One Minimum Wage to bind them all:

Industries' collective agreements and the NMW in France

Master in Public Policy and Development

Andrea Ibba

Supervisor: Thomas BREDA (PSE)

Referee: Philippe ASKENAZY (PSE)

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Abstract

This exploratory paper studies the effects of an increase in the minimum wage on wages and employment at the industry level in France. To do so, it exploits the system of the collective agreements that generates the coexistence of different binding minimum wage across industries. This setting permits to compare industries that are not affected by an increase of the national minimum wage in a given quarter - as their minimum binding wage lied above the national one - to those that had to raise their minimum wage. The two strategies performed show that an uprate of the minimum wage raises wages in the affected industries. However, the impact on employment remains ambiguous, as the first strategy points to a zero effect whereas the second strategy indicates a small detrimental repercussion.

Keywords: Minimum wage, collective bargaining, France, wages, employment, welfare.

"O frati", dissi "che per cento milia
perigli siete giunti a l'occidente,
a questa tanto picciola vigilia

d'i nostri sensi ch'è del rimanente,
non vogliate negar l'esperienza,
di retro al sol, del mondo sanza gente.

Considerate la vostra semenza:
fatti non foste a viver come bruti,
ma per seguir virtute e canoscenza."

"O brothers, who amid a hundred thousand
Perils," I said, "have come unto the West,
To this so inconsiderable vigil

Which is remaining of your senses still
Be ye unwilling to deny the knowledge,
Following the sun, of the unpeopled world.

Consider ye the seed from which ye sprang;
Ye were not made to live like unto brutes,
But for pursuit of virtue and of knowledge."

Dante - La Divina Commedia, Inferno, canto XXVI: vv. 112-120.

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Lastly, thank you *mamma e papà* for always being my polar stars and giving me the gift of curiosity and the opportunity to widen my knowledge.

I would like to add that any errors or shortcomings are entirely those of the author.

List of abbreviations

APE: Activité principale exercée

BMB: Base des Minima de Branche

BW: Binding wage

CMW: Collective bargained minumum wage

DARES: Direction de l'Animation de la Recherche, des Études et des Statistiques

DGT: Direction Générale du Travail

DiD: Difference in differences

DOM: France d'Outre-Mer

IDCC: Identifiant des conventions collectives

ILO: International Labour Organisation

ILRR: Industrial and labour relations review

INSEE: Institut National de la Statistique et des Études Economiques

LFS: Labour Force Survey

MW: Minimum wage

NACE: Nomenclature statistique des activités économiques dans la Communauté européenne

NAF: Nomenclature des activités françaises and activité principale exercée

NMW: National minimum wage

SMIC: Salaire minimum interprofessionnel de croissance

SMIG: Salaire minimum interprofessionnel garanti

TC: Table de Conversion

TDP: Table de Passage

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1 Introduction

The literature upon the economic effects of a binding minimum wage (MW) is one of the richest and most controversial within the field of economics. Researchers of the last generation have tried to make sense of the sometimes contradictory evidence focusing on both thoroughness of the empirical analysis (taking more seriously endogeneity issues) and alternative theoretical explanations. Different identification strategies, from regression discontinuity designs (Dickens et al., 2011) to difference in differences estimations (Dube et al., 2010), have been employed to assess the causal effect of the MW on the affected workers. Similarly, since the standard neoclassical model was not fully satisfactory in providing explanations for the contradictory evidence, non-standard approaches were introduced (institutional and dynamic monopsony models). Hence, studies started to take into consideration the impact of the MW on the several adjustment channels predicted by the theoretical models (Hirsch et al., 2015). Following an increase in the MW, the neoclassical framework still allows for higher prices to consumers, reductions in non-wage benefits such as health insurance and retirement plans, reductions in training, and shifts in the composition of employment. The institutional model, assuming that firms are not operating at their peak efficiency, allows for an increase in productivity. This is generated by both the employers demanding for more effort given the higher wages paid and the employees being willing to deliver more effort due to the higher wage incentive (efficiency wage). Lastly, the monopsony model predicts that a MW increase actually reduces the costs of turnover to low-wage employers.

The key question raised by the vast literature on the MW focuses on how its employment effects can be reliably identified. Indeed, the identification of the MW effects requires both a focus on potentially affected workers and the construction of a valid counterfactual, so-called “control” group, to proxy what would have happened absent the increase in the MW. The latter is critical to account for other influences on the employment of potentially affected workers that may be confounded with the effects of changes in the MW. In other words, the power of the statistical analysis lies in the reliability of a good counterfactual.

One widely used identification strategy is the so-called difference-in-differences (DiD) that, in practice, looks at how the difference between “treated” and the “control” groups change after the policy has been introduced. It relies on the validity of the common trend assumption: in the absence of the policy (MW) the difference between employment levels, in this case, in the “treated” and “control” group before and after the policy would have been the same, hence any deviation from this common trend represents the effect of the policy. Such “controls” are difficult to find since, as stressed by Dube et al. (2010) and Allegretto et al. (2011), one must compare places that are geographically proximate in order to have valid controls, since MW changes can be correlated with unobserved economic shocks. Another issue concerns the possible regional differences in employment growth that are unrelated to the MW (Dube et al., 2010). Hence, statistical analyses that do not control for this "spatial correlation" in the MW will attribute the better employment performance in low minimum-wage states to the lower MW.

In France, since the implementation of the SMIG in 1950 and its replacement, SMIC, in 1970, there has been the coexistence of two systems of wage floors, the national minimum wage (NMW) and the collective bargained minimum wage (CMW). Whereas the NMW covers between 10% and 15% of the labour force (Fougère et al., 2016), the CMW is the dominant level of wage setting for one third of French firms[1] (Luciani, 2014). The relationship between the two is interesting since, if the trade unions (the workers) and the firms find an agreement on a MW that is above the NMW (SMIC), at the level of the firm or industry[2] the CMW has to be applied rather than the national one (SMIC). Hence, the French case presents a particularity that can be exploited to identify the economic impact of a MW.

The objective of my study is therefore twofold. First, by exploiting the coexistence of the NMW and the CMW I attempt to assess the effects of a national MW on actual wages and employment at the industry level. In fact, this setting allows me to focus on the whole distribution of workers in a specific industry rather than only on youngsters or individuals at the MW, as the previous literature has done. Hence it can be considered as an improvement with respect to the literature both in terms of statistical and identification precision. Statistical precision increases since a larger number of observations will be used[3]. Similarly, this identification strategy allows to exploit the whole spectrum of workers affected by the MW rather than a proxy for people at the MW (young individuals, for instance). Besides, it can offer a good counterfactual if we believe that individuals bound to different wage floors simulate the outcome evolution of the "treated" group better than the people just above the MW. This may be considered as an advancement with respect to previous studies in France such as Abowd et al. 1999, which focuses on youth, and Abowd et al. 2000, which focuses on people at the MW. Second, the analysis contributes to the state of the art of the literature on the MW as, to the best of my knowledge, the setting used represent a novelty. In this work, I am able to observe the effects of a MW increase only on industries' actual wages and employment rates. However, future research may use such setting (supported by more exhaustive firm-level and individual panel data) to investigate the MW impact on other aspects of interest that have not been yet thoroughly analysed, such as firms' national and international competitiveness.

The datasets I will employ to perform the analysis are the French Labour Force Survey and the Base de Minima des Branche[4]. The LFS is a cross-sectional dataset having quarterly data on

[1] 50% of the firms with less than 250 employees.

[2] Note that different wages can be bargained at the firm and at the industry level. The highest is always binding, where two are present. As this study did not have data on firm-level bargaining, the focus will be on industry-level CMW.

[3] This is true only on a theoretical point of view since the study suffers from both statistical power and precision, as it will be addressed in the next sections.

[4] An exhaustive description of the datasets used and the way the final dataset has been constructed can be found in section 3.

individuals. It is the official source used by the government to measure unemployment, employment and underemployment rates according to the International Labour Organization definition. It also provides other important information on the occupations (including their sector code), the activity of women and young people, working time, precarious jobs and the practices of many professions. If needed to, consecutive years of the survey can be integrated creating a panel. Conversely, the Base de Minima des Branche allows me to know which industry is under the NMW, which one is under a collective agreement and when the latter has been bargained. Combining these two datasets I am able to associate each worker in the Labour Force Survey with a sector and collective agreement, identifying the workers (industries) that are affected by an increase of the NMW to those that are not.

In order to assess the impact of a price floor on wages and employment, I will pursue two strategies. The first strategy employs a DiD design to four consecutive quarters having an increase of the NMW occurring at the beginning of the third quarter. Hence, if t corresponds to the policy change, $t-1$ and $t-2$ represent the "before", while t and $t+1$ the "after". This requires a common assumption to hold true - that is - the fact that an industry follows its CMW, rather than the NMW, is not correlated to the evolution of any of the relevant labour market's outcomes. This assumption may be tenable, but it needs to be explored through intuitive thinking (i.e. sectors having a CMW may signal strong unions and vice versa, like in Germany; or that firms are willing to pay more for productivity reasons and so forth) and empirical analysis (statistical comparisons between sectors). So, the analysis will perform conventional methods to render the hypothesis more credible, such as plotting historical employment trends that are similar before the change of the MW and diverge after it. The DiD would be a viable approach for this setting as it can account for biases resulting from idiosyncratic and unobservable characteristics of the two groups under comparison as long as the common trend hypothesis holds. In this case, there are many ways of proceeding with the identification strategy, each having their specific attractive features and limitations. For the first strategy, "treated" and "control" groups are formed as follows:

- The "treatment" group is composed by the industries that in quarter $t-1$ were under the NMW and in quarter t went under a new (higher) NMW; the "control" group comprises those industries that through the four quarters were under a CMW (hence, above the NMW) that did not change.

The first strategy suggests that the NMW increase occurred in 2004 raised, on average, actual wages in the affected industries by approximately 5 percent. It does not find any significant impact on employment of the "treated" industries, which may imply that the NMW can be raised[5] without

[5]Note that an increase in the MW may not have a linear effect. This study treats MW growths that are between 1 to 5 percent in real terms, which can be deemed rather standard increases. Hence, these results may not be generalisable to larger increases.

being detrimental to employment. Conversely, the second strategy suggests that 1 percent increase in NMW leads to 0.16 percent higher increase in average actual wages for those industries that are affected in the quarter with respect to those that are not. Besides, contrarily to the findings applying the first strategy, these NMW increases seem slightly detrimental for employment in the affected industries, as we obtain a negative and significant coefficient.

These inconsistencies in the findings stem from the structural differences between the two strategies. In fact, the second strategy exploits the longitudinal feature of the data and performs the analysis on the whole available period. In this strategy a permanent "treated" and a "control" group do not exist, rather industries are "treated" or "control" in a given point in time. For instance, if industry A is affected by an increase in the NMW in quarter t, while industry B is not, the former will be considered as "treated" while the latter as "control". However, if in the quarter of the next increase in the NMW industry A is not affected (as its minimum wage is now well above the NMW), it will not be considered as "treated" but as "control". Hence, taking a wider perspective, the second strategy compares the industries that raised their binding minimum wage[6] when the NMW increased with those whose binding wage remained unaltered. To do so, it applies a fixed effects estimation to control for all time-invariant characteristics.

The study suffers from several limitations. Overall, the internal validity of the results is threatened by the measurement error arising from linking individuals to their respective industries according to a probability[7]. As far as the first strategy is concerned, the common trend assumption may be violated for both the study on wages and employment, since its evolution does not seem to be similar before the policy change (Figure A.8 in the Appendix). Besides, it lacks of statistical power, as there is a trade-off between implementing its identification and the number of industries in the analysis. A further implication of the first strategy concerns the generalisation of the findings. The two groups of industries that are compared are a small sub-sample most likely not representative of the initial sample of industries. Therefore results are probably local, which means that with this strategy it is estimated a LATE. Regarding the second strategy, any time-variant occurrence, not accounted for by the basic controls, poses a threat to the identification. Furthermore, it is to be determined the external validity of the results. In fact, the paper takes advantage of a very peculiar setting in which different wage floors simultaneously exist. Hence, their very existence and the complex interrelationships between the various collectively bargained wages and the NMW render the study difficult to replicate elsewhere.

The paper is structured as follows. A brief review of the literature on the MW is presented in the next section. Section 3 provides an overview of the institutional setting and its dynamics, i.e. how the NMW and the collective agreements work in France. Sections 4, 5 and 6 examine the different

[6] Note that the increase can either be due to the industry automatically following the NMW (because its coefficient lied below), or the industry deciding to raise it at the same time as the NMW.

