THE SHORT-TERM IMPACT OF THE MINIMUM WAGE ON EMPLOYMENT: EVIDENCE FROM SPAIN

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The Short-Term Impact of the Minimum Wage on Employment: Evidence from Spain

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Abstract

Minimum wages have been widely discussed in the literature. The minimum wage impact on employment strongly depends on labor market concentration and the point at which it is located in the income distribution. Therefore, its study essentially involves exploring whether it has been set too far, beyond the competitive market wage. In 2019, the Spanish government decided to raise the minimum wage by 22.3%. This increase is of a previously unseen magnitude. Using rich administrative data, we combine Propensity Score Matching and a Difference-in-Differences model to evaluate the short-run employment effect of this policy. We find that the reform increased the probability of job loss within a range of 0.38 pp. (7.8%) and 0.44 pp. (9.2%) for workers below the new minimum wage, which implies an employment elasticity between 0.3 and 0.4. In addition, our results suggest that the bulk of this effect is concentrated in the group of workers furthest from the new minimum wage. This is the segment of the income distribution that bore the bulk of the employment costs of the minimum wage increase.

Keywords —Minimum Wage, Employment, Job Loss, Propensity Score Matching, Difference-in-Differences

JEL Codes —J23, J31, J38, J42

1 Introduction

The increase in income inequality in most OECD countries (La Caixa, 2022) and the negative trend in labor income shares (FMI, 2017) have generated a debate on the need to implement policies to protect wages, especially those located in the lower part of the income distribution. Minimum wages have been one of the most popular economic policy tools in this regard. In Spain, income inequality is one of the highest among its European counterparts, and the middle-income population is progressively shrinking, which has led the country to an intense debate about income distribution and, eventually, to a gradual update of the minimum wage over the last decade (Figure 1). In this paper, we evaluate the impact on employment of the increase in the minimum wage in 2019 in Spain, a reform that stands out for its magnitude. Using data from the Spanish Social Security, we assess the effect on the probability of job loss for those workers directly impacted by the policy. Thus, we combine Propensity Score Matching and a Difference-in-Differences strategy to estimate the causal impact of the reform, comparing these workers to those located just above the new minimum wage.

The introduction of a minimum wage pursues protecting workers against meager wages, pushing the lower part of the income distribution to the right. This policy presents two essential advantages compared to other economic instruments: it is often relatively easy to implement, and such implementation does not require an immediate fiscal effort. Consequently, minimum wages have become the policy of reference both in terms of social demand and political feasibility. This has led governments to view them favorably and periodically update their amount. However, it also presents potential costs that may outweigh the benefits. Nowadays, there is no agreement in the literature about its consequences, and new evidence seems to be pointing to more ambiguous conclusions than those drawn two decades ago. Hence, further research is needed to understand to what extent unintended effects occur and at which point they emerge. This paper provides additional evidence in this regard, exploring the impact of an unusually high minimum wage increase in Spain, where it rose from 735.9€ to 900€[1], an increment of 22.3 %.

In most cases, only a small share of workers is directly impacted by minimum wages. Hence, it is not possible to study its effect by exploring aggregate outcomes (Dube, 2019a). In the literature, different causal inference methods have been used in order to estimate this

[1] Magnitudes expressed in 14 payments, as usual in Spain.

effect. In general, Difference-in-differences (DID) is the most employed method, although alternative techniques have been used in many ocasions[2]. Other jurisdictions where the policy was not implemented, higher-wage workers, different demographic groups, or low-wage regions usually conform the control group (Dube, 2019a). A classical procedure is to focus on incumbent workers, identifying as treated those workers whose pre-policy salary is below the threshold established by the minimum wage. Following this approach, we restrict our sample around the new minimum wage and identify these workers directly impacted by the reform. Next, we combine a Difference-in-Differences and a Propensity Score Matching technique to estimate the impact of the policy. Our results suggest that the reform increased the salary of our treatment group by around 5-7%. In addition, we find a negative effect of the minimum wage increase on employment. Thus, the reform would have increased our treatment group's probability of job loss within a range of 0.38 p.p. (7.8%) and 0.44 p.p. (9.2%), which implies an employment elasticity between 0.3 and 0.4. The direction and magnitude of this impact are consistent with the literature – see, for instance, Figure 1 in Neumark & Shirley (2021) – and, for the case of Spain, are in line with Barceló et al. (2021). Finally, we analyze how this impact varies depending on the distance with respect to the new minimum wage. We find that the bulk of the effect of the minimum wage is concentrated in the group of workers located furthest from the new minimum wage. This is the segment of the income distribution that bore the bulk of the employment costs of the minimum wage increase in 2019.

The remaining of the paper is organized as follows: Section 2 reviews the literature on the impact of minimum wages, and section 3 presents the institutional framework. Next, section 4 is devoted to explaining our data and sample selection, and section 5 summarizes descriptive evidence on the incidence of the minimum wage reform. Finally, in section 6, we present the identification strategy and the results, and we conclude in section 7.

2 Literature Review

The effects of the minimum wage are multidimensional, potentially impacting various economic agents and outcomes. The literature has traditionally focused on employment [Neumark & Shirley (2021); Dube (2019a); Belman, D. & Wolfson, P. (2016)], as a reduction

[2]See Kreiner et al. (2019) or Dayioglu et al. (2020) for a Regression Discontinuity Design (RDD) or Cengiz et al. (2019) for a Bunching Method.

in labor demand induced by the increase in labor costs constitutes the classic unintended effect anticipated by economic theory. Nevertheless, further evidence is available on additional outcomes, such as income inequality [Carl Lin & Myeong-Su Yun (2016); Autor et al. (2016)], poverty [Gindling (2018); Dube (2019b)], prices (Harasztosi & Linder, 2019), profits [Drucker et al. (2019); Harasztosi & Linder (2019)] or productivity (Riley & Bondibene, 2016). As known, the positive effects of the minimum wage are expected to come from an increase in wages located in the left tail of the distribution, which may reduce poverty and income inequality. Thus, minimum wages necessarily imply an increase in labor costs, inducing a wealth redistribution that will ultimately generate losers and winners. Three different economic agents may suffer or benefit from a minimum wage increase: workers, firms, and consumers. Eventually, how this luck is distributed among agents will depend on how companies react to the increase in labor costs the policy entails.

The economic theory predicts several channels through which firms react: labor demand, prices, profits, and productivity. As mentioned, the minimum wage effect on labor demand is the most explored channel of the four. However, this attention is often not proportional to its importance. According to Harasztosi & Lindner (2019), for instance, prices constitute one of the most significant reaction margins for companies. In this document, the authors assess a substantial increase in the minimum wage in Hungary and explore the margins along which firms responded. They find that the final consumer paid 75% of the rise in labor costs induced by the minimum wage. In addition, a small employment elasticity is estimated, which is only larger in non-competitive industries. Similarly, Leung (2021) studies firms' reactions to the minimum wage via prices in the United States and finds these to be lower in those sectors where demand elasticities are high and, therefore, it is difficult for firms to adjust the increase in labor costs via prices. Hence, alternative channels such as profits or productivity seem reasonable to be explored when this occurs.

Under good competitive markets and the absence of firms' monopsony power in the labor market, minimum wage policies are very likely to impact profits. In Hungary, for example, after estimating a low employment elasticity, Harasztosi & Lindner (2019) finds the portion of the cost induced by the minimum wage increase that is not explained by prices to be assumed by firms through a reduction in profit margins. Likewise, Drucker et al. (2021) finds a significant minimum wage increase in Israel to negatively impact firms' profits, an effect that is stronger for minimum-wage-intensive companies. Finally, there is also evidence that shows how minimum wages are capable of increasing labor productivity. In this sense, Riley & Bondibene (2016) find that firms reacted by increasing production efficiency and TFP to offset the minimum wage increases in the unit labor cost in Britain.

Therefore, prices, profits, and productivity constitute essential margins of reaction for firms facing minimum wage policies. However, their study requires specific firm-level data about prices, costs, sales, wages, and TFP, which is often hard to find. This is one of the main reasons why these have not been as much studied as the employment margin. Nonetheless, it is critical to consider them. Furthermore, beyond exploring these margins to understand and anticipate potential unintended effects, it is also essential to evaluate the impact of the policy on wages, the level of poverty, and wage inequality. This is the only way to determine if the benefits outweigh the costs. In this paper, we only focus on the employment margin. Thus, a complete evaluation of the Spanish minimum wage increase in 2019 is beyond the scope of our work, which is only intended to contribute to the debate on employment and provide further evidence on its impact on the probability of job loss. Therefore, our results must be considered within these terms, as a necessary but not sufficient element for the evaluation of the reform.

Economic theory provides precise predictions on the impact of the minimum wage on employment under perfect competition: under this circumstance, the market clearing wage is equal to the marginal product of labor, so the introduction of a minimum wage above this level would reduce employment – as the workers who were previously supplying labor at the equilibrium are still willing to work but are displaced – and generate unemployment – as other workers who were not supplying labor are now willing to work –. Under imperfect competition, however, the employer sets the wage at the point where the marginal cost equals the marginal product of labor. This involves a lower wage and employment level than in a competitive market. Thus, a minimum wage may potentially increase wages and employment as long as it is set between the monopsony and the competitive market wage (Boeri & Van Ours, 2021). In this context, the effects of the minimum wage on employment will depend, therefore, on the competitiveness of the labor market and the point at which it is located. These aspects need to be empirically evaluated.

