# The Interaction between Minimum Wages and Collective Bargaining: Explaining Wage Spillovers*

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**Abstract**

This paper studies the interaction between minimum wages and collective bargaining, focusing on two minimum wage hikes of 6.9% and 21.5% that happened in Spain in 2017 and 2019 respectively. Leveraging geographical-sector variation in the bite of the minimum wage, we find wage spillovers until percentile 80th of the wage distribution, and positive but insignificant effects on employment. Using newly digitized data of collective agreements, we show that wage spillovers are driven by agreements with wage levels below the new minimum wage. The evidence suggests that collective bargaining systems with extended coverage help transmit mw changes further up the wage distribution.

***Keywords:*** Minimum wages, collective bargaining, wage spillovers

***JEL classification:*** J31, J41, J50

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# 1 Introduction

Minimum wages and collective agreements are arguably the two most important wage-setting institutions. Both set wage floors. The former is normally established by governments and applies to all workers in a specific territory, whereas the latter is the result of the negotiations between trade unions and employers, and sets minimum wages for each worker occupation or category, be it at the sector or company level. Given that both institutions establish the lowest wage a specific worker can earn, they are likely to interact. For instance, can collective agreements transmit the wage increases mandated by the minimum wage to the wages of non-affected workers further up in the wage distribution? In other words, do collective agreements propagate minimum wage changes to workers that earn higher wages? We lack precise answers to such questions because of the scarcity of data on wage scales, and because the literature has studied each institution separately (Engbom and Moser, 2022; Cengiz et al., 2019; Card and Cardoso, 2022).

In this paper we aim to fill this gap by analyzing how minimum wages and collective bargaining agreements determine wages jointly. To do so, we combine quasi-experimental variation in minimum wages in Spain with a new dataset of the wage scales and wage increases approved in the collective agreements of the Spanish labor market. This information is hard to access because it is scattered in many different files, each with its own format and characteristics. Nevertheless, equipped with modern web-scraping and optical character recognition techniques, we have put together two main new datasets: one with more than 20,000 wage floors and one with approved wage growth in each unit of negotiation. Furthermore, we use administrative data of the Spanish labor market to document the impact of the minimum wage on employment and wages, on top of the effects on wage floors and wage increases.

Spain offers a unique setting to study the interaction between minimum wages and collective bargaining. First, because the minimum wage was raised consecutively in 2017, 2018 and 2019 by 6.9%, 2.8% and 21.5% in real terms, respectively. Second, there is no automatic tranmission, since collective wage agreements are not indexed by default to the minimum wage. Third, it is a country with a high level of collective bargaining coverage. About 80.8%

of its workers have it wage set by a collective agreement, a similar number to that of the Netherlands (79.3%) or Denmark (83.1%), smaller than that of Italy (100%) or France (98%), and higher than in Germany (56%). These values stand in contrast to that of countries like the USA or the UK, where coverage is much lower and around 12% and 26.3%, respectively. A rather low level to expect an interaction between the two wage-setting institutions.

We begin by exploring the wage and employment effects of the minimum wage increases. We group observations at the province and 3-digit sector codes, and exploit variation in the exposure to the minimum wage across 8520 province-sector groups. To calculate the bite of the minimum wage, we quantify the percent wage increase necessary to bring all workers in a group up to the minimum wage, a measure commonly known as the wage gap in the literature (Dustmann et al., 2022). The empirical strategy is a difference-in-differences. Hence, we compare province-sector groups with a higher incidence of the new minimum wage, with those with a smaller incidence. For all outcome variables, we show the event-study estimates to confirm that more and less exposed province-sector groups were following parallel trends before the minimum wage changes.

We find that wages increase, while there is no evidence of negative effects on employment. Specifically, for a 1 percentage point (pp) increase in the gap measure, average wages increase significanly by 0.8%, and employment increases, not significantly, by 0.375%. These numbers translate into an own-wage employment elasticity of 0.468, with a standard error of 0.507. The 95% confidence interval discards elasticities more negative than -0.526. These point estimate and confidence interval are similar to recent evidence of the effects of the minimum wage in the US (Cengiz et al., 2019).

Next, we repeat the same strategy and estimate the impact on each decile of the wage distribution. We detect positive effects for all deciles, and they are significant up to the 80th percentile. The estimates are larger for the lower deciles, and decline as we move up the wage distribution, though they do not completely fade out. In particular, a 1pp increase in the gap, increases the median wage by 0.59%, and the 90th wage percentile by 0.39%. These wage spillovers are most similar to those found in Brazil over a period of 22 years (Engbom and Moser, 2022). However, there are two differences worth highlighting. The impacts we quantify happen in the short-run, and the spillovers in the upper tail of the distribution do not peter out as much as the estimates in Engbom and Moser (2022). In contrast, the wage spillovers are larger than those found for the US or the UK (Cengiz et al., 2019; Autor et al., 2016; Dickens et al., 2012). Given the high level of collective bargaining coverage in Spain, relative to the US labor market, a plausible hypothesis is that minimum wages trigger wage renegotiation in collective agreements, causing wage increases well above the area of workers directly affected by the minimum wage. In other words, minimum wages may be an anchor for the wage scales, and when the anchor moves, the whole wage structure moves along (Card and Krueger, 2016).

We then turn to the new dataset of collective agreements that we have collected to understand both how the wage floors and wage increases signed in each agreement react to the minimum wage. For that, we follow a similar strategy as that used for wages and employment, and compare agreements that had wage floors bitten by the new minimum wage (treatment), with those not exposed to it (control). We plot the wage floor distributions for treatment and control agreements and compare their evolution before and after each minimum wage increase. The visual evidence shows that the wage scales of bitten agreements shift right after a minimum wage change, while that of control agreements is very similar across periods. The rightward movement of the distribution is more pronounced after 2019, when the largest minimum wage increase took place. Using a difference-in-difference specification, we measure that wage floors for affected agreements increase by 2% on average. Moreover, the effect is very similar across the whole distribution of wage floors, suggesting that relative pay concerns are an important factor driving the movement of the wage scales (Dube et al., 2019).

For robustness purposes, we provide additional evidence using the frequency distribution approach, as in Cengiz et al. (2019); Gopalan et al. (2021); Giupponi et al. (2022). This method has the advantage that it not only estimates wage and employment effects, but also wage spillovers. Since there is not a neighboring Spain that we can use for control, we build a counterfactual using data from the near past and implement a time-shifted difference-in-differences. The estimates confirm the results from the wage gap strategy. There is large and significant wage growth in the area directly affected by the minimum wage change, the employment effects are either positive or quite limited when the sign is negative. Last, there are ripple effects on wages throughout the distribution, and they are more important for groups more affected by the minimum wage increase.

To summarize, we use exogenous variation in minimum wages and newly collected data on wage floors to show that minimum wages and wage scales interact, and that the latter transmit partially the minimum wage increases to the rest of the distribution. These results contribute to several strands of the literature. First, there is a large literature that studies how minimum wages and unions affect employment, wages, prices, profits, productivity, etc. (Card and Kruger, 1994; Freeman and Medoff, 1984; Cengiz et al., 2019; DiNardo et al., 1996; Harasztosi and Lindner, 2019; Dustmann et al., 2022; Engbom and Moser, 2022; Farber et al., 2021; Fortin et al., 2021). A subset of this literature has documented the wage spillovers of minimum wage policy (Cengiz et al., 2019; Autor et al., 2016; Dickens et al., 2012; Lee, 1999; Engbom and Moser, 2022). Relative to these papers, we show that minimum wages increase the wages of affected workers. Moreover, we detect ripple effects of the minimum wage much larger than what has been reported. Additionally, they do not fade away. Most importantly, thanks to the new wage floor dataset, we contribute with evidence of the main mechanism driving these spillovers: after a minimum wage increase, wage floors adjust upwards with effects of similar size throughout the distribution. Last, and despite the larger scope of the wage increases, the evidence shows that negative employment effects are, at most, very moderate, as is the case for most of the minimum wage literature (Dube, 2019).

Second, there are several hypotheses for the existence of spillovers. Among them, firm pay policies and workers reallocation (Engbom and Moser, 2022), labor-labor substitution Teulings (2000), hierarchical matching (de Melo, 2012), hedonic compensation (Phelan, 2019), and educational investment (Bárány, 2016). In general, in models of frictional labor markets, wage spillovers fade away because workers and firms in the upper tail of the wage distribution operate in different labor markets (van den Berg and Ridder, 1998; Engbom and Moser, 2022). An alternative explanation is that non-competitive factors such as preserving internal wage structures can generate ripple effects of the minimum wage (Card and Krueger, 2016). The evidence in this paper is consistent with this mechanism. Moreover, even though workers and firms in the upper tail of the wage distribution search in labor markets that are different than those in the lower end, they are connected to each other through the wage scales, particularly when there is a certain degree of centralization in collective bargaining, as is the case with the agreements at the province-sector level or in large companies. Therefore, in such institutional settings, the spillover effects are felt throughout the distribution. In that sense, wage scales institutionalize fairness concerns (Dube et al., 2019).

Finally, we highlight the relevance of taking into account the interactions between labor market institutions. For example, Gudgeon et al. (2023) show that the unemployment effects of unemployment insurance depend on retirement policies. Closer to this paper, Farber et al. (2021) show that without institutional changes at the US federal level, grassroots union activity might not translate into high union density or coverage. Similarly, Fortin et al. (2021) show that deunionization magnifies the role of declinining minimum wages in explaining increases in inequality. Relative to these contributions, the results in this paper show how the institutional setting might alter substantially the effects of the minimum wage, and suggest that its effects might only be felt at the bottom of the wage distribution in countries with laws that do not favor high levels of union coverage.

The rest of the paper is organized as follows. Section 2 presents the datasets we use and the institutional setting. Section 3 reports the wage and employment effects. It also documents how the wage scales in collective agreements are updated following a minimum wage increase. Section 4 contains robustness exercises of the main results. Finally, Section 5 summarizes the conclusions.

# 2 Data and Wage-Setting Institutions

## 2.1 Data

We combine two administrative datasets to explore the effects of wage-setting institutions on the labor market: the *Muestra Continua de Vidas Laborales* (MCVL) and the *Registro de Convenios Colectivos* (RCC).[1] We provide a general overview of the data here.

The MCVL has individual level data on the working history of a representative sample of workers. The sample contains 4% of all individuals who had some relationship with the social

[1]In English, the dataset names would be Continuous Sample of Work Lives and Register of Collective Agreements, respectively.

security system, that is, people who were working in the formal labor market, were collecting unemployment insurance or unemployment assistance, or were receiving pension benefits.[2] For each individual included, the data contains all information on her social security history.

The *Registro de Convenios Colectivos* has all collective agreements signed in Spain since October 2010. Even though the information is available online, it is not easily accessible, because it cannot be downloaded directly. We build two datasets from its records: one with the agreed wage increases that apply to all workers, and another one with the wage floors for each worker category. On the one hand, we web-scrapped the wage increases together with many other collective agreement variables such as year, firm, sector, dates of begining and end of the agrement. On the other hand, most wage floors are not directly available online, and can only be found in the pdf files of the agreements. Thus, we downloaded all of them, and used both OCR technology and human labor to put them in a format suitable for analysis. The wage floor dataset contains more than 20,000 floors, that come from 747 agreements of the period 2016-2019. Around 34% are sector-wide contracts. 36% are for companies of national scope. The remaining 30% are firm agreements of provincial scope.[3] The agreements cover around 2.5 million workers in 2016, or 14.4% of the workforce.

## 2.2 Wage-Setting Institutions: Minimum Wage and Collective Bargaining

**Minimum Wage Policy.-** MWs in Spain are set at the national level.[4] Every 1st of January, the MW might be revised. In Figure 1, Panel (a), we display the evolution of the real MW since 2008. As can be seen, it remained quite stable between 2008 and 2016. Furthermore, this trend was significantly reversed in 2017, when the first MW increase after many years was approved. In real terms, the increases were of 6.9%, 2.8% and 21.5% in 2017, 2018, and 2019, respectively. During the years we analyze (until December 2019), the increases were

[2]Sampling was random, without any kind of stratification.

[3]In terms of number of agreements, the dataset is similar to the data on Portuguese collective agreements analyzed in Card and Cardoso (2022). In their case, the dataset is based on 988 agreements.

[4]Other countries where no geographic variation in minimum wage applies are France, Germany, Greece, Hungary, Ireland, Israel, the Netherlands, New Zealand, Poland and the United Kingdom.

permanent. Moreover, they were also perceived as permanent. First, because inflation was low and it did not erode the MW. Second, because in 2018 a new left government came to power with a clear commitment to increase the MW.

Table 1 provides summary statistics in December of 2016, 2017 and 2018. That is, a month before changes in the wage-setting institutions happen. We split the sample between workers below the MW that was instituted the following month, and those above it. In 2016 and 2017, around 12% of workers in Spain earned below the MW. In 2018, this number had increased to around 16%. For all the years in table 1, workers in the area affected by the MW increase are relatively more likely to be female, immigrant, younger, and less educated, than the rest of the workforce. Regarding their working conditions, they are also more likely to be employed in the agricultural sector or work as housekeepers, and less likely to be employed in the industry or service sector, when compared to the rest of the workforce.

**Collective Bargaining.-** The first main characteristic of the collective bargaining system in Spain is its high coverage. As can be seen in Figure 1, it was 80.8% in 2016. France, Italy, Sweden and 5 more countries had higher coverage levels. In contrast, the US and the UK, for which most MW causal evaluation work has been done, had a coverage of 12% and 26.3%.[5]

There are two main reasons for the high coverage of collective bargaining in Spain. First, the existence of mechanisms of automatic extension to all workers within the functional/geographical scope of the collective agreements. The second is the predominance of sector and province level bargaining, despite the attempts to decentralize it during the Great Recession. In particular, a reform in 2012 allowed company level agreements to opt-out of sector ones. However, the effects of the policy change were limited, mostly because of the employers' support for the sectoral bargaining structures (Molina, 2023; Bulfone and Afonso, 2020). Firms value this system because it limits industrial conflict, reduces transaction costs related to wage-bargaining and prevents firms from being undercut by rivals offering lower wages (Bulfone and Afonso, 2020). For instance, for establishments with at least 10 employees, only 38% have a recognized body for employee representation. Hence, most firms are

[5]See Cengiz et al. (2019) and Giupponi et al. (2022) for examples for each country.

likely to follow sector agreements.[6]

Hence, the Spanish system of collective agreements has several layers. Negotiation can be at the sector-, company or establishment-level. All can apply nationally or at the province level. Sector-province and sector-national are the most influential. In any case, and to be comprehensive, the dataset we have assembled has both sector and company agreements, with either national or local scope.