[7] Sections 5 and 6 provide detailed information on these probabilities and discussion about the measurement error threat.

dimensions of the data employed in the analysis. In particular, section 4 presents the original data sources focusing on the key information the study drew upon. Section 5 discusses and delineates the methods exercised to link the original sources among each other, critically appraising the issues that such dataset combinations generated. The most worrying of these issues, namely, attenuation bias, is addressed in Section 6. The methodology is presented in section 7, whereas section 8 shows and discusses results from the two strategies. Lastly, the final section outlines the paper's findings and provides some recommendations for future research.

2 Literature review

2.1 A brief history of the minimum wage

For the first half of the twentieth century, invetigations on the MW turned around theoretical arguments (mainly due to lack of data) having two main sides of debate: the neoclassical position and the "economic institutionalism". The former argues that MWs reduce employment and it has been supported by George Stigler (1946) and Fritz Machlup (1946). The second branch, mainly represented by Richard Lester's work (1946), brings forward the theory that product demand rather than wage rates is by far the most important factor determining employment so that fairness was the more appropriate consideration in setting a wage floor.

It was in the second half of the twentieth century, mostly in the US, when empirical research took over in an attempt to clarify the theoretical arguments. Over time, empirical analyses, especially the time-series studies conducted in the 1960s and 1970s, increasingly found that MWs tended to reduce employment among teenagers, who were viewed as a proxy for low-skilled labour. An influential paper by Brown, Gilroy, and Kohen, published in 1982, surveyed the existing literature on MWs and established the "consensus" that a 10% increase in the MW would reduce teenage employment by 1 to 3% (Brown et al., 1982). Following that study, economists began to coalesce around the idea that MWs have adverse effects on low-skill employment.

However, a decade later, in the early 1990s, the publication of a special issue of the Industrial and Labour Relations Review (ILRR) that featured four studies using various analytical approaches and regional disparities yielded diverse findings. Disemployment effects similar to the earlier consensus (Neumark and Wascher, 1992) were accompanied to no effect on employment (Card, 1992a) and even to a positive effect of the MW on employment (Card, 1992b; Katz and Krueger, 1992).

The ILRR and Card and Krueger seminal paper (1994) launched a new body of contemporary research on MW, that came to be known as the "new minimum-wage" generation. Its principal innovations were the use of natural experiments and cross-state variation in the "bite" of the MW.

These researchers measured the employment impact of a single instance of a policy change (an increase in a state or the federal MW) by comparing a group of workers directly affected by the change (teenagers in a state where the MW increased, for example) with a similar group that was not affected (teenagers in a neighbouring state where the minimum did not change). More attention was thereby given to the counterfactual outcome - what would have happened in the absence of the policy. The main results obtained by these studies that were exploiting new methods of analysis were somehow contradictory. On the one side, evidence from many studies rejected a large, negative employment effect of the MW (Card and Krueger, 1995). On the other camp, the detrimental effect of the MW on low-wage workers had been documented by many authors (Neumark and Wascher, 2008).

It is this contradiction in findings that led researchers at the wake of the 21st century to focus on both thoroughness of the empirical analysis and alternative theoretical explanations. Indeed, more rigorous research was performed using different identification strategies, from regression discontinuity designs (Dickens et al., 2011) to difference in difference estimations (Dube et al., 2010), to be able to assess the causal effect of MW on the affected workers. Similarly, since the standard neoclassical model was not fully satisfactory in providing explanations for the contradictory evidence, non-standard approaches were introduced (institutional and dynamic monopsony model). Hence, studies started to take into consideration the impact of the MW on the several adjustment channels predicted by the models (Hirsch et al., 2015). Following an increase in the MW, the neoclassical framework still allows for higher prices to consumers, reductions in non-wage benefits such as health insurance and retirement plans, reductions in training, and shifts in the composition of employment. The institutional model, assuming that firms are not operating at their peak efficiency, allows for an increase in productivity. This is generated by both employers demanding for more effort given the higher wages paid and employees being willing to deliver more effort due to the higher wage incentive (efficiency wage). Lastly, the monopsony model predicts that a MW increase actually reduces the costs of turnover to low-wage employers.

This is the state of art regarding the long-lasting debate on the effect of the MW[8]. The main challenges are represented by a reliable identification strategy and the availability of data to assess the various channels of adjustment.

2.2 Identification comparisons

Why scholars, researchers and policymakers pay so much consideration in properly identifying and appraising the economic effects of the MW? The MW is considered a redistributive policy tool, as it can raise the wages of the poorest workers and close the labour income inequality gap

[8]The literature review covered mainly studies in the US. For studies in Europe, see Bazen and Martin (1991), Dolado et al. (1996), Abowd et al. (1997), Laroque and Saloniè (2002), Manning (2012), Garnero et al. (2013), etc.

that started to augment since the 1980s (Piketty, 2013). In economic terms, reducing inequality and poverty restores efficiency leading to economic growth (Persson and Tabellini, 1994; Ostry et al., 2014). However, it is important to bear in mind that the MW is just one of the policy tools that economists can exploit to improve welfare. Since any redistributive tool have some unintended consequences on incentives and behaviour, for instance, it becomes crucial to compare how the MW performs with respect to other methods, such as reductions of unskilled labour taxes, subsidies financed by progressive labour income taxes or the introduction of a non-distortionary negative income tax. To partially answer the question posed, one of the inefficiencies of a MW may consist in the potential loss in employment: convincingly assessing this eventuality, therefore, would provide essential information for policy-making.

To accomplish this purpose, the key question raised by the large literature on the MW is how can we reliably identify its economic effects. Indeed, the identification of minimum wage effects requires both a focus on potentially affected workers, the "treated", and the construction of a valid counterfactual, called "control" group, to proxy what would have happened absent increases in the MW. The latter is critical to account for other influences on the employment of potentially affected workers that may be confounded with the effects of changes in the MW. In other words, the validity of the analysis lies in the reliability of a good counterfactual.

Probably the most widely used criteria to compose comparison groups to assess the effect of the MW are wage distribution position (Abowd et al., 2000) and age (Dickens et al., 2011 and Fidrmuc and Tena, 2013). Depending on the criteria chosen, the analyses capture and address different economic implications. Identifications based on age comparison normally exploit an institutional feature that generates a discontinuity into the exposition to "treatment". Hence, estimates stemming from this strategy may capture some other coincident change that has impact on labour market outcomes (e.g., benefit increases at age 22), or the substitution in production between workers of different age groups. In addition, the findings would be difficult to generalise as the effects of the MW on the workforce, since young people do not represent the bulk of the low paid (Dolado et al., 2000). In this respect, therefore, such studies are answering to the question of whether MW changes are welfare enhancing for the youngsters. Similarly, strategies comparing a group of workers above the MW with a group following the MW may produce estimates that reflect the substitution effect between more skilled workers, that became relatively cheaper to employ, and less skilled workers.

This paper's identification is based on a comparison between industries, some affected and some unaffected by the policy change. Hence, the estimates may capture competition and substitution effects among industries. For example, if workers' mobility across industries is relatively easy because of compatibility of needed skills, workers may flow towards industries whose MW uprated attracted by higher earnings. The intensity of these flows will depend on the degree of skills substitutability across industries and the magnitude of the MW increase. However, the setting supplies, arguably, an answer that can be more easily generalised. In fact, if the "treated" and the "control" were heterogeneous in their composition of industries and firms, the findings could indicate whether a MW uprate was welfare detrimental or not for the entirety of the sectors affected.

3 Institutional setting

3.1 The national minimum wage in France

Two institutional forces determine wages in France: the statutory minimum wage (NMW) and the collective minimum wage (CMW). The government sets the former, which is the binding minimum wage applying to any worker in France[9]. The latter is collectively bargained in a decentralised manner at the sectorial level. The CMW serves its purpose of minimum wage floor for a particular industry as long as it is set above the NMW, otherwise the latter becomes binding.

In France the NMW was firstly implemented in 1950 with a statutory law under the name of SMIG[10] ("guaranteed inter-professional national minimum wage"). The SMIG was partially[11] indexed to the rate of increase in consumer prices, feature that did not allow it to keep pace with the increase in average gross earnings generated by productivity gains in the post-war period (Husson et al., 2012). It was therefore replaced in 1970 by the current system, the SMIC[12] ("inter-professional minimum growth wage"). The SMIC is adjusted following the legal formula below:

$$\Delta NMW_t \;=\; max(0,\ \Delta CPI_t) + \frac{1}{2} \cdot max(\Delta W_t - \Delta CPI_t,\ 0) + \epsilon_t \tag{1}$$

where ΔNMW_t is the NMW growth in year t, ΔCPI_t is the inflation rate in year t and ΔW_t is the blue-collar's hourly wage growth in year t. Finally, ϵ_t represents an additional increase that the government can decide. Hence, the three indicators upon which formula 1 is based can be summarised as follows:

1. The consumer price index (if inflation rises by more than two percent within a year the adjustment takes place immediately and automatically).
2. Half of the growth in the purchasing power of the blue-collar's gross hourly wage.
3. The government's annual reviews (the *coup de pouce*).

[9] There are four exceptions for which a reduced minimum wage applies: interns (36.29% of NMW); workers in apprenticeship (it varies from 25 to 78% of NMW depending on age and seniority); workers under a contract of professionnalisation (it varies from 55 to 80% of NMW depending on age and seniority); below 18 (varying from 80 to 90% of NMW).

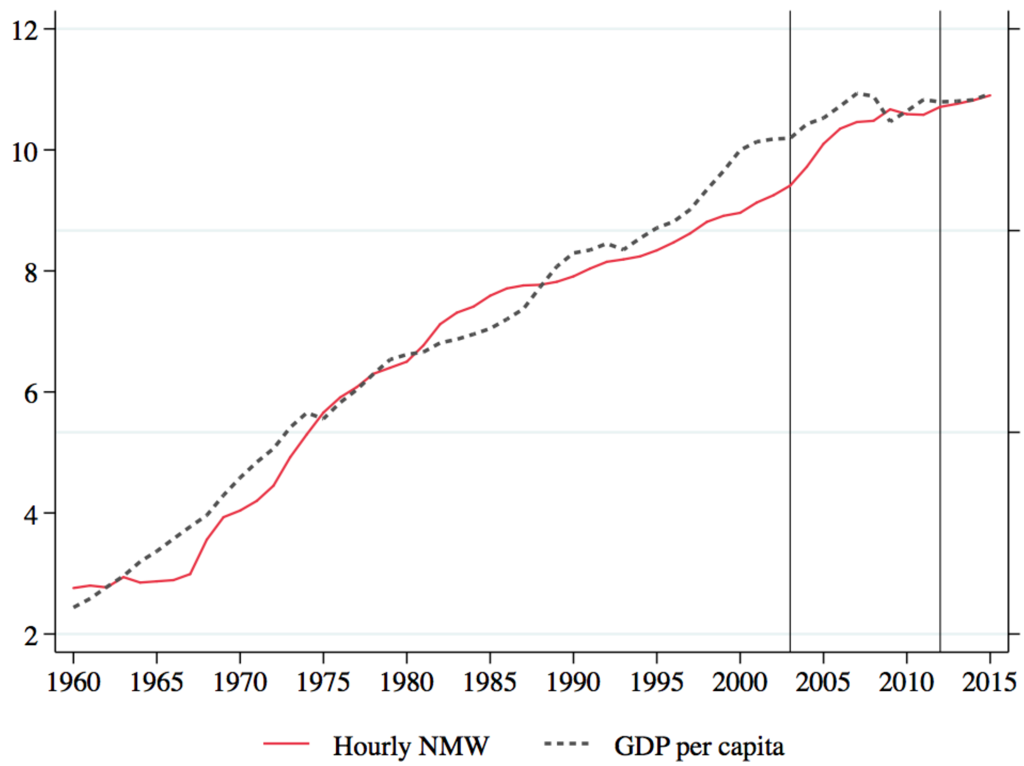
[10] Salaire Minimum National Interprofessionnel Garanti.

[11] The inflation rate had to exceed five percent per year - two percent from 1957 to 1970 - to trigger indexation.

[12] Salaire Minimum Interprofessionnel de croissance.

The SMIC, as the SMIG before, was established as an hourly wage. However, in 1998, as a consequence of the change in the weekly working time from 39 to 35 hours, it was introduced a guaranteed monthly wage.

