



Estudios sobre la Economía Española

Firm-Level Employment Dynamics and Minimum Wages: Evidence from Spain

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Resumen no técnico

Este estudio ofrece una estimación del efecto causal del aumento del salario mínimo interprofesional (SMI) en 2019 sobre las dinámicas del empleo a nivel de empresa. El estudio ofrece importantes novedades. Por primera vez, se han utilizado datos del Panel Empresa-Trabajadores (PET) y se ofrece un análisis exhaustivo de los ajustes en las plantillas de las empresas tras la subida, comparando los efectos sobre las pequeñas y las grandes empresas. Además, el estudio analiza cómo afectó la subida del SMI a los flujos tanto de nuevas contrataciones como salidas, así como la calidad del empleo en términos del tipo de contrato y duración de la jornada.

Nuestra estrategia empírica se basa en el influyente estudio de Harasztosi y Lindner (2019). A partir de un modelo de regresión, comparamos la evolución del empleo y los salarios medios en establecimientos con diferentes porcentajes de trabajadores afectados para los que el aumento del SMI era vinculante. Al contrario de Harasztosi y Lindner, no disponemos de datos sobre productividad o precios. Por lo tanto, no podemos estudiar en qué medida las empresas fueron capaces de absorber las subidas del SMI en sus márgenes o de trasladar el aumento en sus costes a los consumidores. Para superar esta limitación, ofrecemos un análisis exhaustivo de los efectos diferenciales según el tamaño de los establecimientos, medido por el número de empleados. En la medida en que la productividad y el poder de mercado de las empresas están correlacionados con su tamaño, se trata de una forma indirecta de captar las diferencias en la capacidad de las empresas de mitigar el impacto de una subida inesperada en el salario mínimo.

Los principales hallazgos son:

- Los datos muestran que la subida afectó al 21% de las empresas y que era vinculante para el 12% de los trabajadores. Además, se observa una considerable dispersión en el grado de afectación de las empresas por la subida del SMI, medida por el porcentaje de trabajadores afectados. Entre las empresas afectadas, este porcentaje es decreciente en el tamaño de las empresas, con un valor mediano del 100% para las empresas con menos de 5 empleados, comparado con un valor mediano de 2% para las empresas con más de 250 empleados.
- Los resultados principales muestran que la subida del SMI tuvo un impacto muy significativo en el empleo durante los primeros 12 meses. En establecimientos donde toda la plantilla estaba afectada, el crecimiento salarial aumentó en 11,4 puntos porcentuales, mientras que el crecimiento del empleo disminuyó en 4,5 puntos porcentuales. Para el resto de los establecimientos los efectos causales son proporcionales al porcentaje de trabajadores directamente afectados por la subida del SMI.
- Los efectos negativos sobre el crecimiento de empleo se concentran en los pequeños establecimientos de hasta cinco empleados, muy expuestos al SMI. Sin embargo, la subida tuvo implicaciones mucho más amplias, ya que provocó un aumento simultáneo de las contrataciones y salidas de trabajadores en las empresas afectadas de hasta 250 empleados. Se trata de un resultado inédito, muy vinculado a la estructura dual del

mercado laboral español. El consiguiente aumento de los flujos llevó a un deterioro de la calidad de los empleos, con menos contratos permanentes y la sustitución de contratos temporales a tiempo completo por otros a tiempo parcial.

- No encontramos evidencia de que los trabajadores desplazados se hayan reubicado en mejores empleos.
- Los efectos observados son grandes en comparación con los estudios disponibles. Casi el 40% de las ganancias potenciales derivadas del aumento del SMI se vieron contrarrestadas por los ajustes de las plantillas. Sin embargo, el efecto agregado fue moderado, con una disminución en la tasa de crecimiento medio del empleo de entre 0.5 y 1.0 puntos porcentuales - de un crecimiento medio observado de 3% - debido al bajo nivel inicial del SMI. Estas cifras ofrecen el límite superior para los efectos agregados de la subida en el SMI.

Firm-Level Employment Dynamics and Minimum Wages: Evidence from Spain*

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Abstract

This paper estimates the effects of a sharp rise in the Spanish minimum wage on firm-level employment and worker flows. Our analysis uses a novel dataset of linked employer-employee data and exploits the variation in the share of workers of each establishment who were directly affected by the increase in the minimum wage using a difference-in-differences design. We find that the 22% minimum wage hike caused an increase in wage growth of approximately 11 percentage points and a reduction in employment growth of around 5 percentage points in establishments where all workers were affected relative to firms where no workers were affected, resulting in an own-wage elasticity of -0.39. The negative effects on employment are concentrated in small establishments with up to five employees, but we show that the minimum wage had much broader implications as it caused a simultaneous increase in inflows and worker outflows for affected establishments with up to 250 employees. The resulting increase in gross flows almost doubles the net effects on employment leading to a deterioration of job quality. We link these novel findings to the dual structure of the Spanish labor market.

Keywords: *minimum wage, employment, worker flows*

JEL Classification: *J23, J38, J63*

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1 Introduction

In October 2018, the Spanish government announced an unprecedented 22% increase in the national minimum wage, which came into effect three months later. The announcement marked the end of an era of low minimum wages. Within months, the Kaitz ratio — the ratio of the minimum wage to the median wage - jumped from one of the lowest levels in Europe to a level comparable to that of Germany and the Netherlands. This was followed, a year later, by an agreement to raise the minimum wage to a level of 60% of the mean wage within four years.¹ In this article, we exploit the unanticipated nature of the initial increase in 2019 to examine its impact on employment growth at the establishment level.

The policy change in Spain is not an isolated case. In recent years, numerous governments have significantly increased or introduced minimum wages. However, minimum wages remain a controversial policy instrument as the evidence on their employment effects is mixed. The traditional view is that binding minimum wages reduce employment, while the new minimum wage literature emphasizes the prevalence of small and often statistically insignificant effects found in recent studies (Dube & Lindner, 2024; Neumark & Shirley, 2021). A growing body of research explores the mechanisms behind this muted response of employment. Consistent with economic theory, minimum wages are shown to have an ambiguous effect on employment when firms have monopsony power (Azar et al., 2019; Popp, 2023). Other studies emphasize that market power in product markets allows firms to pass on part of the increase in labor costs to customers (Harasztosi & Lindner, 2019). Furthermore, there is evidence that minimum wages may induce a reallocation of workers to more productive firms (Dustmann et al., 2021; Rao & Risch, 2024).

In the particular case of Spain, the effects of the minimum wage cannot be understood without taking into account the dual structure of its labor market. In 2018, one in four Spanish workers was employed on a temporary contract.² These contracts offer worse conditions and are much cheaper to terminate than permanent contracts. In addition, a relatively large share of Spanish establishments are small and medium-sized companies (in 2018, 81% had 5 or fewer workers).³ An interesting empirical question is to what extent these characteristics amplify the effects of large unanticipated changes in the national minimum wage.

¹In total, the Spanish government has increased the minimum wage by 61% since 2018. The subsequent increases in following years are outside the scope of our analysis.

²National Statistical Agency (January 29th, 2019), Labor Force Survey of 4th Quarter 2018, available at this link.

³National Statistical Agency (July 31st, 2018), Structure and dynamism of the business community in Spain, available at this link.

Following Harasztosi and Lindner (2019), we use administrative data and a difference-in-differences strategy that exploits the variation in the share of workers in each establishment who were directly affected by the minimum wage increase. An affected worker is someone whose wage at the time of the announcement was below the 2019 minimum wage, while our main outcome variables measure the annual percentage change in the mean wage and employment at the establishment level as a function of the share of affected workers. In an extension, we decompose the changes in employment by looking separately at the flows of hires and separations. In addition, we examine whether the displaced workers from the affected establishments moved to better jobs.

Unfortunately, we do not have information on productivity or output prices, nor do we observe firms' margins. Thus, we cannot analyze to what extent firms were able to absorb the increased labor costs in their margins or to pass them on to their customers. To overcome this limitation, we provide an exhaustive analysis of the differential effects by size of the establishments, measured by the number of employees. To the extent that firms' productivity and market power are correlated with size, this is an indirect way of capturing differences in the capacity of firms to mitigate the impact of higher minimum wages. Furthermore, an important feature of our dataset is that we observe the complete working histories of all the individuals who, at some point in time, worked in one of the establishments in our sample. This feature allows us to construct precise monthly indicators for the exposure of all establishments, including the smallest ones. Their inclusion turns out to be crucial for our results. To assess the robustness of our results, we also replicate our baseline specification at the worker level.

Our main findings highlight the strong impact of the increase in the minimum wage in 2019 on highly exposed establishments. In establishments with an exposure of 100%, wage growth rose by 11.4 percentage points while employment growth declined by 4.5 percentage points over the first twelve months relative to establishments where there were no exposed workers. For the remaining establishments, the effects are proportional to the share of directly affected workers, as we do not find evidence of any non-linear effects. Our estimates are consistent with an own-wage elasticity (OWE) of -0.39, suggesting that almost 40% of the potential earnings gains from the minimum wage increase were offset by job losses. This OWE is located in the upper tercile of the distribution of the studies reviewed in Dube and Zipperer (2024) and more than doubles the OWE reported in Harasztosi and Lindner (2019).⁴ Finally, we find some evidence that establishments reduced hours

⁴The own-wage elasticity is obtained by taking the ratio of the point estimates for employment and wage growth. The advantage of the OWE is that it can be compared across studies as it scales the effect on employment by the change in wages that can be attributed to the minimum wage. For a detailed discussion of its properties, see Dube

and/or shortened the length of temporary contracts to mitigate the impact of the higher minimum wage. In full-time equivalent terms, the maximum impact on mean wages and employment is 14.6 and 5.9 percentage points, respectively. However, in the case of employment the difference with respect to our baseline is not statistically significant.

We find that the negative effects are concentrated in the smallest establishments with up to five employees. The coefficient estimates vary little by size, but exposure among affected establishments declines sharply as size increases. In particular, the median exposure among affected establishments is 100% for those with up to five employees, compared to 11% for establishments with 10 to 49 employees. For larger ones the effects on employment are not statistically significant. Reassuringly, the worker-level analysis yields similar results. The probability that a worker from an establishment with a 100% exposure is employed twelve months after the reform is 5.1 percentage points lower than for workers from unaffected establishments. This finding discards the risk that we overestimate the effects by capturing movements of workers from highly exposed establishments to less exposed ones.

Our analysis of worker flows provides further evidence of the comparatively strong impact of the minimum wage increase. We start by showing that the effect on employment was entirely driven by an increase in the separation rate, which disproportionately affected vulnerable groups such as youth, women and low-skilled workers. This latter result is common in the minimum wage literature, but what is striking about our findings is that the hike led to a simultaneous increase in both the hiring and separation rates of affected establishments with up to 250 employees. The resulting increase in gross flows almost doubles the net change in employment and led to a decline in overall job quality. In particular, we observe a significant decline in the creation of permanent jobs and a shift from full-time temporary jobs to part-time temporary jobs. These offsetting changes in the quality of jobs have negative implications for the earnings stability of the affected workers and are more extensive than the adverse effects on employment. Finally, we find no evidence that displaced workers transitioned to better-quality jobs.

This paper contributes to a small body of literature that studies the effects of the minimum wage on firm-level outcomes.⁵ The novel element of our analysis is the dual structure of employment protection in Spain. Our analysis provides strong indications that this key institutional feature,

and Zipperer (2024).

⁵Beyond the seminal work of Card and Krueger (1993), studies by Harasztosi and Lindner (2019), Dustmann et al. (2021), Machin et al. (2003), Draca et al. (2011), Mayneris et al. (2018), Chava et al. (2019), and Luca and Luca (2019) have also contributed to the literature.

present in many European countries, magnifies the effects on employment and turnover. The OWE is high by international standards and, to the best of our knowledge, we are the first to document a positive effect of minimum wages on job creation and excess turnover. Some existing studies suggest that minimum wage policies reduce worker turnover by reducing the arrival rate of better-paying jobs (Dube et al., 2016; Portugal & Cardoso, 2006; Wiltshire et al., 2023), while others find no significant changes in turnover (Hirsch et al., 2015). These results make sense if turnover costs are high. In contrast, in Spain turnover costs for temporary jobs are very low, allowing firms to respond quickly to changes in the minimum wage. We find that the predominant response of the smallest firms is to downsize, while the larger ones tend to switch to cheaper contracts. This negative effect of the minimum wage on the quality of jobs is our second contribution. Recent survey evidence for the UK⁶ indicates that a considerable fraction of firms use zero-hour contracts as a means to mitigate the cost of higher minimum wages, but according to the UK Low Pay Commission there is no systematic evidence of a causal link between higher minimum wages and the incidence of zero-hour contracts.