In terms of wage-setting, collective agreements in Spain are similar to those in other European countries and specify a set of wage floors for different occupation groups. They also define idiosyncratic wage premiums on top of the floors, or wage cushions (Card and Cardoso, 2022). Additionally, the agreements also determine the yearly wage growth, for all workers regardless of their category. To put it in other words, wage floors determine the structure of wages, or the wage distribution, while wage increases influence the evolution of this wage structure.

Table 2 summarizes some of the main features of the wage floor dataset. On average, an agreement has 16 floors, with a minimum of 3 and a maximum of 70. As can be seen in the table, the floors are located throughout the wage distribution, though they are concentrated below the median wage. For example, the first quartile of wage floor is at the 20th percentile of the wage distribution, the median wage floor is at percentile 33rd, and the third quartile is at percentile 54th, slightly above the median wage. The highest wage floor is at the 97th percentile of the wage distribution. Taking into account these numbers, it seems plausible that if the minimum wage increases and bites the lowest wage floors of a collective agreement, the whole wage scale updates and the effects are felt until the upper tail of the wage distribution.

Figure 2 plots the distribution of wage floors for the years 2015 to 2018. To visualize how many wage floors are exposed to minimum wage increases each year, the vertical red dashed line is the minimum wage of the next year. 8% of the wage floors in 2016 were below the 2017 minimum wage. The number increased to 10% in 2017, and to 23% in 2018, the year before the largest increase. In the empirical section, we follow the intuition from these graphs and analyze if the agreements with floors affected by the changes in the minimum

[6]The estimate is from the European Company Survey 2019 performed by Eurofound.

wage update more, and with higher increases, their wage scales, relative to those not affected by it.

Last, Table 3 displays the average wage increase by negotiation unit. As can be seen, the mean approved wage growth is slightly higher at the sector level and affects many more workers. Finally, Table 4 shows wage increases by sector. It shows that these are agreements in all sectors, with household services and international organizations being the only two sectors for which collective bargaining is not very important.

# 3 Province-Sector Analysis: Wage and Employment Effects

To measure the impact of the minimum wage on wages and employment, we combine the time variation offered by the policy changes, with heterogeneity in the exposure to the minimum wage for each province-sector group. There are 52 provinces in Spain, and the data has 3 digit NACE codes. It allows us to identify 8520 province-sector groups of economic activity for which we have continuous information for the period 2014-19. For each group, we estimate the bite of the minimum wage using the gap measure that has often been used in the minimum wage literature (Dustmann et al., 2022). The equation is:

$$GAP_{gt} = \frac{\sum_{i \in g} d_{it} \max\left(0, MW - w_{it}\right)}{\sum_{i \in g} d_{it} w_{it}} \tag{1}$$

where $w_{it}$ refers to the daily wage of employee $i$ for each province-sector group $g$ in year $t$, $MW$ is daily the minimum wage. $d_{it}$ are the number of days worked in year $t$ earning $w_{it}$. The $GAP_{gt}$ multiplied by 100 is the percent wage increase that is necessary in each group to comply with the new minimum wage.

Table 5 reports descriptive statistics of the wage gap for each year before a minimum wage increase. Panel A is for 2016, Panel B is for 2017, Panel C is for 2018, and Panel D shows the average gap, $\overline{GAP}$, for these three years. The gap was, on average, 0.6% for the first increase, 0.4% for the second and smallest increase. The gap was the highest in 2018, before the largest increase, and equal to 1.6%, a level quite similar to the gap in Germany before the introduction of its minimum wage and studied in Dustmann et al. (2022).[7] The mean gap for the whole period was 0.9%.

For the province-sector groups with the 10% highest gap, the most common sectors are agriculture, manufacturing, and administration and auxiliary services -the sector that contains most outsourced workers-. In terms of provinces, the most common are Santa Cruz de Tenerife, Murcia, Córdoba and Cuenca, which are not at the top of economic performance in Spain. At the other end, for the group with a 0% gap, manufacturing is the most common sector (46%) and all regions are represented quite evenly in this group.

We relate the continuous gap measure of group $g$ to the log average wage, the log wage deciles and the log months of employment of each group. We estimate the combined effects for the increases since 2017. In addition, we do it separately for the 2017 and 2019 increases, respectively. For the first estimation, we use annual data and the following equation:

$$y_{gt} = \alpha + \delta_g + \delta_t + \sum_{j=-3, j\neq -1}^{3} \beta_j 1[j = t]\overline{GAP}_g + \sum_{j=-3, j\neq -1}^{3} \gamma_j 1[j = t] X_g + \epsilon_{gt} \tag{2}$$

where $y_{gt}$ denotes the outcome of interest. $\delta_g$ and $\delta_t$ are province-sector group and year fixed effects. $\overline{GAP}_g$ is the average gap for group $g$, and its the continuous treatment or "shock" variable. The coefficients $\beta_j$ capture its yearly effect on outcomes, relative to the pre-policy year. The specification also includes interactions of year dummies with controls at baseline, $X_g$. Specifically, we control for the province unemployment rate, a dummy for coastal provinces, and group averages of firm size; share of part-time, open-ended and short-term workers, and job duration. The regressions are weighted by the log number of workers of each group at baseline. Additionally to all the controls mentioned above, for wages we also detrend the data using province-sector specific linear trends, estimated using pre-policy years. We report the results with and without the detrending procedure.

The specification to analyze the 2017 and 2019 increases separately uses monthly data. The equation is:

$$y_{gm} = \alpha + \delta_g + \delta_m + \delta_c + \sum_{j=-12, j\neq -1}^{12} \beta_j 1[j = m] GAP_{g\tau} + \sum_{j=-12, j\neq -1}^{12} \gamma_j 1[j = m] X_g + \epsilon_{gm} \tag{3}$$

[7] In that case, the gap was 1.7% with a standard deviation of 1.

where the subindex $m$ refers to a specific month and $\delta_c$ are calendar-month fixed effects. $GAP_{g\tau}$ is the gap measure in year $\tau$, that is, 2017 or 2019, depending on the estimation. In this case, the control variables, $X_g$, are interacted with month dummies. Additionally, we include an interaction between each group $g$ and the calendar month to account for heterogenous seasonal effects. As in the previous specification, we weight by the log number of workers of each group at baseline. We also detrend the data using province-sector specific linear trends, estimated using pre-policy months and report the results with and without detrending the data.

The results for the year-to-year effects are in Figure 3. The top-left panel depicts the visual evidence of how wages for province-sector groups very exposed to the minimum wage increase evolve relative to wages in groups less exposed to it. We plot the coefficients $\beta_j$ from Equation 2 using the detrended log average wage of each group. The graphs without detrending the log average wage are in the online appendix, Figure A1. The figures show that the groups for which the minimum wage had a stronger bite were experiencing similar wage growth compared to those with a weaker bite, for the year before a minimum wage increase. However, wage growth was significantly higher in the groups more affected by the policy in 2017, the year of the first minimum wage raise. The effect was persistent in 2018 and becomes much larger in 2019, the year of the second and largest change.

The top-right graph displays the evolution of employment. As can be seen, the year estimates are similar for the years before and after each policy, suggesting the minimum wage had small effects on employment.

The middle and bottom figures show the effects on wages and employment of 2017 and 2019 minimum wage increases, respectively, using monthly data. The evidence is consistent with the yearly estimates: after each minimum wage change there are clear increases in the log average wage, whereas the employment estimates are similar for the months before and after each policy.

We provide estimates of the wage and employment effects of the minimum wage in Table 6. The specification with yearly data is:

$$y_{gt} = \alpha + \delta_g + \delta_t + \beta_1 Gap_g + \beta_2 Post_t + \beta_3 GAP_g \times Post_t + \epsilon_{gt} \tag{4}$$

And with monthly data:

$$y_{gm} = \alpha + \delta_g + \delta_m + \delta_c + \beta_1 Gap_g + \beta_2 Post_m + \beta_3 GAP_g \times Post_m + \epsilon_{gm} \tag{5}$$

The top panel shows the results for the combined effect of the two increases, and the middle and bottom panels contain the estimates for the 2017 and 2019 changes, respectively. For all cases, the estimates clearly indicate that the minimum wage increased wages. For a 1 percentage point increase in the gap measure, average wages increased by 0.8% (combined effect), 0.697% (2017) or 0.436% (2019). The third column shows the results for log employment (without detrending). As can be seen, we fail to estimate significant effects. The combined effect is positive (0.375), as is the case for the 2019 increase (0.196). Only the effect in 2017 is negative (-0.0781), though an order of magnitude smaller.

Column 5 provides estimates of the own-wage employment elasticities. For the estimation of the combined effects of both increases, the elasticity is 0.468 (standard error of 0.507). The confidence intervals rule out any own-wage elasticity more negative than -0.526. These numbers are very similar to those in Cengiz et al. (2019), who estimate an elasticity of 0.411 with a lower bound 95% confidence interval of -0.45. The 2017 and 2019 own-wage employment elasticities also discard negative and significant effects on employment: -0.112 for 2017 and 0.451 for 2019.

Next, we turn to investigate if the minimum wage had spillovers on wages higher up in the wage distribution. For that, we run Equation 2 but for each detrended log wage decile. The results are in Figure 4. The left column displays the event-studies for the log median wage. The right column plots the estimate of the effect for each decile. As can be seen in Panel (a), the median wage increased more in the groups more exposed to the minimum wage bite. The coefficient is positive and grows in 2017, though is not yet significantly different from zero. It is quite stable in 2018 and jumps up and becomes significant in 2019. Panel (b) shows that the effects were felt at each decile of the wage distribution. As expected, the effects were stronger for workers closer to the minimum wage. The size of the coefficients declines as we move up the wage ladder. However, it does not fade out and we detect significant effects up to percentile 80th, or almost significant at the 90th percentile. The figures in the middle and bottom panels display the impact across the distribution for the 2017 and 2019 changes. In general, the impact was more concentrated in the left-tail in 2017, and arrived further up in 2019, probably because the minimum wage increase in 2019 was much larger, but also because some time might need to pass by since the first minimum wage change for its effects to be transmitted to workers with higher salaries.

For a correct interpretation of the impact on the wage distribution, it is important to know to what extent the groups more exposed to the bite of the minimum wage are more likely to have a distribution with lower wage deciles than the groups less exposed to it. For instance, the 90th percentile for the 20% of groups with highest gap is a wage of 54.98 euros per day (those with a gap between 1.4-11.47%). For the next 20%, the 90th percentile is at 66.86 euros per day, and for the next 20% is at 75.1 euros per day.[8] For the groups with a gap of 0%, the 90th percentile is at 69.48 euros. Hence, the 90th percentile of the 20% of groups most affected by the minimum wage is quite different relative to less affected groups. Relative to the overall wage distribution, 54.98 euros per day is at percentile 62, and 66.86 is at percentile 66. Therefore, the estimates suggest the impact of increasing the minimum wage is felt well above the median wage.

We report the estimates at the 10th, 50th and 90th percentiles in Columns 6, 7 and 8 in Table 6. For a 1pp increase in the wage gap, we measure an increase of 3.2% for the bottom decile, 0.59% for the median wage, and 0.39% for the top decile. We extract three remarks from them. First, at the lower end of the distribution, wage increases are higher than those needed to comply with the new minimum wage and close the gap. We interpret this result through the lens of relative pay concerns Dube et al. (2019). The mandated increases, both in 2017 and 2019, were quite large. The distance in wages between the most and less affected workers was 8% in 2017 and 22.4% in 2019. Hence, if firms need to preserve the hierarchical ranking of workers, either because of the wage scales set in collective agreements, or to avoid an increase in quits from workers who could be felt to be treated unfairly, the wage increases will more than compensate the wage gap. Second, the spillovers we find are much larger than those found in Cengiz et al. (2019); Autor et al. (2016); Dickens et al. (2012), who detect effects up to the percentile 25th. They are more similar, but still larger, to those in Lee (1999). Very importantly, the estimates in this paper are unlikely to suffer from

[8] These are groups with a gap between 0.48-1.4% and 0.25-0.48%, respectively.

measurement error because we use administrative data (Autor et al., 2016).

Third, the spillover estimates in this paper are most similar to those in Engbom and Moser (2022), though they do not fade out as much. For instance, wage growth at the median is 18.4% of that of the bottom decile. This number is similar to that reported in Engbom and Moser (2022) for Brazil, who find that the minimum wage spillover at the median represents 21% of wage growth at the lowest decile. In contrast, we detect that growth at the top equals 12.2% of growth at the first decile, while Engbom and Moser (2022) find a 3.6%.

Why are there such differences in the spillovers captured in each paper? Why in our case the ripple effects do not peter out as much? The theoretical literature offers many explanations for the spillovers: labor-labor subtitution (Teulings, 2000), hierarchical matching (de Melo, 2012), educational investment (Bárány, 2016), hedonic compensation (Phelan, 2019), or firm pay policies and worker reallocation (Engbom and Moser, 2022). However, these papers are not very specific about how far the spillovers could go. In fact, in models of frictional labor markets the spillovers fade out because workers and firms in the upper end of the wage distribution are operating in different labor markets.

An alternative explanation is that the ripple effects are a result of non-competitive factors such as preserving internal wage structures (Card and Krueger, 2016). In that case, workers and firms in the upper tail of the wage distribution, even though they are acting in different labor markets, they are connected through the wage scales in sector and firm agreements. Moreover, minimum wages may be an anchor for these wage structures, and if the anchor moves, the whole structure moves along. With this idea in mind, we now turn to analyze the new data that we have assembled to check if we also detect the spillovers on the wages set in the collective agreements.