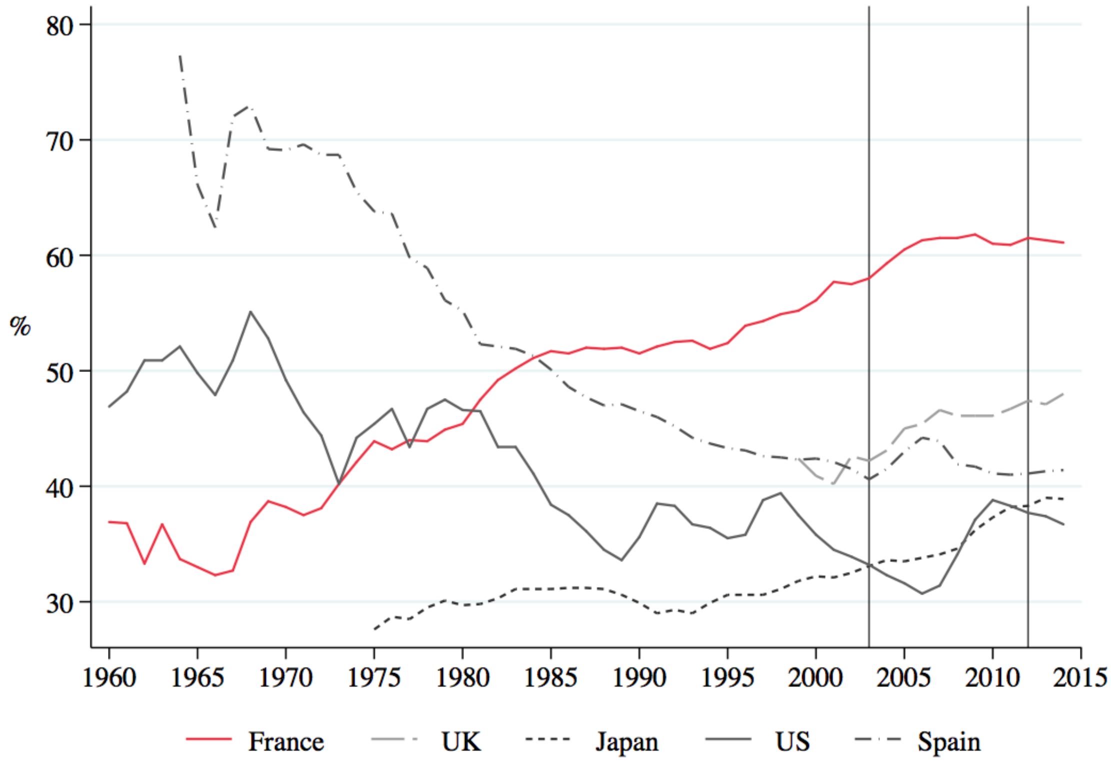
Figure 1: Real hourly minimum wage and GDP per capita



Note: left-hand axis for hourly minimum wages and right-hand axis for GDP per capita (thousands). Both measures are expressed in U.S. dollars, at constant prices and constant PPPs. *Source:* own elaboration based on OECD statistics.

Figure 1 shows the evolution of the real hourly minimum wage and GDP per capita in France from 1960 to 2015 (the two parallel black lines indicating the years of this study). The passage from SMIG to SMIC is clear-cut, as the trend was relatively flat before 1970, although GDP per capita was rising fast. The sharp increase for the NMW that can be observed at 1968, just two years before the introduction of the new system, represents a government's attempt to reverse the trend (Husson et al., 2012). Unexpectedly, the two trends follow the same pattern with GDP per capita leading greater or smaller real NMW increases.

Figure 2: NMWs relative to median wages, across countries



Note: median earnings refer to full-time employees only for all countries and generally include overtime and other supplementary pay. *Source:* own elaboration based on OECD statistics.

Between 1960 and 2015 the French NMW has risen at an average rate of 2.6 percent in real terms, reaching a peak of 19 percent in 1968. How does this compare internationally? In order to make NMW levels comparable across countries, a relevant figure is the minimum wage as a ratio of the median wages of full-time workers. Figure 2 plots this ratio for some of the world larger economies. Despite the fact that these countries' economies grew over the period, their ratio of minimum wages to median wages steadily dropped - it is the case of the U.S. and Spain - or remained fairly stable - as for the U.K. and Japan. Conversely, since the adoption of the SMIC, France presents a trend that flattened out only starting with the recent crisis in 2007. A cross-section of the most recent data on minimum wages as a share of median wages among the OECD countries shows how France has a figure well above the average OECD (the second highest), with the SMIC accounting for a fraction of 62 percent of the French median wage in 2014 (see Figure A.1 in Appendix).

3.2 Collective agreements and wage floors in France

Collective agreements, or collective bargains, refer to a process of negotiations between federations of employers and trade unions (on behalf of the employees) at the level of their contractual industry[13] or the enterprise level. Notice that sorting into contractual industries depends mainly on firm's activity and geographical location[14]. Just around half of the 700 contractual industries present in France cover more than 5,000 workers and frequently bargain on wages (Fougère et al., 2016).

The first institutional framework for collective agreements in France dates back to a law in 1919. However, it was only in 1936 thanks to the Matignon[15] agreement that the system was put into practice (Bernier, 1969). Through the years it has undergone many systematic changes in the reach of its decisional power and its meeting obligations[16]. Today, at the sectorial level, collective agreements negotiate over several aspects of the terms and conditions of employees in their workplace. For instance, they set wage floors and scales, decide working hours, health and safety measures, take action on gender equality and discuss lay-off conditions, training contracts and so forth.

For the sake of conciseness, the rest of the section will consider only wage negotiations. Parties within a certain contractual industry have the institutional obligation to meet at least once a year to bargain on wages[17], but do not need to come up with an agreement. In the case that new wage floors are not agreed upon, old ones remain binding, as long as they are above the NMW. When new wage floors are set, they are extended to all firms belonging to the relevant contractual industry[18] (Fougère et al., 2016). Within contractual industries there are as many wage minimum floors as categories of workers. Workers are in fact assigned a position in a job classification according to various criteria (years of tenure, skills, qualifications and so forth). No one can be paid less than the minimum bargained for their job classification. Although there are several minimums, this analysis concentrates only on the lowest minimum wage floor bargained in a contractual industry.

The NMW has a fixed timing[19] and a predictable magnitude (except for circumstances in which high inflation triggers an automatic rise), as it follows a legal formula. Conversely, increases in wage floors resulted from collective bargains can theoretically occur at any time of the year and can be of any magnitude the parties involved agree upon. Which are the main determinants that affect wage floor adjustments? Some determinants that may play a role in the timing and magnitude of wage adjustments are inflation, unemployment, productivity and actual wages. While the first

[13] "Branches conventionnelles" in French.

[14] Contractual industries may differ from conventional classification of economic activities. This divergence will be addressed in the next section.

[15] The agreement included also a 40 hours weekly working time without loss of pay, paid holidays and social insurance.

[16] The main laws affecting it being the laws Auroux (1982) and law Fillon (2004).

[17] According to article L.132-12 of the labour code (du code du travail).

[18] The Ministry of Labour is in charge of the extension mechanism, which occurs quasi-automatically and it is quickly implemented.

[19] In 2010, the timing of NMW increases in France changed from July 1st to January 1st.

three have been mostly investigated in macroeconomic analyses[20], actual wages particularly apply to a within-industry competitive scenario. Wages' updates in a given contractual industry may be triggered by past actual wage increases that are independent of previous wage adjustments. These actual wage increases may be due to part of the sector experiencing productivity gains or related to exogenous wage increases in the largest firms of the industry (Fougère et al., 2016).

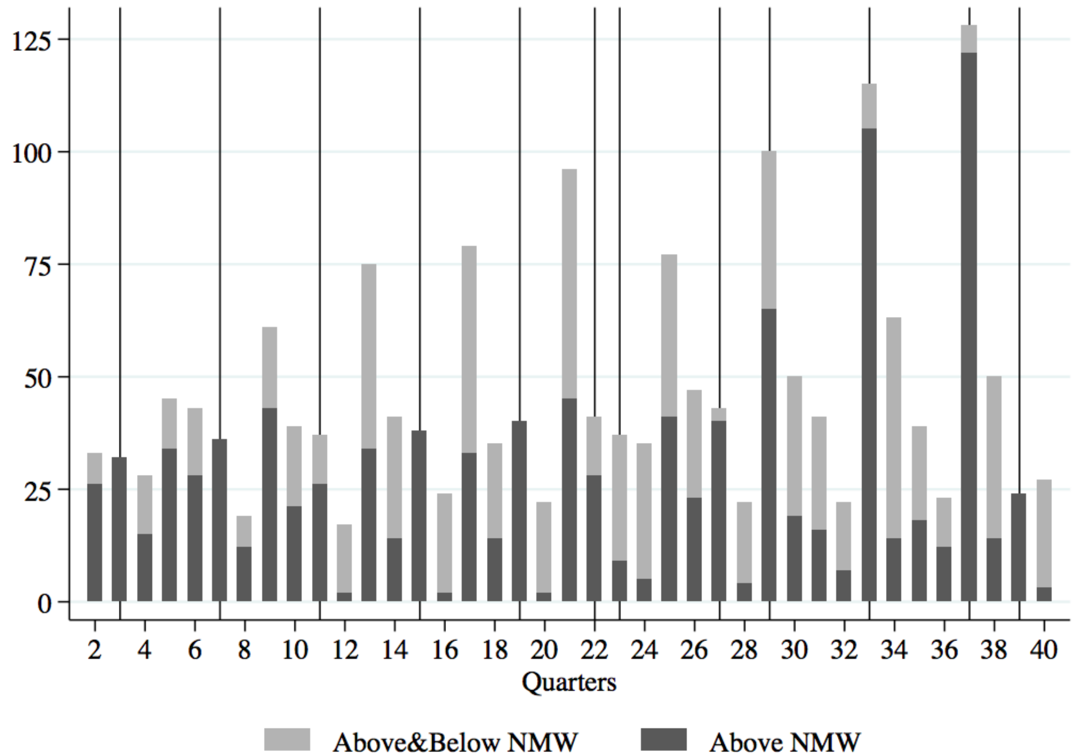
The NMW plays certainly a key role in the decisions made at the contractual industry level. Its impact is twofold. First, it directly affects industries whose lowest collective bargained wage falls below the NMW after the uprating. Those industries will receive pressures from the Ministry of labour to start negotiations and set higher lowest wage floors (Fougère et al., 2016). Besides, the updated wage needs to be at least as large as the NMW to be binding. Hence, this direct impact affects both the timing and the magnitude of wage adjustments.

Second, it influences also those industries whose lowest CMW lies above the NMW through spillover effects. Indeed, if firms compete on wages to attract skilled workers, an increase in the NMW will shorten the differential between low-wage and high-wage firms. High-wage firms will have to increase wages accordingly to keep hiring skilled workers (Manning, 2003). As Grossman (1983) shows, employing an efficiency wage model, the relative wage deterioration of those workers that are not directly affected by the NMW increase may lead them to reduce their effort. If firms want to maintain the same level of effort, they need to increase wages along all the distribution. Also a market mechanism can occur: firms increase the demand for higher skilled workers since they are relatively cheaper after a rise in the NMW. Due to greater demand, higher skill workers' wages go up. Lastly, some research (Falk et al., 2006) points towards the long-lasting behavioural impacts of a NMW increase. Employers may change their perception of a fair wage and employees' reservation wage may rise, resulting in higher wages. Whereas these indirect impacts will affect the magnitude of the wage adjustments, it is not clear whether they impact the timing of adjustments. In fact, spillover effects may not affect homogeneously firms within and across industries. It is therefore likely that only those industries having most of the firms experiencing the abovementioned dynamics will see wage adjustments[21].

[20]Some works include Blanchard and Katz (1999) and Gali (2011).

[21]For a thorough analysis on the interrelationships between the NMW and collective wage bargaining in France, see Fougère et al. (2016).

Figure 3: Industries above the NMW before increasing their minimums and industries below the NMW before increasing their minimums



Note: the left axis represents the number of industries. Quarters start at year 2003 and end at year 2012. First quarter is omitted. Vertical black lines identify an increase in the NMW. *Source:* own elaboration based on the Base des Minima de Branches.

