t + 12$
Employment with labor intensity reduction	0.000952 (0.000637)	0.00165* (0.000850)	0.004446*** (0.00109)	0.00415*** (0.00124)	0.00312*** (0.00136)	0.00375** (0.00146)	0.00429*** (0.00154)	0.00263 (0.00165)	0.00258 (0.00168)	0.00346** (0.00173)	0.00756*** (0.00180)	0.00921*** (0.00184)
Unemployment	0.000128 (0.000128)	– 0.00127 (0.000127)	– 0.000780 (0.000780)	– 2.03e – 06 (0.00268)	0.00563** (0.00274)	0.00789*** (0.00281)	0.0102*** (0.00288)	0.0107*** (0.00301)	0.00931*** (0.00314)	0.0207*** (0.00309)	0.0160*** (0.00308)	0.0170*** (0.00307)
Observations	61.172	61.021	60.920	60.831	60.722	60.610	60.418	60.205	60.101	60.153	60.255	60.162

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6 Impact of MW on employment (men) *Source:* Own calculations using CSWL

	<i>t</i> + 1	<i>t</i> + 2	<i>t</i> + 3	<i>t</i> + 4	<i>t</i> + 5	<i>t</i> + 6	<i>t</i> + 7	<i>t</i> + 8	<i>t</i> + 9	<i>t</i> + 10	<i>t</i> + 11	<i>t</i> + 12
Employment with labor intensity reduction	0.00142	0.00351***	0.00613***	0.00586***	0.00393**	0.00385*	0.00517**	0.00220	0.00276	0.00516**	0.00637**	0.00764***
	(0.000880)	(0.00119)	(0.00153)	(0.00176)	(0.00191)	(0.00207)	(0.00214)	(0.00232)	(0.00238)	(0.00238)	(0.00249)	(0.00253)
Unemployment	0.00490*	− 0.00148	− 0.00182	0.00233	0.00569	0.0111**	0.0178***	0.0162***	0.0116**	0.0244***	0.0206***	0.0231***
	(0.00285)	(0.00376)	(0.00410)	(0.00424)	(0.00433)	(0.00442)	(0.00451)	(0.00460)	(0.00481)	(0.00479)	(0.00478)	(0.00474)
Observations	25,695	25,600	25,538	25,492	25,423	25,371	25,266	25,141	25,078	25,143	25,206	25,175

Average marginal effects. Standard errors in parentheses. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$

Table 7 Impact of MW on employment (women) *Source:* Own calculations using CSWL

	<i>t</i> + 1	<i>t</i> + 2	<i>t</i> + 3	<i>t</i> + 4	<i>t</i> + 5	<i>t</i> + 6	<i>t</i> + 7	<i>t</i> + 8	<i>t</i> + 9	<i>t</i> + 10	<i>t</i> + 11	<i>t</i> + 12
Employment with labor intensity reduction	0.000587 (0.000905)	0.000193 (0.00120)	0.00314** (0.00152)	0.00293* (0.00172)	0.00260 (0.00189)	0.00352* (0.00202)	0.00353 (0.00216)	0.00296 (0.00227)	0.00234 (0.00232)	0.00195 (0.00243)	0.00838*** (0.00252)	0.0102*** (0.00260)
Unemployment	– 0.00351 (0.00223)	– (0.00295)	– (0.00331)	– (0.00345)	0.00446 (0.00354)	0.00447 (0.00363)	0.00376 (0.00374)	0.00568 (0.00396)	0.00696* (0.00412)	0.0175*** (0.00404)	0.0113*** (0.00402)	0.0111*** (0.00402)
Observations	35,477	35,421	35,382	35,339	35,299	35,239	35,152	35,064	35,023	35,010	35,049	34,987

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 8 Impact of MW on employment (30 years old or younger) *Source:* Own calculations using CSWL

	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	$t + 6$	$t + 7$	$t + 8$	$t + 9$	$t + 10$	$t + 11$	$t + 12$
Employment with labor intensity reduction	0.000923 (0.00134)	0.00268 (0.00171)	0.00979*** (0.00218)	0.00787*** (0.00252)	0.00600** (0.00279)	0.00175 (0.00301)	0.00420 (0.00317)	0.00136 (0.00339)	0.00326 (0.00343)	0.00714*** (0.00356)	0.0107*** (0.00369)	0.0150*** (0.00380)
Unemployment	0.000232 (0.00329)	– (0.00436)	0.00274 (0.00488)	0.00973** (0.00506)	0.0154*** (0.00516)	– (0.00525)	– (0.00539)	5.34e–05 (0.00553)	– (0.00573)	0.00418 (0.00574)	–0.00183 (0.00578)	–0.00177 (0.00575)
Observations	20,535	20,505	20,465	20,442	20,412	20,380	20,318	20,256	20,218	20,225	20,246	20,212

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 9 Impact of MW on employment (more than 30 years old) *Source:* Own calculations using CSWL