The evidence on the employment effects of minimum wages is extensive. Nevertheless, there is no consensus in the economic literature. A first branch of the literature considers the overall body of evidence to be pointing toward a small and generally non-statistically significant effect [Dube (2019a); Bellman & Wolfson (2014)]. On the contrary, however, documents like Neumark & Shirley (2021) defend the existence of a "clear preponderance of negative estimates in the literature". In any case, there seems to be an agreement on why discrepancies in employment elasticities exist across studies: the degree of monopsony in the labor market. Yet, no convincing evidence was available on this matter until recently.

The first piece of compelling evidence in this sense is Azar et al. (2019). Using a rich dataset that includes information on US firms' online vacancies in a particular low-wage sector, this paper is able to estimate the degree of monopsony at the county level. Thus, it exploits the regional variation in the minimum wage incidence – the so-called MW *bite* –, and shows that the effect of the minimum wage is negative in those regions where labor markets are competitive, while becoming positive in concentrated ones. These results are consistent with the monopsony explanation. Since Azar et al. (2019), new evidence has shown similar results – see Munguía (2020) –. Simultaneously, Ahlfeldt et al. (2019) provide further evidence supporting the idea that "there is no such thing as one minimum wage effect". They construct a monopsonistic labor market model with heterogeneous firms and calibrate it using region-specific treatment effects estimated from a DID that explores the impact of a minimum wage increase in Germany in 2014. Results prove that the minimum wage effect is a bell-shaped function of local productivity. Hence, there exists a point at which the minimum wage maximizes employment – 48% of the median income in Germany – and a limit from which its impact starts to reduce it – 80% –. Again, this bell-shaped effect perfectly fits the monopsony explanation. Therefore, the impact of the minimum wage will depend on the labor market concentration and the point at which it is located in the income distribution. These aspects can only be empirically assessed.

3 Institutional Framework

In 2018, the government of Spain embarked on a mid-term minimum wage reform to reach 60% of the median income at the end of the legislature. In Figure 1, we present the path minimum wages have followed in Europe during the last 20 years. As seen, the increase in the minimum wage in Spain since 2016 has far exceeded its European counterparts and, most in particular, the update introduced in January 2019 constitutes an interesting episode due to its unprecedented magnitude: from December 2018 to January 2019, the minimum wage was increased from 735.9€ to 900€/month[3], an increment of 22.3 %.

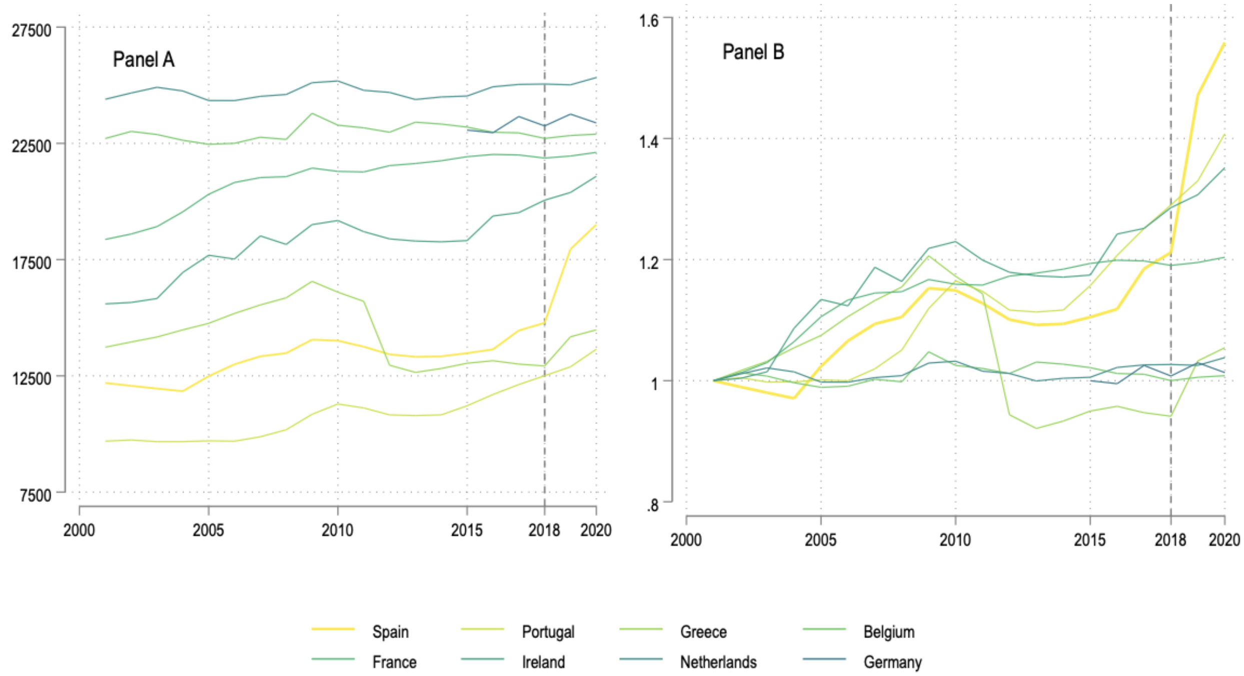
Despite this, however, the evidence regarding this reform is scarce. Initially, several evaluation attempts were made by the Bank of Spain (BdE) and the Independent Authority for Fiscal Responsibility (AIREF) to anticipate the effect of the reform before the publication of social security data (Muestra Continua de Vidas Laborales, MCVL) for the year 2019. In this sense, the Bank of Spain (Lacuesta et al., 2019) was the first to predict a large impact based on the employment elasticity resulting from the evaluation of the 2017 minimum wage increase. Moreover, contrary to what is found a year before in AIREF (2019), which provides no evidence of a negative impact on employment using aggregate data, AIREF (2020) estimated a job loss within a range of 19.000 and 33.000 affiliates.

Nevertheless, these documents are only preliminary evaluations prior to the publication of the 2019 MCVL. Among the ones that employ this dataset, the most important document is Barceló et al. (2021), published as well in the occasional documents line of the Bank of Spain. This paper provides evidence on the impact of the minimum wage increase in 2019 using two differentiated strategies. First, the authors carry out a similar analysis to Lacuesta et al. (2019), exploring the reform's effect on the probability of job loss. The results of this exercise are consistent with the latter document, which predicted a larger impact in 2019 than in 2017 considering the larger magnitude of the last reform. However, the main contribution of Barceló et al. (2021) is a difference-in-differences estimation that compares the growth rate of the monthly average number of contracts in a range around the threshold established by the new minimum wage and the segment immediately above this range. In this second approach, it can be observed how the segment of the income distribution below the selected threshold behaves comparatively worse. This result is robust to several specifications of these ranges and indicates a negative impact of the policy on employment. In particular, the minimum wage increase is estimated to have reduced employment within a range of 6% and 11%.

These documents constitute the whole set of publicly available evidence on the 2019 reform. However, there are several unpublished documents that are worth to be considered. First, in a regulatory impact analysis of the update in the minimum wage for 2022 elaborated by the Ministry of Labor and Social Economy (2021), an internal work of the ministry still

[3]Magnitudes expressed in 14 payments, as it is usual in Spain.

Figure 1: The Evolution of Minimum Wages in Europe, 2000-2020



Notes: The figure depicts the evolution of the minimum wage for Spain, Portugal, Greece, Belgium, France, Ireland, Netherlands, and Germany. Minimum wages are shown in 2020 constant prices at 2020 USD PPPs. Panel A represents the natural trends, while these are expressed in the 2001 base year in Panel B.
Source: OCDE.stat — Own Elaboration

in the process of elaboration is mentioned. According to this report, the 2019 minimum wage increase would have reduced employment. Moreover, the same document refers to an internal note from the Ministry of Economic Affairs and Digital Transformation, which would also found a job loss quantified in 36.000 jobs caused by the increase in the minimum wage in 2019. Finally, it is public knowledge that the Ministry of Labor commissioned another evaluation of this reform, conducted by Professor Sara de la Rica, whose results have not been published.

In conclusion, the evidence we have on the recent minimum wage updates in Spain is scarce. However, the one we have seems to be pointing out a negative impact on employment. The present document intends to contribute to the Spanish literature, posing a compelling method to estimate the effect of the increase in the minimum wage and providing further evidence in this regard.

4 Data

The present paper employs administrative data from the Muestra Continua de Vidas Laborales con Datos Fiscales (MCVL). This extremely rich dataset contains individual-level and anonymized social security, income tax, and census information for a 4% random sample of Spanish workers, pensioners, and unemployment benefit recipients, who have had any relationship with the Spanish Social Security during the year of reference. Starting in 2004, one edition of the MCVL has been annually published. In this document, we generally use the 2019 edition, although in some cases, we complement this information with the 2017 and 2018 editions.

The MCVL is composed of six different files: (1) People, (2) Affiliates, (3) Social Security Contributions, (4) Pensions, (5) Cohabitants, and (6) Income Tax Data. Each of these files provides an anonymized and unambiguous personal identifier that allows the researcher to connect the whole set of available information for every person in the dataset, distributed among these files. The crucial table of the MCVL is the affiliates' file, which contains the complete labor market history for every individual in the sample. Starting from this file, we recover personal information such as gender or age, extracted from the Social Security records, and educational attainment, nationality or place of birth and residence, extracted from the Spanish Continuous Census of Population, from the people's file. In addition, we match this information with data on individual Social Security contributions. As known, these earnings are censored. This can be solved using income tax data, available in the corresponding file. However, we are only interested in the left-hand half of the income distribution, and data on Social Security contributions is slightly wider. Hence, we decided to stick to these earnings.