## 3.1 Effects on Wages Set in Collective Agreements

### 3.1.1 Wage Floors

We begin the exploration of wage floor renegotiation after a minimum wage increase by plotting the distributions of wage floors. The intuition we follow is similar to the identification strategy of the previous section. Collective agreements with wage floors that fall below the new minimum wage are pushed to negotiate higher wage floors for the affected occupations. However, raising the lowest wage floors is a tide that lifts all boats, or all wage floors, and the distribution of wage floors shifts to the right. The evidence is in Figures 5 and 6, which show the distributions for the period 2016-17 and 2018-19, respectively. The blue lines are for agreements that were exposed to the minimum wage, and the red lines for those that were not. The dashed lines are for the year after a minimum wage change, and the solid lines for the year before it. Panel (a) shows all agreements, and Panels (b), (c) and (d) display agreements at the sector level, for national companies and for the rest of companies, respectively.

As can be seen in Figure 5, Panel (a), the wage floor distribution for the control group is very similar in 2016 and 2017. In contrast, the wage floor distribution for affected agreements gains mass to the right of the new minimum wage (grey vertical dashed line). The other panels in Figure 5 display evidence in the same direction, disaggregated by type of agreement.

The results for the years 2018 and 2019 are even more clear-cut. Panel (a) in Figure 6 shows how the distribution of wage floors for affected agreements moves to the right. This happens both for sector and national company agreements, the contracts that cover more workers (recall Table 3, discussed in Section 2). For small companies, we see that the control group also moves right, which suggests that the renegotiation of sector agreements might have spillovers on small companies.

To put numbers to the preceeding discussion, we estimate the following equation:

$$y_{at} = \alpha + \delta_a + \delta_t + \beta_1 Treatment_a + \beta_2 Post_t + \beta_3 Treatment_a \times Post_t + \gamma X_{at} + \epsilon_{at} \quad (6)$$

where $y_{at}$ is either the log average wage floor or a log wage floor decile, in agreement $a$ at time $t$. $\delta_a$ and $\delta_t$ are agreement and year fixed effects. $Treatment_a$ is a dummy that indicates an agreement with wage floors that were bitten by a minimum wage increase. $Post_t$ equals 1 for years after a minimum wage increase. $X_{at}$ are time-varying controls. Specifically, we include interactions of sector and year dummies, province and year dummies, and how many years have passed since the first agreement was signed.

We report the results in Table 7. The first panel combines the 2017 and 2019 minimum wage increases. The middle and bottom panel are for each year separately. The first column is for all agreements, the second for sector agreements, the third for national companies and the fourth for all the other companies.

The estimates show that the average wage floor increased by a significant 1.9% more for affected agreements. The effect is mostly driven by sector and national company agreements. For the former, the impact is a 1.7% significant, and for the latter it is a larger, though insignificant, 4%. Looking at the panels for 2017 and 2019, we conclude that, even though there is evidence for changes starting in 2017, after the first minimum wage increase, the effect on wage floors is larger and significant after the change in 2019. This is not at all surprising, since this was the increase that bit the most wage floors (recall Figure 2, discussed in Section 2).

In Figure 7, we plot the coefficients when the endogenous variable is the log wage floor at each decile. There are two important insights in the graph. First, the minimum wage increase moves up the entire wage scale. Second, the effects are very similar across the wage scale, suggesting the relevance of relative pay concerns (Dube et al., 2019).

As we did with the results of the province-sector analysis for the distribution of wages, it is important to understand where is the 90th percentile wage floor for affected agreements in the distribution of all wage floors. The 90th percentile for treated agreements, in 2018, is at 54.76 daily euros. Relative to the overall wage floor distribution in 2018, this is the 82nd percentile. Hence, it confirms that the right tail of the distribution is also affected by the minimum wage increases.

All in all, we confirm the role that wage scales have in transmitting the effects of the minimum wage. This relationship has proven difficult to establish, mainly due to the lack of high quality micro data on wage scales. In fact, the empirical literature studying whether minimum wage increases have a spillover effect dates back to at least Grossman (1983). More recently, Engbom and Moser (2022); Cengiz et al. (2019); Autor et al. (2016) have documented spillovers of different sizes using modern causal econometric techniques. However, none of these papers link the ripple effects to the wage scales that unions and employers bargain over.

### 3.1.2 Wage Growth

We follow the same identification strategy and use Equation 6 to estimate if the agreements bitten by the minimum wage increase also sign higher wage increases, relative to those not directly affected by the minimum wage change. The endogenous variable is the percentage wage increase that is signed in the agreement and that applies to all workers, no matter what their actual category is.

The evidence is in Table 8. Panel A shows the effect when we pool together the 2017 and 2019 minimum wage increases. The effect is largely insignificant, suggesting that there was no systematic difference in the wage growth of agreements exposed to minimum wage changes and those not exposed to them. In fact, this result is consistent with the evidence from the real wage floor distributions. For instance, in Figures 5 and 6, the control distributions overlap almost perfectly.

This result, together with the fact that the real wage floor distributions for the control group overlap almost perfectly, indicates that wage increases are likely to follow the inflation rate. Hence, the signed wage increase that affects all workers is used to maintain the purchasing power, whereas wage floors reflect relative pay concerns within a sector or firm and, thus, react to changes in the minimum wage.

# 4 Robustness: Frequency Distribution Approach

In this section, we provide robustness checks of all the results we have documented so far. We follow the frequency distribution approach (Cengiz et al., 2019; Gopalan et al., 2021; Giupponi et al., 2022). This method has the advantage of conveying estimates for workers all along the wage distribution. Hence, it is useful to confirm the presence of ripple effects of the minimum wage further up the wage distribution, but also to obtain additional estimates of the employment and wage effects.

The key empirical challenge is to estimate how wage growth and employment would have been in the absence of the minimum wage increases. In Spain, as in many other countries, the minimum wage is a national policy. Therefore, there is no neighboring "Spain" that we can exploit for identification.

To circumvent this problem, we estimate counterfactual distributions using data from the close past, and implement a time-shifted difference-in-differences. More precisely, we construct the counterfactual with data from the period 2015 and 2016, the last two years with no significant changes in the minimum wage. Figure A4 helps build intuition for this method. In Panel (a) we show the distributions in December 2015 and January 2016. Between these two months, there was minimum wage increase, and the two distributions sit on top of each other. In contrast, Panels (b), (c) and (d) display the distribution across the months of December and January of 2016-17, 2017-18 and 2018-19, respectively, all years in which the minimum wage increased. In this case, the distributions do not overlap perfectly and many workers move to the new floor. To be concise, we focus our explanation on the change in 2018-19. Nevertheless, the comments for 2018-19 also hold for the other years with changes in the minimum wage. The figures suggest that the distributions in December 2018 and January 2019 would have been the same if there had been on minimum wage increase. In other words, that the distributions in 2018-2019 would have evolved following the dynamics of 2015-2016 if there had been no changes in the MW.

The descriptive evidence we just discussed suggests that we use the Spain of 2015-16 as the control group for the Spain of 2018-19 in a difference-in-differences framework.[9] Therefore, we define treatment as:

$$treatment_s = \begin{cases} 0, & \text{for the Spain of 2015 and 2016} \\ 1, & \text{for the Spain of 2018 and 2019} \end{cases} \tag{7}$$

We also need to define a $post_t$ period, that is, a time when a change in wage-setting institutions happens or could have happened (as in the case of year 2016 for control Spain. Thus, $post_t = 1$ for the 2019 year and $post_t = 0$ for the 2018 year. Similarly, for $treatment_0$, $post_t = 1$ for 2016 and $post_t = 0$ for 2015. Therefore:

$$post_t = \begin{cases} 0, & \text{for the years of 2015 and 2018} \\ 1, & \text{for the years of 2016 and 2019} \end{cases} \tag{8}$$

[9] To keep the explanation concise, we refer to the Spain of 2018-19 as that for which we want to construct a counterfactual. Nevertheless, what we say also applies to develop counterfactuals for the Spain of 2016-17 and 2017-18.

The specification is identical to that in Cengiz et al. (2019). The main difference is that the identification variation that we exploit comes from a Spain in the near past, and not from neighbouring counties. To estimate the effects on wages, we use individual data and a time-shifted difference-in-differences specification. Specifically:

$$y_{isjt} = \alpha + treatment_s + post_t + \sum_k \gamma_j \mathbb{1}[bin_j = k] + \sum_k \beta_j \mathbb{1}[bin_j = k] \times treatment_s \times post_t \\ + \delta_t + \delta_i + \rho X_{isjt} + \epsilon_{isjt} \tag{9}$$

where $y_{isjt}$ is the log wage of individual $i$, Spain $s$ (either treatment or control Spain), in bin $j$, at time $t$. $bin_j$ is a dummy that equals 1 for individuals with a wage at bin $j$ before the policy. $\delta_t$ and $\delta_i$ are month and individual fixed effects, and $X_{isjt}$ are time-varying covariates such as sector, contract type (short-term or open-ended), job duration, province, share of part-time hours, and the social security contribution group. The $\beta_j$ are the coefficients we are interested in and measure the wage or employment effect, for each bin $j$, of the wage-setting institutions. In addition to the bin-level estimates, we will also report the average effect for all workers, and the average below and above the new minimum wage.

To calculate the employment effects, we bin the workers in 1 euro bins of daily wage. The endogenous variable is the number of workers in each wage bin, relative to the average number of employed workers the year before the minimum wage increase. In this case, the specification is:

$$y_{sjt} = \alpha + treatment_s + post_t + \sum_i \gamma_j \mathbb{1}[bin_j = i] + \sum_i \beta_j \mathbb{1}[bin_j = i] \times treatment_s \times post_t \\ + \theta_m + \mu_{sj} + \rho_{jm} + \epsilon_{sjt} \tag{10}$$

where $y_{sjt}$ is the fraction of workers in bin $j$ and at month distance $t$ from a change in the wage-setting institutions. The subindex $s$ refers to either treatment or control Spain. $bin_j$ is the wage in bin $j$. $\theta_m$ are calendar-month fixed effects that capture seasonality, $\mu_{sj}$ are "state"-by-wage-bin fixed effects that control for employment shocks at the wage-bin level for the treatment and control Spain. $\rho_{jm}$ are calendar-month-by-wage-bin fixed effects that control for shocks at the wage-bin level that are common for each calendar-month across years. To put it in other words, they control for seasonality at the wage-bin level. $\epsilon_{sjt}$ is the error term.

For the case of employment, we use the $\beta_j$ to measure the missing and excess mass created by changes in the MW ($m_t$ and $e_t$, respectively).

$$m_t = \sum_{j<MW_t} \beta_j \qquad (11) \qquad\qquad e_{tJ} = \sum_{j \geq MW_t}^{J} \beta_j \qquad (12)$$

where $MW_t$ is the new minimum wage at time $t$. $J$ represents the highest wage bin for which we add the excess mass. The sums of the missing and excess mass are informative as to what extent the changes in the MW might destroy employment: $s_{tJ} = m_t + e_{tJ}$ and. In section X, we discuss the translation of these measures into employment elasticities.

Before concluding, it is worth discussing the identification assumptions of a cross-year difference-in-differences. In fact, they are equivalent to those of a standard difference-in-differences. First, parallel trends. We will show evidence consistent with it in the empirical section. Second, that in 2017, 2018 and 2019 there were no additional changes affecting wages and employment, other than the WSI, relative to the year 2016. In that sense, the persistency of the daily wage distributions over time, in the absence of changes in wage-setting institutions, is reassuring (see figures XXX). It suggests that jobs were created and destroyed proportionally to their share in the wage distribution, except when there were significant changes in the minimum wage.

## 4.1 Wage Effects

Each panel in figure A2 shows the differential wage growth that we measure in January 2017, 2018, and 2019. We plot coefficients for each euro bin along the wage distribution. The figures are shown in chronological order. The red-dashed line marks the position of the old minimum wage, whereas the first grey-dashed line that of the new minimum wage. The four black dot-dashed lines represent the 10th percentile, the median wage, and the percentiles 80 and 90.

The two graphs show that the minimum wage has a clear positive impact on wage growth of low-wage workers and that there are also smaller but significant wage increases for workers not directly affected by the MW (anyone above the first grey line). Specially in 2019, the year of the largest minimum wage increase. Thus, the figures confirm that the minimum wage has ripple effects in the wages of workers all along the distribution.

If we focus on the results for 2019, there are four points worth highlighting. First, the increase for workers at the old MW is the largest and equal to 19.57%. Second, for employees earning wages between the old and the new MW, the increases are smaller the closer they are to the new MW. Third, wage growth for workers at bins -3 to 0 places them at salaries above the new minimum wage. For instance, at bin -1 wages increase, on average, by 7.87%, whereas an increase of 3.45% was enough to comply with the policy. Therefore, the distribution of wages at the bottom is compressed by the policy and, at the same time, there is weak wage-rank preservation. This is expected if the wage scales in collective agreements are updated after a minimum wage increase, or if relative pay concerns matter (Dube et al., 2019). Fourth, there are wage spillovers well above the minimum wage. At the median (second black dot-dashed line), wage growth is significant at 1.3%, at percentile 80 (third grey-dashed line) is equal to a significant 1%, whereas the effect is no longer significant at percentile 90.

It is important to keep in mind that these wage estimates reflect the impact on wages for workers both in province-sector groups with a high and a low wage gap. Thus, it is not surprising we obtain more nuanced spillover estimates. For instance, at the median, wage growth is 6.6% of growth at percentile 10. To reconcile the spillover estimates of each method, we repeat the wage analysis of the frequency distribution approach splitting the sample into three groups, depending on their exposition to the minimum wage. The high gap are workers in the top 25% province-sector groups in terms of wage gap. The medium gap are those between the first and the third quartiles of wage gap. And the low gap are those in the bottom 25% province-sector groups in terms of wage gap. The results splitting the sample are in Figure A6 and Tables 11 and 10. In the figure, the left column displays results for the 2017 increase, and the right column for the 2019 policy change. The evidence is qualitatively similar for both years. To be concise, we focus our explanation in the outcomes of 2019. The figures are in ascending order of wage gap. Not surprisingly, the high gap group has a distribution with lower wages. For instance, the median daily wage is 33 euros, and the 90th percentile is at 59 euros. In contrast, for the groups with low gap, the median is at 67 euros, and percentile 90 is at 77 euros. We draw several conclusions from these graphs.