	<i>t</i> + 1	<i>t</i> + 2	<i>t</i> + 3	<i>t</i> + 4	<i>t</i> + 5	<i>t</i> + 6	<i>t</i> + 7	<i>t</i> + 8	<i>t</i> + 9	<i>t</i> + 10	<i>t</i> + 11	<i>t</i> + 12
Employment with labor intensity reduction	0.000935	0.00103	0.00167	0.00207	0.00148	0.00456***	0.00404**	0.00279	0.00161	0.00116	0.00572***	0.00592***
	(0.000673)	(0.000943)	(0.00121)	(0.00137)	(0.00148)	(0.00159)	(0.00168)	(0.00179)	(0.00185)	(0.00189)	(0.00196)	(0.00201)
Unemployment	0.000590	0.000325	0.00444	0.00822***	0.0175***	0.0150***	0.0175***	0.0164***	0.0195***	0.0290***	0.0247***	0.0262***
	(0.00208)	(0.00273)	(0.00299)	(0.00312)	(0.00319)	(0.00328)	(0.00337)	(0.00357)	(0.00372)	(0.00363)	(0.00359)	(0.00359)
Observations	40,637	40,516	40,455	40,389	40,310	40,230	40,100	39,949	39,883	39,928	40,009	39,950

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 10 Impact of MW on employment (full-time) *Source:* Own calculations using CSWL

	<i>t</i> + 1	<i>t</i> + 2	<i>t</i> + 3	<i>t</i> + 4	<i>t</i> + 5	<i>t</i> + 6	<i>t</i> + 7	<i>t</i> + 8	<i>t</i> + 9	<i>t</i> + 10	<i>t</i> + 11	<i>t</i> + 12
Employment with labor intensity reduction	0.000438 (0.000711)	0.00276*** (0.000949)	0.00601*** (0.00119)	0.00578*** (0.00138)	0.00519*** (0.00152)	0.00629*** (0.00166)	0.00743*** (0.00176)	0.00564*** (0.00190)	0.00385* (0.00197)	0.00642*** (0.00202)	0.0102*** (0.00206)	0.0118*** (0.00210)
Unemployment	8.71e – 05 (0.00213)	6.66e – 05 (0.00277)	– 0.00231 (0.00306)	– 0.000756 (0.00318)	0.00770** (0.00324)	0.00983*** (0.00331)	0.0106*** (0.00339)	0.0133*** (0.00354)	0.0125*** (0.00369)	0.0231*** (0.00361)	0.0190*** (0.00359)	0.0220*** (0.00357)
Observations	43,474	43,340	43,266	43,204	43,135	43,063	42,926	42,750	42,654	42,727	42,857	42,792

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 11 Impact of MW on employment (part-time) *Source:* Own calculations using CSWL

	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	$t + 6$	$t + 7$	$t + 8$	$t + 9$	$t + 10$	$t + 11$	$t + 12$
Employment with labor intensity reduction	0.00222 (0.00136)	– (0.00179)	0.000919 (0.00238)	0.000351 (0.00266)	– (0.00287)	– (0.00300)	– (0.00312)	– (0.00325)	0.000150 (0.00324)	–0.00345 (0.00338)	0.00118 (0.00364)	0.00314 (0.00376)
Unemployment	0.000522 (0.000429)	– (0.00267)	0.00517 (0.00473)	0.00355 (0.00496)	0.00140 (0.00513)	0.00311 (0.00529)	0.00911* (0.00547)	0.00423 (0.00567)	0.00226 (0.00590)	0.0156*** (0.00592)	0.00954 (0.00594)	0.00511 (0.00598)
Observations	17,698	17,681	17,654	17,627	17,587	17,547	17,492	17,455	17,447	17,426	17,398	17,352

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 12 Impact of MW on employment (North) *Source:* Own calculations using CSWL

	$t+1$	$t+2$	$t+3$	$t+4$	$t+5$	$t+6$	$t+7$	$t+8$	$t+9$	$t+10$	$t+11$	$t+12$
Employment with labor intensity reduction	– 0.000121	– 0.000631	0.00381** (0.00124)	0.00410** (0.00180)	0.00339* (0.00194)	0.00373* (0.00210)	0.00328 (0.00221)	0.00417* (0.00235)	0.00302 (0.00239)	0.00318 (0.00248)	0.00558** (0.00259)	0.00727*** (0.00264)
Unemployment	– 0.000168	– 0.00226	– 0.00609* (0.00381**)	– 0.00180 (0.00410**)	– 0.000891 (0.00339*)	– 0.000616 (0.00373*)	0.00453 (0.00328)	0.00610 (0.00417*)	0.0137*** (0.00302)	0.0219*** (0.00318)	0.0175*** (0.00558**)	0.0136*** (0.00727***)
Observations	31,415	31,361	31,325	31,287	31,239	31,204	31,159	31,115	31,093	31,081	31,046	30,970

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 13 Impact of MW on employment (South) *Source:* Own calculations using CSWL