Following the codes employed in De La Roca & Puga (2017)[4], we track affiliates over their working lives and construct a panel that provides information about the most important job relationship every month from Jan 2017 to Dec 2019 for employees who belong to the general Social Security regime and have worked at least the equivalent to 30 full-time days during a calendar year. Most of the evidence presented in this paper only uses the panel constructed from the 2019 edition. However, some results and figures require using the 2017 and 2018 waves, so this procedure is carried out for these editions as well.

As a result, every observation in the dataset refers to a particular individual, year, and

[4]Published in https://diegopuga.org/data/mcvl/

month, and contains information about the number of days worked, daily income, type of contract and working day, occupation, educational attainment, economic activity, etc. Moreover, we identify the last month of a period with a strictly positive number of days worked followed by a month of unemployment as a month in which job loss occurs. A dummy identifying these periods is constructed. This will be the main dependent variable in our analysis.

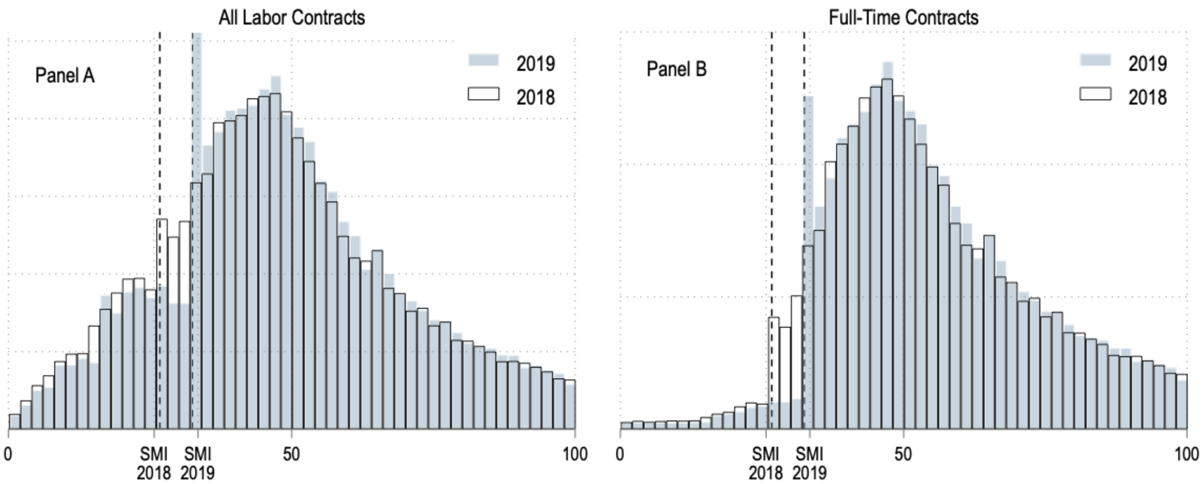
5 Descriptive evidence

5.1 The Incidence of the Minimum Wage

The new minimum wage was established in 900€/month and introduced in January 2019. In Figure 2, we represent the evolution of the frequency distribution of real daily earnings from 2018 to 2019, using data from both editions of the MCVL. As shown, the minimum wage raised from 26.72€ – represented by the first dashed line – to 32.45€ – second dashed line –, which constitutes an increase of 21.4% in real terms. In Panel A, we represent the earnings distributions for both years and all contracts, including part-time contracts. Unfortunately, the MCVL does not include data on the daily number of hours worked, and the variable intended to provide information on the percentage of a full-time working day specified in the contract is unreliable. This prevents us from computing the hourly wage. Thus, non-zero frequencies to the left of the 2018 minimum wage arise in Panel A due to part-time contracts. Hence, Panel B represents the same frequency distribution of earnings, excluding these latter. In this case, frequencies to the left of the 2018 minimum wage are much lower, being the remaining cases probably due to full-time jobs of less than 40 hours a week and other exceptional circumstances. Nevertheless, some unexpected contract type errors cannot be ruled out either.

As shown, the substantial increase in the minimum wage clearly modified the income distribution. First, as expected from an adequately enforced legislation, jobs below the new minimum salary disappeared. Moreover, we observe how these jobs concentrate right at the new minimum wage level, generating a spike in the 2019 distribution that is considerably more pronounced than the 2018 minimum wage's. We barely perceive additional jobs appearing further up in the income distribution, which sharply contrasts with other similar

Figure 2: The Frequency Wage Distribution in Spain: 2018 and 2019



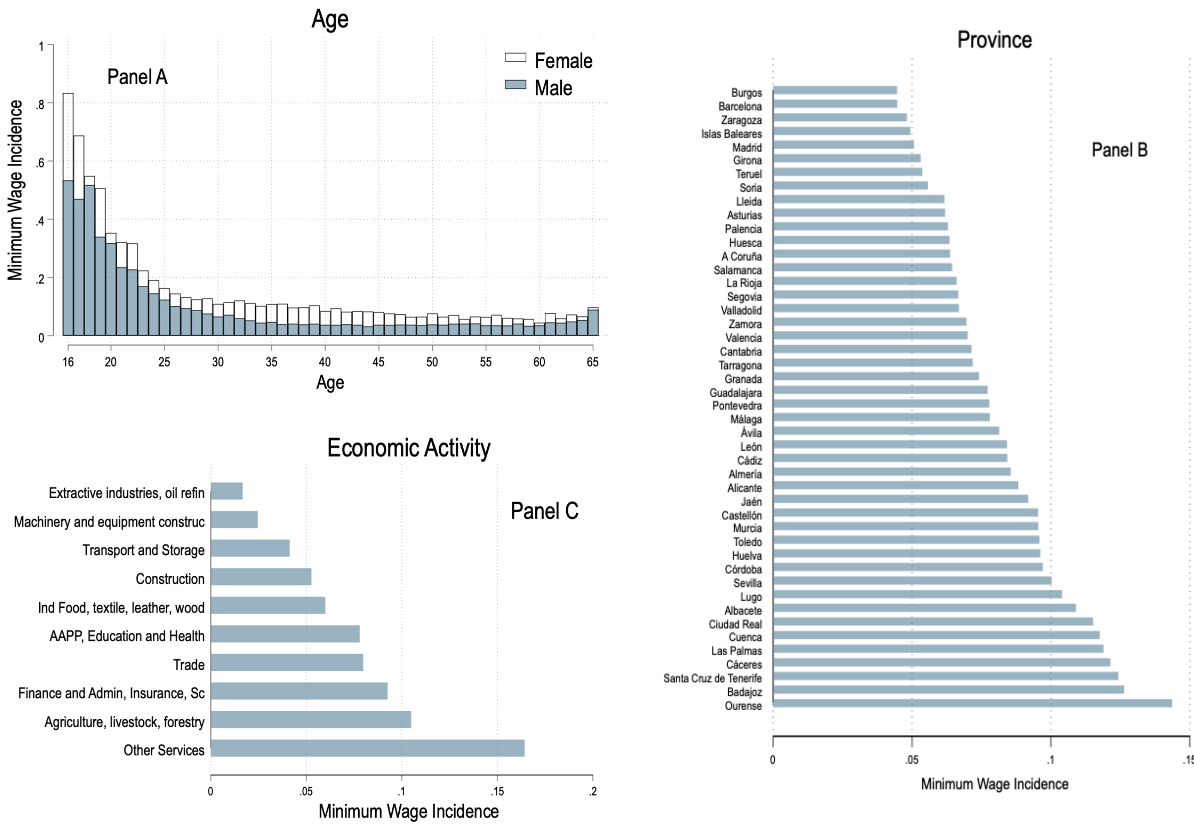
Notes: The figure represents the Spanish October frequency distribution of real daily earnings for the years 2018 and 2019. Panel A includes all types of labour contracts, while Panel B only includes full-time contracts. The vertical lines represent the minimum wage each year.
Source: 2018, 2019 MCVL — Own Elaboration

analyses, like Harasztosi y Linder (2019)'s. Thus, while this reallocation of jobs seems to occur more uniformly above the new minimum wage in Hungary, this process is more abrupt and concentrated in Spain, probably due to the magnitude of the increase.

According to our final dataset, a 9% of employees present social security contributions below the new minimum wage. This percentage is similar to Barceló et al. (2021)'s and is considerably larger than the share of workers directly impacted from the last minimum wage updates in 2018 – 4% – and 2017 – 2.4% –. Moreover, this minimum wage *bite* presents important differences across geographic groups. In Figure 3, Panel A, we represent the share of workers impacted by the minimum wage by gender and age. As seen, the minimum wage *bite* is a convex function of age, reaching its maximum levels during the earliest stages of access to the labor market and then progressively decreasing until 60 years old, the point at which a slight rebound occurs. Thus, 60% of the youngest workers present earnings below the minimum wage, while this share is less than 10% for 50 years old workers. In addition, female workers seem to systematically present a higher incidence of the minimum wage than men, with the most remarkable difference being during youth.