Figure 3 tries to disentangle the NMW effects on the timing of the wage adjustments. A bar as a whole, i.e. independently of its colours, tells us the number of contractual industries that adjust their wages in a given quarter. The plot distinguishes those industries whose lowest CMW lied above the NMW before the wage adjustment took place (dark grey) from to those industries whose lowest coefficient was below the NMW before the wage adjustment (light grey). Two patterns can be observed. First, the former industries, which are not directly affected by the NMW increase, adjust their wages in the last three quarters of the year, between quarter 2 and 22. It is not clear, however, whether this adjustment corresponds to an anticipation of the imminent NMW increase, or rather comes following the NMW increase. In the second half of the period, they seem to follow the timing of the NMW[22]. Second, the former industries, i.e. those directly affected by the NMW,

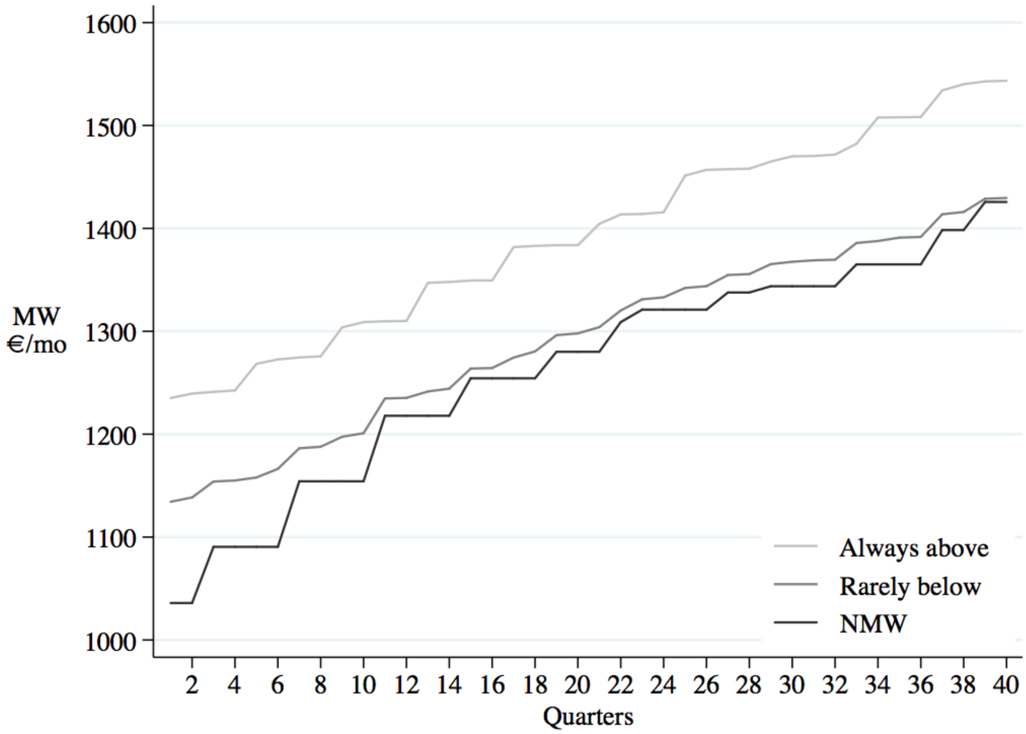
[22]This change is probably due to the fact that the years 2003 to 2006 (quarters 2 to 16) experienced large increases

seldom follow the NMW timing and their timing appears fairly homogeneous within a given year. The first pattern suggests that industries above the NMW may have more precise and planned bargaining system than industries that systematically fall below, but it does not seem to tell us about spillover effects on timing. Conversely, the second pattern may show the Ministry of Labour's pressure-effect to uplift the wage floor on those industries that fell below the NMW.

Over the years considered in this study, we can observe a wide spectrum of contractual industries lying below or above the NMW. In particular, the group of industries whose minimum coefficient lies always above the NMW accounts for 11 percent of the sample, the group whose coefficient rarely falls below the NMW (less than 10 times) represents 16 percent of the industries, whereas those industries whose coefficient occasionally falls below the NMW (between 20 and 30 times) constitutes the larger part of the sample being 59 percent of the industries. Lastly, those industries whose minimum coefficient almost always falls below the NMW (more than 30 times) account for 13 percent of the sample (see Table A.1 in the Appendix for more details).

in the NMW. Subsequent years are subject to relatively smaller increases, making it easier for industries to comply with the NMW.

Figure 4: Nominal monthly minimum wages evolution over the period



Note: the averages for industries 'always above' and 'rarely below' the NMW are constructed using their minimum binding wages in a given quarter, weighted for the number of workers covered by their collective agreement. MW are expressed in gross values. *Source:* own elaboration based on the Base des Minima de Branches.

Independently of whether the contractual industry has a minimum wage that is systematically higher than the NMW or not, the evolution of its wage adjustments closely resembles that of the NMW (Figure 4). It is now possible to observe how those industries that are not directly affected by the NMW increase respond to it by raising their lower coefficient. These increases seem to be lagged with respect to the timing of the NMW increase, which is unexpected since their minimum coefficients are well above the NMW.

4 Data description

The analysis relies on the interaction between two datasets: the Base des Minima de Branche (henceforth BMB) and the French Labour Force Survey (henceforth LFS). The core idea of the paper is to exploit the information on the contractual industry (in particular the timing of wage adjustments) present in the BMB to then identify in the LFS those workers belonging to an industry directly (or indirectly) affected by a given NMW increase.

4.1 Base des Minima de Branche (BMB)

The BMB[23] is an administrative panel dataset containing all industry-specific scales of wage floors for 275 French industries (covering today more than 80%[24] of private sector's workers) comprising more than 2,000 workers over the period 2003-2014[25], for a total sample size of 41,277 observations. For any industry, there are quarterly data on four collectively agreed wage coefficients, both minimum and maximum (one per socio-economic category: blue collar, employee, intermediate profession and white collar). In the dataset, it is indicated the IDCC[26] number for the contractual industry, the title of the collective agreement, the level of the wage[27] coefficients, the number of workers covered by the collective agreement, the date the wage adjustment was bargained and in which it became effective and other related information (see Table A.1 in Appendix for a summary of descriptive statistics).

The BMB provides data on the status of a contractual industry with respect to the NMW (i.e. directly affected or not). In fact, by focusing only on the lowest CMW it is possible to monitor any movement of a particular contractual industry, giving the opportunity to determine A) whether in a specific quarter the industry adjusted its CMW, B) whether it followed the NMW (i.e., its CMW fell below the binding NMW) or its own coefficient. Finally, any collective agreement can negotiate up to three different types of salaries: monthly guaranteed, annual guaranteed and hierarchical. Since the hierarchical salary is the closest one to the NMW[28], it has been the benchmark for comparison in the analysis. In the cases in which the hierarchical wage is not available, monthly and annual guaranteed salaries have been employed instead[29].

[23] Result of the joint effort between the Ministry of Labour, the Direction de l'animation de la recherche, des études et des statistiques (Dares) and the Direction Générale du Travail (DGT).

[24] *Source:* own elaboration using Insee data on total employment in France and BMB data on covered workers.

[25] The relevant years for the analysis are between 2003 and 2012. This is due to technical problems in using the years 2013 and 2014 of the LFS, as I will explain in the next subsection.

[26] In French, "Identifiant Des Conventions Collectives".

[27] Wages are in gross monthly or yearly figures (35 weekly hours base).

[28] As specified in the official BMB's data description.

[29] These differ from the hierarchical in that they may include bonuses related to seniority or special working conditions. They have been applied to roughly 52% of the sample of industries.

4.2 Labour Force Survey (LFS)

The main dataset used in this paper comes from the French LFS. It is exploited to have individual level data on wages and employment, which are the final outcomes of the analysis. The LFS is a national household survey conducted by the French national institute of statistics and economic studies (Insee), representative at the national level[30]. It is the official survey to provide employment figures in France and the only source that allows labour international comparisons, following the concepts of the International Labour Organization (ILO). It offers information on the characteristics of individuals inside and outside the labour market, including the spheres of demographics, education, labour conditions, income, shelter, etc.

The LFS presents a sample of roughly 57,000[31] households, which are interviewed for six consecutives quarters. Every member of the household above 15 years old is followed, provided he or she does not move during the period[32], and assigned a household identifier and a within-household number. The interaction between these two variables provides a unique identifier for each individual. If contingent quarters are pooled together using the identifier, the LFS can become a small panel. Finally, one sixth of the sample is drawn anew each quarter.

Insee does not provide figures for net monthly or hourly NMW for the years before 2005[33]. Although for comparison reasons it would be useful to have a measure for gross wages of workers, the LFS does not have a precise variable for gross monthly earnings[34]. Only net monthly wages are therefore used in the analysis. In addition, from the moment that Insee does not dispense an official figure for the net monthly NMW before 2005, available gross NMW figures have been converted into net applying the 2005 share of non-wage labour costs to previous years[35]. Note, also, that the questions on earnings are asked only twice to each individual, once in the first quarter and a second time in the last quarter of the survey. Hence, regarding wages, not only the sample is smaller but also adjacent quarters become repeated cross-sections. The LFS exercises the same definition of employment as the ILO, so the employment status in quarter t is equal to 1 for all individuals who worked for pay for at least one hour in the reference week, 0 otherwise.

Each worker is assigned a code APE or NAF[36]. Insee attributes this code to groups of enterprises purely for statistical purposes. Even if the structure of the grouping may resemble sectors or

[30]The geographic are it covers is mainland France (France métropolitaine). It exists an independent survey for the overseas Departments (DOM).

[31]These numbers are based on the LFS 2012, which is the most recent year used in the analysis. As the coverage of the LFS changed across years, previous surveys have less interviewed.

[32]If the household moves, the new household that will live in the house will be surveyed. Hence, the surveyed unit is the physical household.

[33]This is due to the change of the weekly working hours from 39 to 35.

[34]'Salmee', which is the closest one, can include both gross and net figures independently.

[35]Since for 2005 I know both the net and gross monthly NMW, I extrapolate the share and apply it to years 2004 and 2003.

[36]Standing for 'nomenclature des activités françaises and activité principale exercée'.

industries (which is, in fact, the objective), the code APE does not represent a contractual industry identifier, which is the IDCC number. Hence, to link the information obtained in the BMB dataset on contractual industries with the sectors in the LFS, we need to use a third source that converts the APE codes into IDCC number (this key passage is addressed in the next section). Besides, in 2008 Insee renewed the nomenclature of the APE codes to make them conform to European standards[37]. This change complicates the conversion from APE codes to IDCC numbers, as it involves a further transformation (i.e. for years before 2008: old APE to new APE to IDCC).

The LFS provides other useful information that I include as controls in my regressions such as age, gender, region, years of tenure and education. Additionally, thanks to detailed variables on part-time, internships, training[38] and apprenticeship contracts, I was able to construct[39] a NMW line below which workers should not be legally paid.

5 The sample

Table 1: Datasets employed and their interactions

	LFS	Conversion Table	BMB
Key information per dataset	• Individual info • APE code	• APE to IDCC • Probability	• Minimum wage floors per IDCC • Timing of wage adjustments

	Dataset	Merging variable		Merging variable	Dataset		Dataset
1.	LFS	APE	+	APE	Conversion Table	=	Merged LFS
2.	Merged LFS	IDCC	+	IDCC	BMB	=	**Complete LFS**

Source: own elaboration.

The final sample of my analysis is the result of a double dataset matching (Table 1). Firstly, the

[37] Nomenclature NACE, which is the statistical classification of economic activities in the European community.

[38] 'Contrat de professionnalisation'.

[39] These occupations are exceptions to the NMW. Depending on age and years of tenure they may be entitled to different minimums. So, I have constructed a grid to apply to these occupations their binding NMW. To produce the grid I used the following links. Internships: https://www.service-public.fr/professionnels-entreprises/vosdroits/F32131. Apprenticeships: http://travail-emploi.gouv.fr/formation-professionnelle/formation-en-alternance/article/le-contrat-d-apprentissage. Training: https://www.service-public.fr/particuliers/vosdroits/F15478.

joint LFS dataset from 2003 to 2012 is merged to a table that probabilistically converts the APE codes into their corresponding IDCC numbers. Secondly, the "merged LFS" is further matched with the BMB. From this last merging is produced my final dataset, called for the rest of the paper either final sample or complete LFS.

The complete LFS covers 10 years of data, from 2003 to 2012 (therefore comprising 40 quarters). One direct consequence of the double matching, regards the sample size. In fact, the latter would have, in its entirety, over 10 millions observations (this because observations are duplicated for as many IDCC possibilities they have, given the APE, as it is explained in the next subsection). Managing this amount of data resulted difficult for the software available and, consequently, the sample was restricted to only the duplicates having a probability of belonging to a given IDCC at least equal to 0.05. After the restriction, the sample counts half of the observations (i.e. almost 5 millions).

A second consequence concerns the number of contractual industries in the sample. The merged LFS has over 350 different contractual industries, whereas the BMB reports information on 275 industries. The final sample comprises a total of 264 contractual industries on which it has the full set of information[40], 144 contractual industries coming from merged LFS that did not find the correspondent IDCC in the BMB, and 16[41] contractual industries present in the BMB that did not match with the merged LFS (see Table A.2 in the Appendix for descriptive statistics). As I do not know the timing or the magnitude of the minimum wage adjustments in the 144 contractual industries not matched with the BMB, I will exclude them from the analysis, otherwise specified. When I drop those contractual industries from the analysis, the sample size is reduced to 4,006,985 observations.