	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	$t + 6$	$t + 7$	$t + 8$	$t + 9$	$t + 10$	$t + 11$	$t + 12$
Employment with labor intensity reduction	0.00193** (0.000895)	0.00379*** (0.00116)	0.00509*** (0.00151)	0.00437** (0.00170)	0.00315* (0.00189)	0.00418** (0.00202)	0.00573*** (0.00213)	0.00136 (0.00229)	0.00241 (0.00236)	0.00358 (0.00242)	0.00931*** (0.00250)	0.0109*** (0.00258)
Unemployment	– 5.47e – 05 (0.00258)	– 0.00127 (0.00339)	0.00372 (0.00376)	0.000972 (0.00395)	0.0118*** (0.00404)	0.0162*** (0.00414)	0.0149*** (0.00428)	0.0152*** (0.00453)	0.00436 (0.00475)	0.0191*** (0.00464)	0.0127*** (0.00456)	0.0185*** (0.00449)
Observations	29,757	29,660	29,660	29,595	29,595	29,544	29,544	29,483	29,483	29,406	29,406	29,259

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 14 Impact of MW on employment (primary sector) *Source:* Own calculations using CSWL

	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	$t + 6$	$t + 7$	$t + 8$	$t + 9$	$t + 10$	$t + 11$	$t + 12$
Employment with labor intensity reduction	1.46e – 05	0.000475	–	0.00333	0.00678**	0.000543	0.00693*	0.00210	–	–	0.00410	0.00808**
			0.000439						0.000837	0.000111		
Unemployment	(0.00112)	(0.00141)	(0.00235)	(0.00255)	(0.00301)	(0.00353)	(0.00365)	(0.00421)	(0.00452)	(0.00427)	(0.00406)	(0.00408)
	0.0339***	0.0275***	0.00317	– 0.0112	– 0.00792	–	0.0126	0.00855	0.0128	0.00753	0.0130	0.0125
						0.000200						
Observations	(0.00823)	(0.0103)	(0.0108)	(0.0112)	(0.0114)	(0.0113)	(0.0122)	(0.0135)	(0.0141)	(0.0136)	(0.0125)	(0.0117)
	4,355	4,259	4,236	4,217	4,187	4,170	4,075	3,944	3,893	3,996	4,147	4,132

Note: Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 15 Impact of MW on employment (secondary sector) *Source:* Own calculations using CSWL

	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$	$t + 6$	$t + 7$	$t + 8$	$t + 9$	$t + 10$	$t + 11$	$t + 12$
Employment with labor intensity reduction	– 0.00154 (0.00172)	0.00116 (0.00221)	0.00422 (0.00305)	0.00228 (0.00308)	– 0.00240 (0.00353)	0.000293 (0.00394)	– 0.00168 (0.00424)	– 0.000888 (0.00461)	– 0.000903 (0.00467)	– 0.00203 (0.00469)	– 0.00429 (0.00496)	– – (0.00487)
Unemployment	0.00578 (0.00649)	0.00854 (0.00863)	0.0154* (0.00915)	0.0345*** (0.00973)	0.0209** (0.0100)	– 0.0120 (0.0102)	– 0.00154 (0.0100)	– 0.00530 (0.0103)	– 0.0141 (0.0110)	– 0.000889 (0.0108)	– 0.00585 (0.0108)	0.00142 (0.0107)
Observations	5,186	5,179	5,179	5,173	5,173	5,166	5,166	5,158	5,158	5,148	5,148	5,143

Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 16 Impact of MW on employment (tertiary sector) *Source:* Own calculations using CSWL

	<i>t</i> + 1	<i>t</i> + 2	<i>t</i> + 3	<i>t</i> + 4	<i>t</i> + 5	<i>t</i> + 6	<i>t</i> + 7	<i>t</i> + 8	<i>t</i> + 9	<i>t</i> + 10	<i>t</i> + 11	<i>t</i> + 12
Employment with labor intensity reduction	0.00125*	0.00156	0.00476***	0.00406***	0.00308**	0.00403**	0.00458***	0.00326*	0.00358*	0.00468**	0.00885***	0.0100***
	(0.000720)	(0.000962)	(0.00122)	(0.00140)	(0.00153)	(0.00164)	(0.00173)	(0.00183)	(0.00187)	(0.00194)	(0.00202)	(0.00209)
Unemployment	− 0.00187	− 0.00131	0.00263	0.00681**	0.00935***	0.0148***	0.0171***	0.0192***	0.0178***	0.0298***	0.0241***	0.0216***
	(0.00186)	(0.00254)	(0.00286)	(0.00297)	(0.00307)	(0.00315)	(0.00323)	(0.00335)	(0.00353)	(0.00348)	(0.00347)	(0.00349)
Observations	52,762	52,713	52,641	52,575	52,504	52,418	52,324	52,249	52,202	52,152	52,105	52,035

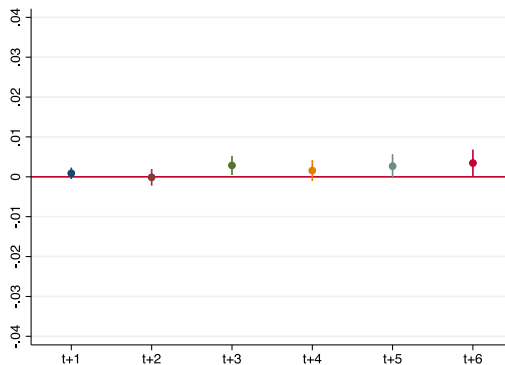
Average marginal effects. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Appendix 3: Placebo test