First, the spillovers at the median wage for the high gap workers are larger than those for low gap workers. For the former, they are 4% or 28.17% of wage growth below the minimum wage (Table 11, Panel C). In contrast, they are 1.4% for the latter, or 6.8% of the wage growth below the minimum wage for this group (Table 11, Panel A). At percentile 80 we still detect significant effects for the high gap group. They are 1.8%, or 12.68% of growth below the new minimum wage. In comparison, the effect at percentile 80 is not significant for the low gap group and close to zero. Thus, the bite of the minimum wage is a key driver of the wage spillovers, consistent with the results in Section 3.

Second, the figure for low gap workers also shows significant spillovers. They are larger for workers near the minimum wage. For instance, at percentile 10 wage growth is 2.1%. They are still significant at the median and equal to 1.4%. On average, wage growth above the new minimum wage is a significant 2%. Therefore, the evidence suggests the minimum wage has the largest ripple effects in sectors and geographical areas where it bites a sizable part of the bottom of the wage distribution. However, the results also show spillovers of lesser magnitude in less affected province-sector groups, indicating that most workers benefit from, at least, slightly higher wages after a minimum wage increase.

In figure **??**, we display the event studies of wage growth. The graphs on the left column are for workers affected by the MW increases. The evidence is based on XX months before and after each MW hike. As is visually evident, wage growth only happens the month of the policy change, with no further increases after it. Reassuringly for the empirical strategy, there is no significant estimate for any of the pre-treatment coefficients.

Now, we focus on the results for workers above the new MW. On average, we measure significant wage growth of 2.75% (0.36). The right column in figure **??** plots the event studies of wage growth for these employees. The dynamics of wage increases for these individuals are very similar to those of MW workers. They occur the same month the MW is updated, and there is no significant effect before or after it.

## 4.2 Employment Effects

Next, we estimate the effects on employment. Figure ?? shows the results from our baseline specification (equation 10) and plots the $\beta_j$ coefficients, that is, the changes in employment shares for each euro bin $j$. The graphs are displayed in chronological order. The red line is the sum of the negative and positive employment effects up to each bin ($s_{tJ}$ for all possible $J$). The dashed red lines are the confidence intervals of the cumulative sum. The top figure is for the 2017 change, the middle for 2018, and the bottom for 2019. In each graph, we report employment estimates $m_t$, $e_{t,w^*}$, $s_{t,w^*}$ averaged over the next 12 months after each policy change. $w^*$ is a wage bin in the higher part of the wage distribution for which it is visually evident that the employment effects have converged to a stable estimate. We reach several conclusions from this analysis.

First, for each change there is a significant drop in the number of jobs below the new minimum wage. It amounts to -5.5% and -9.6% for the 2017 and 2019 changes, respectively. Second, there is also a clear and significant increase in jobs just at the new minimum wage. Specifically, the excess mass represents 4.9% in 2017 and 5.5% in 2019. Third, there are smaller and positive employment effects above the minimum wage. They are more visible in 2019. Furthermore, and consistent with the wage spillovers, the cumulative sum shows many workers move up in the wage distribution. The cumulative sum stabilizes around wage bin XX, which is only a little bit above the highest wage floor in our dataset (NÚMERO). This is quite normal given that after a minimum wage increase, both wage floors and wage cushions might be updated. Since our data does not have information about wage cushions, by looking only into wage floors we are likely to underestimate the highest wage affected by the renegotiation of the wage scales. Fourth, the differences between the missing and excess mass, once the cumulative sum stabilizes, is positive in 2017 and equal to 1.1%. In 2019, it is negative and equivalent to -0.4%.

The own-wage employment elasticity in 2017 is positive and significant (0.584), whereas it is negative in 2019 (-0.106). The lower bounds of the 95% confidence interval for each year are 0.235 and -0.182. These numbers are similar to the estimates reviewed in Dube (2019) and are not statistically different at the 95% level than those obtained using the wage gap method.

Relative to recent papers using the frequency distribution approach to estimate the effects of the minimum wage on employment (Cengiz et al., 2019; Gopalan et al., 2021; Giupponi et al., 2022), we show that in contexts of high collective bargaining coverage it is important to understand if the interaction between the minimum wage changes and collective bargaining shifts the wage distribution rightwards. Not taking this shifting behavior into account might lead to overestimate any potential negative employment effect.

Next, in figure **??** we show the excess, missing, and sum mass, for the area under the MW, in an event study design for a few months before and after each change. We estimate $m_{t,MW}$, $e_{t,MW}$, and $s_{t,MW}$ separately for each month and we plot them relative to the estimate of the month just before treatment. We highlight several remarks from these figures. First, there is no indication of pretreatment trends. Therefore, these results provide a strong validation of the identification assumption of our approach. Namely, that the frequency distribution of daily wages in treatment and control Spain would have moved in parallel in the absence of the reforms. Second, the missing and excess mass become significant in the first month of treatment. The quick response shows that firms were complying with the minimum wage increases. Third, the difference between the excess and missing mass is significant for each case. Moreover, there is a common pattern across changes, though it is perhaps clearer for the 2018 and 2019 reforms. The employment destruction is more negative the month immediately after the change and shrinks the following month, suggesting that firms might first overreact. From then on, though, the negative effect on employment grows over time. For instance, it increased from -1.23% in February 2019 to -3.04% in December 2019. Similarly, in 2018 it grew from -0.49% to -1.63%. For the 2017 policy, though we can only measure it over an horizon of 5 months, it grew from 0.4% to -1.13%. The evidence is consistent with firms adjusting over time. However, the effects grow quite fast, suggesting that short-run estimates can be a good approximation of long-run effects. In particular, in contexts where the minimum wage increases are consecutive and are not eroded by inflation (Sorkin, 2015; Hurst et al., 2022). Moreover, in labor markets with high turnover, as is the case in Spain (Felgueroso et al., 2018), and a high incidence of short-term contracts, the adjustment of the workforce can happen quickly. We return to this point when we discuss the heterogeneity of the results and the evidence for flows.

# 5 Conclusions

In this paper, we have shown that minimum wages and collective bargaining interact. Increases in the former are transmitted to workers throughout the wage distribution through the renegotiation of higher wage floors. Furthermore, the employment results show either non-negative effects, or a quite small negative impact, as in most of the recent minimum wage literature (Dube, 2019).

The findings in this paper are for a institutional setting in which labor law promotes a high level of collective bargaining coverage. Further research could focus on understanding the transmission of minimum wages to non-affected workers by comparing firms with and without wage scales in contexts with lower levels of coverage. Moreover, the list of labor market institutions is long: unemployment insurance, employment protection, apprenticeship contracts, retirement, on-the-job training, short-term and open-ended contracts, etc. Shedding light on the interactions between them, and with minimum wages and collective bargaining looks like a fruitful avenue for future research.

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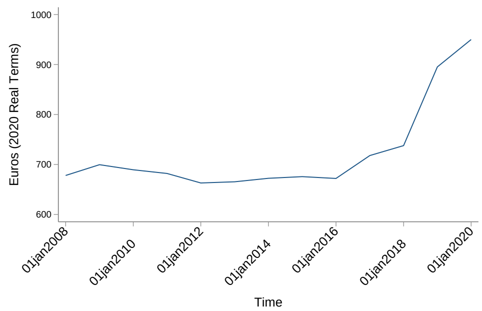
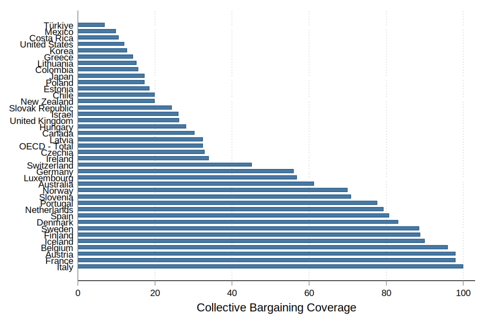
# A Figures

Figure 1: **Minimum Wage Evolution & Collective Bargaining Coverage**



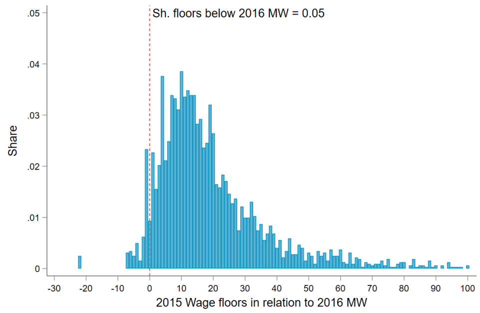

**(a) Minimum Wage Evolution**



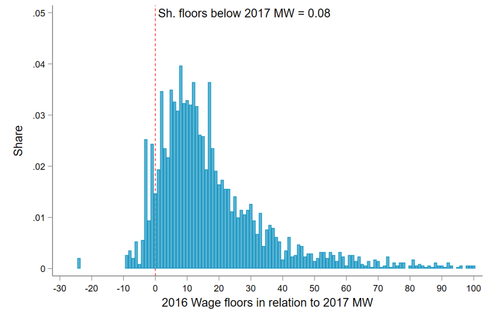

**(b) Collective Bargaining Coverage**

Notes: Panel (a) shows the evolution of the minimum wage between 2008 and 2020. Panel (b) displays the percentage of workers covered by collective agreements in OECD countries. Sources: OECD and authors' own calculations.

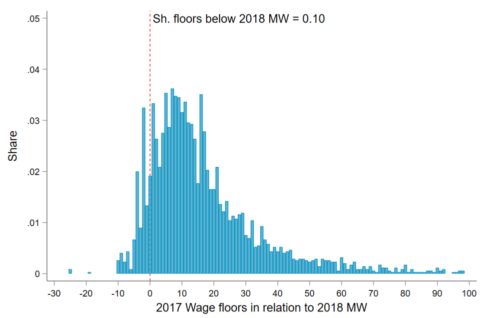
Figure 2: **Wage Floors and the Minimum Wage Bite**



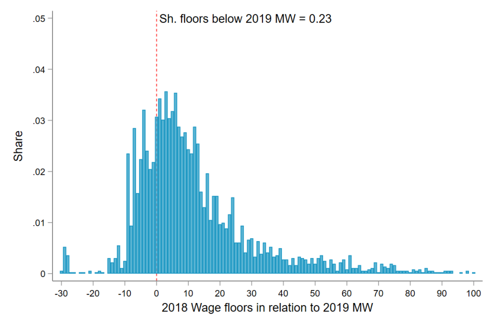

**(a)**




**(b)**



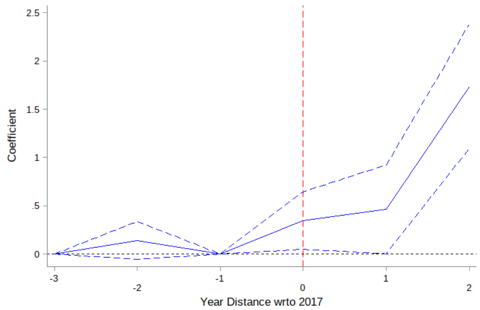

**(c)**



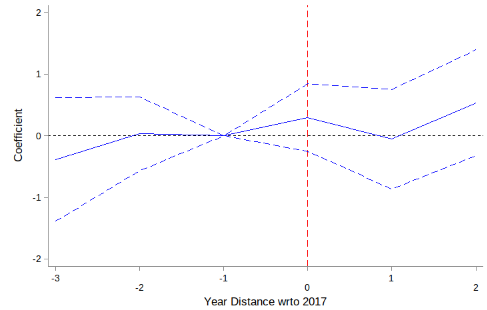

**(d)**

Notes: The figures are histograms representing the distribution of wage floors in 2016, 2017, 2018 and 2019. The horizontal axis display the values of the wage floors with respect to the minimum wage. The vertical red dashed lines represent the minimum wage of the following year. For each figure, we report the share of floors that fall below the minimum wage of the next year.

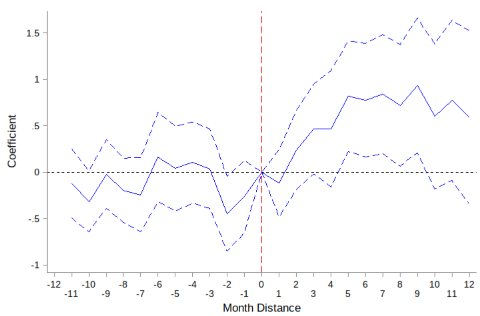
Figure 3:



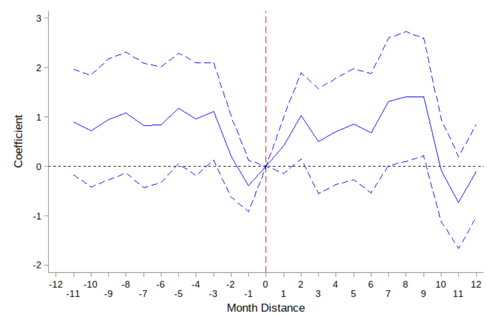

**(a) Log Wage**



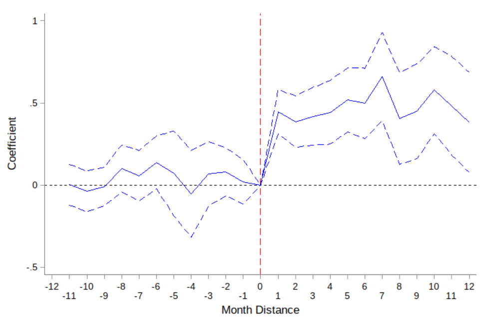

**(b) Log Employment**



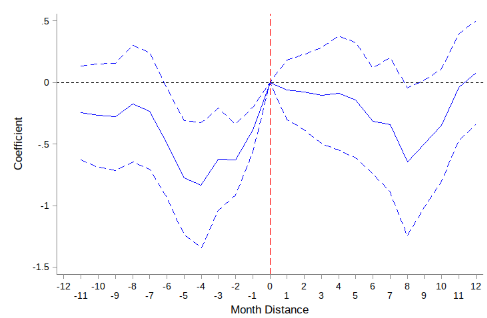

**(c) Log Wage**




**(d) Log Employment**



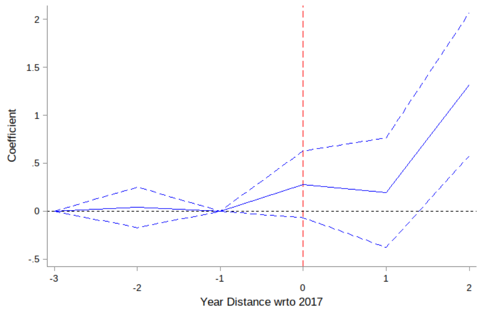

**(e) Log Wage**



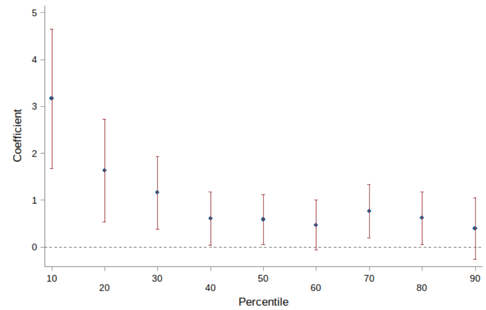

**(f) Log Employment**

Notes: The figures in the left column display the event-study estimates on log average wages. The figures on the right depict the effects on log employment. The top panels are for the year-to-year estimation (Equation 2). The middle and bottom panels show the results using monthly data and for the minimum wage change in 2017 and 2019, respectively (Equation 3). Regressions are weighted by log population at baseline. Controls include interactions of time dummies with the province unemployment rate, coastal dummies, and province-sector averages of firm size, part-time work, open-ended and short-term contracts, and job duration. Additionally, for wages we also detrend the data using province-sector linear trends estimated with pre-policy data. The blue solid lines are the coefficient estimates and the blue-dashed lines 95% confidence intervals of robust standard errors clustered at the province-sector level. The vertical dashed line depicts the moment of the reform.