Finally, it is also important to note that prior research has examined the unprecedented increase in the Spanish minimum wage in 2019. Some recent papers have studied the same episode using worker-level data. All of them report negative effects on employment, but the differences in empirical specifications, outcome variables and sample selection criteria make comparisons difficult. Three out of four (Gorjon et al., 2024; Hijzen et al., 2023; Laporta, 2022) use individual-level data to analyze the effect of the minimum wage on the probability of employment or the transition rate into unemployment of those directly affected. The work of Barcelo et al. (2021) is closer in spirit to ours. They estimate the effect of the 2019 increase on employment growth by comparing the evolution of the mass of jobs above and below the new minimum wage and provide an upper bound for the reduction in the aggregate growth rate of employment. As it turns out, this upper bound is virtually identical to the average reduction in employment growth we obtain, which is between -0.5 and -1 percentage points. In both cases, the calculations exclude possible general equilibrium effects due to higher consumption growth.

The structure of the paper is as follows. In Section 2, we describe the institutional context. In Section 3, we describe our data and the identification strategy and report descriptive statistics. In Section 4, we present our main results at the establishment-level. In Section 5, we decompose our main results in worker flows. In Section 6, we test the robustness of our results at the worker-level.

⁶Resolution Foundation (2024).

Finally, Section 7 concludes.

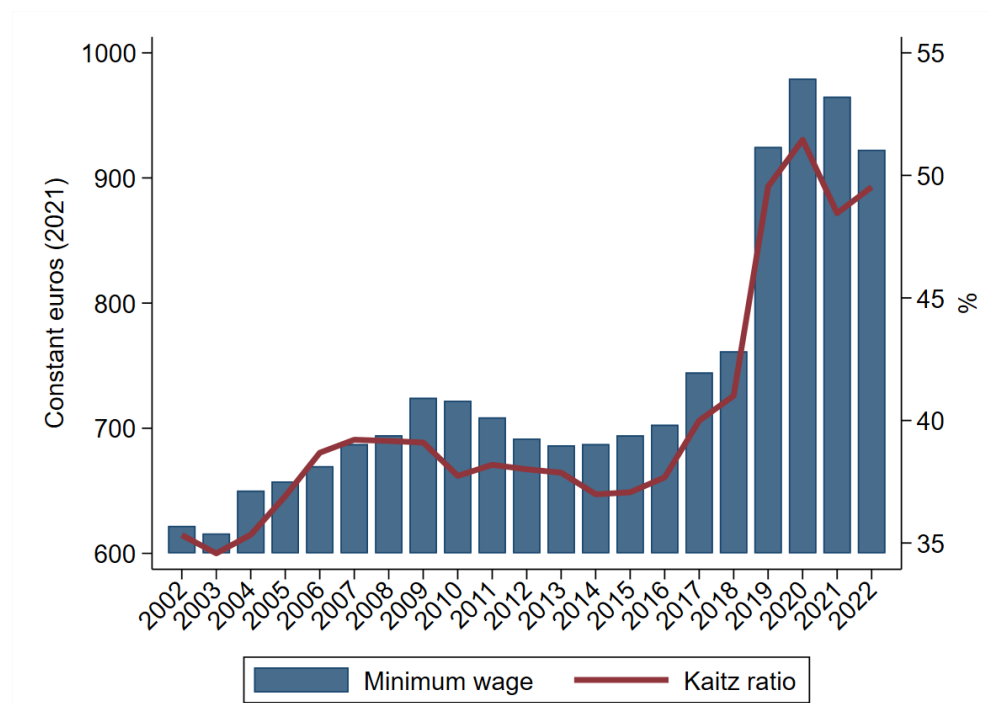
2 Institutional and economic context

Institutional background

In Spain, all workers over the age of 16 are entitled to the same statutory minimum wage. By law, the minimum wage must be set annually by the national government after consultation with the social partners, taking into account the consumer price index, average productivity, the labor share, and general economic conditions.⁷ If inflation is higher than expected, the minimum wage may be revised mid-year.

Figure 1 shows the evolution of the real monthly minimum wage and the corresponding Kaitz ratio between 2002 and 2022. The figure highlights the exceptional nature of the decision of the government to raise the minimum wage in 2019 by 22%, from 736 to 900 euros.⁸

Figure 1: The evolution of the real monthly minimum wage and the Kaitz ratio



Notes: The reported figures correspond to the monthly minimum wage for full-time workers discounted by the 2021 CPI. The Kaitz ratio is the ratio of the minimum wage to the median wage.

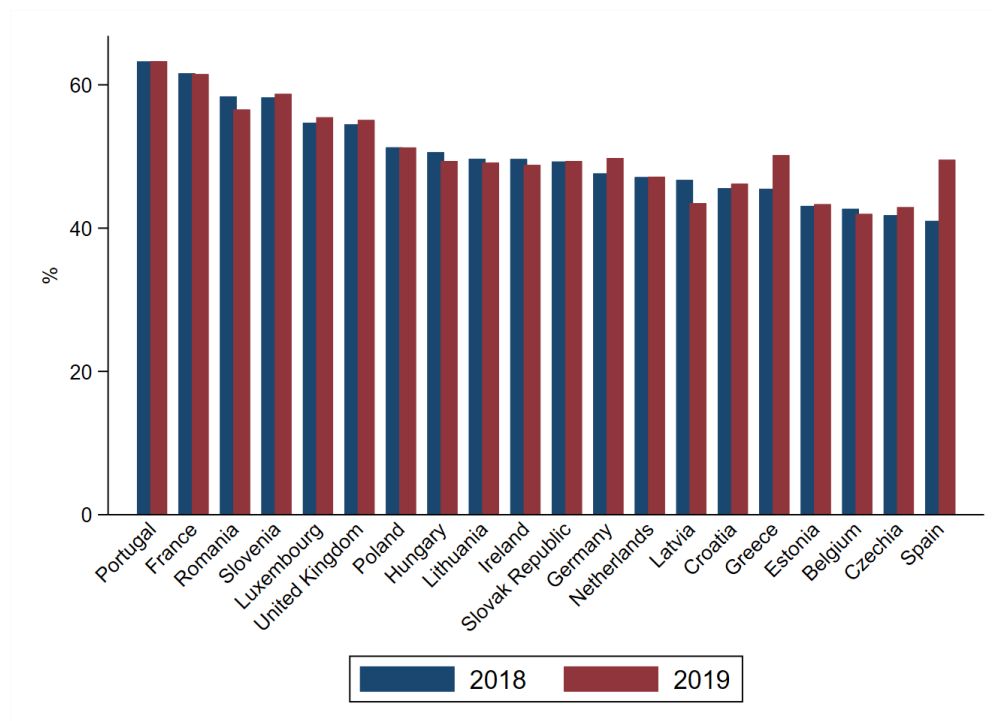
Sources: Own elaboration using data from the Ministry of Labor and Social Economy, the National Statistical Agency INE and the OECD Minimum Wage dataset.

⁷The minimum wage is regulated in Article 27 of the Workers' Statute.

⁸On an annual basis, this corresponds to an income of 11,600 euros, as workers in Spain are entitled to fourteen monthly salaries.

This increase was the largest since 2002 and pushed the Kaitz ratio above the 50% threshold for the first time in history. Due to this unprecedented change, Spain moved from the bottom position to the upper half of the distribution of Kaitz ratios among the European member states of the OECD (Figure 2).

Figure 2: Kaitz ratio in European OECD countries, 2018-2019



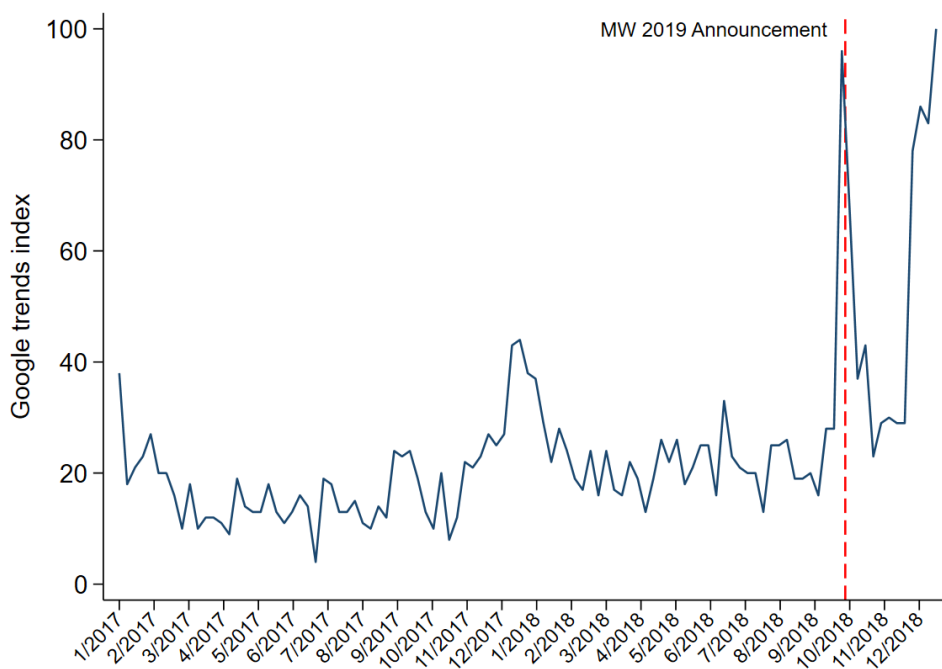
Notes: The Kaitz ratio is defined as the ratio of the minimum wage to the median wage of all salaried workers.
Source: Own elaboration using the OECD Minimum Wage dataset.

The increase in the minimum wage was announced three months in advance, in October 2018, as part of an agreement between the ruling socialist party, the PSOE, and Podemos. The former party came to power in June 2018 following a vote of no confidence, which was supported by Podemos. The minimum wage hike came as a surprise. A clear indication of the surprise nature of the measure is the peak in the number of Google searches for the Spanish term for minimum wage in October 2018 (Figure 3). In practical terms, the measure constituted a unilateral departure from an existing multi-annual agreement between the ousted government and the social partners. This agreement foresaw a step-wise increase in the minimum wage until a level of 850 euros in 2020. The objective of our empirical analysis is to analyse how this departure from the scheduled path of minimum wage increases affected the employment decisions of firms.

Finally, it is important to stress the role of collective bargaining. Collective agreements are legally

binding in Spain and cover over 80% of all salaried workers. In most cases, these collective agreements set wage floors above the level of the statutory minimum wage. Despite the existence of these wage cushions, our data reveal that the 22% increase in the minimum wage was binding for 21% of the establishments and 12% of the individuals in our sample, as we will show in more detail below.

Figure 3: Google searches in Spain of the Spanish term for minimum wage



Notes: Google Trends is an index built by Google to measure the interest of a particular topic. The index is built using a random sample of searches and is normalized by the proportion of all searches on all topics on Google at that time and location. For more details, see <https://trends.google.com/trends>.

Source: Own elaboration using data from Google Trends.

Labour market conditions

At the time of the minimum wage increase, salaried employment was growing at an annual rate between 1 and 2% for two consecutive years.⁹ Nevertheless, the recovery from the Great Recession was far from complete. In the fourth quarter of 2018, the overall unemployment rate still stood at 14.5% and more than one-fourth of all salaried workers was employed on a temporary contract, characterised by relatively short durations and low termination costs.¹⁰ These numbers help to understand why the large increase in the minimum wage generated an intense debate, with institutions like the Bank of Spain predicting a loss of up to 125 thousand jobs (Lacuesta et al.,

⁹National Institute of Statistics (INE). (2017;2018). Annual Wage Structure Survey, 2017 and 2018.

¹⁰National Institute of Statistics (INE). Labour Force Survey (EPA) - Fourth Quarter of 2018.

2019).

3 Data and empirical strategy

This section offers a description of our dataset, the sample selection criteria and our empirical strategy.

Data

Our primary source of data is the Panel Empresa-Trabajador (PET). This is a matched employer-employee dataset, elaborated by the social security administration, that offers exhaustive information on the dynamics of employment, worker flows and working conditions for a stratified random sample of establishments – so-called *cuentas de cotización secundarias* – and that is representative of the establishments within each of the strata.¹¹ The second wave of the PET covers the period 2017-2020. The distinguishing feature of the PET is that it provides the complete employment histories of all the individuals who were employed in each of the establishments during at least one day in a given year. The PET includes only establishments that fall under the General Regime of Spain’s Social Security system. As a result, we lack data on the self-employed, civil servants and the salaried workers included in special regimes, such as agriculture and fisheries.

The PET is made up of four separate files with information on (1) the individuals, (2) the establishments, (3) their affiliations and (4) the social security contribution base wages. Each of these files includes a unique identifier that allows to match individuals and establishments and to follow them over time.

The first file contains basic demographic characteristics such as the age, sex, nationality, and municipality of residence of the individuals. The second file contains the list of sampled establishments with information on their location, average number of employees, sector of activity (four-digit CNAE), ownership structure (public or private company), and a firm identifier (*cuenta de cotización primaria*). This variable is relevant if the establishment belongs to a multi-establishment firm.¹² Next, the third file contains information on all employment spells of the individuals, from their first affiliation to the date of extraction. We observe the start and end dates of each spell, the type of contract,

¹¹The sample includes 3% of the establishments with less than 10 employees, 5% of the establishments with 10 to 49 employees, 8% of the establishments with 50 to 249 employees, 10% for establishments with 250 to 499 employees and 15% of the establishments with more than 500 employees. For more details, see this link.