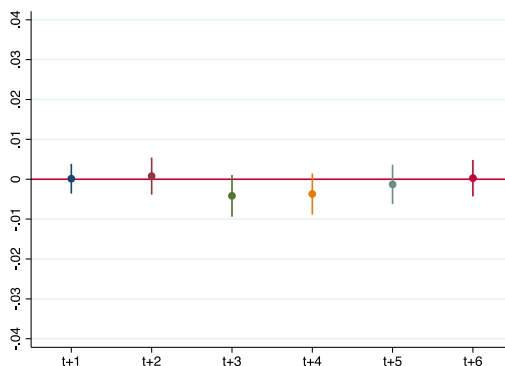
Finally, a placebo test was carried out in which a fictitious increase in the MW was considered. Specifically, the month of May 2018 is set as the time at which this fictitious increase would have taken place. The purpose of this test is to confirm that the results obtained in the estimates are indeed due to the SMI and not to other factors unrelated to the measure, such as differences in productivity between treated and controls. Given that this increase did not take place, and applying the same methodology and identification strategy explained in the body of the report, the expected result is that there is no impact on the probabilities of transitioning to the defined scenarios. If this were not the case, it could not be argued that the results obtained in this work are really due to the rise in the MW.

Figure 14 shows that, according to the results of this placebo test, there is no significant impact. Therefore, and at least during the months prior to the increase in

a) *Employment with work intensity reduction*



b) *Unemployment*



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.

Source: Own calculations using CSWL.

Fig. 14 Results of placebo test

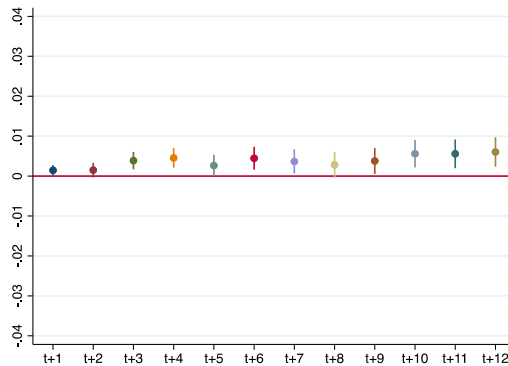
the MW, there do not seem to be any significant differences between controls and treated patients.

Appendix 4: Additional sensitivity analysis (I)

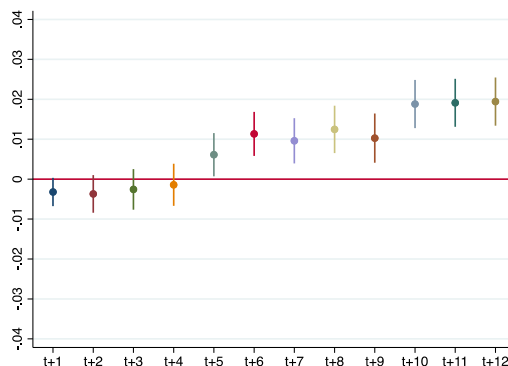
The second robustness analysis consists of changing the reference day on which individuals are observed across the panel. Specifically, this dated is switched from the second Tuesday of each month to the second Saturday, keeping November 2018 as t_0 . The objective of this approach is to include individuals who work only on weekends and who, with the original approach, are excluded from the analysis. Since this is a group that, presumably, may be more affected by labor precariousness and, therefore, could be particularly affected by the increase in the SMI. The matching process applied in this robustness test is the same as that applied in the impact assessment section.

Estimates made following this approach also yield an impact of the SMI increase very similar to those presented in the report and in Appendix 2. Again, Fig. 15 shows

a) *Employment with work intensity reduction*



b) *Unemployment*



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

Fig. 15 Impact of MW raise on employment, setting Saturday as reference day

that, in the short term, there is no significant impact of the increase on employment. From $t + 5$ onwards, we begin to see an impact that grows month by month until it reaches 1.94 p.p. in the case of the probability of transitioning to non-employment. The impact on work intensity is very small.

Appendix 5: Additional sensitivity analysis (II)

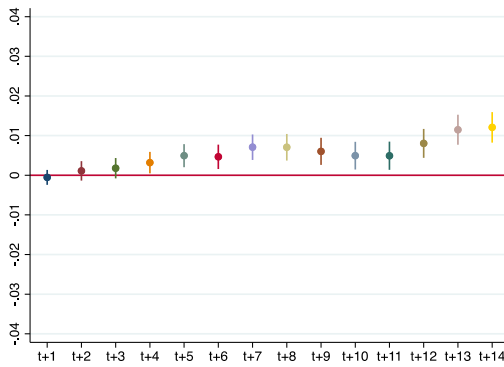
In their analysis of the impact of the MW increase on employment survival, Cebrián et al. (2020) find that the MW increase has a negative impact prior to the implementation of the increase. This is because employers, knowing in advance that the MW increase is going to occur, make the decision to make labor adjustments before the MW increase takes effect. Therefore, this third robustness test consists of setting a month prior to November -September 2018- as $t0$, in order to rule out the existence of such an effect, which would imply that the estimated impact of the MW increase would be incorrect. As in the original approach, individuals are observed on the second Tuesday of each month (Fig. 16).