Lastly, when I perform the analysis only on certain quarters, as I am not constrained by sample size, I am able to keep all the duplicates (i.e. the duplicates having a probability below 0.05 are not dropped). Besides, before computing any kind of analysis (wages or employment), individuals reporting a figure for the net monthly wage that is three standard deviations[42] higher or lower than the logarithm of the median net monthly wage in the quarter have been dropped, as they have been considered outliers.

The next subsection explains and discusses the several issues, assumptions and implications that I had to make in order to construct the complete LFS.

[40] That is, each source (LFS, conversion table and BMB) conveyed the relevant information for the study.

[41] Note that these numbers do not add up because some industries, such as that of the "Bâtiment ouvriers", fall under the same IDCC number but have different minimum wages according to their region. In the analysis, when they are treated together, we will have 224 industries, otherwise 264.

[42] Taking three standard deviations can be deemed rather safe. In fact, in a normally distributed variable, 99.73% of the values should lie within three standard deviations around the mean. Formally, $Pr(\mu - 3\sigma \leq x \leq \mu + 3\sigma) \approx 0.9973$, where x is an observation from a randomly distributed random variable, μ is the mean of the distribution and σ its standard deviation.

5.1 Linking the BMB with the LFS

The final sample of the analysis is restricted in length by the reach of the BMB, which spans from 2003 to 2014. Besides, the LFS underwent structural changes in 2013, making it difficult to match years after 2013 with previous years. Hence, the final sample ranges from 2003 to 2012 both included. Now, to perform the study, information on contractual industries (from BMB) need to be conveyed to the LFS. If the latter dataset had a variable appointing to workers the number of their collective agreement (IDCC), a simple match between the BMB and the LFS would be enough.

However, there are two main hindrances. First, the LFS does not have an IDCC number. It has an APE code that can be converted into its corresponding IDCC number using a third source, which is called "Table de Passage[43]" (henceforth TDP). In the TDP, a code APE does not correspond to a unique IDCC number and vice versa. In other words, a given APE code can be linked to multiple potential IDCC numbers and, similarly, a specific IDCC number can correspond to many APE codes. The TDP provides us with the share of workers in a given APE code that belong to a given IDCC number. If we aggregate all the shares belonging to a given APE code, they add up to 1. This measure is essentially a conversion probability from a code APE to an IDCC number. It can therefore be interpreted as the probability of an industry belonging to a specific contractual industry (IDCC) given the APE code assigned in the LFS. This share can be formalised as the following conditional probability, $Pr(IDCC \mid APE)$.

Table 2: Probabilistic conversion between APE and IDCC

		Share
– APE 1	IDCC 1	20%
	IDCC 2	80%
– APE 2	IDCC 1	40%
	IDCC 2	60%

Source: own elaboration.

In the TDP, a given APE code can be linked from a minimum of 1 to a maximum of 40 distinct IDCC numbers (the average being about 7 IDCC numbers per APE code)[44]. Although simplified, Table 2 serves as fictitious example to understand the dynamics of the conversion. It tells us, for instance,

[43] Insee provides these "Conversion Tables" online.

[44] Figures do not change from Table de Passage 2009 to Table de Passage 2012.

that 80% of the workers being assigned in the LFS to a code APE 1 belong to the contractual industry having as identifier IDCC 2. Equivalently, we can say that the probability of the workers coming from a code APE 1 that belong to IDCC 2 is 0.8, or $Pr(IDCC\ 2 \mid APE\ 1) = 0.8$.

Second, since the new APE codes adopted in 2008 have a different nomenclature (passing from 4 to 5 figures), there is the need to use a TDP for years before 2008. To the best of my knowledge, this TDP does not exist making necessary a double conversion. First, old APE codes are converted into their corresponding new APE codes, employing a "Table de Correspondence[45]" (henceforth TC). Then they are linked to their given IDCC number through the TDP[46]. Note that the same problematic as before arises: an old APE code does not correspond to a unique new APE code and vice versa. The TC is therefore provided with the share of workers in a given old APE code that belong to a given new APE code. As for before, we can read this share as a probability, formally $Pr(APE\ new \mid APE\ old)$. The possibilities are lower than the TDP (see Figure A.2 in the Appendix for a visual comparison): a given old APE code can be linked from a minimum of 1 to a maximum of 14 distinct new APE codes (the average being about 2 new APE codes per old APE code).

Table 3: Probabilistic conversion between APE old, APE new and IDCC

		Share TC		Share TDP	Combined (1)	Combined (2)
– APE old 1	– APE new 1	20%	IDCC 1	30%	6%	6%
			IDCC 2	70%	14%	30%
	– APE new 2	80%	IDCC 2	20%	16%	
			IDCC 3	80%	64%	64%
	First conversion		Second conversion		100%	

Source: own elaboration.

For years before 2008, the share of workers in a given new APE code that belongs to a given IDCC number needs to be adjusted accounting for the share of its old APE code. In other words, we need to multiply these two conditional probabilities. This tree situation is explicated in Table 3. Following this fictitious example:

$$Pr(APE\ 1 \mid APE\ new\ 1) \neq 30\%, \text{ rather it will be}$$
$$Pr(IDCC\ 1 \mid APE\ new\ 1) \cdot Pr(APE\ new\ 1 \mid APE\ old\ 1) = 0.3 \cdot 0.2 = 6\%$$

[45] Insee provides these "Corresponding Tables" online.

[46] The TDP used is the oldest available, TDP 2009. By doing this, we are so assuming that the number of workers covered by a given APE code before 2008 is similar to that APE code in 2009. This hypothesis may not be tenable if events occurred in between, such as the 2008 financial crisis, changed structurally the demographics of industries in France.

The adjusted probabilities are therefore those listed in column 1 (Table 3). In some cases, an old APE code that split in several new APE codes can end up belonging to the same IDCC number. As it is shown in column 2, we can add up the probabilities and remove an option.

5.2 First merge: LFS and TDP

Due to these two issues, after merging TDP with LFS the individuals in the sample are duplicated as many times as there are IDCC possibilities given their APE code[47]. Each duplicate differs only in the contractual industries he or she is assigned to and in the probability of belonging to that industry. In order to be more confident that the worker actually belongs to the given industry, in some specifications I may restrict the sample to only those workers above a certain probability threshold, or I may just keep the duplicate having the highest probability for an individual. It is therefore crucial to know what happens to the sample if I want to increase the precision of the contractual industry assigned to a worker. Let us imagine, for example, that I decide to keep only observations having a probability of 0.8. I would lose from the sample all the individuals belonging to APE codes that do not have a share of workers as large as 80% employed in a particular IDCC. This loss therefore depends on the density of the number of possibilities that a given APE code has[48], the incidence of the probabilities for each possibility[49] and the level of the threshold chosen. To have an idea of it, I performed two checks that take into account these three dimensions: first, what I call an *ex ante study* and, second, an *ex post study*.

5.2.1 Ex ante study

In the LFS, every worker is duplicated for as many IDCC numbers his or her APE code is linked to. Before assessing how many workers I will lose by raising the threshold of the probability, I am able to conduct the same exercise directly using the APE codes duplicated for any IDCC number in the TDP. This *ex ante study* is helpful as it shows beforehand what I should be seeing in the LFS if the latter had all the information of the TDP. Specifically, I am interested in plotting the evolution of the parameter:

$$\frac{x(Pr)}{N} \cdot 100$$

Where N represents the total sample size of APE codes, excluding duplicates; and $x(Pr)$ is the total number of APE codes I still have given a specific probability threshold. So, N is fixed and x

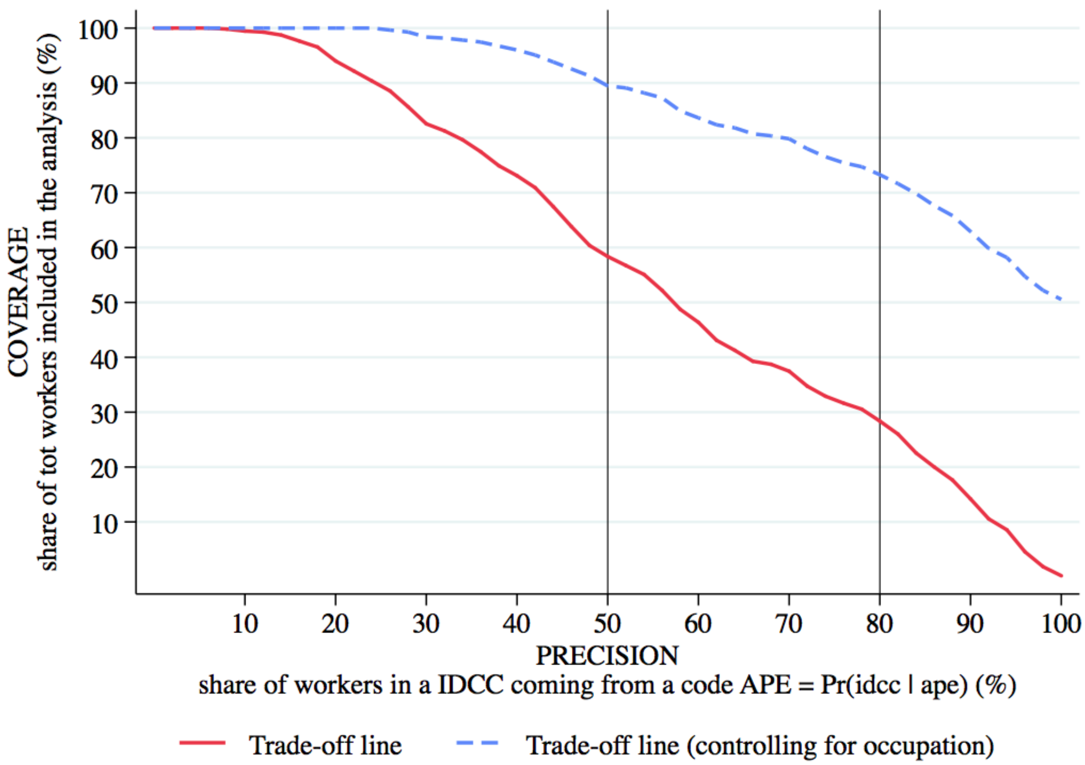
[47]Individuals who are inactive or unemployed do not have an APE code. Hence, they are not duplicated.

[48]Figure A.2 in the Appendix.

[49]Imagine the following scenarios. A) APE 1 is linked to ten IDCC numbers, with an equal probability of 0.1. B) APE 2 is linked to nine IDCC numbers with an equal probability of 0.01 and to one IDCC number with a probability of 0.91. To decide which threshold to use, it is important to know whether it is more prevalent scenario A) or B).

varies with the threshold.

Figure 5: Coverage precision trade-off using TDP



Note: dashed line constructed keeping APE codes constant for any level of probabilities when the IDCC description gave us information on the professional categories. *Source:* own elaboration based on Table de Passage 2012[50].

The red line in Figure 5 can be thought of as a trade-off line since it shows how many individuals are lost (or how much coverage is lost) the more the threshold of our probability is increased, which represents the precision of the analysis (i.e. the higher the probability, the more likely that the worker is actually belonging to that IDCC). Hence, Figure 5 suggests that if we choose as a threshold a probability equals to 50%, we would perform the analysis on approximately 60% of the population, losing 40% of it. Similarly, if we were to set the threshold at 80%, we would perform the analysis on roughly 30% of the population. We can also observe that at very high threshold of probability, i.e. 95%, the population we would perform the analysis upon converges quickly to zero. The dashed line represents the same evolution, assuming that in the LFS we are able to control for occupation. In other words, sometimes it is possible to find in the description of the IDCC number

[50]Using TDP 2012 or 2009 produces virtually identical plots.

a broad indication of which professional category[51] it covers. As in the LFS workers have reported their professional category, I can assign with certainty[52] the belonging to a given IDCC number when worker's profession and IDCC description match. The dashed line, therefore, shows how the coverage precision trade-off improves matching for occupation.

5.2.2 Ex post study

Now, we turn to observe the relationship between precision and coverage after I merged the TDP with the LFS. Due to the size of my sample at this stage, I restrict the analysis to the first quarter of 2012[53]. So, N is the total sample size[54] in the first quarter of 2012, excluding duplicate workers. As we raise the probability threshold, $x(Pr)$ decreases by one for any worker that completely disappears from the analysis. In other words, we do not lose a worker until all of his or her duplicates have an assigned probability lower than the chosen one (following, therefore, the same rationale as for the *ex ante study*).