¹²The sampling strategy implies that we usually only observe detailed data for a subset of the establishments of multi-establishment firms. This explains why we do not include an analysis of employment adjustments at the firm level.

the contribution group (*grupo de cotización*), and a part-time indicator that measures the hours of work as a share of the regular full-time working week. Finally, the fourth file contains the corresponding monthly social security contribution base which is our proxy for earnings. For each establishment-worker pair the PET provides the monthly contribution base. If a worker is employed by several establishments in the same month, we observe the contribution base corresponding to each of these establishments. Although the contribution wage is top-coded, it is still useful as we are interested in individuals at the lower end of the wage distribution. For these individuals, the social security contribution base coincides with their gross monthly earnings from work.

The PET has certain limitations. First, the sample of establishments varies somewhat over time, but we do not observe the reason why establishments drop from the sample. The disappearing establishment may have closed, merged, or been acquired by another company, or the establishment may not have had any employees in a given year. Any result for adjustments on the extensive margin must therefore be interpreted with caution. Second, the PET includes comparatively little personal information. The main restrictions are the lack of information on the country of origin and the educational attainment of the individuals. Where needed, we use the contribution group as a proxy for the individual’s skills.¹³ Third and last, we can follow the individuals who move to an establishment outside our sample, but we only observe basic characteristics of these establishments such as their total number of employees and sector of activity. As a result, we cannot analyze whether displaced workers move to better firms that are less affected by the minimum wage.

Panel design

To study the impact of the increase in the minimum wage, we constructed a balanced panel dataset of establishments with monthly information on employment levels, average wages, and worker flows (inflows and outflows). To avoid confounding effects of the pandemic, we limit the analysis to the period 2017-2019.

The calculations of the monthly statistics are based on the records of persons employed in each establishment on the second Tuesday of the month.¹⁴ Our initial dataset comprised 69,845 establishments, but to improve the robustness of our results, we excluded establishments with no recorded employment activity during 2017 or 2018. In addition, we excluded the establishments belonging

¹³Several studies have shown the accuracy of this proxy using data from the Muestra Continua de Vidas Laborales.

¹⁴A similar sampling method is used in Gorjon et al. (2024). This sampling procedure eliminates the volatility due to the pronounced peaks in the worker flows on the first and last day of each week and month (Conde-Ruiz et al., 2018).

to the public and primary sectors,¹⁵ and the ones with an annual growth rate in the number of employees above the 99th percentile.

Our final sample consists of 28,928 establishments. Of these, 27,322 continue to report a positive number of employees in 2019. We use the former sample to compute the changes in employment, while the latter sample is used to calculate the changes in mean wages.¹⁶

Finally, we use annual comparisons to construct our measures for the inflows and outflows of workers. In particular, let n_{it} denote the level of employment in establishment i in month t and let Δn_{it} denote the change in employment period between t and $t - 12$. We calculate the inflow of workers by counting the number of workers who were not part of the establishment’s pool of employees at time $t - 12$ but are so at t , and the outflow by counting the number of workers who were so at time $t - 12$ but are no longer so at t . Similarly, we define an incumbent worker as someone who is employed in the same establishment at time $t - 12$ and t . Accordingly, our estimate of total employment can be decomposed as follows:

$$n_{it} = inflows_{it} - outflows_{it} + incumbents_{it} = \Delta n_{it} + incumbents_{it}$$

Dividing both sides of the above expression by n_{it-12} , we derive the corresponding worker flow rates. This transformation enables us to calculate the relative contribution of worker inflows and outflows to the proportional change in employment $\Delta n_{it}/n_{it-12}$.

Empirical strategy

We estimate the effects of the minimum wage increase by comparing the evolution of employment, mean wages and worker flows at establishments with many workers affected by the minimum wage increase to those establishments with few affected workers. In line with Harasztosi and Lindner (2019), we estimate the following regression model:

$$\frac{y_{it} - y_{it-12}}{y_{it-12}} = \beta_0 + \beta_1 FA_i + X_i \phi + \epsilon_{it} \tag{1}$$

¹⁵We identify public establishments using two fields: the identifier of public companies from the establishment files and the type of employer identified in the affiliation files.

¹⁶For the 2017-2018 sample, we take January 2019 as the upper bound of sample selection due to the way the PET is constructed. For each annual wave, Social Security excludes from the sample establishments that no longer had any active workers during the previous year. Still, we also detected cases for which there were workers at some year but they no longer appeared in the following. By using January 2019, we guarantee that those establishments we observe can only change their status due to their affiliation records and not due to sampling extraction reasons from Social Security.

where $(y_{it} - y_{it-12})/y_{it-12}$ denotes the annual growth rate in the outcome variable for establishment i in a given month t . FA_i , our measure of exposure, is the fraction of workers in establishment i with a wage below the new minimum wage, X_i is a set of time-invariant establishment characteristics and ϵ_{it} is a random error term. The coefficient of interest is β_1 . It measures how the change in the annual growth rate of the outcome variable varies with the fraction of workers for whom the minimum wage increase was binding. Our baseline specification assumes a linear relationship, but in the extensions we also allow for non-linear effects. Furthermore, the reference period for our outcomes variables is December 2019, while our baseline measure of exposure is calculated using data for September 2018, i.e. the last month before the announcement of the minimum wage increase.¹⁷

Throughout the analysis, we report separate estimates of the impact of the exposure to the minimum wage increase on mean wages and employment. In our baseline, we use the number of active employees in each establishment and their mean wage as our outcome variables. However, as many temporary employment spells last less than a month, we also report results for their full-time equivalents. This way we are able to capture potential changes in the duration of employment spells or the length of the working day in response to the minimum wage increase. Furthermore, we explore the robustness of our results to alternative measures of exposure, such as the wage gap measure analyzed in the following subsections, and we analyze how the impacts vary with establishment size. Finally, in an extension we analyze the separate impact of the minimum wage on the inflows and outflows of workers and the quality of jobs.

Identification

Our identification strategy is based on the assumption that the changes at establishments with fewer affected workers are a valid counterfactual for establishments with many affected workers, and so these establishments would have followed parallel trends in the absence of the minimum wage increase. While this assumption cannot be tested directly, we implement an event study design to test whether the parallel trends assumption holds before the announcement of the minimum wage increase.¹⁸ Reassuringly, we find no evidence of differential trends in either employment or mean wages prior to the announcement of the minimum wage increase.

We also consider possible violations of the Stable Unit Treatment Value Assumption due to spillovers

¹⁷Strictly speaking, our baseline specification is a difference equation in growth rates. This is formally equivalent to a difference-in-differences specification in levels of y_{it} that includes establishments fixed effects. These fixed effects disappear when we take first differences.

¹⁸In a robustness check we use a matching estimator that compare establishments with similar characteristics. This alternative specification delivers similar results as our baseline specification.

on unaffected establishments. While some of the displaced workers may move to unaffected establishments, we find that these effects are small or negligible. In particular, we show that our results do not change if we restrict our sample to establishments with at least one affected worker.¹⁹ Lastly, our set of controls includes the age of the establishment²⁰ and its legal form,²¹ sector fixed effects (three-digit CNAE-09), province fixed effects (NUTS3) and the average percentage of temporary employees of the establishment in 2017. The inclusion of the sector and province fixed effects implies that we obtain identification from the within-sector variation around the mean exposure in each province.

Finally, the observations are weighted using the strata-specific selection probabilities of the PET and all standard errors are clustered at the stratum level.

Descriptive statistics

Before discussing our results, we present descriptive statistics that highlight the significant exposure of Spanish establishments to the minimum wage increase and its variation by establishment size, region and sector of economic activity.

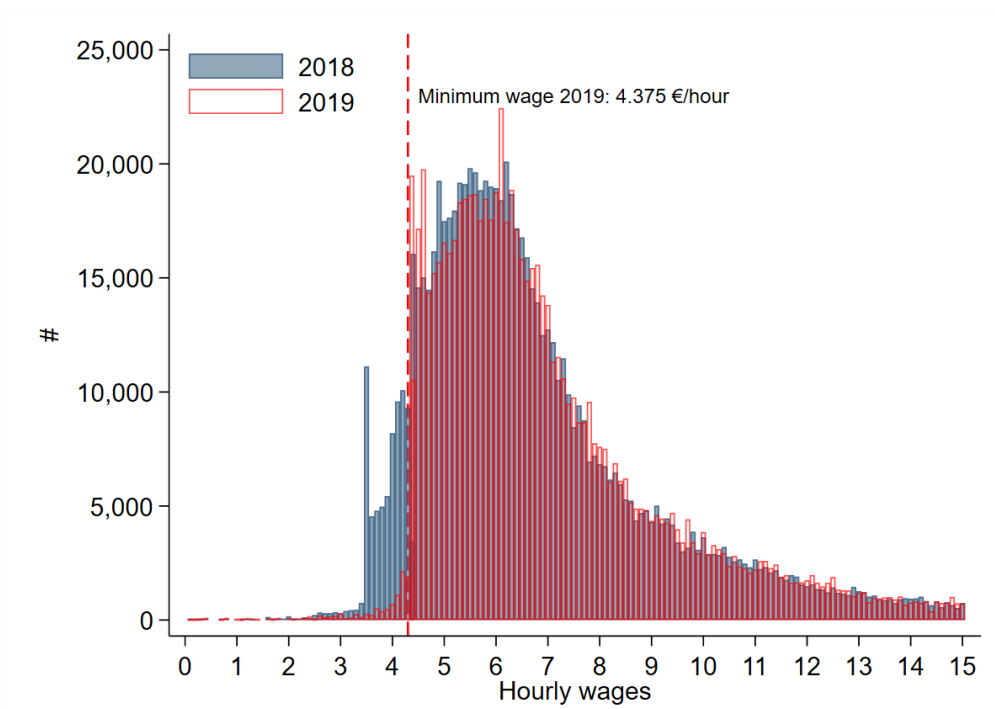
Figure 4 shows the change in the distribution of mean hourly wages between December 2018 and December 2019. Two facts stand out. One month before the new minimum wage became effective, a considerable fraction of establishments were paying lower mean hourly wages. Twelve months later, most of these establishments had moved upwards in the distribution to the three bins immediately to the right of the new minimum wage. We also see some changes in the right tail of the distribution, but it is less likely that these changes are driven by the change in the minimum wage.

¹⁹Similarly, in the Appendix we show that the increase in the minimum wage does not seem to have had a differential impact on the entry of establishments in highly exposed sectors, although we do observe a marginally significant decrease in entry in more exposed regions relative to less exposed regions. However, this is not a problem. It suggests that we may actually underestimate the negative effects on employment in the most affected regions as our baseline specification does not account for changes in establishments entry.

²⁰We build a measure of establishment's age using the first date of affiliation and estimate it in September 2018.

²¹The PET data provides a variable describing the legal form of the establishment. We group the alternative forms into six broad categories: sole trader, Public Limited Company, Private Limited Company, Cooperative, Public Company and others.

Figure 4: Establishment-level mean hourly wages, 2018-2019



Notes: The figure reports mean hourly wages for the sample of establishments with non-zero workers in the 2017 - 2019 period. The observations are weighted by the inverse of the sampling probabilities of the PET.
 Source: Own elaboration using PET data.

Next, Table 1 shows how exposure to the minimum wage varies by firm size, sector of economic activity and region. The 2019 minimum wage increase was binding for 21% of the establishments and 12% of the workers.²² While the share of affected establishments increases with size, the opposite is true for our measure of exposure. Thirteen percent of those working in the smallest establishments were directly affected by the minimum wage increase, compared to five percent of those working in the largest firms in our sample. The relatively strong exposure at small establishments becomes even more evident when we restrict attention to the establishments with at least one affected worker. For this subsample, the median exposure in establishments with up to five employees is 100%, compared to 2% in the largest establishments. We also observe a considerable degree of variation in the median exposure of affected establishments by sector of economic activity and the region they are located in, but along these dimensions the differences are less pronounced than between establishments of different size. Not surprisingly, mean exposure is highest in low-pay sectors and in regions with relatively high unemployment rates. To account for these facts, all our regressions

²²The statistics reported in the first row are weighted by the inverse sampling probability of establishments of different sizes, and therefore represent economy-wide figures.