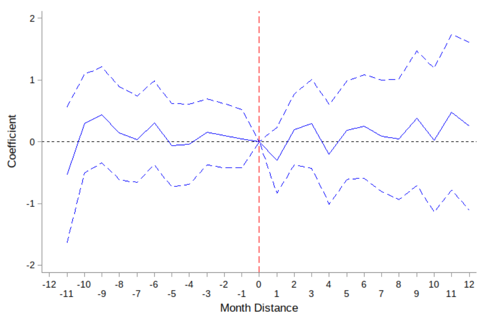
Figure 4:



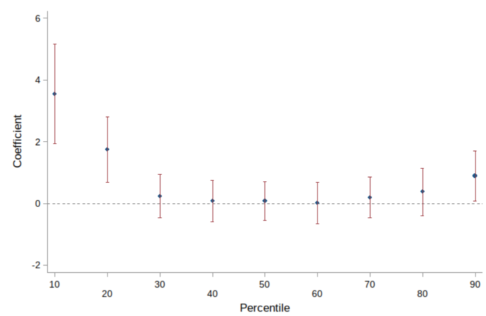

**(a) Log Median Wage**



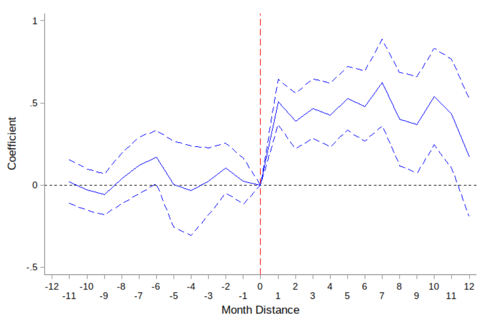

**(b) Wage Effects by Decile**



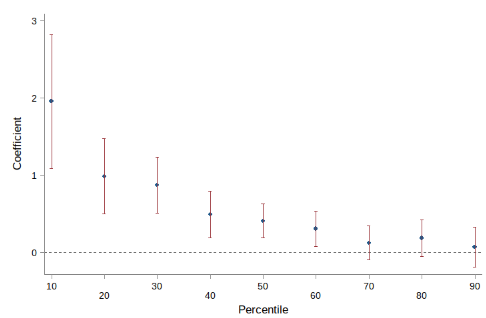

**(c) Log Median Wage**




**(d) Wage Effects by Decile**



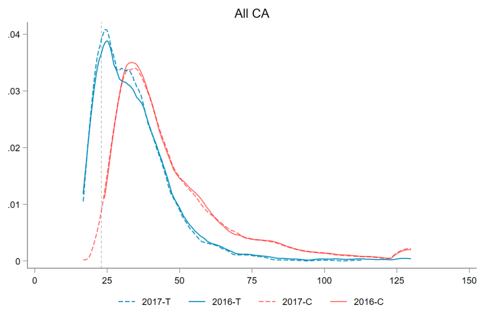

**(c) Log Median Wage**



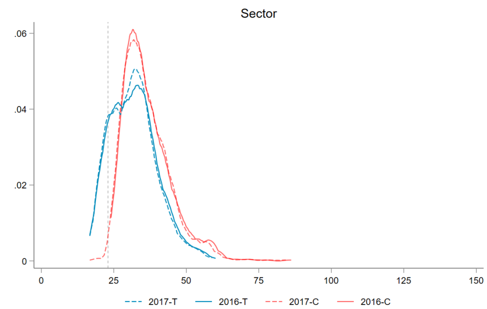

**(d) Wage Effects by Decile**

Notes: The figures in the left column display the event-study estimates on log median wages. The figures on the right depict the effects on the log of each wage decile. The top panels are for the year-to-year estimation (Equation 2). The middle and bottom panels show the results using monthly data and for the minimum wage change in 2017 and 2019, respectively (Equation 3). Regressions are weighted by log population at baseline. Controls include interactions of time dummies with the province unemployment rate, coastal dummies, and province-sector averages of firm size, part-time work, open-ended and short-term contracts, and job duration. Additionally, for wages we also detrend the data using province-sector linear trends estimated with pre-policy data. The blue solid lines are the coefficient estimates and the blue-dashed lines 95% confidence intervals of robust standard errors clustered at the province-sector level. The vertical dashed line depicts the moment of the reform.

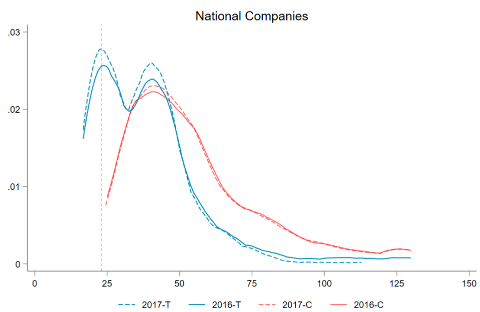
Figure 5: **Wage Floors Distribution: 2016-2017**



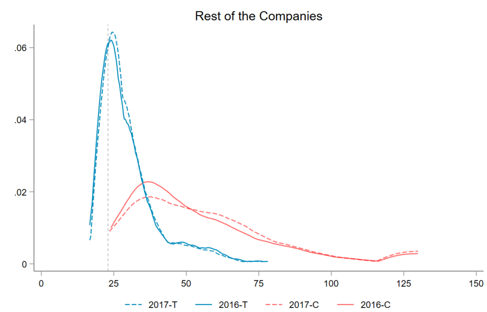

**(a) All Agreements**




**(b) Sector Agreements**



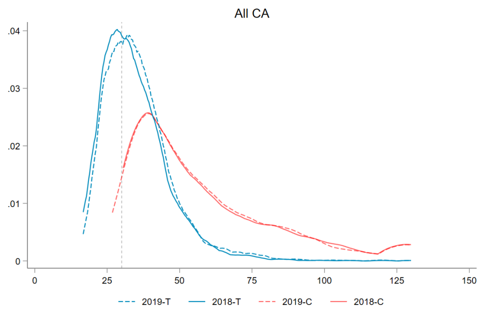

**(c) National Company Agreement**



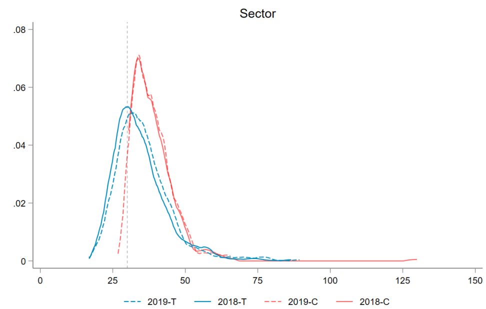

**(d) Other Companies Agreement**

Notes: The figures display the wage floor distributions for the years 2016 and 2017. The blue lines are agreements that were bitten by the minimum wage (treatment), and the red lines are agreements that were not (control). The solid lines are for the year before the increase (2016), and the dashed lines for the year after (2017). The vertical grey dashed line represent the minimum wage.

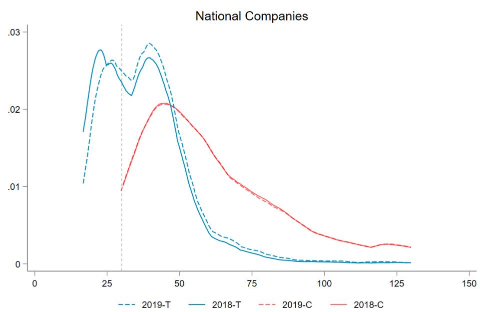
Figure 6: **Wage Floors Distribution: 2018-2019**




**(a) All Agreements**




**(b) Sector Agreements**

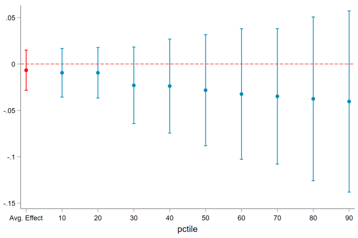



**(c) National Company Agreement**

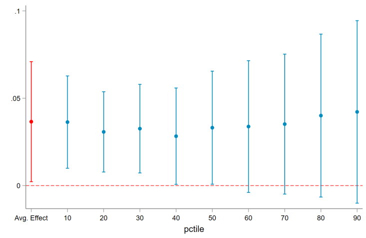

**(d) Other Companies Agreement**

Notes: The figures display the wage floor distributions for the years 2018 and 2019. The blue lines are agreements that were bitten by the minimum wage (treatment), and the red lines are agreements that were not (control). The solid lines are for the year before the increase (2018), and the dashed lines for the year after (2019). The vertical grey dashed line represent the minimum wage.

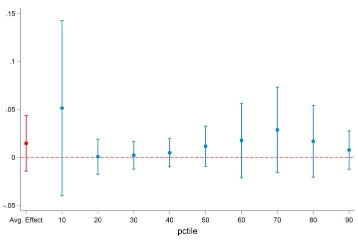
Figure 7: **Effects on Wage Floors when MW is Biniding: By Percentiles**



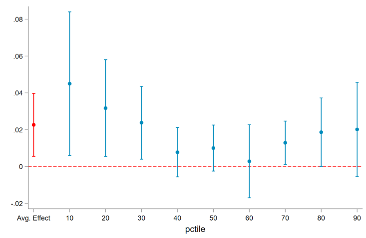

**(a) All Agreements (2017)**



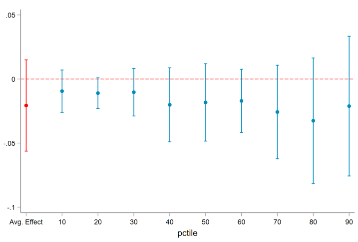

**(b) All Agreements (2019)**



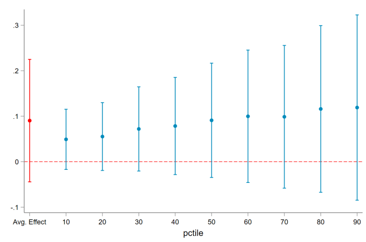

**(c) Sector Agreements (2017)**



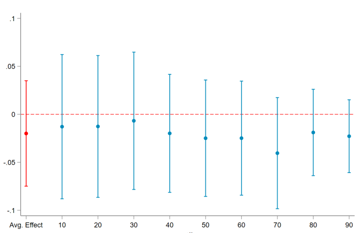

**(d) Sector Agreements (2019)**



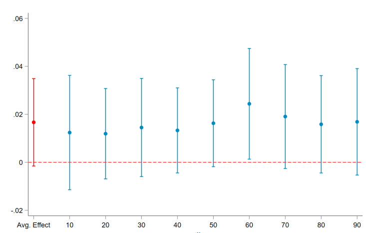

**(e) Large Company Agreements (2017)**




**(f) Large Company Agreements (2019)**



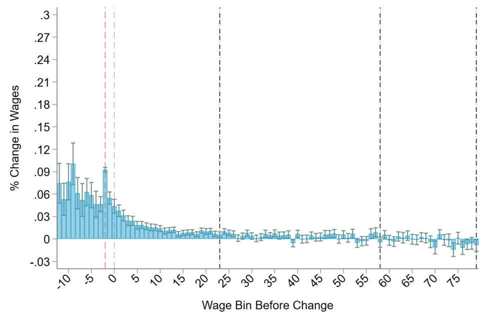

**(g) Rest of Companies Agreements (2017)**



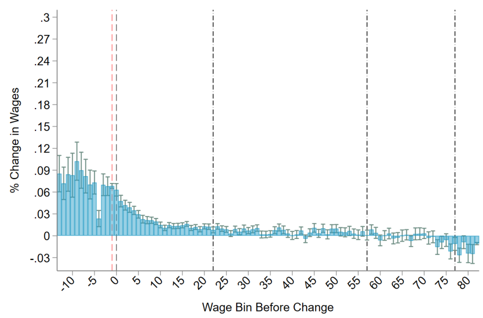

**(h) Rest of Companies Agreements (2019)**

Notes: The figures display the growth of wage floors at each percentile. The estimation is based on Equation 6. The blue dot are the point estimates, and the capped lines the 95% confidence intervals. Similarly, the red dot and red solid lines are the estimates for the average effect. The left column is for the minimum wage increase in 2017, and the right column for that of 2019. The panels depict the evidence for all collective agreements ((a) and (b)), sector agreements ((c) and (d)), large company agreements ((e) and (f)), and for the rest of companies ((g) and (h)).