Finally, as it can be observed, there are remarkable differences across provinces, economic

Figure 3: The Incidence of the Increase in the Spanish Minimum Wage in 2019



Notes: The figure represents the incidence of the increase in the minimum wage in Spain in 2019. It is computed for all employment relationships in the general social security regime that cover the entire month of December. This coverage is equal to the percentage of workers whose salary in 2018 is lower than the new minimum wage established in 2019. First, Panel A represents the incidence of the minimum wage by age and gender. Secondly, Panel B provides information on this coverage by province (regional level). Finally, Panel C represents the incidence of the minimum wage by economic activity.
Source: MCVL 2018 — Own Elaboration

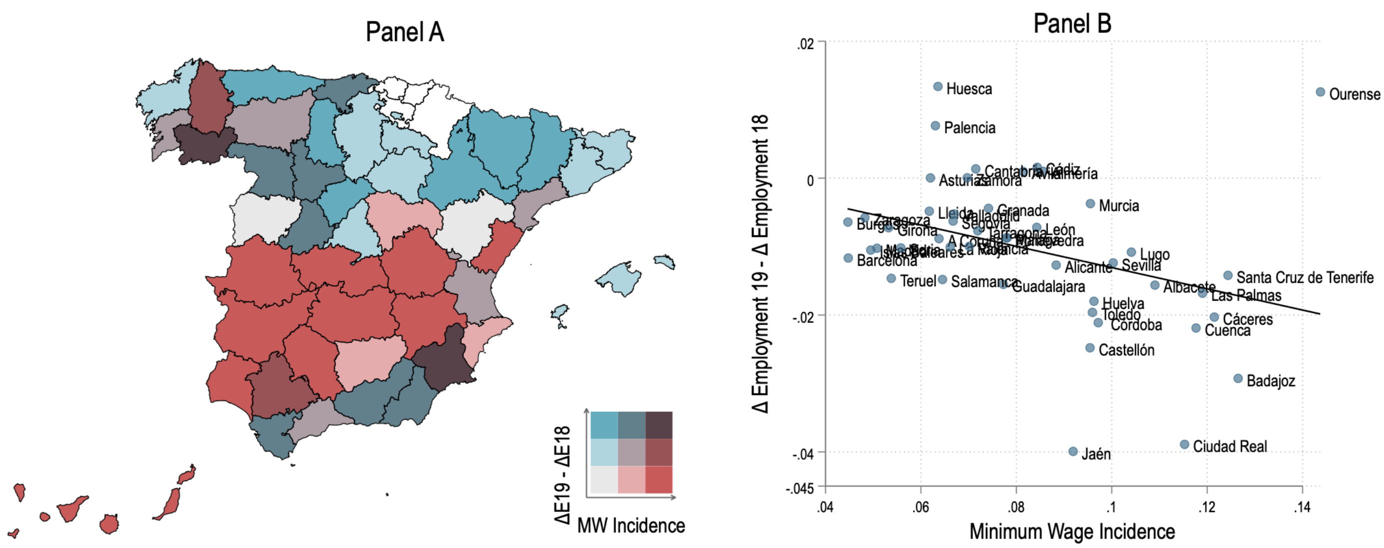
activities and sectors as (Figure 3, Panels B and C). Agriculture, livestock, forestry, fishing, commercial activities, and other low-productivity services present a higher minimum wage *bite*. Moreover, the provinces where these activities are more important are those for which the incidence of the minimum wage is larger. Thus, while provinces where large cities – such as Madrid or Barcelona – are located have levels below 5%, other territories like Extremadura, Castilla La Mancha, or the Canary Islands present levels above 10%.

5.2 Employment and Incidence of the Minimum Wage

As previously mentioned, minimum wage policies only affect a small share of workers. As a consequence, it is not usually possible to study its effect by exploring aggregate outcomes (Dube, 2019a). The case at hand, however, could be different due to the magnitude of the increase. Thus, Barceló et al. (2021) provide evidence of a negative correlation between the evolution of employment and the incidence of the minimum wage. In addition, they assessed the effect of the policy by exploring differences in the growth path of the number of active contracts in the MCVL in several income segments around the new minimum wage before and after its implementation. As shown, Spain suffered a slowdown in the path of employment growth during 2019. Naturally, this fact should not be attributed to the increase in the minimum wage. Hence, in this document we analyze whether the reform may have marginally contributed to the worst employment behavior during 2019. In particular, in this section, we delve into the analysis of the correlation between the aggregate evolution of employment and the minimum wage *bite*, paying special attention to the differences by age, economic activity, and regional areas.

In Figure 4, we represent the relationship between the difference in the employment interannual growth rate in Dec 2019 and Dec 2018 and the share of workers directly affected by the policy at the province level. As seen, these variables are negatively correlated, which implies that employment slowdown was stronger in those regions more affected by the minimum wage. In Panel A, we observe the highest minimum wage incidences concentrated in the country's south and northwest, revealing a high correlation between this index and the gross domestic product per capita. Therefore, the minimum wage *bite* is not randomly distributed across provinces but strongly depends on their economic and socio-demographic structure (Figure 3). Thus, it is not difficult to imagine a similar correlation in the absence of an increase in the minimum wage, as employment is likely to behave better in more prosperous regions. Hence, Figure 5 analyzes this correlation by cells that group age segments, economic activities, and territorial divisions. In this figure, the composition of these cells specifically intend to dissolve this effect and control for these variables. In general, it can be observed how this correlation attenuates. This is especially the case in Panel A, where it almost disappears when age is considered. However, the results still show a negative relationship between employment and the minimum wage *bite* in Panels B and C, where we consider differences in sectoral composition across provinces, and age and sectoral composition across Autonomous

Figure 4: Employment and Incidence of the Minimum Wage by Provinces



Notes: The figure evaluates the relationship between the evolution of aggregate employment and the incidence of the minimum wage by provinces. The incidence of the minimum wage is computed using the MCVL for all employment relationships in the general social security regime that cover the entire month of December. This coverage is equal to the percentage of workers whose salary in 2018 is lower than the new minimum wage established in 2019. Panel A maps this coverage and the the evolution of employment, which is measured as the difference between the interannual variation of the number of affiliates between Dec 2018 and Dec 2019 using Social Security Affiliation Data. Panel B shows the correlation between these two variables.
Source: MCVL 2017, 2018, 2019 — Own Elaboration

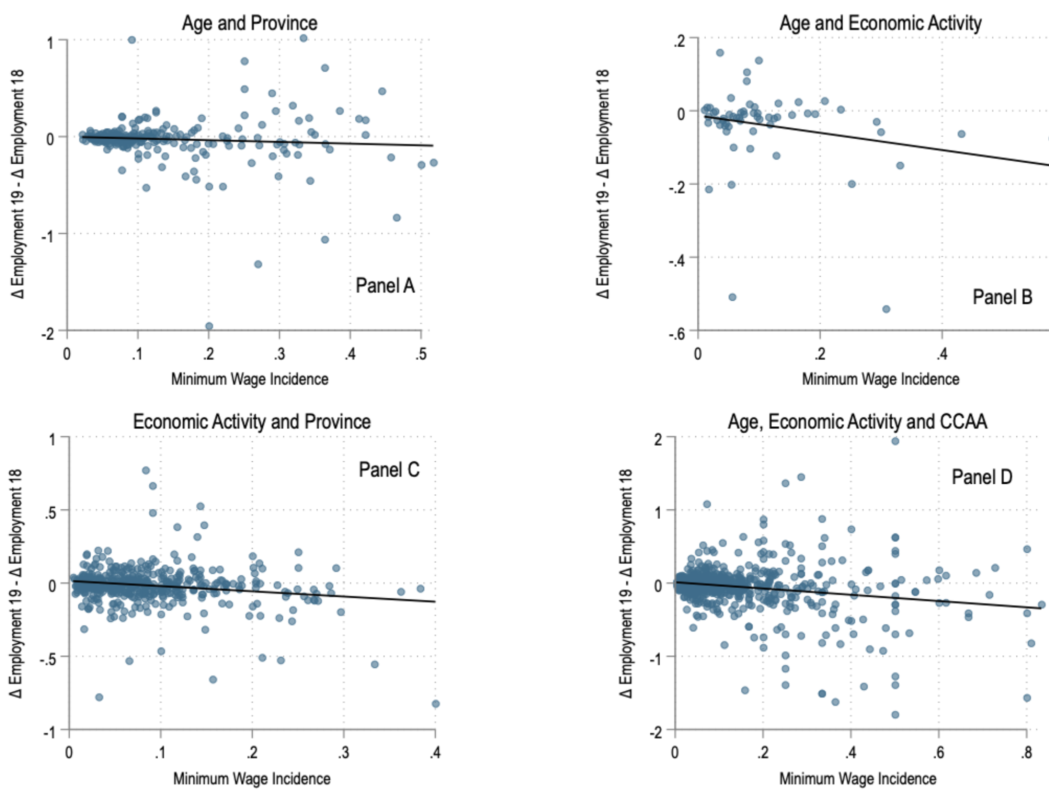
Regions, respectively. Lastly, the same results are found in Panel B, where this correlation is represented by age and economic activity cells.

In general, therefore, we observe a negative relationship between employment behavior during 2018 and 2019 and the incidence of the minimum wage. Thus, regions and cells where the minimum wage *bite* is larger behave comparatively worse than those where this share is low. However, despite providing valuable information, this correlation cannot be interpreted as causal, as lower employment growths may result from additional factors not considered so far. Thus, in the next section, a causal inference method that combines Difference-in-Differences and Propensity Score Matching is posed to isolate the causal impact of the reform on employment.