Table 1: Summary statistics of establishments

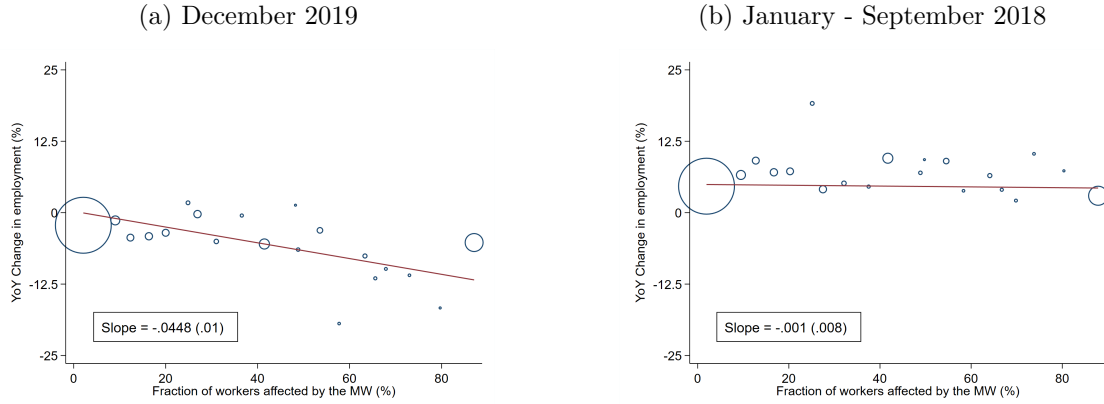
Establishment size	# establishments in PET	# establishments	% establishment with MW workers	% incidence	P25 % incidence with MW workers	P50 % incidence with MW workers	P75 % incidence with MW workers	% temporary workers in 2018
Total	28,928	827,366	21%	12%	18%	50%	100%	20%
Panel A: Establishment size								
1-5	16,796	559,867	7%	13%	50%	100%	100%	17%
6-9	3,224	106,867	11%	10%	14%	25%	62%	25%
10-49	6,657	133,947	14%	7%	6%	11%	33%	25%
50-249	1,853	23,275	28%	5%	2%	4%	14%	26%
250-500	222	2,224	45%	4%	1%	2%	6%	24%
+500	176	1,187	67%	5%	0%	2%	6%	29%
Panel B: Economic sector								
Administrative and auxiliary services	1,587	40,019	31%	12%	9%	25%	60%	24%
Communications	603	15,457	25%	12%	11%	33%	100%	16%
Construction	2,306	67,130	8%	4%	17%	38%	100%	34%
Education	847	20,795	44%	20%	17%	33%	75%	31%
Finance, insurance and real estate	1,246	38,749	21%	15%	50%	100%	100%	7%
Health	1,418	38,956	30%	13%	10%	33%	73%	19%
Hostelry	3,051	86,813	19%	7%	10%	25%	50%	27%
Household employed, art and recreational activities	2,937	91,978	30%	23%	50%	100%	100%	15%
Industry	3,513	90,737	15%	8%	12%	33%	100%	19%
Retail	7,656	226,766	21%	13%	25%	50%	100%	17%
Scientific and technicals	2,245	67,222	20%	11%	20%	50%	100%	13%
Transportation and storage	1,519	42,744	13%	7%	13%	44%	100%	20%
Panel C: Autonomous community								
Andalucía	4,478	131,230	24%	14%	21%	50%	100%	28%
Aragón	691	19,148	16%	7%	10%	33%	75%	18%
Asturias	557	16,242	20%	11%	19%	50%	100%	21%
Canarias	1,389	40,178	38%	26%	30%	67%	100%	19%
Cantabria	260	7,224	22%	11%	11%	33%	100%	19%
Castilla y León	763	20,028	20%	7%	8%	20%	50%	20%
Castilla-la Mancha	820	22,782	22%	11%	15%	38%	100%	24%
Cataluña	6,044	171,824	18%	9%	16%	50%	100%	16%
Ceuta y Melilla	28	665	25%	10%	10%	14%	50%	19%
Comunidad Valenciana	3,382	98,445	21%	11%	20%	50%	100%	21%
Extremadura	369	10,546	29%	18%	27%	57%	100%	26%
Galicia	1,574	45,769	21%	11%	17%	50%	100%	21%
Islas Baleares	823	23,674	16%	8%	14%	45%	100%	18%
La Rioja	121	3,030	17%	7%	5%	20%	50%	21%
Madrid	5,208	149,022	20%	12%	20%	50%	100%	14%
Murcia	939	27,313	30%	17%	25%	50%	100%	24%
Navarra	318	8,341	11%	4%	10%	25%	100%	22%
Pais Vasco	1,164	31,907	13%	6%	10%	33%	80%	18%

Notes: This table shows the descriptive statistics at the establishment level of the minimum wage incidence. We consider the baseline sample of existing establishments in 2017 - 2018 and define affected workers as those whose full-time equivalent wage in September 2018 is below the minimum wage. The establishment size categories are defined using the average number of active workers in 2018. The sector and region are defined using information at the establishment level. The P25, P50 and P75 correspond to the 25th, 50th and 75th percentile distribution of the minimum wage incidence among the subsample of establishments with at least one affected worker ($FA > 0$). The observations are weighted by the inverse of the sampling probabilities of the PET in all columns except for "# establishments in PET".
Source: Own elaboration using PET data.

include province and sector fixed effects.

Our final piece of descriptive evidence concerns the change in the correlation between our measure of the incidence of the minimum wage, FA_i , and annual employment growth at the establishment level, $\Delta n_{it}/n_{it-12}$. To make this bin-scatter plot, we created 20 equally spaced bins for the fraction of workers affected. Figure 5a confirms the existence of a clear negative correlation between these two variables in the time period between December 2018 and December 2019. In contrast, both variables are uncorrelated in the first nine months of 2018.

Figure 5: The correlation between annual employment growth and minimum wage incidence



Notes: Panel A shows the relationship between minimum wage incidence and the annual employment growth at the establishment level in December 2019, after controlling for the same observable characteristics as in our benchmark specification. Panel B shows the same relationship against the employment growth between January and September 2018. The size of the hollow circles reflects the density of establishments with a certain incidence of the minimum wage and the red line is the linear fit. The observations are weighted by the inverse of the sampling probabilities of the PET.

Source: Own elaboration using PET data.

4 Results

Main results

This section summarizes our findings.

Table 2 presents our main results. The first two columns refer to our baseline specification in which we regress the annual growth rate in mean wages and employment in December 2019 on exposure in September 2018.

According to our results, the minimum wage increase added 11.4 percentage points to wage growth and reduced employment growth by 4.5 percentage points in establishments where all workers were directly affected by the increase, relative to establishments where no workers were directly affected by the minimum wage increase. Both coefficient estimates are statistically significant at the 1% level and together imply an own-wage elasticity of -0.39. While these effects may seem large, it should be borne in mind that the mean exposure across all establishments was 0.12.

Table 2: Main results

Variables	Baseline		Full time equivalent		n \geq 50		Employees in 2019	
	Wages (1)	Employment (2)	Wages (3)	Employment (4)	Wages (5)	Employment (6)	Wages (7)	Employment (8)
Fraction affected	0.1141*** (0.0052)	-0.0448*** (0.0100)	0.1462*** (0.0108)	-0.0593*** (0.0102)	0.1266*** (0.0235)	-0.0563 (0.0680)	0.1141*** (0.0052)	-0.0313*** (0.0081)
Observations	27,322	28,928	27,322	28,928	2,207	2,251	27,322	27,322
Controls	YES	YES	YES	YES	YES	YES	YES	YES
OWE		-0.392		-0.406		-0.445		-0.274

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parenthesis. Columns (1) and (2) report the results for our baseline specification using growth in the number of employees in each establishment and their mean wages as outcome variables. In Columns (3) and (4) we replaced the baseline outcome variables by their full-time equivalents, while Columns (5) and (6) reproduce our baseline specification for the sample of establishments with more than 50 workers in 2018. Finally, in Columns (7) and (8) all establishments with zero workers in 2019 are excluded from the sample. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and PET's probability of selection weights observations.

Source: Own elaboration using PET data.

The next two columns report the estimates when the outcome variables are measured in full-time equivalent terms. The associated point estimates are larger than in our baseline, suggesting that some firms responded to the minimum wage increase by shortening the duration of the contracts and/or the length of the working day of the affected workers. However, the difference in the impact on employment growth is not statistically significant, and the OWE is virtually unchanged.

In the next exercise, we only consider establishments with more than 50 employees. This sample restriction results in somewhat larger point estimates than in our baseline. Nevertheless, in absolute terms the impacts are small compared to the impact in small establishments, as less than 5% of the employees in establishments with more than 50 employees is directly affected by the minimum wage increase. The lack of statistical significance of the point estimate for employment is less relevant and should be interpreted with caution due to the sixfold increase in the standard errors.

Finally, the last two columns report the results when we restrict the sample to the 27,322 establishments that continue to employ workers in December 2019. In this case, the effects on wages and employment are estimated for the same sample. As expected, the point estimates are smaller than in our baseline, with employment growth falling by 3.1 percentage points in establishments with a 100% exposure.

Overall, our results confirm that the minimum wage increase led to an increase in wage growth and a reduction in employment growth. The employment effects are statistically significant for small establishments and affect both the internal and external margin of adjustment.

Monthly evolution

In this section, we report monthly estimates of the impact of FA_i on the annual growth rates of our outcome variables before and after the minimum wage increase. Our event study design includes a full set of monthly dummies for the period between January 2018 and December 2019 and their interactions with FA_i . In addition, we include establishment fixed effects to control for potential differential trends at the establishment level.²³ The results are shown in Figure 6.

Reassuringly, we do not observe any significant difference before January 2019 in the growth rates of employment and wages between establishments with a high and a low exposure to the minimum wage increase. This is compelling evidence in support for our assumption of parallel trends.

Next, coinciding with the jump in the minimum wage, we observe a sharp increase in the growth rate of mean wages for affected establishments. In establishments with a 100% exposure the effect on impact is a 10 percentage point increase in the annual growth rate of mean wages relative to establishments with zero affected workers. Thereafter, the difference in the pace of wage growth continues to widen until it peaks in October. The negative effect on employment growth builds up gradually and peaks after nine months, but the adverse effects are statistically significant from the second month onwards. This quick response of employment growth is a reflection of the low turnover costs for temporary contracts.

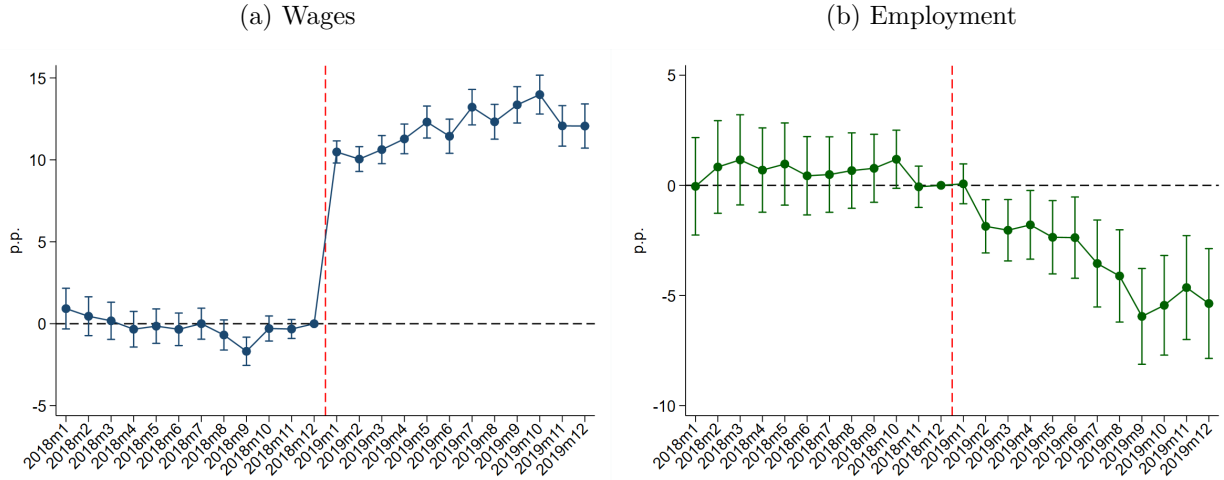
In the following subsections, we perform a decomposition to analyze how the dual structure of employment protection shaped the response of establishments' inflow and outflow decisions and the choice of contracts. But before doing so, we first present the results of a broad set of robustness tests.

²³Formally speaking, the equation model would be as follows:

$$\frac{y_{it} - y_{it-12}}{y_{it-12}} = \beta_0 + \sum_{t \neq 2018m12} \beta_{1t} D_t + \sum_{t \neq 2018m12} \beta_{2t} (FA_i \times D_t) + \gamma_i + \epsilon_{it},$$

where D_t is an indicator variable that takes value of 1 in monthly date t .

Figure 6: Monthly estimates of the impact on employment and earnings growth, 2018-2019



Notes: The figures report the estimated coefficients of the minimum wage incidence on wage growth (Panel A) and employment growth (Panel B) at the establishment level. We interact our measure of exposure FA_i with a full set of month fixed effects and set December 2018 as the base period. The red dashed line marks the moment the increase in the minimum wage came into effect. Both specifications further include establishment and time fixed effects. Standard errors are clustered at the establishment level and PET's probability of selection weights observations. The sample of establishments for regressions on wages includes establishments that had workers in 2017 and 2018, while the sample for regressions on employment also had workers in 2019. 95% confidence intervals are shown.