The estimation results suggest that, in the case of the MW increase, there is no negative impact before the measure takes effect -before $t + 4$, with this approach-. Once the increase has taken place, the results obtained are similar to those presented in the impact analysis section of the report, since a null impact is observed in the short term, which increases over time to values close to 2 p.p. in the case of job losses and around 1 p.p. in the adjustment in hours worked.

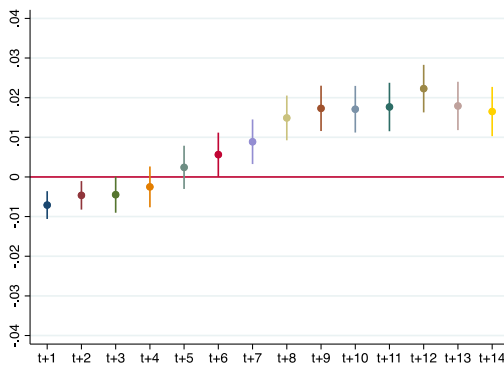
Appendix 6: Regression discontinuity design tables

See Tables 17, 18 and Fig. 17.

a) *Employment with work intensity reduction*



b) *Unemployment*



Note: In the figure, we plot average marginal effects for the probability of decreasing number of hours worked (Panel A) and the probability of transitioning to unemployment (Panel B). The points represent regression point estimates from the multinomial logit model specified in section 4. Lines represent the 95% confidence interval based on standard errors.
Source: Own calculations using CSWL.

Fig. 16 Impact of MW raise on employment, setting September as $t0$

Table 17 Effect of MW reform on probability of unemployment. RDD estimation results

	January	February	March	April	May	June	July	August	September	October	November	December
PANEL A: Parametric RDD (bandwidth = [€3.57, €5.47])												
Pol. Order = 1	–	–	– 0.00741	– 0.00509	– 0.00404	–	– 0.00414	–	0.0147***	0.0182***	–	–
	(0.00519)	(0.00496)	(0.00511)	(0.00501)	(0.00510)	(0.00495)	(0.00502)	(0.00513)	(0.00509)	(0.00521)	(0.00514)	(0.00526)
Pol. Order = 2	–	0.0150**	0.0216***	0.0332***	0.0209***	0.0259***	0.0306***	0.0154*	0.0356***	0.0130	0.0314***	0.0378***
	0.000938											
Pol. Order = 3	(0.00790)	(0.00763)	(0.00779)	(0.00761)	(0.00775)	(0.00752)	(0.00770)	(0.00790)	(0.00775)	(0.00794)	(0.00782)	(0.00809)
	0.00171	0.00293	0.00698	0.0217**	0.0102	0.00592	0.0283***	– 0.00410	0.0209***	– 0.00190	8.69e–07	0.0102
Observations	103,057	111,067	107,493	109,012	108,706	112,228	112,173	110,669	111,184	109,932	112,548	111,723
PANEL B: Nonparametric RDD												
Adjusted bandwidth [4.25–4.5]	0.0100	0.0151**	0.0186***	0.0278***	0.0175**	0.0162**	0.0260***	0.00600	0.0284***	0.00610	0.0192***	0.0215***
Observations	10,981	11,741	11,299	11,725	11,488	12,21	11,937	11,727	12,198	11,656	12,110	11,753
Optimal bandwidth	0.00985	0.0180	0.0195*	0.0343***	0.0148	0.00880	0.0250**	– 0.00188	0.0290***	–	0.0158	0.0343***
										0.000704		
Observations	20,682	20,071	24,786	18,327	31,525	27,324	27,965	25,587	29,985	20,263	30,506	19,407
	(0.0117)	(0.0123)	(0.0106)	(0.0121)	(0.00942)	(0.0100)	(0.0103)	(0.0111)	(0.00986)	(0.0126)	(0.00991)	(0.0129)

Each coefficient comes from a different regression (standard errors in parentheses). Panel A reflects results of regression specification in Sect. 6 using different order of polynomials. The sample includes workers employed in each month with wages between 3.57 and €5.47 per hour hourly wages. Panel B displays the nonparametric approach that use a $\pm$ – €0.125 narrow bandwidth around the €4.375 per hour MW cutoff and the resulting optimal bandwidth following the procedure by Cattaneo et al. (2017). This last specification employs the default options (i.e., local linear regressions and triangular kernels). The dependent variable is an indicator for the worker being unemployed 12 months after the baseline period; the independent variable is an indicator for the worker earning a wage below the MW cutoff in the reference period. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 18 Balance in covariates around the MW cutoff, alternative specifications. Sample of employers working in November 2018