6 Empirical Approach

In this section, we estimate the causal impact of the minimum wage increase on the probability of job loss. So far, we have shown that those regions and cells where a larger share of workers was directly affected by the minimum wage increase behaved comparatively worse in

Figure 5: Employment and Incidence of the Minimum Wage



Notes: The figure evaluates the relationship between the evolution of aggregate employment and the incidence of the minimum wage by cells that group age segments, economic activities, and/or territorial divisions (Provinces and Autonomous Regions (CCAA)). The incidence of the minimum wage is computed for all employment relationships in the general social security regime that cover the entire month of December. This coverage is equal to the percentage of workers whose salary in 2018 is lower than the new minimum wage established in 2019. The graph shows the correlation between this share and the evolution of employment, which is measured as the difference between the interannual variation of the number of affiliates between Dec 2018 and Dec 2019. These cells are made up of age groups and provinces in Panel A, age groups and economic activities in Panel B, economic activities and provinces in Panel C, and age groups, economic activities, and CCAAs in panel D.
Source: MCVL 2017, 2018, 2019 — Own Elaboration

terms of employment. However, this fact cannot be interpreted as causal, as several factors and circumstances affect and determine both the demand and supply of labor and differ across these units. Hence, we need a causal inference method to estimate the real impact of the minimum wage on employment. In this sense, the standard strategy exploits variation in the minimum wage regulation. This variation is often geographical, although it can also occur between groups of workers for whom the minimum wage legislation differs. For the Spanish case, however, this regulation is strictly homogeneous across regions, and there are no important specificities regarding the application of the law. Therefore, alternative methods that exploit different sources of variation are needed.

6.1 Identification Strategy

A classical procedure in these cases focuses on incumbent workers. Following this approach, we use data from the 2019 MCVL to define as treated those employees with a pre-policy wage below the new minimum salary, while including those whose salary is slightly above this threshold in the control group. To do so, we first restrict our sample to those employees who have worked at least the equivalent of 30 full-time days during 2018. In addition, we create a variable that reports the last wage perceived in 2018 and eliminate from the sample those workers with a part-time contract in this period. Since hourly wages cannot be computed, the latter is done to identify treated individuals more precisely. Next, based on this variable, we restrict our sample to workers whose salary lies within the interval [MW2018, 1.3*MW2019], which ensures some comparability between the treatment and the control group. Finally, to avoid measurement errors and possible spillover effects, we exclude from the sample workers just above the new minimum wage – see Figure A1 –. The complete procedure leaves us with a sample of 76.002 individuals whose information expands over 36 months. Then, as explained, we identify those employees below the new minimum wage (19.331) as treated while including the rest (56.671) in the control group.

From here, we present two different estimation methods that will be combined with a Propensity Score Matching technique. As a first step, we propose the following linear regression model:

$$Y_{it} = \beta_0 + \beta_1 Treated_i + \lambda_t + \delta X_{it} + \epsilon_{it} \tag{1}$$

where $Treated_i$ is a dummy for treated individuals, λ_t are time fixed effects dummies, and X_{it} is a vector of covariates that includes gender, age, province of residence, country of birth, type of contract and working day, economic activity and occupation. In this equation, the parameter β_1 is intended to capture the post-policy average difference in the probability of job loss between the treatment and the control group. Therefore, Equation 1's estimation sample will always be restricted to post-policy observations. Once the propensity score matching is applied, we call this parameter the matching estimator. When it is not, we call it the unmatched or naïve OLS estimator. Using this matching method, we create a statistical comparison group based on a logit model that estimates the probability of being treated conditional on a set of observable characteristics:

$$Y_i = h(X'\sigma) + \epsilon_i = \frac{exp(X'\sigma)}{1 + exp(X'\sigma)} + \epsilon_i \tag{2}$$

$$Pr(\widehat{T = 1}|X) = \frac{exp(X'\hat{\sigma})}{1 + exp(X'\hat{\sigma})} = \frac{exp(\hat{\sigma_0} + \hat{\sigma_j} X_i^j)}{1 + exp(\hat{\sigma_0} + \hat{\sigma_j} X_i^j)} \tag{3}$$

where X is a vector of covariates that include those mentioned above plus the pre-policy average share of days worked a month and months with a fixed-term contract, and the number of months worked during 2017 and 2018. Based on the predicted propensity score, different algorithms can be used to match every treated and control units. In our baseline setting, we use the nearest neighbor matching algorithm with replacement, allowing ties, and imposing a caliper of 0.001.

Under this approach, β_1 will constitute the average treatment effect on the treated (ATT) whenever two identification assumptions are satisfied: conditional independence and common support. The first assumption implies that no unobserved characteristics determine the treatment. Thus, given a set of observable characteristics, potential outcomes are independent of the treatment assignment. From this assumption follows the balancing property, which ensures that observations with the same propensity score present the same distribution of covariates, being the assignment to the treatment random for a given propensity score. On the other hand, the common support assumption requires that treatment units have comparison observations nearby in the propensity score distribution. These aspects will be discussed in more detail in the next section.

In a second step, we propose an alternative approach, a Difference-in-Differences model:

$$Y_{it} = \beta_0 + \beta_1 Treated_i * Post_t + \gamma_i + \lambda_t + \delta X_{it} + \epsilon_{it} \tag{4}$$

where $Post_t$ is a dummy that takes value one for periods after the implementation of the policy, γ_i are individual-fixed effects dummies, and the resting elements are explained above (See Equation 1). In this case, β_1 is the result of a double difference between the distance before and after the implementation of the policy and the difference between the treatment and control group. Thus, the first difference purges the error term from any time-invariant unobservables that may differ between the treatment and the control group, while the second difference does the same for any time-varying unobservables that are equal for treated and untreated units. Hence, our parameter β_1 will capture the treatment effect of the policy as long as there are no time-varying differences in unobservables correlated with the treatment between the treatment and the control group. Given the difficulty in justifying this assumption, Difference-in-Differences models are often combined with Propensity Score Matching. As we will see in the next section, one of our preferred specifications employs this resource.

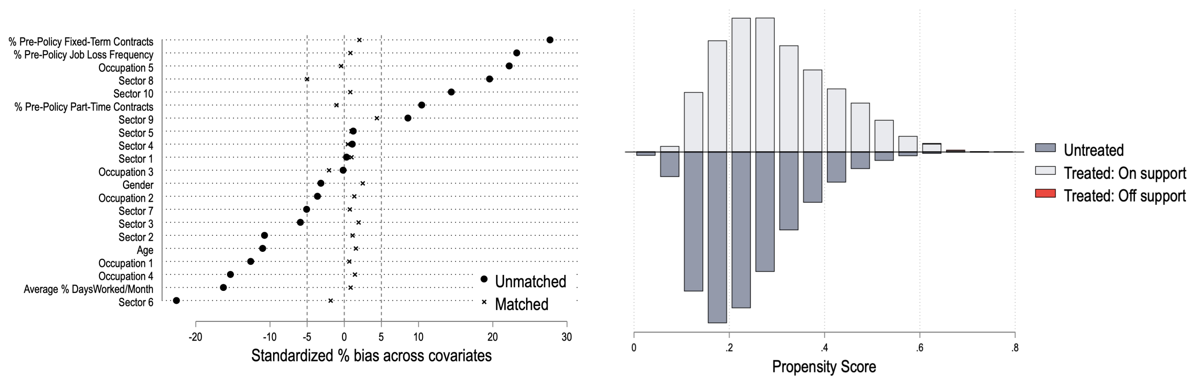
6.2 Main Results

First, we analyze the distribution of covariates between the treatment and control groups before propensity score matching is applied. In Figure 6, Panel A, we represent the standardized percentage bias across the most representative covariates included in the logit model. As seen, workers in the treatment group are younger, more likely to have a fixed-term and part-time contract, more likely to lose their job, work fewer days a month, and are employed in low-responsibility occupations and sectors with a higher incidence of the minimum wage. Therefore, our treatment and control groups significantly differ in covariates that are strongly correlated with our treatment. This implies that a simple post-policy comparison between these groups would yield a biased estimate. We can clearly see this in Figure 7, Panel C, where we show the evolution of the probability of job loss for these groups. As observed, the likelihood of job loss is substantially higher for the treatment group, so the resulting post-policy difference cannot be attributed to the minimum wage increase.

We propose two different causal inference methods to overcome this problem and isolate