Source: Own elaboration using PET data.

Robustness

Our first set of robustness tests considers modifications to our baseline specification. In the first exercise, we add controls for sector-specific trends at the regional level by interacting the sector fixed effects with a region fixed effect. In the next exercise, we measure the impact on wages through changes in the growth rate of average hourly earnings rather than average monthly earnings. In the third exercise, we include a quadratic term in FA_i among the regressors to control for possible non-linear effects of the minimum wage increase, and in the fourth exercise we estimate the effects using a Coarsened Exact Matching procedure. In this last exercise, we divided our sample of establishments into a control group with zero affected workers and a treatment group with positive exposure. Then, we matched treated and control establishments using a list of variables. Our resulting sample uses 10,280 establishments (5,140 per group), which we use in our baseline specification to estimate the effect of the minimum wage hike.²⁴

²⁴The Coarsened Exact Matching is a multivariate matching method used to improve covariate balance between treated and control groups by temporarily coarsening variables into meaningful categories and performing exact matching on those. See Iacus et al. (2012) for more details.

As shown in Table 3, the only significant change is the stronger response of wages when we use hourly wages as the outcome variable (Column 3). The difference between the sensitivity of hourly and monthly wages is another indication that employers managed to mitigate the impact of the increase in the minimum wage on labor costs through adjustments in hours of work and/or the length of short-duration contracts.²⁵ Importantly, we do not find any evidence of non-linear effects on employment. The introduction of the quadratic term gives rise to a stronger and weakly non-linear effect on wages, while we observe no significant changes in the response of employment.

Table 3: Alternative specifications

Variables	Sector * region FE		Hourly wages		Non-linear effects		Matching	
	Wages (1)	Employment (2)	Wages (3)	Employment (4)	Wages (5)	Employment (6)	Wages (7)	Employment (8)
Fraction affected	0.1134*** (0.0052)	-0.0438*** (0.0101)	0.1462*** (0.0108)	-0.0448*** (0.0100)	0.1556*** (0.0207)	-0.0748** (0.0370)	0.1144*** (0.0065)	-0.0495*** (0.0128)
Fraction affected ²					-0.0005** (0.0002)	0.0003 (0.0004)		
Observations	27,322	28,928	27,322	28,928	27,322	28,928	9,671	10,280
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The specifications in Columns (1) and (2) include an interaction of the sector fixed effect with autonomous community region fixed effects. In Columns (3) and (4) we used average growth in hourly wages instead of monthly wages as our outcome variable, while Columns (5) and (6) present the results when we allow for non-linear effects through the introduction of a quadratic term in FA_i . Finally, in Columns (7) and (8) we estimate the effects using Coarsened Exact Matching comparing establishments with zero exposure (control) to establishments with at least one affected worker (treated group). The latter matches establishments using their 2-digit economic sector, autonomous community, legal form of the establishment, age of establishment in September 2018 and share of temporary of workers in 2017. Standard errors in parenthesis. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Table 14 in the Appendix reports the pre and post-matching mean differences. Standard errors are clustered at the stratum level and the observations are weighted using the inverse of the sampling probabilities of the PET. The sample of establishments for regressions on employment includes establishments that had workers in 2017 and 2018, while the sample for regressions on wages also had workers in 2019.

Source: Own elaboration using PET data.

Our next set of robustness tests includes two placebo tests which replicate our baseline using pre-treatment outcomes corresponding to 2018. The first placebo test uses annual growth rates as in our baseline, while the second exercise uses growth rates for the nine months prior to the announcement of the minimum wage increase.

In both cases we obtain a null effect for employment (Columns 2 and 4, Table 4), in line with the insignificant pre-treatment effects in the event study reported in the previous section. Before the announcement of the minimum wage increase, wage growth was apparently slightly less favorable at

In this paper, we use the following variables to match the treated and control groups: 2-digit economic sector, autonomous community, legal form of establishment, age of establishment in September 2018 and share of temporary workers in 2017. Table 14 in the Appendix reports the pre and post-matching mean differences.

²⁵During our sample period, approximately 40% of the contracts signed each month lasted less than 30 days.

establishments with high exposure than at establishments with low exposure (Column 3), but this finding does not alter our conclusions. If anything, it strengthens our results for wage growth in the aftermath of the minimum wage increase.

Finally, in the third exercise we replicate the baseline specification for the sample of establishments with at least one affected worker. The associates point estimates (Columns 5 and 6) are very similar to our baseline estimates, confirming that our findings are driven by the establishments that were directly affected by the increase in the minimum wage.

Table 4: Placebo tests

Variables	Placebo				At least one MW worker	
	December 2018		January-September 2018		Wages (5)	Employment (6)
	Wages (1)	Employment (2)	Wages (3)	Employment (4)		
Fraction affected	-0.0056 (0.0045)	-0.0010 (0.0080)	-0.0087*** (0.0033)	0.0013 (0.0061)	0.1026*** (0.0088)	-0.0416** (0.0166)
Observations	28,928	28,928	260,352	260,352	6,497	6,873
Controls	YES	YES	YES	YES	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Columns (1) and (2) replicate our baseline using data for December 2018, while columns (3) and (4) report the results when we use data for the period between January and September 2018. In columns (5) and (6), we exclude establishments that are unaffected by the minimum wage increase. Standard errors in parenthesis. All specifications further include the legal form of the establishment, establishment age in September 2018, province fixed effects and 3-digit economic sector fixed effects. In addition, in columns (5) and (6), we control for the percentage of temporary workers in 2017. Standard errors are clustered at the stratum level and the observations are weighted using the inverse of the sampling probabilities of the PET. In columns (1)-(4), the sample of establishments is restricted to those that had workers in 2017 and 2018.

Source: Own elaboration using PET data.

Finally, in our third set of exercises we consider alternative ways to measure the degree of exposure. In the first three exercises we use difference reference periods for the computation of FA_i — December 2018, January-September 2018 and January-December 2018.

In all three cases, we obtain a significantly higher point estimate for wages. The point estimates for employment are also considerably higher than in our baseline, but the differences are not statistically significant at conventional confidence levels. One possible explanation for this finding is the strong seasonality of the Spanish labor market. In our baseline, exposure is measured right before the end of the tourist season. In the final quarter of the year many of the jobs in this sector are destroyed, affecting many workers with wages near the minimum wage. Hence, there are good reasons to use measures of exposure that use data for more than one month. Nonetheless, we prefer to use September 2018 as our benchmark reference. It allows us to capture possible anticipation effects and our baseline estimates are more conservative than the alternatives reported in Table 5, although

all specifications lead to similar own wage elasticities.

Our final set of robustness checks explores alternative measures of exposure to the minimum wage. We find that the results are largely unchanged when exposure is defined at different points in time. Additionally, we assess the sensitivity of our findings using an alternative metric: the average distance between the wages of affected workers in an establishment and the new minimum wage. Once again, we obtain highly significant effects on both wages and employment, with an implied own-wage elasticity consistently close to -0.40 .

Table 5: Alternative measures of exposure

Variables	Incidence in December 2018		Incidence in January - September 2018		Incidence in 2018		GAP measure	
	Wages (1)	Employment (2)	Wages (3)	Employment (4)	Wages (5)	Employment (6)	Wages (7)	Employment (8)
Fraction affected	0.1280*** (0.0053)	-0.0543*** (0.0102)	0.1268*** (0.0056)	-0.0502*** (0.0110)	0.1303*** (0.0055)	-0.0521*** (0.0109)	0.4147*** (0.0209)	-0.1651*** (0.0335)
Observations	27,322	28,928	27,322	28,928	27,322	28,928	27,322	28,928
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. In Columns (1) and (2) exposure is measured in December 2018. Instead, in Columns (3) and (4) it is measured using the average share of workers in an establishment whose wage is below the 2019 minimum wage in the nine months from January to September 2018, and in Columns (5) and (6) we use the corresponding annual average in 2018. Columns (7) and (8) measure exposure using an establishment-level wage gap measure that is defined as the average percentage distance to the minimum wage of workers earning below the minimum wage in September 2018. Standard errors in parenthesis. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and the observations are weighted using the inverse of the sampling probabilities of the PET. The sample of establishments for regressions on employment includes establishments that had workers in 2017 and 2018, while the sample for regressions on wages also had workers in 2019.

Source: Own elaboration using PET data.

Heterogeneity

Our baseline specification assumes a linear relationship between the impact of the minimum wage increase and exposure FA_i . In this section, we extend our baseline by allowing the gradient to depend on other characteristics of the establishments. The main characteristic of interest is the size of the establishments, but we also consider differences by sector or region. The motivation for this exercise was explained in the introduction. If market power is positively correlated with establishment size, larger establishments may have more scope to absorb the minimum wage increase in their margins or to pass on the cost increase to their customers. In this case, we would expect the own-wage elasticity to decrease with the size of the establishment. Conversely, if labour demand is more elastic in larger establishments, the own-wage elasticity could actually increase with establishment size over a certain range.

To examine this issue, we divided the establishments into five groups. Table 6 reports the results when we estimate our baseline on each of the five subsamples. The advantage of this approach is that the fixed effects are now effectively transformed into region-times-size and sector-times-size fixed effects. Identification now comes from the variation within the same sector and region between establishments of similar size, but with different levels of exposure. However, this comes at the expense of a reduction in sample size, especially for the largest establishments.²⁶

Inspection of Table 6 shows that the point estimate for mean wages is highly significant and roughly stable for establishments with fewer than 250 employees, which make up more than 98% of our sample. On the contrary, in line with before (see Table 2), we find that the negative effect on employment growth is no longer significant for establishments with more than 50 workers. While the point estimate increases with size, so do the standard deviations. As a result, we only obtain a highly significant negative effect for the smallest establishments. The alternative specification with interactions produces similar results, except that in this case the negative point estimate remains highly significant for establishments with up to 49 employees.

The above results confirm that our baseline results are mainly driven by the effects for the smallest establishments. Based on our results we cannot reject the hypothesis of null effects for establishments with more than 50 employees, but the estimates for these groups are too imprecise to draw firm conclusions.

Table 6: Robustness on establishment size

Variables	1-5		6-9		10-49		50-249		+250	
	Wages (1)	Employment (2)	Wages (3)	Employment (4)	Wages (5)	Employment (6)	Wages (7)	Employment (8)	Wages (9)	Employment (10)
Fraction affected	0.1132*** (0.0060)	-0.0476*** (0.0112)	0.1174*** (0.0140)	-0.0261 (0.0326)	0.1177*** (0.0124)	-0.0534* (0.0289)	0.1312*** (0.0255)	-0.0615 (0.0742)	0.0589 (0.0584)	-0.0806 (0.1079)
Observations	15,587	16,796	3,087	3,224	6,441	6,657	1,810	1,853	397	398
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Columns (1) and (2) replicate the baseline specification for establishments with 1 to 5 employees, (3) and (4) for establishments with 6 to 9 employees, (5) and (6) for establishments with 10 to 49 employees, (7) and (8) for establishments with 50 to 249 employees and (9) and (10) for establishments with 250 or more employees. Standard errors in parenthesis. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and PET's probability of selection weights observations. The sample of establishments for regressions on employment includes establishments that had workers in 2017 and 2018, while the sample for regressions on wages also had workers in 2019.

Source: Own elaboration using PET data.

Next, looking at the differences between regions and sectors (Tables 11 and 12 in the Appendix), we find that the significant negative effects on employment are concentrated in three sectors —

²⁶Table 13 in the Appendix shows the results for the alternative when we interact FA with dummies for each group and Figure 10 the resulting OWEs.

construction, retail trade and hostelry — and three of the coastal regions — Andalusia, Canary Islands and Comunidad Valenciana.²⁷ The two findings are related. Both retail trade and hostelry are characterized by relatively low wages and a high proportion of small establishments and have a strong presence in the coastal regions due to their reliance on tourism. Another factor that may contribute to our results is the high share of temporary contracts in sectors such as construction and hostelry. To analyze this issue, the next section looks at the changes in worker flows.

5 Worker flows and job quality

So far, our analysis has focused on the impact of a higher minimum wage on employment growth. But in addition to a reduction in labor demand, a higher minimum wage can also induce other effects, such as changes in the relative demand for different types of workers or in the quality of jobs. The strength of our data is that we have monthly observations on the composition of the workforce of each establishment in our sample. This allows us to decompose the changes in employment as the difference between the inflow and outflow of workers and to examine how the increase in the minimum wage affected these flows.

To simplify the analysis, we consider annual flows. Inflows are computed as the sum of the active employees of an establishment in December 2019 who were not part of the active workforce of the same establishment in December 2018. Conversely, outflows are defined as the active employees of an establishment in December 2018 who are no longer employed in the same establishment one year later. Both the inflows and the outflows are normalised by the employment level of the establishment in December 2018 and therefore take values between 0 and 1.