	Order 1	Order 2	Order 3	Adjusted bandwidth (+ / − €0.125)	Optimal bandwidth
Female	0.0140** (0.00641)	0.0176* (0.00973)	0.0756*** (0.0131)	0.0343*** (0.00892)	0.024 (0.01451)
Age	0.830*** (0.145)	− 1.821*** (0.219)	0.988*** (0.295)	− 0.660*** (0.199)	0.084 (0.38147)
Native	− 0.0336*** (0.00489)	− 0.00197 (0.00743)	− 0.0256** (0.00995)	− 0.0224*** (0.00674)	− 0.02* (0.01178)
Hours worked	− 0.288*** (0.0243)	0.240*** (0.0379)	− 0.483*** (0.0509)	− 0.173*** (0.0341)	− 0.126 (0.07944)
Permanent contract	0.0819*** (0.00629)	− 0.0980*** (0.00960)	0.0169 (0.0130)	− 0.0427*** (0.00870)	0.004 (0.1845)
Observations	112,548	112,548	112,548	12,110	− −

Each coefficient comes from a different regression (standard errors in parentheses). Initial three columns reflect results of regression specification in Sect. 6 using different order of polynomials. The sample includes workers employed in each month with wages between 3.57 and €5.47 per hour hourly wages- The fourth column displays comparison of means between treated and control units in a + / − €0.125 narrow bandwidth around the €4.375 per hour MW cutoff. The last column displays results from optimal bandwidth following the procedure by Cattaneo et al. (2017) This last specification employs the default options (i.e., local linear regressions and triangular kernels). The dependent variable of each regression is the variable displayed in each row; the independent variable is an indicator for the worker earning a wage below the MW cutoff in the reference period. ***p < 0.01, **p < 0.05, *p < 0.1

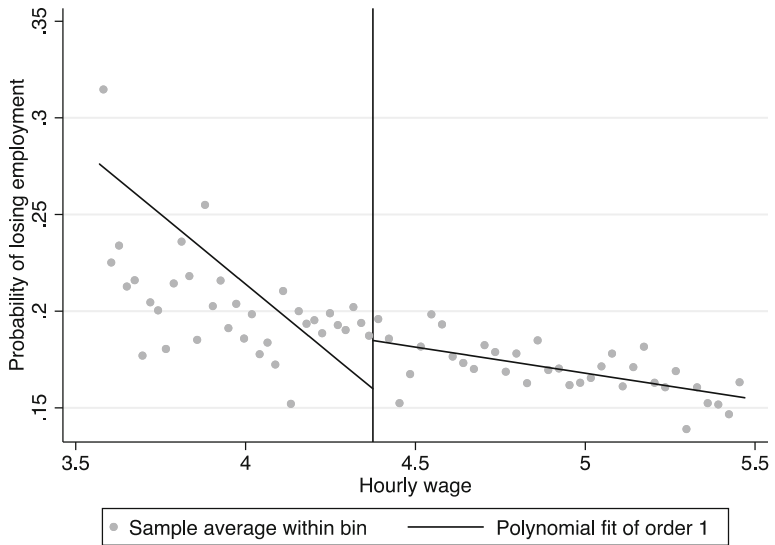
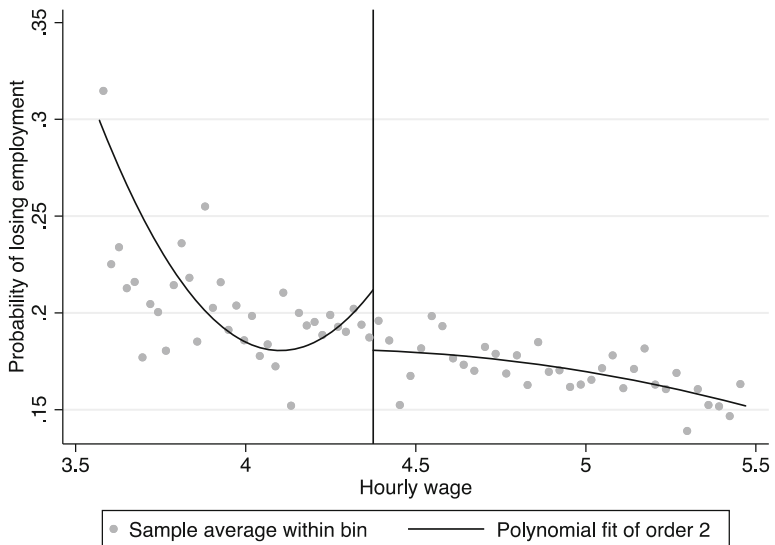
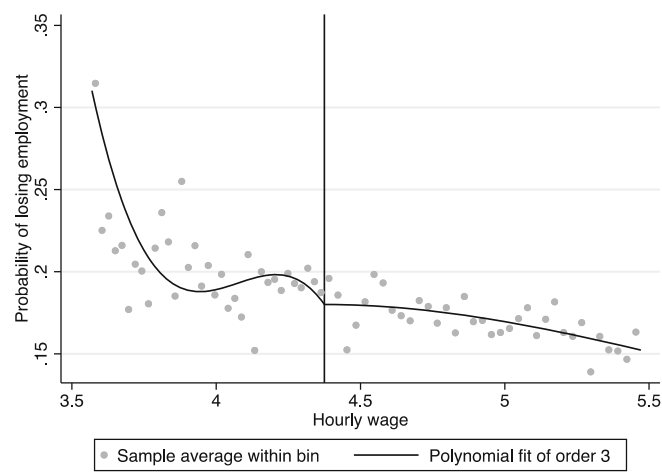
a) *Polynomial of order 1*b) *Polynomial of order 2*