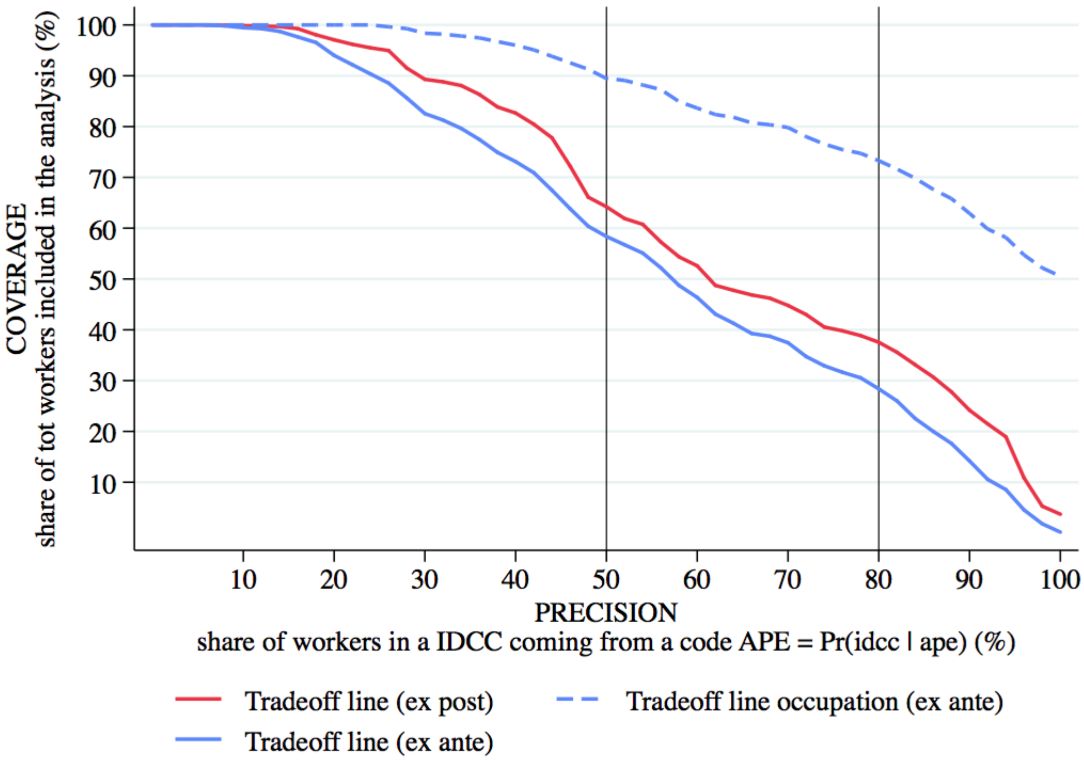
[51]The categories are: blue collar, ETAM, employees and white collars.

[52]Independently of the probability.

[53]The year was chosen to have this study comparable with the ex ante study. The choice of the quarter is arbitrary. The ex post study has been conducted also on various other quarters in other years, but the results are fairly similar. So, the figure shown is representative at the sample level.

[54]This analysis is restricted only to employed individuals in the quarter.

Figure 6: Coverage precision trade-off using “merged” LFS



Note: both dashed line and red line constructed controlling for occupational category. *Source:* own elaboration. The data on the ex ante study come from Table de Passage 2012; the data on the ex post study come from the Labour Force Survey 2012.

Figure 6 shows that the ex post trade-off line controlling for occupation improves with respect to the ex ante line not accounting for occupation, but it falls significantly below its respective ex ante line. This is due to two reasons. First, the ex ante line assumes that, whatever occupation is specified in the TDP, a perfect matching is possible. However, in the LFS only a small proportion of workers have their professional category reported[55]. Second, the ex ante line further assumes that a worker is linked to only one IDCC with occupation, while it can happen that several IDCC present the same occupation, making impossible to perfectly match. We are finally able to quantify the sample loss that occurs when we want to increase the precision of our analysis: if the analysis is performed only on those who are 80% likely to actually belong to their IDCC, the sample loss would be roughly 60%; while a threshold of 50% would allow the analysis to be conducted on around 65% of the sample.

[55] In the LFS 2012, for instance, only about 20% of the workers reported their category.

6 Measurement error

Since belonging to a given contractual industry determines the treatment status, i.e. whether the worker is in an industry that raises its lowest wage floor, not having a precise industry assignment generates a source of bias[56]. As we have a probability of being in an industry raising the lowest wage floor instead of a certain assignment to that industry, our independent variable for the treatment status presents measurement error.

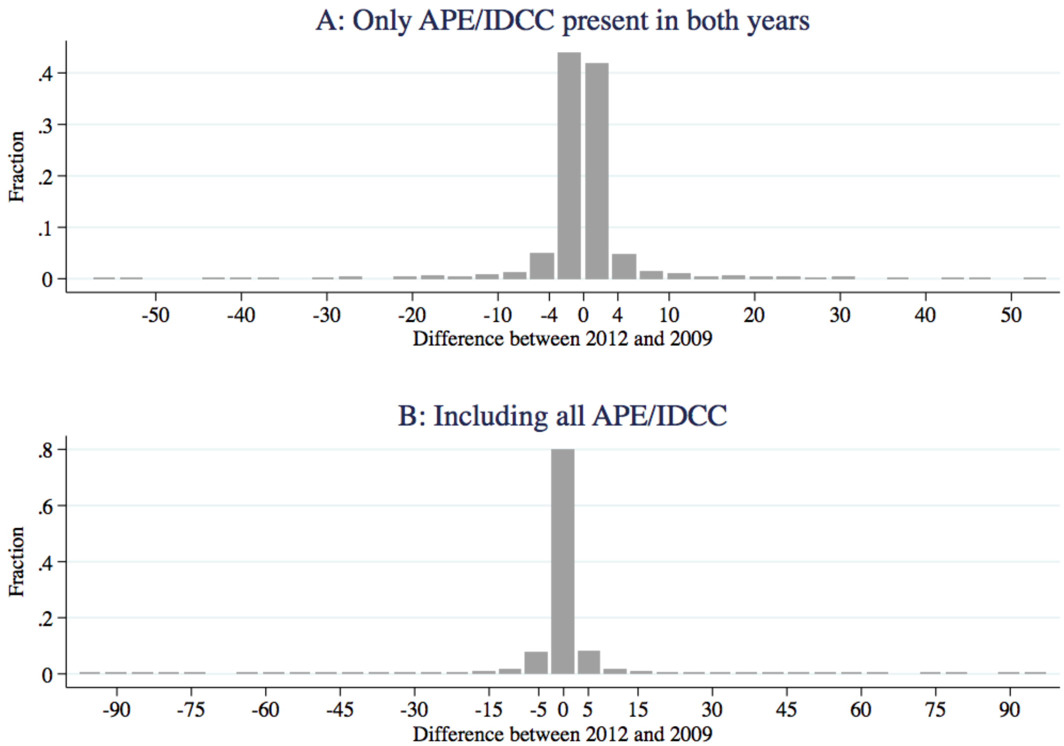
There are two types of measurement errors: classical and non-classical. Classical measurement error arises when we believe that the classical errors-in-variables (CEV) assumption (i.e. the measurement error is uncorrelated with the unobserved explanatory variable) is tenable. In the presence of classical measurement error, the standard result is that the coefficient estimate on the miss-measured variable is biased toward zero (Griliches, 1986). This is called attenuation bias in OLS: on average (or in large samples), the estimated OLS effect will be attenuated. In particular, if the effect of being in an affected contractual industry is positive, the estimated effect will tend to be underestimated it. If the CEV assumption is relaxed, there is non-classical measurement error. The latter may not only result in attenuation bias, but also cause the estimated treatment effect to be of the wrong sign (Kreider, 2010).

It is likely that classical measurement error applies to our case. In fact, non-classical measurement error arises mostly in surveys, for example when individuals report incorrect earnings depending on their true income. Since our treatment status relies on administrative data on contractual industries (BMB) and the probabilities are extracted from official data on covered workers in an industry (TDP and TC), we can deem the CEV assumption to be tenable. Note, however, that we are not able to assign probabilities to each year of the analysis based on the official number of workers covered in that given year, since we have TDP only for 2009 and 2012. This may pose a threat to the CEV if the change of workers covered by industries across years is systematically correlated to the industry treatment status. In other words, if the true number of workers covered by an industry that raises its lowest wage floor increases from 2009 and 2010[57], the probability I assign to it - not accounting the increase - is lower than its true value. If that was the case and errors were correlated with the explanatory variable (treatment status) there would be non-classical measurement error.

[56]For an introduction to measurement error and attenuation bias, see Wooldridge (2015). For an examination of measurement error in treatment assignment, see Aigner (1973). For a discussion of the classical measurement error and its implications, see Griliches (1986). Finally, for the growing literature on non-classical measurement error, see Bound et al. (1994), Black et al. (2000) and Kreider (2010) among others.

[57]This would be a change I cannot detect as I apply the same share of workers in 2009 and 2010, as I apply TDP 2009 to both years.

Figure 7: Change in share workers in a specific APE belonging to a given IDCC



Note: difference calculated as workers covered in the same APE belonging to the same IDCC in 2012 minus the equivalent in 2009. The difference between $Pr(IDCC\ X \mid APE\ X)$ in part A has been calculated restricting the sample to only those probabilities present in both TDP 2009 and TDP 2012. In panel B the differences are computed on the whole sample. Hence, if TDP 2009 was missing an observation for $Pr(IDCC\ X \mid APE\ X)$, to that probability has been assigned a 0. The resulting greater differences between part A and part B are therefore the consequence of probabilities (i.e. APE to IDCC conversions) not existent in either the TDP 2009 or TDP 2012. *Source:* own elaboration based on Table de Passage 2009 and Table de Passage 2012.

Figure 7 is an attempt to assess whether non-classical measurement error is a serious threat. Part A shows us that workers covered by the same APE do not change substantially between 2009 and 2012. In particular, 90% of the changes are in the range of -4 and +4 and distributed in a bell around zero. Part B suggests that between 2009 and 2012 either some industries have disappeared or new ones have been formed, or both. Although this figure cannot tell us whether these changes occurred independently of the industry adjusting frequently its wage minimum floors or not, it shows that these changes are not considerable over a three years period. However, these changes may largely increase over a longer period of time, such as the one used in the analysis ranging from 2003 to 2008, to which the TDP 2009 is applied. Unfortunately, this analysis cannot be conducted for those years.

7 Methodology

This exploratory paper tries to identify the impact of the NMW firstly on actual wages and, secondly, on employment. For that purpose, it exploits the existence of wage floors above the NMW for certain industries in France. Notwithstanding several limitations, the data allow many procedures to approach the research question. Two main paths have been followed in this analysis.

7.1 Identification strategy

In principle, we would like to compare a group of individuals that was affected by a NMW increase ("treated") to another identical group that was not ("control"). Probably the most difficult part in identifying the effect of the NMW relates to being able to single out the "treated" from the "control". France presents a stimulating setting as the collective wage bargaining system in vigour places entire sectors in constant relationship with the NMW. In particular, a given contractual industry can be in at least five statuses after an increase in the NMW in t[58]:

1. The industry's collective wage lied above the NMW in $t-1$; in t it does not adjust its minimum wage floor; the industry's collective wage is still above the NMW.
2. The industry lied above the NMW in $t - 1$; in t it adjusts its minimum wage floor; the industry's collective wage is still above the NMW.
3. The industry's collective wage lied above the NMW in $t-1$; in t it does not adjust its minimum wage floor; hence, following an increase in the NMW, the industry's collective wage falls below the NMW.
4. The industry's collective wage lied below the NMW in $t - 1$; in t it adjusts its minimum wage floor; the industry's collective wage moves above the NMW.
5. The industry's collective wage lied below the NMW in $t-1$; in t it does not adjust its minimum wage floor; the industry's collective wage remains below the NMW.

Only industries falling in the first category do not experience an increase in their binding wage in t. In fact, even though sectors in categories three and five do not adjust their minimum coefficients, they automatically follow the NMW, as they were respectively already below or fell below the

[58]See Table A.3 in the Appendix for statistics.

NMW. Similarly, we can be sure that only sectors falling in the fifth category experience the full extent of the statutory NMW increase in t. Indeed, industries in the fourth category experience a greater increase; those industries in the third category face a smaller one; while industries in two can either undergo a greater or a smaller increase. Both strategies employed in this paper exploit, in different ways, the scenarios generated by these five categories.