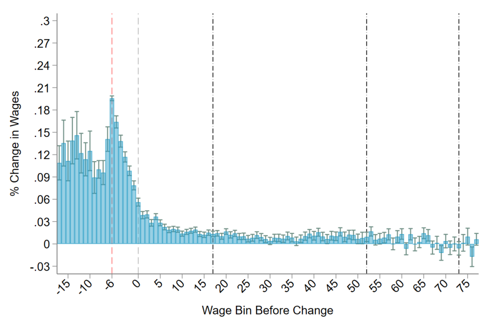
Figure 8: **Wage Effects of the Minimum Wage Increase**




**(a) 2017 MW Increase**



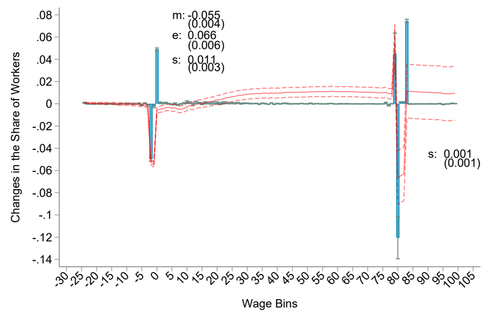

**(b) 2018 MW Increase**



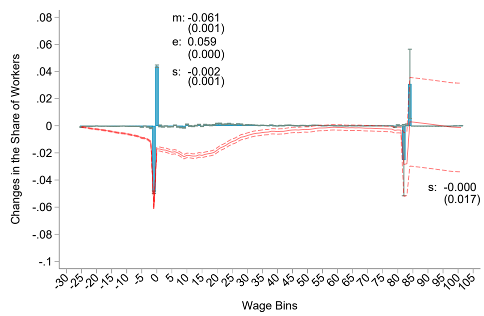

**(c) 2019 MW Increase**

Notes: The figures show the main results from the difference-in-difference specification (see equation 10) on wages. They display the wage change between December, the month before the minimum wage is updated, and January, the month after, for the 2017 (Panel a), the 2018 (Panel b) and the 2019 (Panel c) MW increases. The blue bars show for each euro bin, relative to the minimum wage, the estimated average change in wages in that bin. The error bars show the 95% confidence interval clustered at the individual level. The vertical dashed red line indicates the MW level before the increase and the vertical dashed gray line indicate the new MW level.

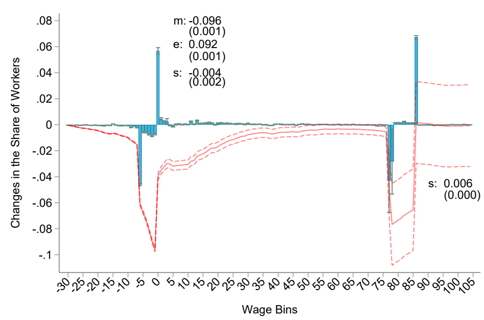
Figure 9: **Employment Effects of the Minimum Wage Increase**




## (a) 2017 MW Increase




## (b) 2018 MW Increase




## (c) 2019 MW Increase

Notes:This figure shows the main results from the difference-in-difference specification (see equation 10) on employment. Panel (a) and (b) show the results for the 2019 MW increase. Panel (c) and (d) show the results for the 2018 MW increase. Panel (e) and (f) show the results for the 2017 MW increase. The blue bars show for each euro bin, relative to the minimum wage, the estimated average employment changes in that bin during the twelve months after the minimum wage increase. The error bars show the 95% confidence interval clustered at month of the year level. The vertical dashed read line indicates the MW level before the increase and the vertical dashed gray lines indicate the new MW level and the new minimum contribution base for the highest contribution group respectively.

# B Tables

Table 1: Summary Statistics

| | December 2016 | | December 2017 | | December 2018 | |
|---|---|---|---|---|---|---|
| | Below 2017 MW | Rest of the Workers | Below 2018 MW | Rest of the Workers | Below 2019 MW | Rest of the Workers |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **A. Demographic Characteristics** | | | | | | |
| Female | 0.571 | 0.472 | 0.575 | 0.471 | 0.574 | 0.465 |
| Spanish Citizens | 0.771 | 0.917 | 0.772 | 0.913 | 0.774 | 0.913 |
| By age: | | | | | | |
| Share less than 24 | 0.023 | 0.010 | 0.023 | 0.012 | 0.014 | 0.006 |
| Share 24-44 | 0.363 | 0.453 | 0.337 | 0.442 | 0.322 | 0.411 |
| Share 45-65 | 0.394 | 0.411 | 0.389 | 0.391 | 0.358 | 0.404 |
| **B. Socioeconomic Characteristics** | | | | | | |
| By education: | | | | | | |
| Share less than HS | 0.347 | 0.137 | 0.343 | 0.137 | 0.317 | 0.133 |
| Completed HS | 0.543 | 0.565 | 0.546 | 0.566 | 0.562 | 0.564 |
| Some Higher Education | 0.081 | 0.264 | 0.082 | 0.262 | 0.092 | 0.267 |
| **C. Contract Characteristics** | | | | | | |
| Open Ended | 0.552 | 0.624 | 0.544 | 0.619 | 0.526 | 0.634 |
| Short-Term | 0.447 | 0.284 | 0.453 | 0.288 | 0.471 | 0.268 |
| Public Worker | 0.001 | 0.093 | 0.003 | 0.092 | 0.003 | 0.098 |
| Outsourced | | | | | | |
| Temp. agency | | | | | | |
| Full Time | 0.929 | 0.821 | 0.927 | 0.824 | 0.875 | 0.832 |
| Agriculture | 0.174 | 0.034 | 0.190 | 0.029 | 0.181 | 0.026 |
| Industry | 0.025 | 0.138 | 0.024 | 0.138 | 0.030 | 0.141 |
| Construction | 0.011 | 0.053 | 0.009 | 0.056 | 0.011 | 0.060 |
| Services | 0.534 | 0.782 | 0.527 | 0.783 | 0.602 | 0.778 |
| Housekeeping | 0.229 | 0.005 | 0.229 | 0.004 | 0.177 | 0.002 |
| Share of workers | 0.126 | 0.873 | 0.122 | 0.878 | 0.164 | 0.836 |

*Notes:* This table shows summary statistics for workers affected and not affected by a minimum wage increase.

Table 2: Summary Statistics for Wage Floors

| **Wage Floor Statistics** | **Value** | **Percentile in Wage Distribution** |
|---|---|---|
| **Min Number of Wage Floors** | 3 | - |
| **Mean Number of Wage Floors** | 15.98 (13.49) | - |
| **Max Number of Wage Floors** | 70 | - |
| **Min Wage Floor** | 17 | 4 |
| **First Quartile** | 30 | 20 |
| **Second Quartile** | 37 | 33 |
| **Third Quartile** | 48 | 54 |
| **Max Wage Floor** | 135 | 97 |
| **Mean Wage Floor** | 41.88 (18.15) | 41 |

Notes: This table shows descriptive statistics of wage floors for the year 2016, before any increase in the minimum wage happened. The first three rows show the minimum, average and maximum number of floors in the agreements. The next five rows display the values of wage floors across the distribution. The last row is the average wage floor. For each of these last six moments, the right column shows its percentile in the wage distribution.

Table 3: Summary Statistics for Wage Increases, by Negotiation Unit

| Negotiation Unit | Wage Increase | Duration (Years) | Affected Workers | Number of Agreements |
|---|---|---|---|---|
| Establishment | .39 | 5 | 119 | 1268 |
| Company | .32 | 6 | 262 | 3869 |
| Sector | .47 | 4 | 21048 | 1069 |

Notes: This table shows the average of wage increases, agreement duration, affected workers, and number of agreements by negotiation unit (establishment, company and sector). The statistics are for the year 2016, before any minimum wage increase happened.

Table 4: Summary Statistics for Wage Increases, by Sector

| Sector | Wage Increase | Duration (Years) | Affected Workers | Number of Agreements |
|---|---|---|---|---|
| Agriculture | .32 | 5 | 7617 | 128 |
| Mining | .32 | 4 | 27861 | 84 |
| Manufacturing | .44 | 4 | 1127 | 1464 |
| Energy | .32 | 5 | 839 | 40 |
| Water | .41 | 5 | 365 | 570 |
| Construction | .19 | 7 | 1780 | 176 |
| Trade | .47 | 5 | 3228 | 571 |
| Transport | .41 | 5 | 1287 | 628 |
| Hospitality | .29 | 6 | 7728 | 277 |
| Telecommunications | .19 | 6 | 716 | 164 |
| Finance | .24 | 5 | 7879 | 42 |
| Real Estate | .39 | 6 | 5417 | 14 |
| Science | .31 | 6 | 1686 | 205 |
| Administration | .27 | 6 | 1826 | 602 |
| Public Administration | .04 | 8 | 175 | 337 |
| Education | .11 | 6 | 5260 | 78 |
| Health | .6 | 5 | 1531 | 417 |
| Arts | .2 | 6 | 43838 | 233 |
| Other Services | .25 | 5 | 357 | 168 |
| Households | 0 | 11 | 328 | 2 |
| International Organizations | .17 | 9 | 74 | 6 |

Notes: This table shows the average of wage increases, agreement duration, affected workers, and number of agreements by sector. The statistics are for the year 2016, before any minimum wage increase happened.

Table 5: Wage Gap

| | Mean | St. Dev. | Min | Max | N |
|---|---|---|---|---|---|
| Wage Gap (2017) | 0.006 | 0.011 | 0.000 | 0.080 | 9348 |
| Wage Gap (2018) | 0.004 | 0.007 | 0.000 | 0.040 | 9285 |
| Wage Gap (2019) | 0.016 | 0.033 | 0.000 | 0.224 | 9586 |
| Wage Gap (2017-19 Average) | 0.009 | 0.016 | 0.000 | 0.115 | 8971 |

Notes: The table shows the wage gap for the minimum wage increases in 2017, 2018, 2019, and the average of the three. We calculate is following Equation 1.

Table 6: Wage, Employment and Elasticity Estimates

| | (1) Log Wage Detrended | (2) Log Wage Not Detrended | (3) Log Employment Not Detrended | (4) Elasticity Detrended | (5) Elasticity Not Detrended | (6) Log Wage 1st Decile | (7) Log Median Wage | (8) Log Wage 9th Decile |
|---|---|---|---|---|---|---|---|---|
| Panel A: Combined Effect of 2017 and 2019 Minimum Wage Increases | | | | | | | | |
| $Treatment \times Post$ | 0.802*** | 0.181 | 0.375 | 0.468 | 2.071 | 3.166*** | 0.585** | 0.393 |
| | (0.226) | (0.127) | (0.406) | (0.507) | (2.620) | (0.758) | (0.273) | (0.333) |
| Observations | 120174 | 120174 | 120174 | 120174 | 120174 | 119983 | 120171 | 120174 |
| Panel B: 2017 Minimum Wage Increase | | | | | | | | |
| $Treatment \times Post$ | 0.697*** | 1.144*** | -0.0781 | -0.112 | -0.0683 | 3.539*** | 0.0694 | 0.888** |
| | (0.0289) | (0.148) | (0.343) | (0.487) | (0.299) | (0.824) | (0.324) | (0.415) |
| Observations | 473352 | 473339 | 473352 | 473352 | 473339 | 472144 | 473260 | 473339 |
| Panel C: 2019 Minimum Wage Increase | | | | | | | | |
| $Treatment \times Post$ | 0.436*** | 0.576*** | 0.196 | 0.451 | 0.341 | 1.933*** | 0.412*** | 0.0736 |
| | (0.0106) | (0.0612) | (0.135) | (0.296) | (0.232) | (0.415) | (0.112) | (0.132) |
| Observations | 494664 | 494664 | 494664 | 494664 | 494664 | 494227 | 494660 | 494664 |

Notes: The table shows the wage, employment and elasticity estimates of the reforms. The top panel displays the estimates for the strategy that combines the 2017 and 2019 minimum wage increases (Equation 2). The middle and bottom panels show the results using monthly data and for the minimum wage change in 2017 and 2019, respectively (Equation 3). Regressions are weighted by log population at baseline. Controls include interactions of time dummies with the province unemployment rate, coastal dummies, and province-sector averages of firm size, part-time work, open-ended and short-term contracts, and job duration. Additionally, for wages we also detrend the data using province-sector linear trends estimated with pre-policy data. Robust standard errors clustered at the province-sector level are shown in parentheses. The standard errors for the elasticities were obtained using the delta method. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Effects on Wage Floors when MW is Binding

| | All CA | Sector Level | National Companies | Rest of the Companies |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | | | ***Panel A: All Years*** | |
| $T_i X Post_t$ | 0.019* | 0.017** | 0.040 | -0.002 |
| | (0.010) | (0.008) | (0.037) | (0.011) |
| Observations | 3795 | 1625 | 1436 | 734 |
| | | | ***Panel B: 2017*** | |
| $T_i X Post_t$ | -0.007 | 0.015 | -0.020 | 0.000 |
| | (0.011) | (0.014) | (0.016) | (0.019) |
| Observations | 1809 | 751 | 724 | 334 |
| | | | ***Panel D: 2019*** | |
| $T_i X Post_t$ | 0.037** | 0.023*** | 0.096 | 0.005 |
| | (0.017) | (0.009) | (0.066) | (0.005) |
| Observations | 1986 | 874 | 712 | 400 |
| Controls | Yes | Yes | Yes | Yes |
| Collective Agreement FE | Yes | Yes | Yes | Yes |

Notes: Robust standard errors clustered at the collective agreement level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Effects on Agreed Wage Growth

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | All CA | Sector Level | National Companies | Rest of the Companies |
| | | | ***Panel A: All Years*** | |
| $Treatment_i X Post_t$ | -0.164 | -0.239 | -0.0598 | 0.112 |
| | (0.119) | (0.195) | (0.140) | (0.250) |
| Observations | 3254 | 1849 | 904 | 501 |
| | | | ***Panel B: 2017*** | |
| $Treatment_i X Post_t$ | 0.0281 | 0.247 | -0.0904 | -0.488 |
| | (0.142) | (0.198) | (0.171) | (0.305) |
| Observations | 1541 | 867 | 482 | 192 |
| | | | ***Panel C: 2019*** | |
| $Treatment_i X Post_t$ | -0.343** | -0.609** | -0.253 | 0.325 |
| | (0.167) | (0.228) | (0.262) | (0.320) |
| Observations | 1713 | 982 | 422 | 309 |