Fig. 17 Regression Discontinuity Plots (RD Plots): Probability of losing employment after 12 months. Sample of employees working in November 2018

c) Polynomial of order 3



d) Non-parametric comparison of means. Bandwidth [€4.25 – €4.5 hourly wages] (+/- €0.125 around the cutoff)

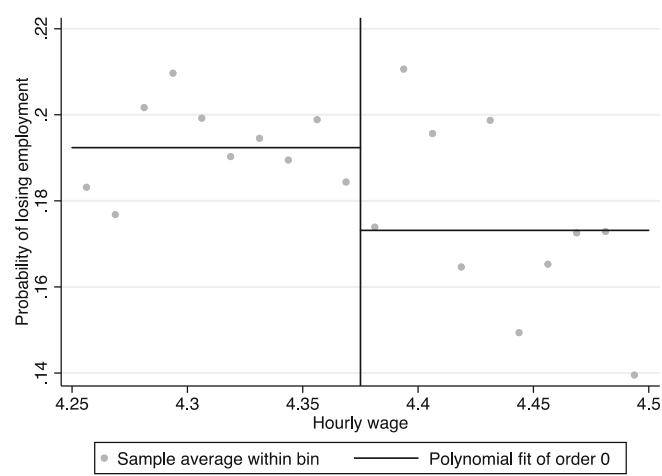
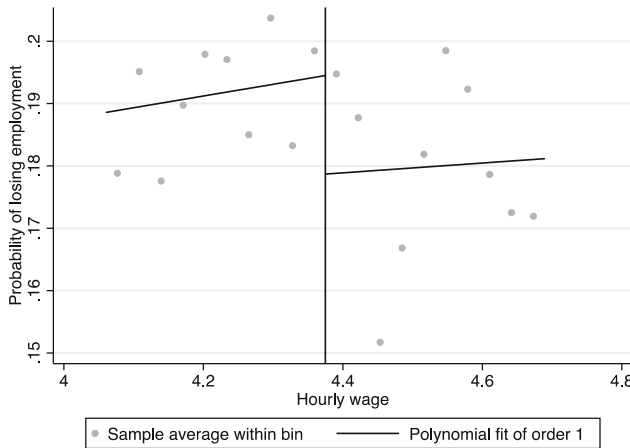


Fig. 17 continued

e) Optimal bandwidth



Note: Circles are local averages within each hourly wage bin. Black lines are polynomial fits using regression specifications in Section 6.

Fig. 17 continued

References

- Aaronson D, French E (2007) Product market evidence on the employment effects of the minimum wage. *J Law Econ* 25(1):167–200. <https://doi.org/10.1086/508734>
- Aaronson D, French E, Sorkin I, To T (2018) Industry dynamics and the minimum wage: a putty-clay approach. *Int Econ Rev* 59(1):51–84. <https://doi.org/10.1111/iere.12262>
- Arellano Espinar FA, Jansen M (2014) Salario mínimo interprofesional y empleo juvenil ¿necesidad de cambios?. *ICE, Revista De Economía*, 1(881). Retrieved from <http://www.revistasice.com/index.php/ICE/article/view/1734>
- Autoridad Independiente de Responsabilidad Fiscal (2020) *Impacto sobre el empleo de la subida del Salario Mínimo Interprofesional a 900€ mensuales*
- Barceló C, Izquierdo M, Lacuesta A, Puente S, Regil A, Villanueva E (2021). Los efectos del salario mínimo interprofesional en el empleo: nueva evidencia para España. Banco de España. (Documentos Ocasionales No. 2113)
- Blázquez M, Llorente R, Moral J (2009) Minimum wage and youth employment rates in Spain: New Evidence for the Period 2000–2008, Working Papers in Economic Theory, 2009/02. Universidad Autónoma de Madrid
- Calonico S, Cattaneo MD, Farrell MH, Titiunik R (2017) rdrobust: software for regression-discontinuity designs. *Stand Genomic Sci* 17(2):372–404. <https://doi.org/10.1177/1536867X1701700208>
- Card D (1992) Do minimum wages reduce employment? A case study of California, 1987–89. *Ind Labor Relat Rev* 46(1):38. <https://doi.org/10.2307/2524737>
- Card D, Krueger A (1994) Minimum wages and employment: a case study of the fast-food industry in New Jersey and Pennsylvania. *Am Econ Rev* 84(4):772–793
- Card D, Krueger A (1995) *Myth and measurement: the new economics of the minimum wage*. Princeton University Press, Princeton
- Cebrián I, Pitarch J, Rodríguez C, Toharia L (2010) Análisis de los efectos del aumento del salario mínimo sobre el empleo de la economía española. *Revista De Economía Laboral* 7(1):1–38
- Cebrián I, Jiménez Martínez M, Jiménez Martínez M (2020) The impact of the minimum wage on employment survival. A quasi-experimental evaluation using an administrative database for Spain.
- Cengiz D, Dube A, Lindner A, Zippper B (2019) The effect of minimum wages on low-wage jobs*. *Q J Econ* 134(3):1405–1454. <https://doi.org/10.1093/qje/qjz014>