Figure 6: Reduction in Bias after Matching and Common Support Histogram

Panel A **Panel B**

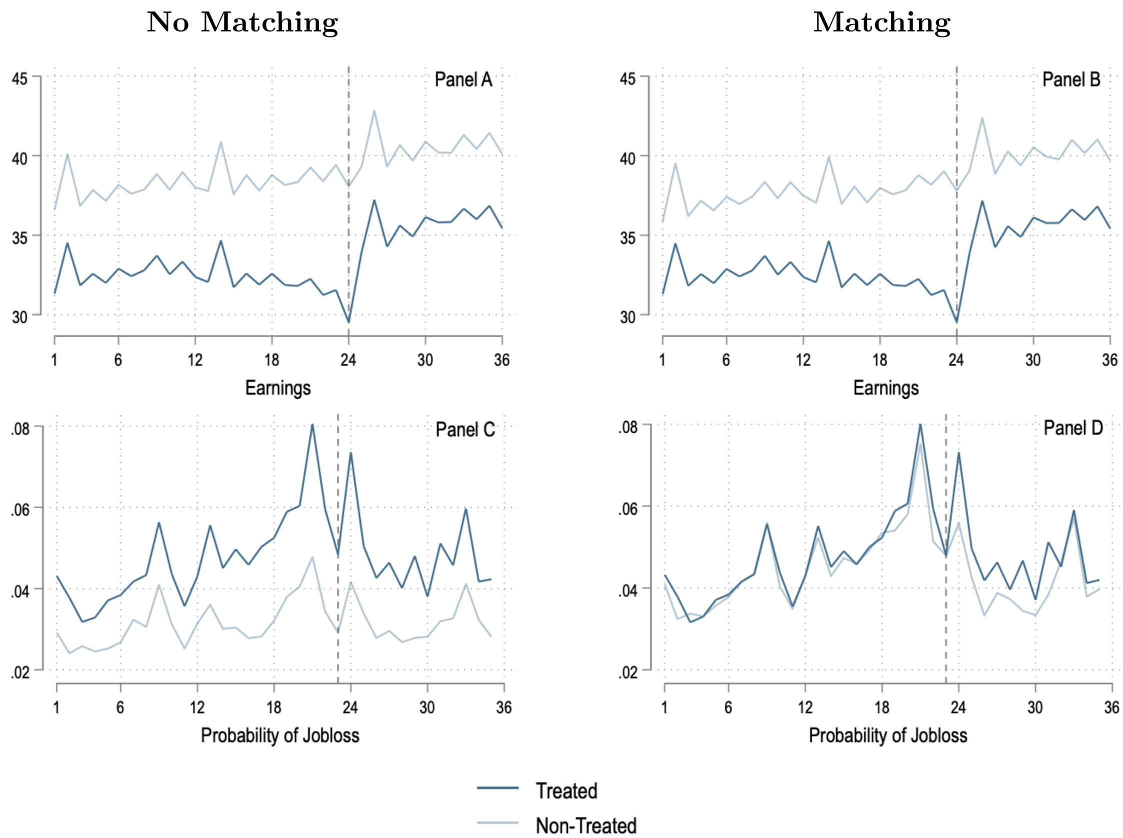


Notes: The figure ilustrates important aspects of the matching procedure. We identify as treated those workers for whom their 2018 wage lies whithin the interval [MW2018 , MW2019] and control those for whom their pre-policy wage is whithin the inverval (MW2019, 1.3*MW2019]. Panel A represents the bias across covariates between the treatment and the control group before and after the matching. Panel B represents the propensity scores distribution for the treatment and control group.

the impact of the policy on the probability of job loss. First, we explore the results for the propensity score matching. As mentioned, we employ the nearest neighbor matching algorithm with replacement. This algorithm chooses for every treated unit the closest individual in terms of propensity score from the comparison group. The replacement allows untreated units to be used more than once as a match. In addition, we allow ties, so more than one match occurs whenever two untreated individuals have the same propensity score. Finally, we impose a caliper to avoid possible bad matches.

We evaluate the most important aspects of this procedure in Figure 6. In Panel A, we show the reduction in standardized bias between the treatment and control groups after the matching. As observed, this procedure remarkably reduces this bias across covariates. According to Caliendo & Kopeing (2008), a percentage bias below 3% or 5% after matching is often seen as sufficient. Here, the majority of our standardized biases lie below 3%, with only two categories above this threshold and, in any case, below 5%. Therefore, we can safely say that our treatment and control groups are balanced. Secondly, the common support assumption requires that treatment units have observations nearby in the propensity score distribution. Panel B shows the propensity score distributions for both groups. As expected,

Figure 7: Trends in Earnings and the Probability of Job Loss before and after Matching



Notes: The figure represents the trends in wages and the probability of job loss for the treatment and the control group before and after the matching. Trends are represented from January 2017 to December 2019. Data is from the MCVL 2019. We identify as treated those workers for whom their 2018 wage lies within the interval [MW2018, MW2019] and control those for whom their pre-policy wage is within the interval (MW2019, 1.3*MW2019]. Panel A and C represent, respectively, the trends in earnings and the probability of job loss without matching. In addition, Panel B and D depict the same trends when matching is applied.

the distribution of the control group is significantly more skewed to the left than the treatment group's. Still, both distributions overlap to a great extent, ensuring a wide common support zone. In addition, this assumption is further guaranteed by the imposition of a caliper, ensuring that no matches occur when propensity scores differ above 0.001.

In Figure 7, we show the evolution of earnings and the probability of job loss for the treatment and control groups before and after the matching. Trends are represented from January 2017 to December 2019. As observed, the improvement in terms of comparability after matching is substantial for the probability of job loss, but no difference is observed in earnings. This is not surprising, given that treatment is a deterministic function of income. Thus, the matching satisfactorily reduces differences in covariates between the treatment and control groups but cannot solve a difference in earnings that is intrinsic to how we define treatment. On the other hand, as mentioned, the probability of losing the job evolves similarly for both groups once matching is applied, which is crucial in our setting.

The first estimator we propose captures the average post-policy difference in the probability of job loss between the treatment and the control group. This parameter is computed using Equation 1, and the results are presented in Table 1. First, we show the results before the matching is applied as a reference in column 1. Naturally, this value cannot be interpreted as the causal impact of the policy, as it is just the result of imbalances in covariates correlated with the treatment. Even so, it provides valuable information on the post-policy *natural* difference in the probability of job loss between the treatment and control groups. We call this parameter the naive estimator, which amounts to 1.4 percentual points. More interestingly, we present the matching estimator in column 4. As seen in Figure 7, Panel D, pre-policy differences in the likelihood of losing the job evolve similarly and show hardly any differences when matching is applied. Thus, we argue that the procedure succeeds and, therefore, the difference after the implementation of the policy can be interpreted as its causal impact. β_1 in Equation 1 captures this effect. We find that the minimum wage reform increased the probability of job loss by 0.51 p.p. (10.5%) for our treated workers, an effect that is consistent with an employment elasticity of 0.5.

Secondly, a Difference-in-Differences model is proposed in Equation 4. This method offers two benefits beyond simply providing an alternative estimate. First, it allows a reassessment of the previous results. A plausible limitation of the above method may be the slight difference in the pre-policy probability of job loss between our groups after the matching is applied. Although this difference is small and particularly close to zero in the last pre-policy period, it raises the concern that we may have overestimated the impact of the policy. Below, we will argue that the combination of a Propensity Score Matching and a Difference-in-Differences method allows accounting for this possibility. Second, it allows providing an estimate of the policy impact on earnings. This was not possible with the previous method since, as explained, the matching cannot eliminate the systematic difference in earnings between the treatment and control groups.

Table 1: The Impact of the Increase in the Minimum Wage on the Probability of Job Loss

	No Matching	Unmatched DID		Matching	Matched DID	
ATT	0.0139***	-0.0012*	-0.0016**	0.0051***	0.0044***	0.0038***
	(0.0006)	(0.0007)	(0.0007)	(0.0009)	(0.0011)	(0.0011)
% Impact	28.7	-2.5	-3.4	10.5	9.2	7.8
Employment Elasticity	1.3	-0.1	-0.2	0.5	0.4	0.3
Time FE	✓	✓	✓	✓	✓	✓
Individual FE		✓	✓		✓	✓
Controls			✓			✓
N Obs.	715729	2100480	2078413	317728	930843	930843

Notes: Data is from our monthly panel database built from the 2019 MCVL. We employ information for the years 2017, 2018, and 2019. In the first column, the sample is restricted to the year 2019 and the result of equation (1) is presented when no matching is applied. Next, columns 2 and 3 present the results for the unmatched DID – equation (2) –. Third, in column 4 the sample is again restricted to the year 2019, and the result of equation (1) is presented when matching is applied. Finally, we present the results for the matched DID – equation (2) – in the last two columns. The Standard Errors, included within brackets, are clustered at the individual level. *** $p< 0.01$, ** $p< 0.05$, * $p< 0.10$.

Figure 7, Panels A and B, depict the evolution of earnings for the treatment and control groups. Moreover, Table A1, in the Appendix, shows the results for the DID before and after the matching. In this case, it is not clear which of the two specifications may be more compelling. In any case, results are similar and suggest that the minimum wage increase raised earnings by a range within 5% and 7% for our treated individuals. These results, however, must be considered with caution, as the parallel trends assumption is difficult to justify in this context. We observe what looks like an anticipation effect during the last months of 2018. Nonetheless, we do observe parallel trends until this point. Thus, we argue that our estimation is valid as a reference, although it must be carefully interpreted.