Notes: Robust standard errors clustered at the agreement level are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Wage, Employment and Elasticity Estimates: Frequency Distribution Approach

| | (1) Log Wage All | (2) Log Wage Below MW | (3) Log Employment All | (4) Own-Wage Elasticity | (5) Log Wage 1st Decile | (6) Log Median Wage | (7) Log Wage 9th Decile |
|---|---|---|---|---|---|---|---|
| Panel A: 2017 Minimum Wage Increase | | | | | | | |
| *Treatment* × *Post* | 0.019*** | 0.079*** | 0.011*** | 0.584*** | 0.044*** | 0.006*** | -0.008** |
| | (0.0007) | (0.005) | (0.003) | 0.178 | (0.005) | (0.002) | (0.004) |
| Observations | 2248552 | 216878 | 2248552 | 2248552 | 15836 | 36500 | 96656 |
| Panel B: 2018 Minimum Wage Increase | | | | | | | |
| *Treatment* × *Post* | 0.018*** | 0.075*** | -0.002 | -0.088 | 0.068*** | 0.008*** | -0.027*** |
| | (0.0006) | (0.003) | (0.001) | (0.055) | (0.001) | (0.002) | (0.004) |
| Observations | 2248552 | 216878 | 2248552 | 2248552 | 15836 | 36500 | 96656 |
| Panel C: 2019 Minimum Wage Increase | | | | | | | |
| *Treatment* × *Post* | 0.039*** | 0.139*** | -0.004** | -0.106* | 0.196*** | 0.013*** | -0.006 |
| | (0.0005) | (0.001) | (0.002) | (0.039) | (0.001) | (0.001) | (0.005) |
| Observations | 2341141 | 342670 | 2341141 | 2341141 | 57138 | 41032 | 95623 |

Notes: The table shows the wage, employment and elasticity estimates of the reforms. The top panel displays the estimates for the strategy that combines the 2017 and 2019 minimum wage increases (Equation 2). The middle and bottom panels show the results using monthly data and for the minimum wage change in 2017 and 2019, respectively (Equation 3). Regressions are weighted by log population at baseline. Controls include interactions of time dummies with the province unemployment rate, coastal dummies, and province-sector averages of firm size, part-time work, open-ended and short-term contracts, and job duration. Additionally, for wages we also detrend the data using province-sector linear trends estimated with pre-policy data. Robust standard errors clustered at the province-sector level are shown in parentheses. The standard errors for the elasticities were obtained using the delta method. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 10: Wages Effects in 2017: Frequency Distribution Approach

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Log Wage All | Log Wage Below MW | Log Wage Above MW | Log Wage 1st Decile | Log Median Wage | Log Wage 8th | Log Wage 9th Decile |
| **Panel A: Low Level Wage Gap Bite** | | | | | | | |
| *Treatment* × *Post* | 0.006*** | -0.014 | 0.004**** | 0.009** | 0.004 | -0.012*** | 0.012*** |
| | (0.001) | (0.108) | (0.001) | (0.004) | (0.003) | (0.004) | (0.001) |
| Observations | 549514 | 4556 | 544958 | 6848 | 6752 | 55042 | 53810 |
| **Panel B: Mid Level Wage Gap Bite** | | | | | | | |
| *Treatment* × *Post* | 0.013*** | 0.118*** | 0.006*** | 0.021*** | 0.001 | 0.002 | -0.016*** |
| | (0.001) | (0.011) | (0.004) | (0.001) | (0.002) | (0.003) | (0.005) |
| Observations | 1077668 | 46671 | 1030997 | 15675 | 22461 | 8026 | 3487 |
| **Panel C: High Level Wage Gap Bite** | | | | | | | |
| *Treatment* × *Post* | 0.041*** | 0.102*** | 0.021*** | 0.069*** | 0.031*** | 0.018*** | 0.027** |
| | (0.001) | (0.022) | (0.001) | (0.011) | (0.004) | (0.006) | (0.011) |
| Observations | 532470 | 165635 | 366835 | 5132 | 15361 | 8836 | 2217 |

Notes: This table shows the wage estimates of the 2019 minimum wage increase using the frequency distribution approach. Panel A displays the estimates for the province-sectors with low wage gap bite, panel B for those with mid level wage gap bite, and panel C for those with high level of wage gap bite. Regressions are weighted by log population at baseline. Controls include interactions of time dummies with the province unemployment rate, coastal dummies, and province-sector averages of rm size, part-time work, open-ended and short-term contracts, and job duration. Robust standard errors clustered at the province-sector level are shown in parentheses.. * significant at 10%; ** significant at 5%; *** significant 1%

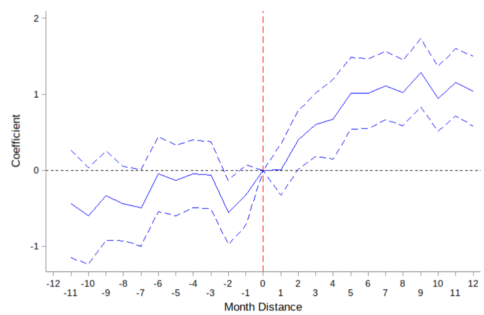
Table 11: Wages Effects in 2019: Frequency Distribution Approach

| | (1) Log Wage All | (2) Log Wage Below MW | (3) Log Wage Above MW | (4) Log Wage 1st Decile | (5) Log Median Wage | (6) Log Wage 8th | (7) Log Wage 9th Decile |
|---|---|---|---|---|---|---|---|
| | | | Panel A: Low Level Wage Gap Bite | | | | |
| *Treatment* × *Post* | 0.022*** | 0.185*** | 0.016*** | 0.021*** | 0.014*** | -0.001 | 0.005 |
| | (0.001) | (0.012) | (0.007) | (0.001) | (0.004) | (0.004) | (0.004) |
| Observations | 573364 | 10512 | 562852 | 8722 | 6703 | 53325 | 53355 |
| | | | Panel B: Mid Level Wage Gap Bite | | | | |
| *Treatment* × *Post* | 0.032*** | 0.174*** | 0.014*** | 0.041*** | 0.012*** | 0.008** | -0.0001 |
| | (0.001) | (0.002) | (0.001) | (0.001) | (0.002) | (0.004) | (0.006) |
| Observations | 1077668 | 93565 | 984103 | 18794 | 22438 | 7895 | 3580 |
| | | | Panel C: High Level Wage Gap Bite | | | | |
| *Treatment* × *Post* | 0.071*** | 0.114*** | 0.024*** | 0.098*** | 0.040*** | 0.018*** | 0.008 |
| | (0.001) | (0.003) | (0.001) | (0.0154) | (0.004) | (0.005) | (0.011) |
| Observations | 551597 | 238462 | 313135 | 2950 | 16810 | 7769 | 2562 |

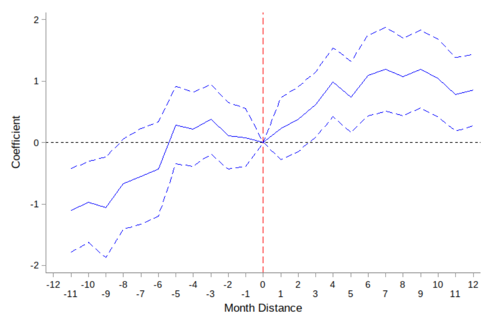
Notes: This table shows the wage estimates of the 2019 minimum wage increase using the frequency distribution approach. Panel A displays the estimates for the province-sectors with low wage gap bite, panel B for those with mid level wage gap bite, and panel C for those with high level of wage gap bite. Regressions are weighted by log population at baseline. Controls include interactions of time dummies with the province unemployment rate, coastal dummies, and province-sector averages of rm size, part-time work, open-ended and short-term contracts, and job duration. Robust standard errors clustered at the province-sector level are shown in parentheses.. * significant at 10%; ** significant at 5%; *** significant 1%

# C Online Appendix (Not for Publication)

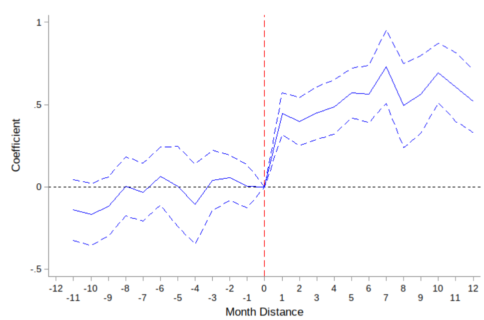
Figure A1: **Wage Effects (Data Not Detrended)**




**(a) Log Wage, 2017 Increase**




**(b) Log Wage, 2018 Increase**

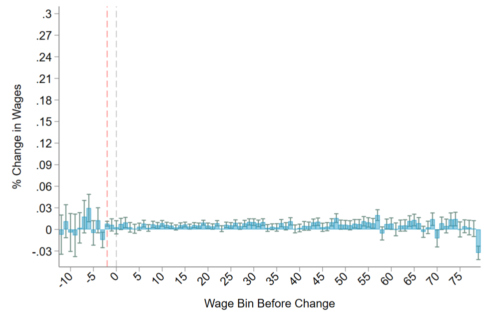



**(c) Log Wage, 2019 Increase**

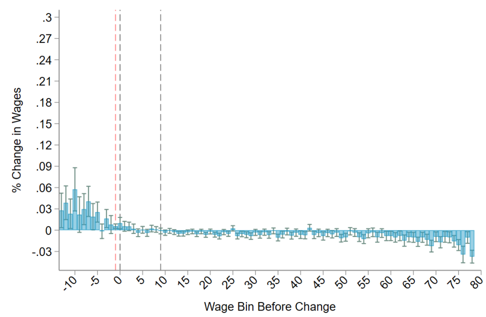
Notes: The figures display the event-study estimates on log average wages. Regressions are weighted by log population at baseline. Controls include interactions of time dummies with the province unemployment rate, coastal dummies, and province-sector averages of firm size, part-time work, open-ended and short-term contracts, and job duration. The blue solid lines are the coefficient estimates and the blue-dashed lines 95% confidence intervals of robust standard errors clustered at the province-sector level. The vertical dashed line depicts the moment of the reform.

## C.1 Wage Effects: Placebo Exercices

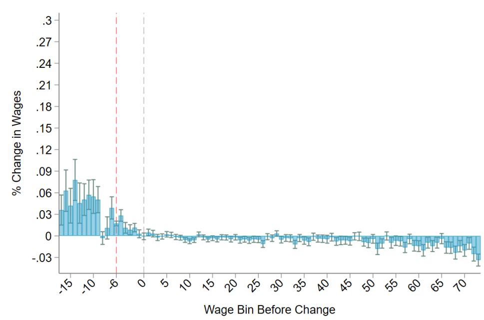
Figure A2: **Wage Effects of the Minimum Wage Increase: Placebo**




**(a) 2017 MW Increase**



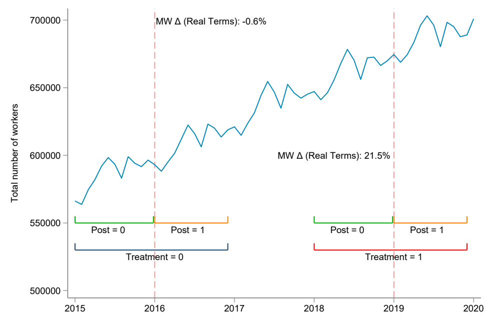

**(b) 2018 MW Increase**



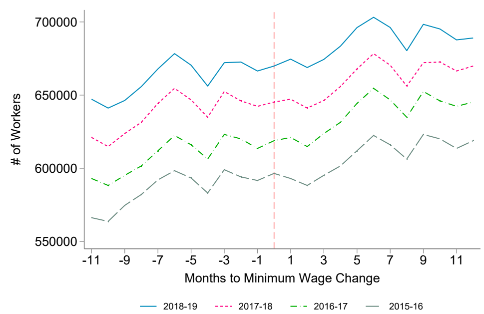

**(c) 2019 MW Increase**

Notes:This figure shows the main results from the difference-in-difference specification (see equation 10) on wages. Each show the result for the wage change between January and February, where the MW does not change, for the 2017 (Panel a), the 2018 (Panel b) and the 2019 (Panel c) MW increases. The blue bars show for each euro bin, relative to the minimum wage, the estimated average change in wages in that bin. The error bars show the 95% confidence interval clustered at month level. The vertical dashed read line indicates the MW level before the increase and the vertical dashed gray line indicate the new MW level.

Figure A3: **Empirical Strategy: Cross-year difference in difference**



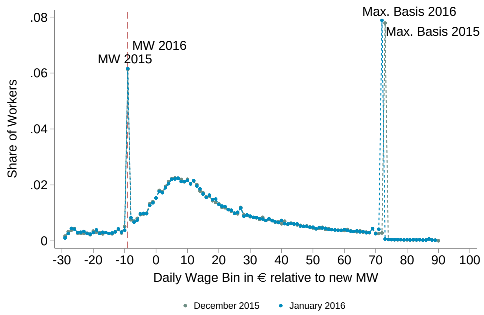

**(a) Difference-in-difference example**



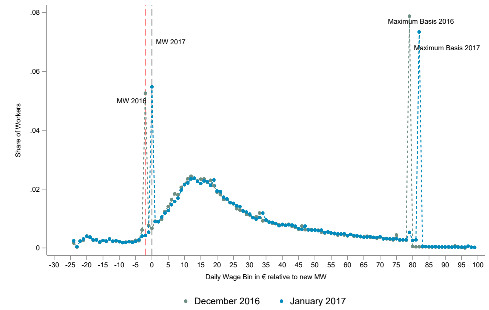

**(b) Employment evolution before and after MW Change**

Notes: This figure panel (a) exemplifies the empirical strategy explained in sections 4. The blue line plots the evolution of the total number of workers affiliated to Social Security. The red dashed lines mark the 1st of January of 2016 and 2019, the moment in which the MW is historically updated. In 2016 there was no increase, this is why we indicate the years 2015-2016 as no being treated (dark blue bracket) and in 2019 MW increased by about 21.5% in real terms, this is why we indicate it as treated (red bracket). The green brackets indicates the months used for the pre-period and the orange one the months used in the post-period. In panel (b) we show the evolution of the total number of workers affiliated to the social security 11 months befora and 12 month after there is a MW change.