Table 7 shows the results for our baseline specification when we use the annual flows as our outcome variables. Again, the point estimates indicate the percentage point change in the outcome variables in establishments with a 100% exposure relative to those with no exposure. In addition, taking the difference between the point estimates for inflows and outflows gives the point estimate for employment growth reported in Table 2.

²⁷Figure 8 and Figure 9 in the Appendix also report the estimated OWEs.

Table 7: The impact on worker flows

Variables	Baseline		Full time		Size above 50		Remaining 2019	
	Inflows (1)	Outflows (2)	Inflows (3)	Outflows (4)	Inflows (5)	Outflows (6)	Inflows (7)	Outflows (8)
Fraction affected	0.0205** (0.0091)	0.0653*** (0.0083)	0.0153 (0.0128)	0.0670*** (0.0083)	0.0645* (0.0383)	0.1208*** (0.0458)	0.0226** (0.0093)	0.0539*** (0.0069)
Observations	28,928	28,928	28,928	28,928	2,251	2,251	28,928	28,928
Controls	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parenthesis. The worker flows are constructed by comparing the composition of the active workforce of each establishment in December 2018 and December 2019. Inflows are defined as the sum of the individuals who are part of the workforce of an establishment in December 2019 and not in December 2018. Conversely, outflows are the sum of those who are part of the establishment's workforce in December 2018 and not in December 2019. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and all observations are weighed using the inverse sampling probability of the PET. The sample of establishments for regressions on inflows and outflows includes establishments with active workers in 2017 and 2018.

Source: Own elaboration using PET data.

We obtain a number of interesting findings. First, a comparison of Tables 2 and 7 shows that the negative effect on employment is driven by an increase in outflows. Indeed, the increase in outflows exceeds the decline in employment growth and the difference increases with exposure. This surprising finding is due to the fact that the higher minimum wage generated a simultaneous increase in both inflows and outflows. As a result, the increase in gross flows almost doubles the net change in employment growth (8.58 versus 4.5 percentage points). However, it should be noted that the effect on inflows is only statistically significant when we use flows based on headcounts (Column 2) rather than their full-time equivalents (Column 4). The second noteworthy finding is that we obtain large and significant effects for the establishments with more than 50 employees on inflows and outflows while the effect on employment in these establishments was shown to be insignificant. To corroborate these findings, Table 8 shows that the simultaneous increase in inflows and outflows is statistically significant for establishments with more than 5 and less than 250 employees. For the smallest establishments the effect on inflows is insignificant and very close to zero. This suggests that the minimum wage increase caused a net outflow from these small establishments to somewhat larger ones. Finally, for the firms who remained attached to the labor market, we obtain a below-average increase in outflows and an above-average increase in inflows.

In the next exercise, we disaggregated the changes in the inflows and outflows using either worker or job characteristics. The results are reported in Table 9. By construction the point estimates for each of the six decompositions add up to the total effect reported in the first rows. As expected, we

find that the negative effects on employment are concentrated among youth, women and low-skilled workers, whose outflows increase substantially in the aftermath of the minimum wage increase. The novel aspect of Table 9 are the reported changes in job quality. The higher minimum wage led to an increase in the destruction of temporary jobs and a drop in the creation of open-ended positions, causing a net deterioration in the quality of jobs. Furthermore, the drop in temporary jobs mainly affected full-time positions, which reinforces the decline in average job quality. These last two findings highlight the importance of taking into account changes in job quality, especially in the case of countries like Spain with a dual labor market and a high share of temporary jobs.

Table 8: Worker flows by establishment size

Variables	1-5		6-9		10-49		50-249		+250	
	Inflows (1)	Outflows (2)	Inflows (3)	Outflows (4)	Inflows (5)	Outflows (6)	Inflows (7)	Outflows (8)	Inflows (9)	Outflows (10)
Fraction affected	0.0086 (0.0102)	0.0563*** (0.0093)	0.0798*** (0.0290)	0.1058*** (0.0234)	0.0774*** (0.0214)	0.1308*** (0.0200)	0.0759* (0.0418)	0.1374*** (0.0478)	0.0121 (0.0762)	0.0685 (0.1066)
Observations	15,587	15,587	3,087	3,087	6,441	6,441	1,810	1,810	397	397
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The worker flows are constructed by comparing the composition of the active workforce of each establishment in December 2018 and December 2019. Inflows are defined as the sum of the individuals who are part of the workforce of an establishment in December 2019 and not in December 2018. Conversely, outflows are the sum of those who are part of the establishment's workforce in December 2018 and not in December 2019. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and all observations are weighted using the inverse sampling probability of the PET. The sample of establishments for regressions on inflows and outflows includes establishments with active workers in 2017 and 2018.

Source: Own elaboration using PET data.

Table 9: Worker flows and wage composition by sub-population groups

Group	Inflows	Outflows	Empl. growth	Wage growth
Total	.021**	.065***	-.045***	.114***
Age				
16-25	.032***	.056***	-.024***	.023***
26-35	.005	.02***	-.015**	.034***
36-44	-.012***	-.001	-.012**	.025***
45-54	-.003	-.006*	.004	.019***
+55	-.001	-.003	.002	.014***
Gender				
Male	.012**	.021***	-.01	.037***
Female	.009	.044***	-.035***	.077***
Nationality				
Spanish	.012	.044***	-.032***	.096***
Foreign	.008**	.022***	-.013***	.018***
Level of skill				
Low	.015**	.055***	-.04***	.071***
Medium	.016**	.025***	-.008	.054***
High	-.01***	-.014***	.003	-.011***
Type of working day				
Full-time	.022***	.07***	-.048***	.067***
Part-time	.002	-.001	.003	.047***
Type of contract				
Permanent	-.019***	.007	-.026***	.081***
Temporary	.022***	.041***	-.019**	.033***
Temporary - Full time	.03***	.053***	-.023***	.025***
Temporary - Part time	-.006	-.01**	.004	.008***

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. This table reports the coefficient estimated of regressing worker flows, employment growth and the growth rate of mean wages in December 2019 on the fraction affected and a set of controls. The outcome variables are measured relative to total employment as of December 2018, ensuring that the sum of all subcategories corresponds to the overall effect shown at the top of the table. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and PET's probability of selection weights observations. The sample of establishments for regressions on employment includes establishments that had workers in 2017 and 2018, while the sample for regressions on wages also had workers in 2019.

Source: Own elaboration using PET data.

6 Reallocation and aggregate effects

Our establishment-level employment results might overestimate the worker-level effects if some of the displaced workers find a job at other, less exposed establishments. Similarly, our employment measures do not take into account possible productivity gains arising from the reallocation of the displaced workers to better jobs, firms or sectors. Both issues can be addressed by assessing the

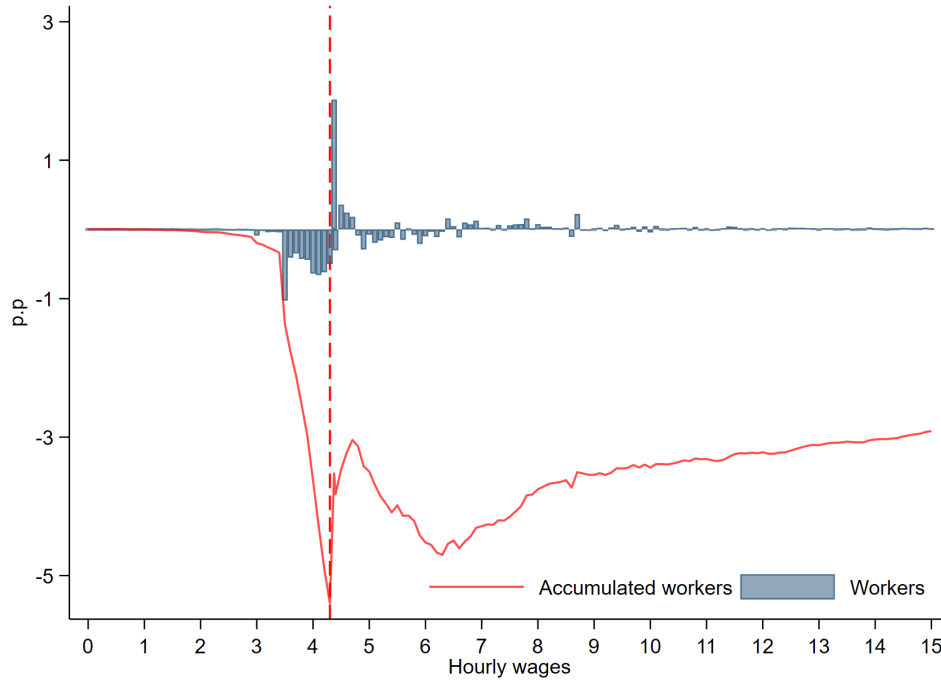
worker-level employment changes.

Bunching analysis

A simple procedure to gauge the worker-level effects of the minimum wage increase is to examine the shifts in the empirical wage distribution around the level of the new minimum wage. The aim is to assess the extent to which the drop in sub-minimum wage jobs is compensated by an increase in jobs at the new minimum wage or at slightly higher wages. The starting point of our analysis is the wage distribution of the individuals in our sample in December 2018. To obtain the corresponding economy-wide distribution, we assign a weight to each observation that is equal to the inverse of the sampling probability of the establishment where the individual is employed. We then repeat the process for the sample of active workers in December 2019 and calculate the difference between the two distributions.

Figure 7 shows the results. The dashed red line marks the level of the minimum wage in 2019, while the grey bars show the change in the number of jobs in each wage bin between December 2018 and December 2019. The latter are normalized by the level of total employment in December 2018. Both the missing sub-minimum wage jobs and the bunching of jobs just above the new minimum wage are clearly visible. To allow a comparison, the solid red line depicts the running sum of employment losses up to each wage bin. As can be seen, the increase in the number of jobs paying above the new minimum wage was clearly insufficient to compensate for the drop in jobs with subminimum wages. The latter account for more than 5% of total employment in December 2018, while the net effect depends on the cut-off used to measure the growth in employment at higher wages. The most conservative estimate points to a total job loss of more than 3%. By contrast, if we expand the analysis to include jobs with hourly wages up to 1 euro above the new minimum wage, we obtain a total loss of more than 4%. This estimate of total job losses is very close to our baseline estimate. Note that this wider window is not unreasonable, as the increase in the minimum wage may have produced spillovers on the wages of workers for whom the minimum wage increase was not binding.

Figure 7: The differences between the empirical wage distributions for 2018 and 2019



Notes: This figure shows the annual change in the distribution of workers from sampled establishments between December 2018 and December 2019. The vertical axis indicates the change in the number of workers in percentage point terms and the dashed red line indicates the 2019 minimum wage in hourly terms (4.375). Workers are weighted by PET's inverse probability of selection.
Sources: Own elaboration using PET data.

Reallocation

It is often argued that higher minimum wages can help improve labor productivity. One of the channels through which this could happen is through the mobility of displaced workers to better jobs or more productive firms or sectors. We do not have information on productivity, but our data do allow us to check whether the displaced workers in the most affected firms moved to better-paying sectors or whether they moved up the job ladder to a higher occupational group (*grupo de cotización*). In addition, we observe whether the displaced workers moved to establishments with more employees, which tend to be more productive than smaller establishments.

To study the reallocation process of the displaced workers, we use the complete working histories of all the individuals in our baseline sample. Notice that these histories include spells at establishments that are not part of the PET sample.²⁸ Our regression analysis uses the following variation on our

²⁸The PET does not report weights for non-sampled establishments, so our analysis relies on those from the establishments in which workers were initially employed. This can introduce some small differences with our results at the establishment level.

baseline specification:

$$y_j = \beta_0 + \beta_1 FA_j + X_j \delta + \epsilon_j \quad (2)$$

Where y_j denotes the individual's employment status in December 2019. The results are reported in Table 10. The first two columns report the effect of exposure on the probability that the individual is employed or remained with the same firm (*cuenta de cotización principal*). For employees in establishments with a 100% exposure in December 2018, these probabilities are, respectively, 5.1 and 5.0 percentage points lower than for individuals from non-exposed establishments (panel A). These probabilities are very similar to our results at the establishment level. The remaining three columns report the effects for those who do not remain with their initial employer on the probability of being employed in a larger establishment, a higher contribution group or a better-paying sector. The negative signs of all point estimates indicate no evidence that we find no evidence of positive reallocation effects. On the contrary, displaced permanent workers from highly exposed establishments are less likely to reallocate to larger establishments or to move to a higher contribution group than the employees of non-exposed establishments.