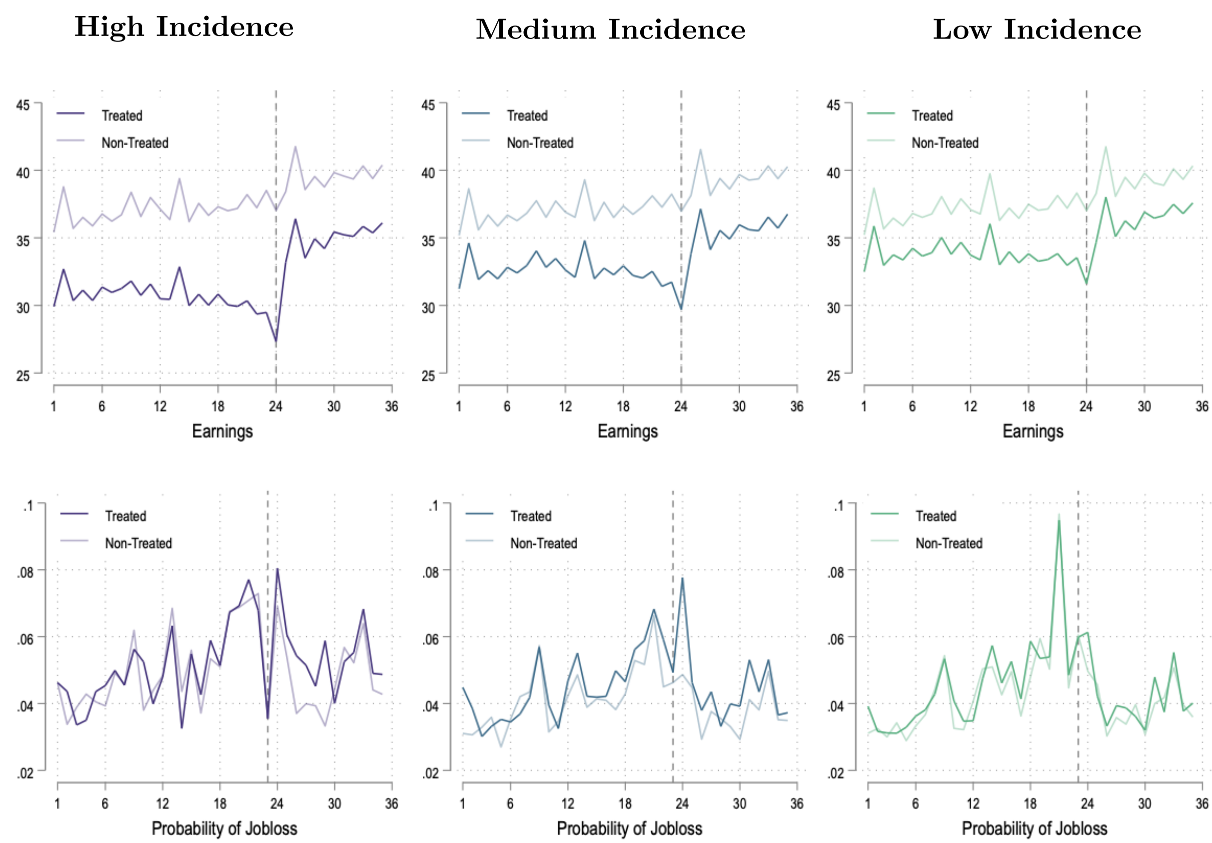
Different is the case of our employment analysis using DID when matching is not applied. As it can be seen in Figure 7, Panel C, the difference in the probability of job loss between the treatment and control groups progressively widens during 2017 and 2018, so the parallel trends assumption does not hold, and the results are not compelling. In this context, as aforementioned, the propensity score matching is successful, creating a comparison group whose probability of job loss is remarkably similar to the treatment group. Thus, the DID setting when matching is applied is our preferred specification. The results for the combination of these methods are presented in Table 1, columns 4 and 5, and suggest that the minimum wage reform increased the likelihood of losing the job in a range within 0.38 p.p. (7.8%) and 0.44 p.p. (9.2%). These estimates are consistent with an employment elasticity between 0.3 and 0.4, which constitutes an effect slightly lower than the one obtained using only propensity score matching. This is because this approach accounts for the slight pre-policy difference in the probability of job loss between the treatment and the control group. In any case, results are similar, and the direction and magnitude of this impact are consistent with the literature – see, for instance, Figure 1 in Neumark & Shirley (2021) –. For the Spanish case, they are also in line with Barceló et al. (2021).

6.3 Heterogeneous Effects

In the previous sections, every individual below the new minimum wage during their last month of work in 2018 is considered equally treated. However, there are important reasons to consider that the intensity of the treatment may vary depending on the distance to this threshold. In other words, the employment effect of the policy is likely to be larger the greater the distance to the new minimum wage. In the first place, this is mainly due to the larger labor costs increase the minimum wage reform entails for these workers. Moreover, this hypothesis is also likely to be mediated by these workers' distribution of covariates. In Figure 6, we show that employees located to the left of the income distribution are younger, more likely to have a fixed-term and part-time contract, work fewer days a month and, ultimately, are more likely to lose their job. In addition, these workers are employed in low-responsibility occupations and sectors with a higher incidence of the minimum wage. Thus, a similar labor cost increase induced by the reform may generate a larger impact on the likelihood of job loss for workers located further to the left in the income distribution. This may occur, for instance, because temporary contracts are cheaper to finish and easier for employers to terminate, as productivity costs related to employees' experience and know-how are lower. On the other hand, however, it is also possible that the labor markets in these sectors and occupations where low-wage workers are overrepresented are more concentrated and, as explained above, this would be a factor pushing in the opposite direction. In this section, we use a similar combination of Propensity Score Matching and Difference-in-Differences employed in the previous exercise to isolate the impact of the increase in labor costs that the minimum wage reform entails, specifically considering differences depending on the distance to the new minimum wage.

We divide our treatment group into three different categories depending on the distance to the new minimum wage. Thus, we call the group of workers located further from the new minimum wage the high incidence treatment group, and the next ones the medium and low incidence groups, respectively. Income thresholds delimiting these groups are set so that they have a similar number of workers – see Figure A1 –. The number of workers per group is 6444. Even so, the number of observations for each group is slightly unbalanced, as high-incidence workers work fewer months a year. Finally, as before, we exclude from the control group those workers just above the new minimum wage, to avoid measurement errors and possible

Figure 8: Trends in Earnings and the Probability of Job Loss before and after Matching



Notes: The figure represents the trends in wages and the probability of job loss for the treatment and the control group before and after the matching. Data is from the MCVL 2019. We identify as treated those workers for whom their 2018 wage lies within the interval [MW2018, MW2019] and control those for whom their pre-policy wage is within the interval (MW2019, 1.3*MW2019]. Panel A and C represent, respectively, the trends in earnings and the probability of job loss without matching. In addition, Panel B and D depict the same trends when matching is applied.

spillover effects.

Table 2: The Impact of the Increase in the Minimum Wage on the Probability of Job Loss

	High Incidence		Medium Incidence		Low Incidence	
	Matching	Matched DID	Matching	Matched DID	Matching	Matched DID
ATT	0.0064***	0.0073***	0.0046***	0.0019	0.0014	0.0008
	(0.0017)	(0.0019)	(0.0013)	(0.0017)	(0.0014)	(0.0017)
% Impact	18.0	20.4	9.4	3.8	2.3	1.3
Employment Elasticity	0.8	0.9	0.4	0.2	0.1	0.1
Time FE	✓	✓	✓	✓	✓	✓
Individual FE		✓		✓		✓
N Obs.	103954	298960	116756	343309	115850	343503

Notes: Data is from our monthly panel database built from the 2019 MCVL. We employ information for the years 2017, 2018, and 2019. In the first two columns, we restrict the sample to the group of workers furthest from the new minimum wage, the high incidence group. In the following two, we only include medium incidence employees and, finally, in the last two columns we only work with the low incidence group. In columns 1, 3 and 5, the sample is further restricted to the year 2019 and the results of equation (1) are presented when matching is applied. On the other hand, columns 2,4 and 6 show the results for the matched DID – equation (4) –. The Standard Errors, included within brackets, are clustered at the individual level. *** p< 0.01, ** p< 0.05, * p< 0.10.

We carry out the propensity score matching procedure for each treatment group. In the appendix, Figure A2 represents the reduction in bias across the treatment and control groups after matching. In Figure 8, we show the evolution of earnings and the probability of job loss for every treatment and its synthetic control group. As expected, the difference in earnings gets smaller as we approach the new minimum wage. Accordingly, the impact of the reform on earnings decreases as they rise – see Table A2 –. On the other hand, series for the likelihood of job loss are considerably noisier than those in Figure 7. Thus, the matching procedure does not present results as solid as the previous ones, probably due to the smaller size of the treatment groups and the greater pre-matching biases in covariates due to the larger distance in earnings among groups. This can be clearly seen in the difference in the amplitude of the x-axis in Figure A2 across incidence groups. Hence, we argue that combining a Difference-in-Difference model and Propensity Score Matching is the most suitable model in this case. Nevertheless, in Table 2, we present the results for this model, as well as those for the simpler propensity score estimator as a reference.

In this table, we see that the impact of the minimum wage reform on the probability of job loss is highly heterogeneous depending on the distance to the established minimum wage. Thus, we find a huge impact of the reform on the group of workers furthest from this threshold. This policy increased their likelihood of losing the job by 0.73 p.p. (20.4%), an effect that is consistent with a surprisingly high employment elasticity of 0.9. On the other hand, we find no evidence of an impact of the policy on the rest of workers in the treatment group. Still, we see a moderate effect in the matching estimator for the medium incidence group. This may arise due to the spike in the probability of job loss that occurs in January 2019, immediately after the implementation of the policy, which would imply that the reform did have a very short-term effect on this second group of workers. Nevertheless, this impact vanishes in our preferred specification when we account for pre-policy differences using DID, which seems especially important for the case of this group of workers – see Figure 8 –.

6.4 Limitations

As aforementioned, the Spanish minimum wage does not show any geographical or legislative heterogeneity in its application that allow the use of methods that exploit this type of variation. Therefore, we focus on incumbent workers. An advantage of this method is that it accurately identifies workers directly affected by the minimum wage. On the contrary, however, it only allows us to estimate the impact of the policy on these workers, so those who were not employed before the reform implementation are left out of our analysis (Dube, 2019a). Furthermore, it is challenging to study the long-term impact of the policy when focusing on incumbent workers, as the age composition of employees changes and the share of employees in the treatment group who are actually treated shrinks (Cengiz et al., 2022). Finally, recent literature has proved the existence of spillover effects above the minimum wage level (Gregory & Zierhan, 2022), which may compromise the stable unit treatment value assumption (SUTVA). We try to deal with this issue by excluding workers just above the new minimum wage from the sample. Nevertheless, alternative methods such as bunching (Cengiz et al., 2019) or matching learnings techniques to identify treated workers (Cengiz et al., 2022) are ways that should also be explored for the Spanish case.

A second limitation of our analysis is the inability to compute the hourly wage, which forces us to exclude from the sample those workers with a part-time contract during the month they perceived the last wage in 2018. Beyond reducing the sample, it prevents estimating the impact of the reform on the number of hours worked, a potentially important margin of reaction for firms in the face of this legislative change. A possible ampliation of this work for the future would be to use matching learning techniques to estimate this missing information following Alhfeldt et al. (2018).