- Cengiz D, Dube A, Lindner A, Zentler-Munro D (2021) Seeing beyond the trees: using machine learning to estimate the impact of minimum wages on labor market outcomes (No. w28399; p. w28399). National Bureau of Economic Research. <https://doi.org/10.3386/w28399>
- Clemens J (2021) How do firms respond to minimum wage increases? Understanding the relevance of non-employment margins. *J Econ Perspect* 35(1):51–72. <https://doi.org/10.1257/jep.35.1.51>
- Conde-Ruiz JJ, García M, Puch LA, Ruiz J (2019) Calendar effects in daily aggregate employment creation and destruction in Spain. *Series* 10(1):25–63. <https://doi.org/10.1007/s13209-019-0187-7>
- Dolado J, Felgueroso F (1997) Los efectos del salario mínimo: Evidencia empírica el caso español. Universidad Carlos III de Madrid, Documentos de Trabajo. Economía
- Dolado JJ, Felgueroso F, Jimeno JF (1997) The effects of minimum bargained wages on earnings: evidence from Spain. *Eur Econ Rev* 41(3–5):713–721. [https://doi.org/10.1016/S0014-2921\(97\)00003-2](https://doi.org/10.1016/S0014-2921(97)00003-2)
- Draca M, Machin S, Van Reenen J (2011) Minimum wages and firm profitability. *Am Econ J Appl Econ* 3(1):129–151. <https://doi.org/10.1257/app.3.1.129>
- Dube A (2019) Impacts of minimum wages: review of the international evidence. Independent Report. UK Government Publication, 268–304
- Dustmann C, Lindner A, Schönberg U, Umkehrer M, vom Berge P (2021) Reallocation effects of the minimum wage. *Q J Econ* 137(1):267–328. <https://doi.org/10.1093/qje/qjab028>
- Galán S, Puente S (2012) Una estimación del impacto de las variaciones del salario mínimo sobre el empleo. *Boletín económico. Banco de España* 12:19–25
- Galán S, Puente S (2015) Minimum wages: do they really hurt young people? *The B.E. J Econ Anal Policy* 15(1):299–328. <https://doi.org/10.1515/bejeap-2013-0171>
- González I, Jiménez S, Pérez C (2003) Los efectos del salario mínimo sobre el empleo juvenil en España: Nueva evidencia con datos de panel. *Revista Asturiana De Economía* 27:147–168
- González I, Pérez C, Rodríguez JC (2012) Los efectos del incremento del salario mínimo interprofesional en el empleo de los trabajadores inmigrantes en España. *El Trimestre Económico* 79(314):379–414
- Harasztosi P, Lindner A (2019) Who pays for the minimum wage? *Am Econ Rev* 109(8):2693–2727. <https://doi.org/10.1257/aer.20171445>
- Iacus SM, King G, Porro G (2012) Causal inference without balance checking: coarsened exact matching. *Polit Anal* 20(1):1–24. <https://doi.org/10.1093/pan/mpr013>
- Lacuesta A, Izquierdo M, Puente S (2019) Un análisis del impacto de la subida del salario mínimo interprofesional en 2017 sobre la probabilidad de perder el empleo. Documentos ocasionales/Banco de España, 1902
- Linneman P (1982) The economic impacts of minimum wage laws: a new look at an old question. *J Polit Econ*, 90(3):443–469. <http://www.jstor.org/stable/1831365>
- Manning A (2021) The elusive employment effect of the minimum wage. *J Econ Perspect* 35(1):3–26. <https://doi.org/10.1257/jep.35.1.3>
- Neumark D, Wascher WL (2010) Minimum wages. MIT Press, Cambridge
- Neumark D, Salas JMI, Wascher W (2014) Revisiting the minimum wage—employment debate: throwing out the baby with the bathwater? *ILR Review* 67(312):608–648. <https://doi.org/10.1177/001979391406705307>
- Neumark D, Shirley P (2021) Myth or measurement: what does the new minimum wage research say about minimum wages and job loss in the United States? (No. w28388; p. w28388). National Bureau of Economic Research. <https://doi.org/10.3386/w28388>
- Comisiones Obreras (2019) La subida del salario mínimo en 2019. Una visión territorial y por federaciones de CCOO. Informe del Gabinete Económico de CCOO
- OECD (2015) In it together: why less inequality benefits all. OECD Publishing, Paris. <https://doi.org/10.1787/9789264235120-en>
- Pérez JIG (2008) La muestra continua de vidas laborales: una guía de uso para el análisis de transiciones. *Revista De Economía Aplicada* 16(1):5–28
- Sorkin I (2015) Are there long-run effects of the minimum wage? *Rev Econ Dyn* 18(2):306–333. <https://doi.org/10.1016/j.red.2014.05.003>