Table 10: Reallocation effects for workers in the initial sample of establishments

	Employed (1)	Same establishment (2)	Larger establishment (3)	Higher occupational group (4)	Better paying sector (5)
Panel A: Continuous form					
Fraction affected	-0.0511***	-0.0503***	-0.0088**	-0.0035	-0.0045*
Panel B: Discrete form					
Fraction affected (0, 18%]	0.0025**	0.0023*	-0.0107***	-0.0003	0.0034**
Fraction affected (18, 50%]	-0.0149***	-0.0145***	-0.0090***	-0.0036**	0.0011
Fraction affected (50, 100%]	-0.0401***	-0.0403***	-0.0089***	-0.0019	-0.0022
Panel C: Population subgroups					
Young workers	-0.0497***	-0.0495***	0.0021	-0.004	-0.0024
Low skill	-0.0484***	-0.0485***	-0.006	-0.005	-0.0019
Female	-0.0464***	-0.0445***	-0.0099*	-0.0074**	-0.0057*
Permanent	-0.0407***	-0.0396***	-0.0207***	-0.0065**	-0.0017
Temporary	-0.0608***	-0.0595***	-0.0009	-0.0017	-0.0066*
Full-time	-0.0592***	-0.0589***	-0.0081	-0.0052	-0.0053
Part-time	-0.0404***	-0.0389***	-0.0085	-0.0052	-0.0042

Notes: ***p<0.01, **p<0.05, *p<0.10. Model (1) reports the effects of the minimum wage incidence on the probability of remaining employed in December 2019, conditional on being employed in December 2018 for one of the establishments from the sample. Model (2) reports the effects on the probability of working for the same firm in December 2019 than in December 2018. Model (3) reports the probability of working for an establishment with a higher number of workers in December 2019 than in December 2018. Model (4) reports the probability of working under a higher contribution group (proxy for skill profiles) in December 2019 than in December 2018, conditional on not being employed for the same firm. Model (5) reports the probability of working in a different sector (2-digit CNAE) with higher average earnings in December 2019 than in December 2018, conditional on not being employed for the same firm. Standard errors in parenthesis. All specifications further include the following information related to worker's establishment in 2018: establishment age in September 2018, the percentage of temporary workers in 2017, province fixed effects and 3-digit economic sector fixed effects. Standard errors are clustered at the stratum level and PET's probability of selection weights observations.
Source: Own elaboration using PET data.

Aggregate effects

In this section, we approximate the aggregate effects of the minimum wage using a simple back-of-the-envelope calculation.

One of the advantages of using data at the establishment-level rather than at the individual level is

that they reflect all the relevant channels that influence the evolution of employment in incumbent firms. Notice that these channels include the possible implications of spillover effects of minimum wage increases on workers for whom the increase is not binding. Furthermore, we find no evidence of a significant impact of the higher minimum wage on firm entry (See Appendix C).

As the PET is designed to be representative at the stratum level, the best approximation is to multiply our baseline employment effect (-4.5 percentage points) by the weighted mean exposure of the affected establishments in the sample (53.6%). This leads to a mean effect of -2.25 percentage points in the employment growth of affected establishments. Moreover, if we multiply this point estimate by the share of workers accounted for the affected establishments (41.2%), the corresponding reduction in aggregate employment growth is -0.99 percentage points.

The second approximation is to use a simple unweighted average. In particular, assuming that the affected workers are evenly spread over all establishments, the aggregate effect would be equal to the product of the share of affected workers (12%), and our baseline estimate for employment (-4.5 percentage points). This leads to a mean reduction in overall employment growth of -0.54 percentage points.

Our proxies for the aggregate effects give rise to employment elasticities in the range of -0.025 and -0.045, while the employment elasticity of the directly affected establishments is -0.10.^{29,30} Finally, it is important to notice that our calculations exclude the positive effects of higher wage growth on consumption. The numbers presented in this section therefore provide upperbounds for the actual reduction in employment growth.

7 Conclusions

In this paper we study the impact of an unanticipated hike in the Spanish minimum wage using a novel matched employer-employee dataset. We find evidence of a significant increase in wage growth and a moderated reduction in employment growth at the establishment level that are proportional to the share of directly affected workers. The negative effects on employment growth are concentrated in small establishments with up to five employees, but we find that the minimum wage increase had a much broader impact through a simultaneous increase in inflows and outflows and a reduction

²⁹Our employment elasticities are very similar to the range of elasticities of $[-0.03, -0.05]$ reported in Barcelo et al., 2021. Their comparison shows that our estimates fall within the range of estimated elasticities of $[-0.018, -0.048]$ reported in the meta-analysis of Belman and Wolfson (2014), although it is not always clear whether the elasticities correspond to aggregate employment or the workers who are directly affected by the minimum wage.

³⁰The employment elasticities are estimated as -0.0054/0.22, -0.01/0.22 and 0.0225/0.22, where 0.22 corresponds to the minimum wage increase in 2019.

in job quality. To the best of our knowledge, we are the first to document a positive relationship between the minimum wage and inflows at firms affected by the minimum wage. We attribute this result and the comparatively high own-wage elasticity to the dual structure of the Spanish labor market.

Future research on minimum wages should help to clarify the quantitative importance of changes in worker flows and the quality of jobs in less segmented labor markets.

The Spanish 2019 hike was also the first of a sequence of minimum wage increases, but it would be inappropriate to extrapolate our results to more recent increases for two reasons. First, there are some indications that minimum wages over and above 60% of the median wage may have non-linear effects (Dube, 2019). Second, the bunching of workers at wages near the minimum wage may introduce an element of downward wage rigidity that amplifies the cyclical fluctuations in unemployment. We need more evidence on the impact of the minimum wage for relatively high values of the Kaitz ratio.

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A Data cleaning

The Panel de Empresa-Trabajador (PET) is a matched employer-employee dataset, elaborated by the social security administration in Spain.³¹ This dataset offers information for a stratified random sample of establishments – so-called *cuentas de cotización secundarias* – and that is representative of the establishments within each of the strata.³²

Initially, the PET sample comprises 69,845 establishments and 2,997,247 workers.³³

We begin the cleaning process at the worker level. Given that the minimum legal working age is 16, we exclude all individuals younger than 16. This step removes 5,453 workers, resulting in a dataset with 2,991,800 workers and 69,704 establishments.

Next, to focus on the effects of the 2019 minimum wage increase, we exclude job spells that began in 2020 or ended before 2017. We also remove observations with inconsistent dates (e.g., end dates preceding start dates) and retain only records corresponding to the second Tuesday of each month. After these filters, the dataset contains 69,684 establishments and 2,465,772 workers.

To identify minimum wage workers, we first construct an hourly wage measure using the contribution base,³⁴ the number of days worked in the month, and the partiality coefficient from the affiliation records.³⁵ Workers are then classified as minimum wage earners if their estimated hourly wage falls below 4.375 €/hr.

In the next step, we construct a balanced panel of establishments with monthly observations. We exclude 16,935 establishments that had no employees in 2018, 2,527 establishments operating in the primary or public sector, and 5,927 establishments exhibiting extreme growth in employment or wages (above the 99th percentile).

The consolidated sample consists of 44,295 establishments for the main analysis, of which 27,322 establishments are part of the subsample covering the 2017-2019 period and 28,928 establishments for the 2017–2018 subsample.

³¹PET data is available upon request to the Spanish social security administration using this link.

³²The sample includes 3% of the establishments with less than 10 employees, 5% of the establishments with 10 to 49 employees, 8% of the establishments with 50 to 249 employees, 10% for establishments with 250 to 499 employees and 15% of the establishments with more than 500 employees. For more details, see this link.

³³These figures reflect the raw counts in the PET and are not weighted by their sampling strata.

³⁴The contribution base is used as a proxy for monthly earnings.

³⁵The partiality coefficient indicates the proportion of a full-time workday that the worker performs in a given job spell.

B Tables and figures

Heterogeneous effects

Table 11: Heterogeneous results by autonomous community

Variables	Wages	Employment
Fraction affected × Andalucía	0.1215*** (0.0119)	-0.0948*** (0.0242)
Fraction affected × Aragón	0.1086*** (0.0339)	0.1359* (0.0740)
Fraction affected × Asturias	0.0945*** (0.0332)	0.0234 (0.0416)
Fraction affected × Canarias	0.1142*** (0.0165)	-0.0737** (0.0308)
Fraction affected × Cantabria	0.2256*** (0.0510)	0.0526 (0.1009)
Fraction affected × Castilla y León	0.1189*** (0.0284)	0.0231 (0.0329)
Fraction affected × Castilla-la Mancha	0.1546*** (0.0283)	-0.0388 (0.0505)
Fraction affected × Cataluña	0.1050*** (0.0117)	-0.0360 (0.0241)
Fraction affected × Ceuta y Melilla	0.0904** (0.0403)	-0.0818 (0.3092)
Fraction affected × Comunidad Valenciana	0.1274*** (0.0146)	-0.0627** (0.0246)
Fraction affected × Extremadura	0.1468*** (0.0394)	-0.0757 (0.0602)
Fraction affected × Galicia	0.0855*** (0.0180)	-0.0483 (0.0393)
Fraction affected × Islas Baleares	0.1222*** (0.0277)	0.0195 (0.0541)
Fraction affected × La Rioja	0.0918 (0.0815)	-0.0909 (0.0933)
Fraction affected × Madrid	0.0935*** (0.0098)	-0.0206 (0.0221)
Fraction affected × Murcia	0.1474*** (0.0264)	0.0328 (0.0507)
Fraction affected × Navarra	0.0545* (0.0331)	-0.0736 (0.2582)
Fraction affected × País Vasco	0.1233*** (0.0328)	-0.0860* (0.0482)
Observations	27,322	28,928
Controls	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parenthesis. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017 and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and PET's probability of selection weights observations. The sample of establishments for regressions on employment includes establishments that had workers in 2017 and 2018, while the sample for regressions on wages also had workers in 2019.

Source: Own elaboration using PET data.

Table 12: Heterogeneous results by economic group sector

Variables	Wages	Employment
Fraction affected × Industry	0.1566*** (0.0205)	-0.0056 (0.0352)
Fraction affected × Construction	0.1198*** (0.0270)	-0.0947** (0.0474)
Fraction affected × Retail	0.1165*** (0.0097)	-0.0844*** (0.0197)
Fraction affected × Transportation and storage	0.1155*** (0.0314)	-0.0617 (0.0314)
Fraction affected × Hostelry	0.1597*** (0.0249)	-0.0759* (0.0405)
Fraction affected × Communications	0.1614*** (0.0242)	-0.0339 (0.0647)
Fraction affected × Finance, insurance and real estate	0.0904*** (0.0150)	-0.0095 (0.0356)
Fraction affected × Scientific and technicals	0.1124*** (0.0186)	-0.0126 (0.0337)
Fraction affected × Administrative and auxiliary services	0.1104*** (0.0213)	0.0005 (0.0528)
Fraction affected × Education	0.0629** (0.0266)	-0.0292 (0.0651)
Fraction affected × Health	0.0990*** (0.0219)	-0.0340 (0.0365)
Fraction affected × Household employees, art and recreational activities	0.0976*** (0.0100)	-0.0174 (0.0199)
Observations	27,322	28,928
Controls	YES	YES

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017 and province fixed effects. Standard errors are clustered at the stratum level and PET's probability of selection weights observations. The sample of establishments for regressions on employment includes establishments that had workers in 2017 and 2018, while the sample for regressions on wages also had workers in 2019. Source: Own elaboration using PET data.

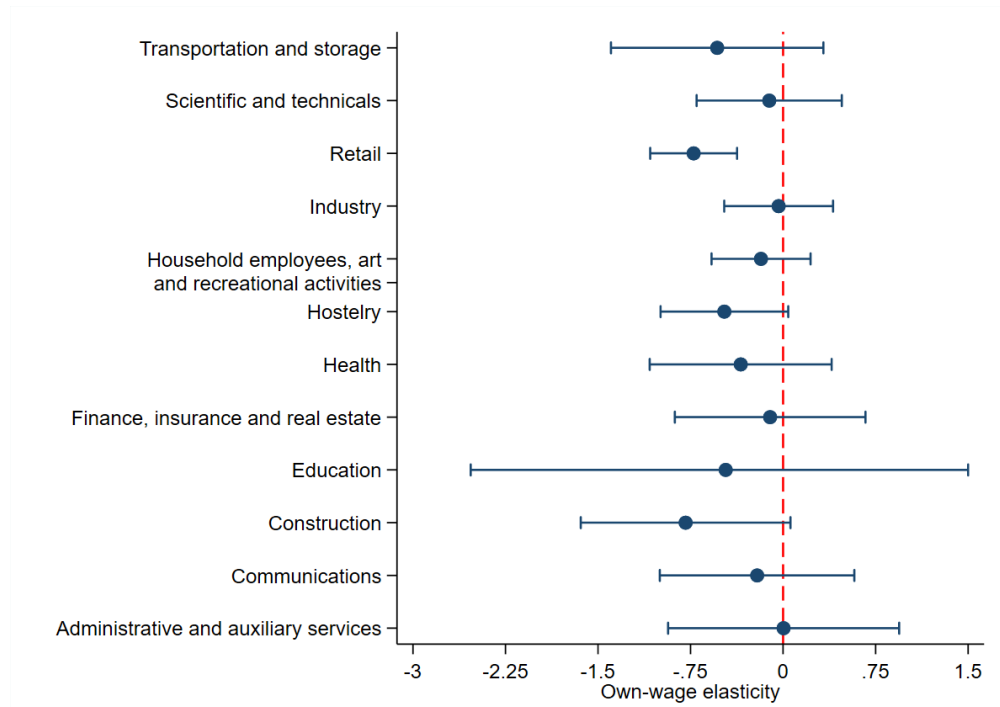
Table 13: Heterogeneous results by establishment size

Variables	Wages	Employment
Fraction affected \times 1–5	0.1142*** (0.0057)	–0.0394*** (0.0109)
Fraction affected \times 6–9	0.1230*** (0.0121)	–0.0526* (0.0301)
Fraction affected \times 10–49	0.1056*** (0.0116)	–0.1022*** (0.0261)
Fraction affected \times 50–249	0.0938*** (0.0259)	–0.0605 (0.0668)
Fraction affected \times +250	0.1517** (0.0589)	–0.1237 (0.0763)
Observations	27,322	28,928
Controls	YES	YES

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors in parenthesis. The controls include the legal form of the establishment, establishment age in September 2018, the share of temporary workers in 2017, province fixed effects and 3-digit sector fixed effects. Standard errors are clustered at the stratum level and PET’s probability of selection weights observations. The sample of establishments for regressions on employment includes establishments that had workers in 2017 and 2018, while the sample for regressions on wages also had workers in 2019.