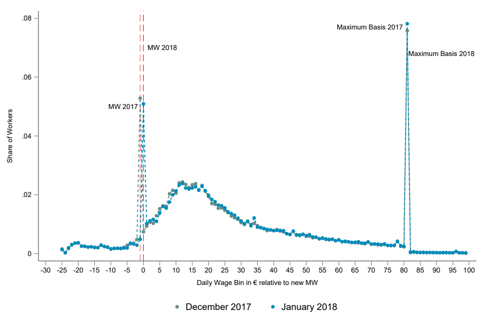
Figure A4: **Daily Wage Distribution Around the Minimum Wage**



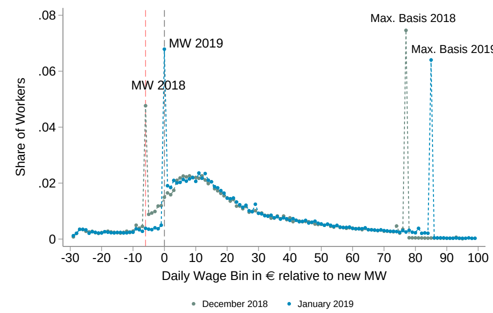

**(a) 2015-2016**




**(b) 2016-2017**



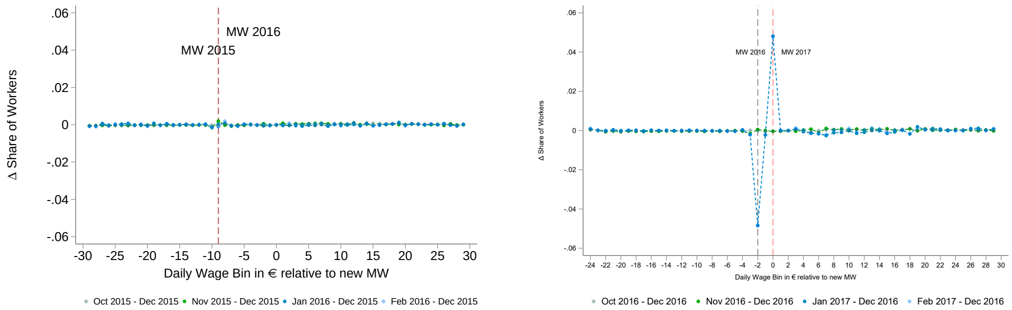

**(c) 2017-2018**



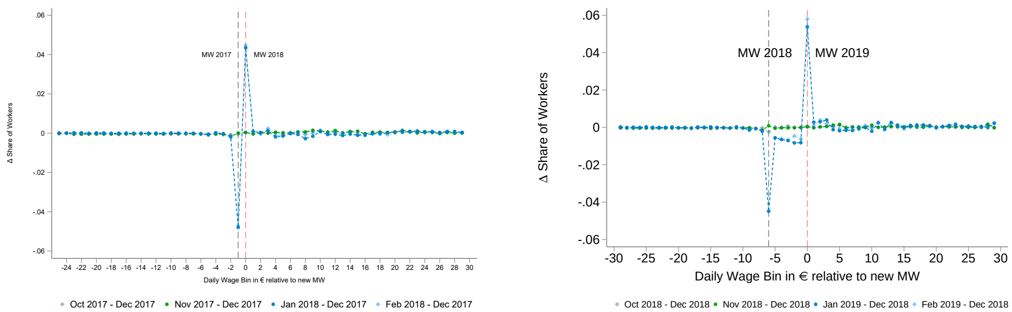

**(d) 2018-2019**

Notes: This Figure shows the frequency distribution of the daily base pay of salaried workers in December in a certain year, $t$, and January of the next year, $t+1$. Panel (a) plots the distributions from December 2018 and January 2019. Panel (b) plots the distributions from December 2017 and December 2018. Panel (c) plots the distributions from December 2015 and 2016. Finally, panel (d) plots the distributions from 2015 and 2016. The vertical lines represent the daily minimum wage level. The red dashed lines corresponds to the daily MW level in year $t$ and the gray dashed line the daily MW year in year $t+1$. The bins have a width of 1and daily wage is calculated in relation to the new daily MW i.e. the MW level of 2019, 2018 and 2017 in panels (a), (b) and (c) respectively. The bins from panel (d) are also set in relation to the MW level from 2019.

Figure A5: **Daily Wage Distribution: Differentiated with respect to December**



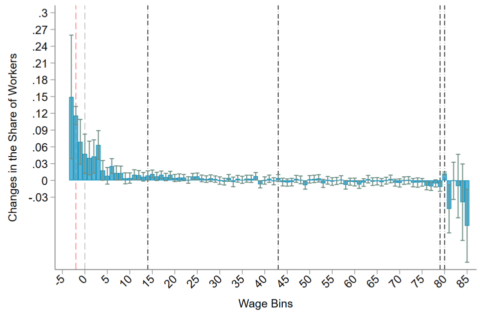

**(a) 2015-2016** **(b) 2016-2017**



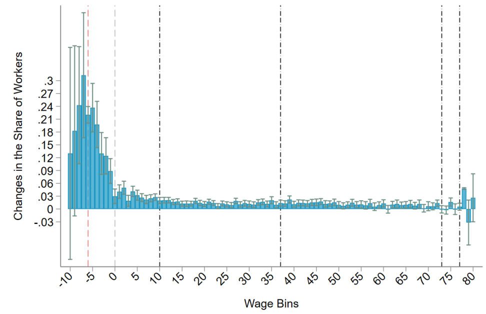

**(c) 2017-2018** **(d) 2018-2019**

Notes:This Figure shows the frequency distribution differentiated with respect to December of year $t$ of the daily base pay of salaried workers in the months before and after December. In particular it shows the differentiated frequency distributions from October and November in year $t$, and January and February in year $t+1$ Panel (a) plots the distributions differentiated with respect from December 2018. Panel (b) plots the distributions differentiated with respect from December 2017. Panel (c) plots the distributions differentiated with respect from December 2016. Finally, panel (d) plots the distributions differentiated with respect from December 2015. The vertical lines represent the daily minimum wage level. The red dashed lines corresponds to the daily MW level in year $t$ and the gray dashed line the daily MW year in year $t+1$. The bins have a width of 1and daily wage is calculated in relation to the new daily MW i.e. the MW level of 2019, 2018 and 2017 in panels (a), (b) and (c) respectively. The bins from panel (d) are also set in relation to the MW level from 2019. The distributions are truncated at 30 euros above the daily MW.

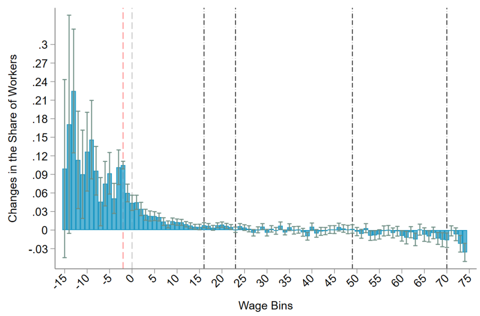
Figure A6: **Wage Effects by Wage Gap: 2017**



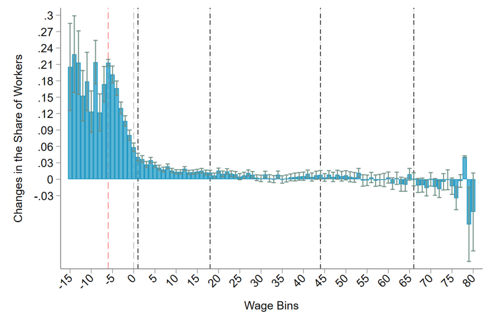

**(a) Low Gap (2017)**



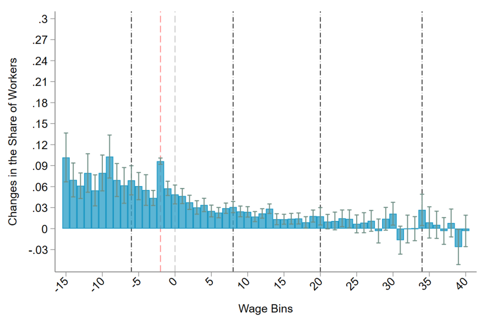

**(b) Low Gap (2019)**



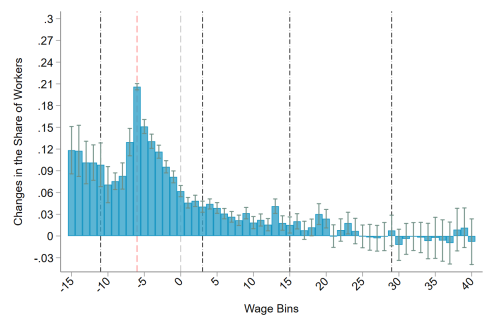

**(c) Mid Gap (2017)**




**(d) Mid Gap (2019)**



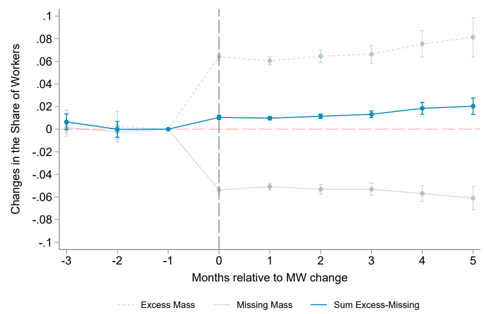

**(e) High Gap (2017)**



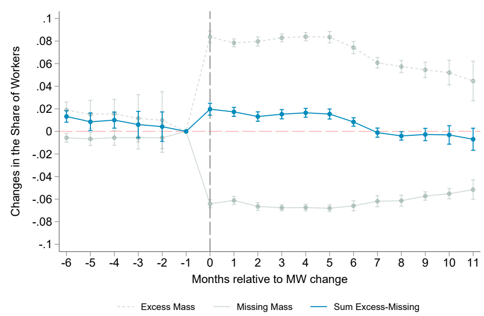

**(f) High Gap (2019)**

Notes: The figures show the effect on wages of the 2017 (left column) and 2019 (right column) minimum wage increases. We split the sample into low'gap province-sectors (Panels (a) and (b)), medium gap province-sectors (Panels (c) and (d)), and high gap province-sectors (Panels (e) and (f)). The specification is Equation 10).The blue bars show the estimated average change in wages in each bin. The error bars show the 95% confidence interval clustered at the individual level. The vertical dashed red line indicates the MW level before the increase and the vertical dashed gray line indicate the new MW level. The black dot-dashed vertical lines are indicate the 10th percentile, the median, and the 80th and 90th percentiles.

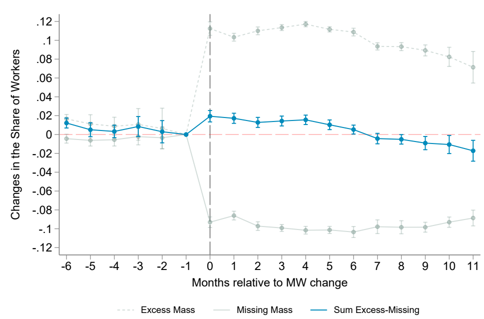
Figure A7: **Employment Effects of the Minimum Wage Increase: Frequency Distribution Approach Event Study**




**(a) 2017**



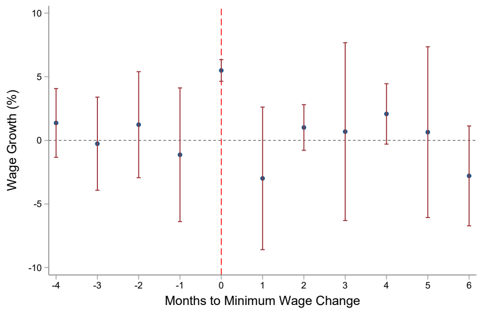

**(b) 2018**



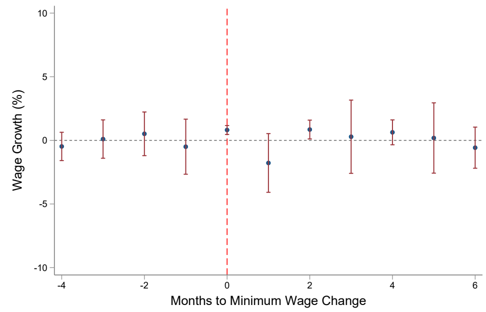

**(c) 2019)**

Notes: This figure shows the main results from the event-study specification (see equation 10) on employment. Panel (a) shows the results for the 2017 MW increase. Panel (b) shows the results for the 2018 MW increase. Panel (c) shows the results for the 2019 MW increase. The figure shows the effect of a minimum wage increase on the excess and missing jobs below and above the new minimum wage (gray dotted line and gray line respectively) and the sum of this excess and missing mass (blue solid line). The vertical lines represent the 95% confidence intervals. For 2018 and 2019 we show the effect for eleven months after the minimum wage increase and six month before it. For 2017 we show the effects for five month after the increase and three months before.

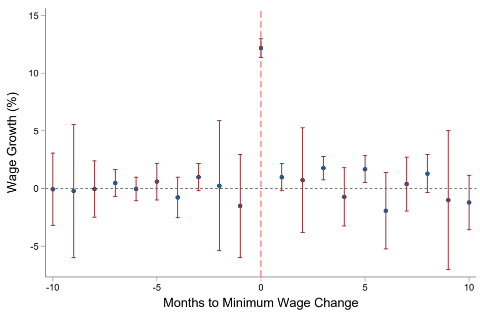
Figure A8: **Wage Effects of the Minimum Wage Increase: Frequency Distribution Approach Event Study**



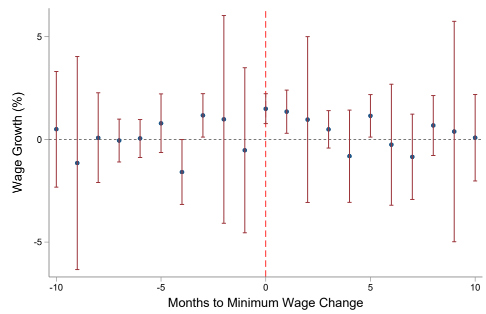

**(a) 2017-Affected by MW**




**(b) 2017-Not affected by MW**




**(c) 2019-Affected by MW**




**(d) 2019-Not affected by MW**

Notes: This figure shows the main results from the event-study specification (see equation 10) on wages. Panel (a) and (b) shows the results for the 2017 MW increase. Panel (c) and (d) shows the results for the 2019 MW increase. Panels (a) and (c) focus on the group of workers directly affected by the MW increase and panels (b) and (d) focuses on the rest of the worker. The vertical lines represent the 95% confidence intervals.