7.2 The first strategy

The first strategy compares individuals working in an industry that falls under the fifth[59] category with individuals belonging to an industry in the first category. Hence, the "treated" group comprises workers whose industry was bound to the NMW both before and after its increase. Conversely, the "control" group is formed by workers whose industry's binding minimum wage lied above the NMW both before and after its increase and, furthermore, it did not move between the two periods. Note that the treatment status is at the industry level and the LFS is never used to assign it, avoiding the generation of an artificial correlation between the indicator of treatment status and actual wages.

The simplest idea to try to estimate the impact of a NMW increase consists in comparing average wages between "treated" and "control" groups in t, i.e. using only one point in time, which corresponds to the quarter of the NMW increase. Since, by construction, industries in the "control" group have higher wages than those in the "treated" group (at least for individuals paid around the minimum wage), such comparison would produce a biased result, probably suggesting that the "control" group is more affected by the NMW increase. Besides, higher wages in an industry may stem from higher productivity, profitability, level of employment, etc., which may render difficult to properly disentangle the true effect of the NMW. In other words, this selection bias arises from the fact that "treated" and "control" groups differ in many observable and unobservable dimensions, making the simple comparison between the two groups at a given point in time trivial.

To obtain unbiased estimators of the effect of the NMW on my outcomes of interest, the first strategy will therefore use a difference-in-differences (DiD) approach. The DiD compares differences in outcomes before with differences in outcomes after the policy change between the "treated" and the "control" group. In this way, it controls for all the observable and unobservable aspects that do not change over time, such as different wage, productivity, employment levels and industries' idiosyncratic characteristics. The validity of its estimate relies on the so-called common trend assumption, which states that, in the absence of the policy, the average change in the outcomes would have been the same for "treatment" and "control". In other words, outcomes' evolutions in the "control" group serves as counterfactual for the "treated" group. If this assumption holds, we can interpret any deviation from the trend as the policy causal impact.

[59]Note that industries falling in what we may call sub-categories of five are included, i.e. an industry being below the NMW in $t-1$ that, although it adjusted its minimum wage in t, remained below the NMW in t.

The model to estimate the first strategy is therefore:

$$Y_{ist} \;=\; \beta_0 + \beta_1 \cdot \mathbb{1}\{t=1\} + \beta_2 \cdot \mathbb{1}\{i,s \in T\} + \beta_3 \cdot \mathbb{1}\{t=1\} \cdot \mathbb{1}\{i,s \in T\} + X_i^{'}\delta + \varepsilon_{ist} \quad (2)$$

where Y_{ist} is the outcome of interest (logarithm of wages) for individual i employed in industry s in quarter t; $\mathbb{1}\{t=1\}$ is an indicator for a dummy equals to one if the quarter corresponds to the NMW increase, 0 otherwise; $\mathbb{1}\{i,s \in T\}$ is a dummy equal to one if individual i works in an industry s which is affected by the NMW increase and 0 otherwise; $X_i^{'}$ is a set of individual covariates, such as age, region and education[60]; finally, ε_{ist} is an error term. The interaction term measures the deviation from the trend that occurs in the industries affected by the increase in the NMW. The DiD coefficient is therefore β_3.

In the case of more than two periods, the model can be generalised as:

$$Y_{ist} \;=\; \beta_0 + \beta_1 \cdot \mathbb{1}\{t=1\} \cdot \mathbb{1}\{i,s \in T\} + X_i^{'}\delta + \gamma_t + \theta_s + \varepsilon_{ist} \quad (3)$$

where γ_t are time fixed effects and θ_s are industry fixed effects. The interaction term β_1 is the coefficient of interest. The advantage of model 3 consists in the fact that more years can be pooled in a single regression producing more precise and robust estimates.

Model 3 is further modified by averaging[61] individual characteristics over time per each industry yielding:

$$\overline{Y}_{ist} \;=\; \beta_0 + \beta_1 \cdot \mathbb{1}\{t=1\} \cdot \mathbb{1}\{i,s \in T\} + \overline{X}_i^{'}\delta + \gamma_t + \theta_s + \overline{\varepsilon}_{ist} \quad (4)$$

where $\overline{Y}_{ist}$ is the averaged outcome of interest (employment rate) in industry s in quarter t; $\overline{X}_i^{'}$ is the average characteristic in industry s, such as average age and average years of tenure; $\overline{\varepsilon}_{ist}$ is the averaged error term. Although conceptually model 4 does not differ from model 3, it reduces the sample size in a given quarter to the total number of industries and it is not flexible in the use of some discrete covariates[62]. However, it allows the treatment of the data as an unbalanced panel[63] of industries by eliminating the numerous individual duplicates[64]

[60]Normally, some of these controls are fixed over time, so that there should not be the need to include them in a DiD framework. However, due to my probabilistic merging and the fact that for outcomes like wages individuals are followed only two quarters, industries' composition may significantly change.

[61]Weighted using probability weights.

[62]For instance, the variable for education and region, being discrete, are excluded. Besides, the gender variable is transformed into the share of males in the industry.

[63]Since in some years the final sample may not include any worker from a given industry

[64]The analysis performed using model 4 could have done also using 3. However, the large number of duplicates excessively slowed the statistical software used for data treatment (Stata).

For both models 2 and 3, the identification assumption is that, without NMW increase, the gap in the outcome of interest between affected industries and not affected industries would be the same in t and in $t-1$. This assumption can be violated in many ways. Indeed, any external shock that affects only one group of industries during the period will confound the effects of the NMW. Imagine, for example, that "control" industries face a productivity shock thanks to a new technology. Even though their binding wages remain the same, their actual wages may increase due to the positive shock. As a result, the DiD estimator would be downward biased and we may interpret it as the effect of the NMW increase having no impact.

Note that, even if the identification assumption holds, the first strategy is very likely to estimate a local average treatment effect (LATE). In fact, the sample used is a non randomly drawn sub-sample of industries, which may not be representative of the average industry in France. As they may differ in industry size, competitiveness, etc. the findings from this strategy may not have an external validity. For example, if our sub-sample comprises large industries, the strategy would estimate the effect of a NMW uprate on large size sectors, which may greatly differ from the effect on small or medium size sectors.

7.3 The second strategy

The second strategy has two objectives. First it tries to circumvent the difficulties encountered during the first strategy in forming a "treated" and a "control" group. Second, it takes advantage of the whole available period to gain more statistical power[65].

Before turning to the final specification, I am interested in detecting the overall impact that an increase in the NMW has on actual wages in the economy. In other words, I want to observe the relationship between the policy change and actual wages increase over the period, independently of whether an industry is affected or not. Note that some industries will not be directly or indirectly affected by the NMW increase. The two models estimated are:

$$Y_{ist} \;=\; logMW_{st}^{'}\,\beta_1 + X_i^{'}\delta + \gamma_t + \theta_s + \varepsilon_{ist} \tag{5}$$

$$Y_{ist} \;=\; logMW_{st}^{'}\,\beta_1 + logMW_{st-1}^{'}\beta_2 + X_i^{'}\delta + \gamma_t + \theta_s + \varepsilon_{ist} \tag{6}$$

where Y_{ist} is the outcome of interest (logarithm of wages) for individual i employed in industry s in quarter t; $logMW_{st}^{'}$ is a vector containing entries of two continuous variables, one is the logarithm

[65] As one of the drawbacks of the first strategy concerned the small sample size deriving from the use of specific "treated" and "control" groups.

NMW level in quarter t, the other is the logarithm of the binding wage level for industry s in quarter t, hence $MW_{st}\left\{NMW_t, BW_{st}\right\}$[66]. Model 6 differs only in that it includes the lagged variables of $logMW^{'}_{st}$. β_1 is a vector for the two coefficients of interest. As it is a log-log specification, β_1 tells us by how many β_1 percent the outcome of interest increase, on average, given a 1 percent increase in MW_{st} controlling for time-invariant characteristics and individual covariates. Note that, contrarily to the first strategy, here we observe the impact of any binding wage increase and not only the NMW increase.

As discussed at length in the previous sections, industries frequently adjust their minimum wage coefficients. It is therefore hard to find a significant number of industries that are not affected for a relevant interval of time. However, in a given point in time, there are several industries that are adjusting their minimum wages and many others that are not adjusting them. This implies that in that precise moment some industries are "treated", while others are not. So the second strategy pools all the years together, from 2003 to 2012, and compares the industries that raised their binding minimum wage[67] when the NMW increased with those whose binding wage remained unaltered. According to the category scheme, the second strategy compares industries falling in the second, third, fourth and fifth category with industries in the first category. In this context, the latter represent the "control" group, whereas the former the "treated" group. Table A.3 in the Appendix provides an account of the share of industries in the categories exploited in the analysis.

It is however misleading to think in terms of "treated" and "control" groups for this strategy. There are not industries that are always "treated" or always "control". Conversely, industries transition between these two statuses depending on whether, at the time of the NMW increase, they do increase their minimum binding wage or they do not. Hence, for the second strategy, the identification relies on the timing of the policy change. To exploit the timing and to control for time-invariant characteristics among industries, model 5 becomes:

$$\overline{Y}_{st} \;=\; \beta_1 \cdot logNMW_t + \beta_1 \cdot \mathbb{1}\left\{\Delta BW_{st}\right\} + \beta_3 \cdot logNMW_t \cdot \mathbb{1}\left\{\Delta BW_{st}\right\} + \overline{X}^{'}_{s}\delta + \gamma_t + \theta_s + \overline{\varepsilon}_{st}{}^{68} \quad (7)$$

where $\overline{Y}_{st}$ is the average outcome of interest (logarithm of wages or employment levels) for industry s in quarter t; $logNMW_t$ is a continuous variable for the NMW level in quarter t; $\mathbb{1}\left\{\Delta BW_{st}\right\}$ is an indicator for a dummy that takes the value of 1 if industry's s binding wage has changed in quarter t, 0 otherwise. The interaction term β_3 is the coefficient of interest.

[66]In practice, models 5 and 6 are estimated twice, once using NMW_t as independent variable and the second using the BW_{st}.

[67]Note that the increase can either be due to the industry automatically following the NMW (because its coefficient lied below), or the industry deciding to raise it at the same time as the NMW.

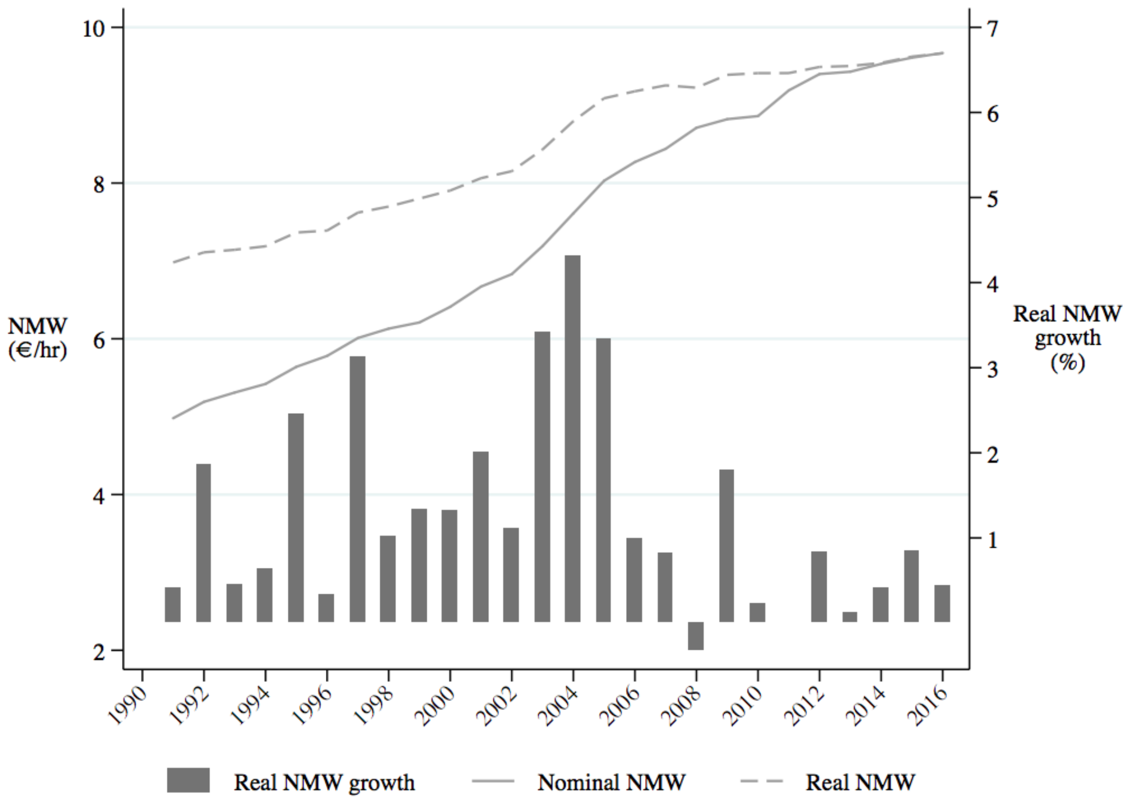
[68]An equivalent model will be run in the next section as a robustness check. A lagged NMW is introduced, the model becomes: $\overline{Y}_{st} \;=\; \beta_1 \cdot logNMW_{t-1} + \beta_1 \cdot \mathbb{1}\left\{\Delta BW_{st}\right\} + \beta_3 \cdot logNMW_{t-1} \cdot \mathbb{1}\left\{\Delta BW_{st}\right\} + \overline{X}^{'}_{s}\delta + \gamma_t + \theta_s + \overline{\varepsilon}_{st}$.