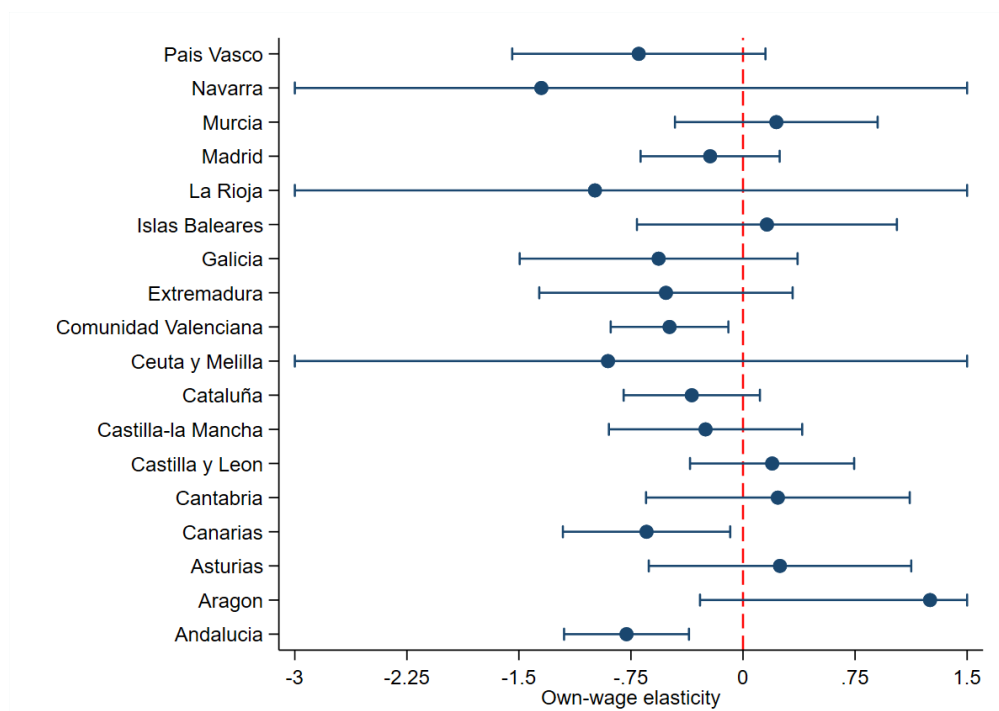
Source: Own elaboration using PET data.

Figure 8: Elasticities by economic sector



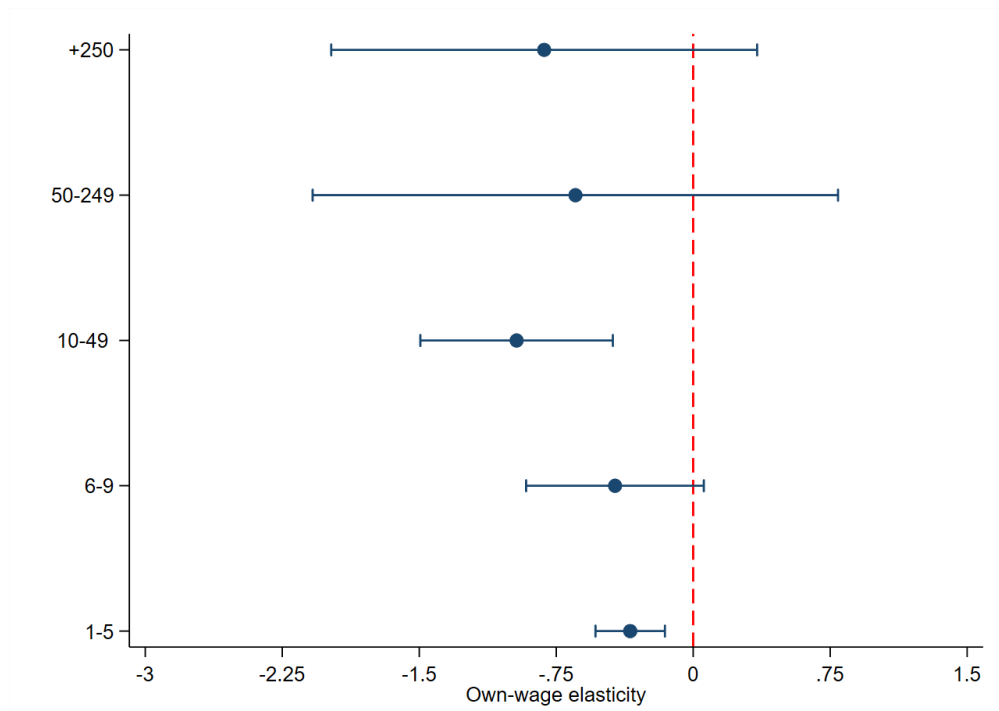
Notes: This figure shows the own-wage elasticity by 2-digit CNAE sector. We construct the 95% confidence intervals using the delta method and assuming independence of the employment and wage elasticities. Confidence intervals were also capped at -3 and +1.5 for visualization purposes. For more details, see Table 11 of the Appendix. Sources: Own elaboration using PET data.

Figure 9: Elasticities by region



Notes: This figure shows the own-wage elasticity by the autonomous community. We construct the 95% confidence intervals using the delta method and assuming independence of the employment and wage elasticities. Confidence intervals were also capped at -3 and +1.5 for visualization purposes. For more details, see Table 12 of the Appendix. Sources: Own elaboration using PET data.

Figure 10: Elasticities by establishment size in 2018



Notes: This figure shows the own-wage elasticity by firm size. We construct the 95% confidence intervals using the delta method and assuming independence of the employment and wage elasticities. For more details, see Table 13 of the Appendix.

Sources: Own elaboration using PET data.

Matching balance check

Table 14: Mean differences of the balanced sample of establishments

Variable	Treated	Control	P-value	Treated (before)
Share of temporary workers in 2017				
0%	23.1%	23.1%	1.00	19.7%
(0, 30%]	40.0%	40.0%	1.00	37.6%
(30, 60%]	21.6%	21.6%	1.00	24.4%
(60, 100%]	15.3%	15.3%	1.00	18.2%
Economic sector				
Administrative and auxiliary services	6.8%	6.8%	1.00	9.3%
Communications	2.1%	2.1%	1.00	2.4%
Construction	3.6%	3.6%	1.00	3.0%
Education	3.3%	3.3%	1.00	5.3%
Finance, insurance and real estate	3.8%	3.8%	1.00	3.8%
Health	5.4%	5.4%	1.00	7.6%
Hostelry	11.1%	11.1%	1.00	9.4%
Household employed, art and recreational activities	12.1%	12.1%	1.00	14.1%
Industry	9.7%	9.7%	1.00	8.5%
Retail	30.9%	30.9%	1.00	26.1%
Scientific and technicals	7.7%	7.7%	1.00	7.1%
Transportation and storage	3.4%	3.4%	1.00	3.5%
Age of establishment				
+20 years	13.2%	13.2%	1.00	13.5%
1-5 years	30.1%	30.1%	1.00	28.8%
11-20 years	33.5%	33.5%	1.00	33.3%
6-10 years	23.2%	23.2%	1.00	24.5%
Legal form of the establishment				
Cooperative	0.3%	0.3%	1.00	1.1%
Others	8.2%	8.2%	1.00	9.1%
Private Limited Company	63.7%	63.7%	1.00	58.6%
Public Limited Company	6.6%	6.6%	1.00	8.9%
Sole trader	21.2%	21.2%	1.00	22.2%
Autonomous community				
Andalucía	19.1%	19.1%	1.00	17.7%
Aragón	1.6%	1.6%	1.00	2.1%
Asturias	1.3%	1.3%	1.00	1.8%
Canarias	6.5%	6.5%	1.00	8.4%
Cantabria	0.6%	0.6%	1.00	1.0%
Castilla y León	2.0%	2.0%	1.00	2.7%
Castilla-la Mancha	2.4%	2.4%	1.00	3.1%
Cataluña	20.3%	20.3%	1.00	17.3%
Ceuta y Melilla	0.0%	0.0%	1.00	0.1%
Comunidad Valenciana	12.2%	12.2%	1.00	11.3%
Extremadura	0.9%	0.9%	1.00	1.7%
Galicia	4.7%	4.7%	1.00	5.2%
Islas Baleares	1.8%	1.8%	1.00	2.2%
La Rioja	0.2%	0.2%	1.00	0.4%
Madrid	20.1%	20.1%	1.00	17.3%
Murcia	3.6%	3.6%	1.00	4.4%
Navarra	0.4%	0.4%	1.00	0.5%
Pais Vasco	2.3%	2.3%	1.00	2.6%

Notes: This table presents the distribution of establishments in the matched sample used in columns (7) and (8) of Table 3. We implement Coarsened Exact Matching (CEM) to compare establishments with zero exposure (control group) to those with at least one affected worker (treated group). Columns "Treated", "Control", and "Treated (before)" report the distribution of establishments by characteristic, while the "P-value" column displays the results of statistical tests assessing whether differences between groups are statistically significant.

Source: Own elaboration using PET data.

C Firm entry

A key confounding factor is the effect of the minimum wage increase on establishment entry. If workers are laid off from firms that are highly impacted by the minimum wage but quickly secure employment at firms outside our fixed sample, we risk overstating the policy’s negative effects (Dube & Lindner, 2024). Conversely, entry rates could decline if increased labour costs deter the creation of new firms, especially in labour-intensive sectors with high shares of affected workers.

To assess the likelihood that entry rates affects the scope for worker reallocation, we constructed a panel dataset with the monthly count of active establishments in the Spanish Social Security system from 2017 to 2019 by sector (2-digit CNAE 2009), size and region. The data come from a register of the Ministry of Labor and Social Economy ³⁶ and are combined with data on mean exposure calculated in September 2018 using our PET sample. We then estimate the following linear regression:

$$\frac{y_{it} - y_{it-12}}{y_{it-12}} = \beta_0 + \beta_1 FA_i \times \mathbf{1}(t \geq 2019m1) + \gamma_t + \gamma_i + \epsilon_{it} \quad (3)$$

Where i stands for the unit of analysis and t for the monthly date. y is the number of active establishments and FA the mean exposure of establishments in September 2018. We further cluster the standard errors at the unit level of analysis.

The results are presented in Table 15. We do not find evidence of significant changes in entry rates in highly exposed sectors or by firm size following the 2019 minimum wage increase, but there is weak evidence of a small decrease in entry rates at the regional level. This is consistent with a null (or broader) impact of the policy on entry rates, posing no threat to our identification strategy.

³⁶Monthly statistics of active establishments in the Social Security System, available here.

Table 15: Analysis of establishments entry rates

Variables	Sector (1)	Firm size (2)	Region (3)
Fraction affected	0.0152 (0.0282)	-0.0443 (0.1126)	-0.0311* (0.0171)
Observations	288	144	432

Notes: ***p<0.01, **p<0.05, *p<0.10. Standard errors in parenthesis and clustered at the relevant level of exposure (sector, firm size or region). Each column reports results from a linear regression of the annual change in the firm entry rate on the average share of workers affected, controlling for cross-sectional and time fixed effects. The sample varies according to the unit of analysis: economic sector (2-digit CNAE), firm size (1–5, 6–9, 10–49, 50–249, 250–500, and 500+ employees), and region (CCAA).

Sources: Own elaboration using data from the Ministry of Labor and Social Economy, and PET.