Finally, despite propensity score matching performing quite well in our setting, the parallel trends assumption is difficult to justify in some specifications. Hence, some results must be cautiously interpreted.

7 Conclusions

In this document, we evaluate the impact on employment of the minimum wage increase in 2019 in Spain, a reform that stands out for its magnitude. Traditionally, minimum wages have been one of the most popular economic policy tools to mitigate income inequality and the negative trend in labor income shares. They are often relatively easy to implement and do not require an immediate fiscal effort. However, they also have important potential unintended effects that have received extensive attention in the economic literature. In general, the most common aspect anticipated by the literature is a reduction of labor demand induced by the increase in labor costs that the minimum wage entails. Nonetheless, the impact of this policy is multidimensional, and the list of potential impacts is large. Ultimately, how the effect of the minimum wage materializes will depend on how firms react to the increase in labor costs. This reaction can be channeled through prices, company profits, labor demand, or productivity. Hence, a detailed assessment of all these aspects is necessary to conclude on the convenience of this policy as a tool against wage dispersion and job insecurity. A complete evaluation of the Spanish reform in 2019 is, therefore, beyond the scope of this document, which is only intended to contribute to the debate on employment and provide further evidence on the minimum wage impact on the probability of job loss. Therefore, our results must be considered within these terms, as a necessary but not sufficient element for the evaluation of the reform.

The employment effect of the minimum wage strongly depends on labor market concentration and the point at which it is located in the income distribution. The literature agrees that there is not a unique minimum wage employment elasticity and, therefore, the effect of this policy surely depends on the context and the characteristics of the labor market. Thus, its study essentially involves exploring whether the minimum wage has been set too far in a given context. In particular, above the competitive market wage. In January 2019, the minimum wage rose in Spain from 735.9€ to 900€/month, an increment of 22.3%. As seen in Figure 1, the magnitude of this reform is unprecedented, which implies a greater need for an adequate evaluation before carrying out similar reforms.

The Spanish minimum wage does not show geographical or legislative heterogeneity in its application that allow the use of causal inference methods that exploit this type of variation. Hence, we focus on incumbent workers. We identify as treated employees with a pre-policy wage below the new minimum salary and include those above in the control group. To do so, we restrict the sample around the new minimum wage and drop workers just above this threshold to avoid measurement errors and potential spillovers. From here, we combine Propensity Score Matching and a Difference-in-Differences model to build a synthetic control group that matches our treatment group's distribution of covariates and estimate the impact of the policy on the probability of job loss. We find that the reform increased the likelihood of losing the job within a range of 0.38 pp. (7.8%) and 0.44 pp. (9.2%) for workers below the new minimum wage, which implies an employment elasticity between 0.3 and 0.4. This result is consistent with previous results in the literature and, in particular, with Barceló et al. (2021). In addition, we evaluate how this result changes depending on the distance with respect to the new minimum wage. We find that the bulk of the impact is concentrated in the group of employees furthest from this threshold. These workers suffered an increase in the probability of job loss of 0.73 pp. (20.4%), an effect that is consistent with a surprisingly high employment elasticity of 0.9.

In sum, we find that the minimum wage increase significantly reduced employment, particularly harming those workers further from the new minimum salary. Thus, we conclude that the reform set the minimum wage above the competitive market wage and, therefore, too far attending to an employment maximizing criteria. These results present essential policy implications. Nevertheless, the polyhedral nature of minimum wages implies the existence of additional margins different from employment that must be considered. In this sense, elucidating whether the benefits outweigh the costs is of enormous importance. Therefore, our results must be considered within these terms, as a necessary but not sufficient element for the evaluation of the reform.

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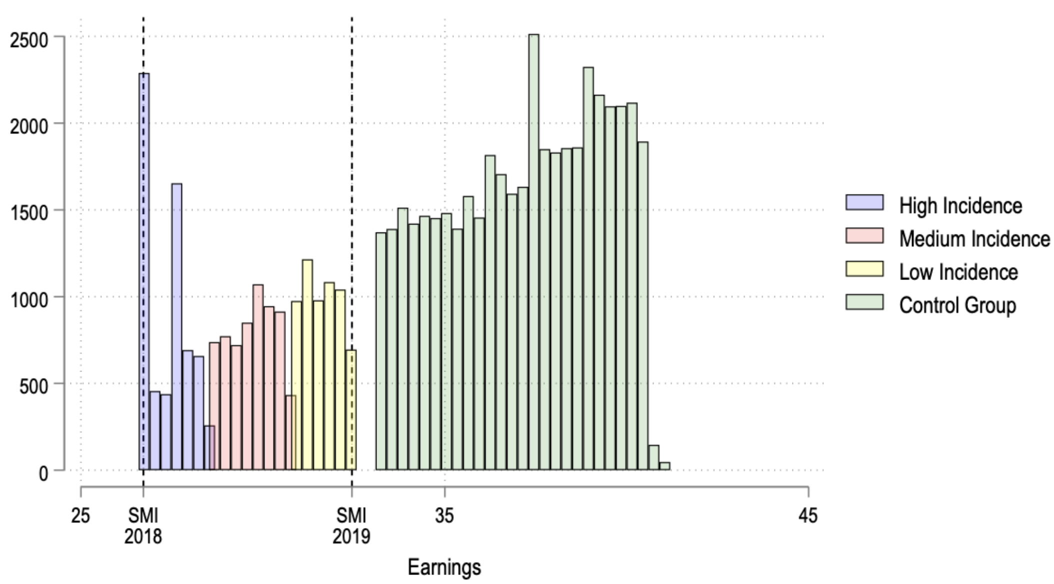
Appendix

Table A1: The Impact of the Increase in the Minimum Wage on Earnings

	Unmatched DID		Matched DID	
ATT	0.0630***	0.0658***	0.0485***	0.0566***
	(0.0023)	(0.0020)	(0.0033)	(0.0028)
Time FE	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Controls		✓		✓
N Obs.	2100480	2078413	930843	930843

Notes: Data is from our monthly panel database built from the 2019 MCVL. We employ information for the years 2017, 2018, and 2019. The table summarizes the results for our DID – equation (2) – before and after the matching. Thus, columns 1 and 2 present the results for the unmatched DID, including two specifications with and without controls. Furthermore, columns 3 and 4 summarize the results for the matched DID, including two specifications that vary in the covariates included as well. The standard errors are within brackets and are clustered at the individual level. *** p< 0.01, ** p< 0.05, * p< 0.10.

Figure A1: Frequency distribution of the last perceived real daily wage for the year 2018



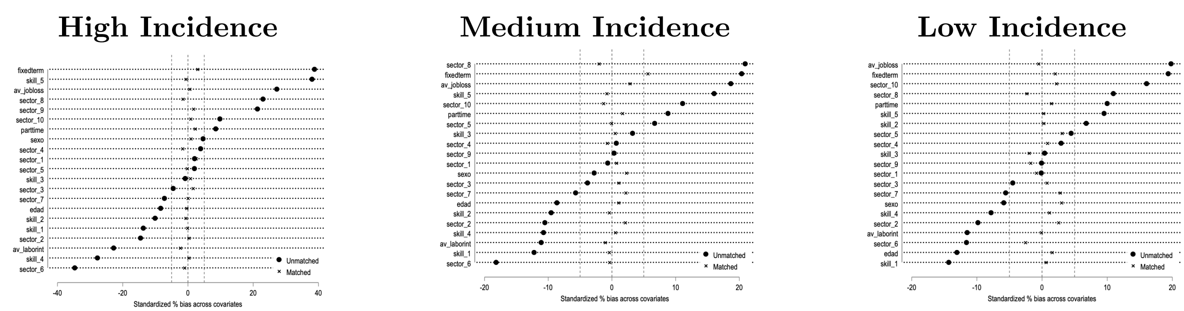
Notes: The figure represents the frequency distribution of the last perceived real daily wage for the year 2018. The graph is depicted following the treatment and control groups defined in the heterogeneous effects section.
Source: MCVL 2019 — Own Elaboration

Table A2: The Impact of the Increase in the Minimum Wage on Earnings

	High Incidence	Medium Incidence	Low Incidence
ATT	0.0871***	0.0442***	0.0285***
	(0.0057)	(0.0050)	(0.0050)
Time FE	✓	✓	✓
Individual FE	✓	✓	✓
N Obs.	298960	343309	343503

Notes: Data is from our monthly panel database built from the 2019 MCVL. We employ information for the years 2017, 2018, and 2019. The table summarizes the results for our DID – equation (2) – before and after the matching. Thus, columns 1 and 2 present the results for the unmatched DID, including two specifications with and without controls. Furthermore, columns 3 and 4 summarize the results for the matched DID, including two specifications that vary in the covariates included as well. The standard errors are within brackets and are clustered at the individual level. *** p< 0.01, ** p< 0.05, * p< 0.10.

Figure A2: Heterogeneous Effects - Reduction in bias between the treatment and control groups after matching



Notes: The figure represents the bias across covariates between the treatment and the control group before and after the matching. We identify as treated those workers for whom their 2018 wage lies whithin the interval [MW2018 , MW2019] and control those for whom their pre-policy wage is whithin the inverval (MW2019, 1.3*MW2019]. Next, we define three treatment groups depending on the distance to the 2019 minimum wage. We call the group of workers furthest from this threshold a high-incidence group, and the other two medium- and low-incidence groups, respectively.