If we were to run model 7 on wages, β_3 would capture the difference between the increase in average wages in industries that raised their binding minimum and the increase in average wages for those industries that did not increase their binding minimum over the period. Therefore one would expect β_3 to be positive and significant.

8 Analysis

8.1 First strategy

Figure 8: NMW evolution in France

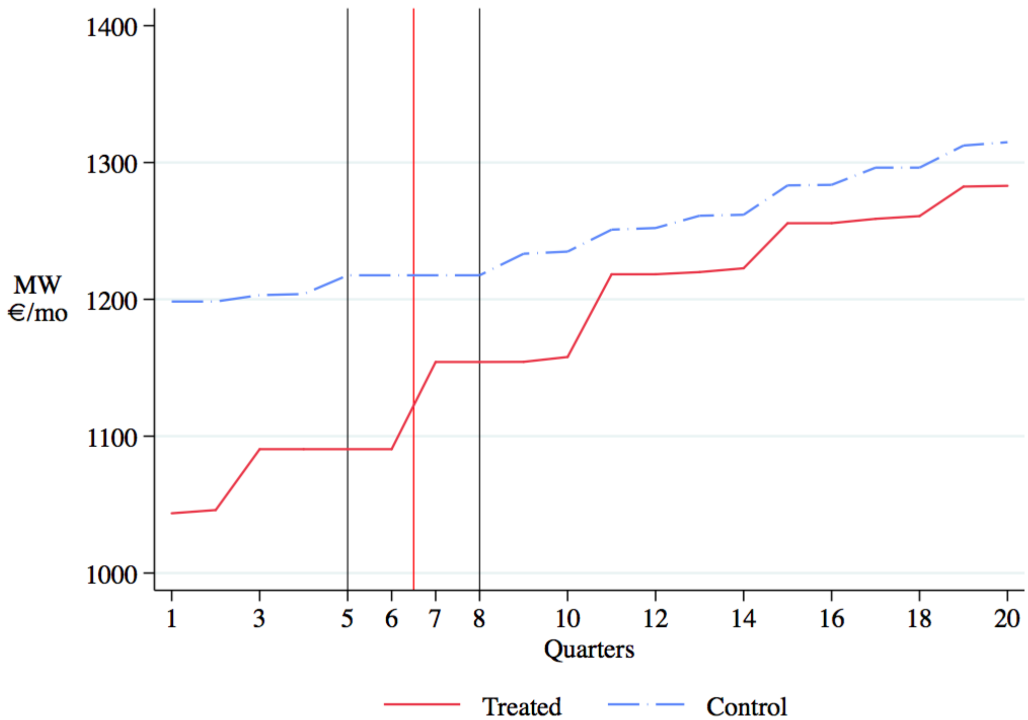


Source: own elaboration based on Insee statistics.

To implement the first strategy, it is extremely important the choice of the period to study. Although it would be natural to exploit all the available data (from 2002 to 2012), there are no industries whose minimum wage coefficient had never been adjusted and remained constant above the NMW; nor there were industries whose binding minimum wage had been the NMW for the whole period. Since "treated" and "control" groups cannot be formed when using the available period, this analysis will focus only on a few quarters. Figure 8 serves as a guide to make the decision. The highest NMW increase in real terms in the last 26 years occurred in 2004, with a real NMW growth of over 4%. Hence, this July 2004 increase will determine the industries' treatment status.

The sample of interest is restricted to the two quarters before and after the increase in 2004[69]. The industries that were following the NMW during the two quarters before form the "treated" group. Conversely, those industries whose lowest wage coefficient was not adjusted and stayed above the NMW both two quarters before and two quarters after the NMW increase compose the "control" group (see Table A.4 in the Appendix for descriptive statistics of the two groups).

Figure 9: Evolution minimum wage coefficients for "treated" and "control"



Notes: the figure covers 20 quarters, from 2003 to 2007, both years included. The black vertical lines delineate the period under study for the first strategy, while the red vertical line indicates the moment of the NMW increase. *Source:* own elaboration based on Base des Minima de Branches.

Figure 9 provides a visual representation of the binding wage trends for both "treated" and "control" from 2003 to 2007. Focusing on the period of the analysis (i.e. quarter 5 to 8, see Figure A.3 in the Appendix for a zoom), we can observe how the "treated" group had experienced a large increase

[69]The choice of the number of quarters before and after is rather arbitrary. However, extending the period to, say, three quarters before and three after results in a consistent loss of industries from both "treated" and "control" groups making the analysis more difficult.

in its binding minimum wage (which corresponds to the NMW) at the beginning of quarter 7[70], whereas the "control" group leaves its binding minimum wage unaltered (as demonstrates the flat trend over the period). Hence, in the absence of other policies, reforms, or external shocks happened concurrently with the NMW increase, wages' growth in the "control" group in quarter 7 and 8 should mimic the counterfactual wages' growth for the "treated" group.

Figure 9 is constructed using administrative data on wage adjustments. The next steps therefore concern showing that actual wages in "treated" industries have been affected by the institutional rise impacting their minimum wages. This impact is addressed both graphically and analytically. The graphical representation has the advantage of allowing us to corroborate the validity of the common trend assumption, as we observe the evolution of wages for both groups over the period. The analytical exercise, instead, attempts to disentangle the effect of the policy change only and provides us with a magnitude of its impact.

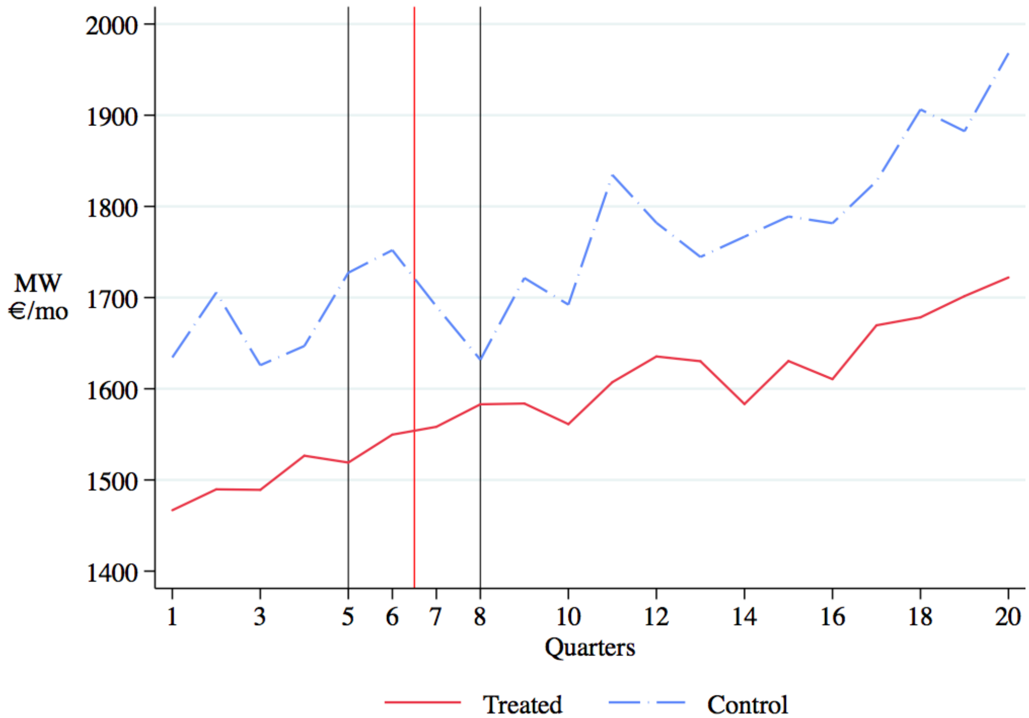
8.1.1 NMW impact on actual wages

Ideally, wages in "control" and "treated" groups follow a similar pattern up to quarter 7 and diverge afterwards[71]. The most natural way to investigate whether the NMW could have affected the wages of individuals working in the concerned industries is by plotting the average actual wages of these groups over the period.

[70] Figure 9 wrongly positions the NMW increase between quarter 6 and quarter 7, for visual purposes. The increase occurred the first day of quarter 7.

[71] Note that, since after quarter 8, many industries forming the "control" group adjust their minimum wages, I consider as 'after' quarters 7 and 8.

Figure 10: Average net monthly wages in “treated” and “control”



Notes: the figure covers 20 quarters, from 2003 to 2007, both years included. The black vertical lines delineate the period under study for the first strategy, while the red vertical line indicates the moment of the NMW increase. Averages weighted using the probability assigned to each worker and individuals below the binding NMW are excluded. For “treated”, group average $N = 14,913$ and average weighted N[72]$= 1,183$. For “control”, average $N = 5,889$ and average weighted $N = 512$. *Source:* own elaboration based on complete Labour Force Survey[73].

Figure 10 does not provide a clear scenario[74]. It is possible to observe the effect of the increase in the NMW on the treated group, as the latter jumps from an average of €1,500 net monthly wage to almost €1,600 from quarter 5 to quarter 8. Furthermore, we can see that, before the NMW increase, average wages in both “treated” and “control” group were raising from quarter 5 to 6. However, it is difficult to explain the sudden drop in average wages in the “control” group just after the NMW increase.

[72] N is the total number of observations per quarter included in the analysis.

[73] Note that for the first strategy I used only half of the complete LFS. Hence, I was able to keep the whole sample without dropping those duplicates having a probability of belonging to a given IDCC lower than 0.05 (as explained in Section 5).

[74] Also the evolution of median wages, average wages capped to €2,200, average wages capped to €2,000 and average wages capped to €1,800 have been plotted. The outcomes are all very similar to Figure 10. Note that averages per decile have been avoided due to the extremely small sample that was employed.

There are three possible reasons. First, the pattern is affected by measurement error as discussed in Section 6. Second, averages are noisy due to the small sample size for the "control" group (which would explain also the overall greater volatility in this group with respect to the "treated"). Third, the drop is the consequence of a change in workers composition and seasonality. Indeed, the NMW increase occurs during summer, which is a period in which many temporal contracts may be agreed upon, resulting in lower average wages.

Although the third option seems less plausible than the others, as it is unlikely that seasonality or a change in workers composition affects only the "control" group, I have performed two checks to assess it. Firstly, I have plotted Figure 10 excluding those workers that had been employed in the current job for less than four months. Accounting for the months of tenure should therefore show whether the drop is due to a change in workers' composition. This new plot, however, still shows a large drop in average wages (see Figure A.4 in Appendix). Secondly, I have estimated the following model to plot the residuals:

$$wage_s \; = \; \sum_{l=1}^{4} \alpha_l \cdot \mathbb{1}\left\{q = l\right\} + \varepsilon_s$$

where $wage_s$ is the average net monthly wage for group of industries s[75]; the indicator $\mathbb{1}\left\{q = l\right\}$ is a dummy equal to 1 one if quarter q is the number l within a year. Hence, the model above controls for seasonality. Figure A.5 in the Appendix plots the residuals of this regression and shows the evolution of average wages over the period in the absence of seasonality effects. Even when seasonality is accounted for, we can still see the large drop occurring after the NMW increase. These two checks, therefore, seem to reject change in workers' composition and seasonality as the causes for the large drop in average wages.

Another widely used graphic approach to reveal how the wage distribution is affected by an increase in the NMW is the Kernel[76] estimate of the density function. The latter is an approximate of the density $f(x)$ from observations on x. In practice, the estimator calculates the density at the centre of the weighted xs found within a rolling interval or bandwidth.

$$\hat{f}_k \; = \; \frac{1}{nh} \sum_{i=1}^{n} K\left(\frac{x - X_i}{h}\right)$$

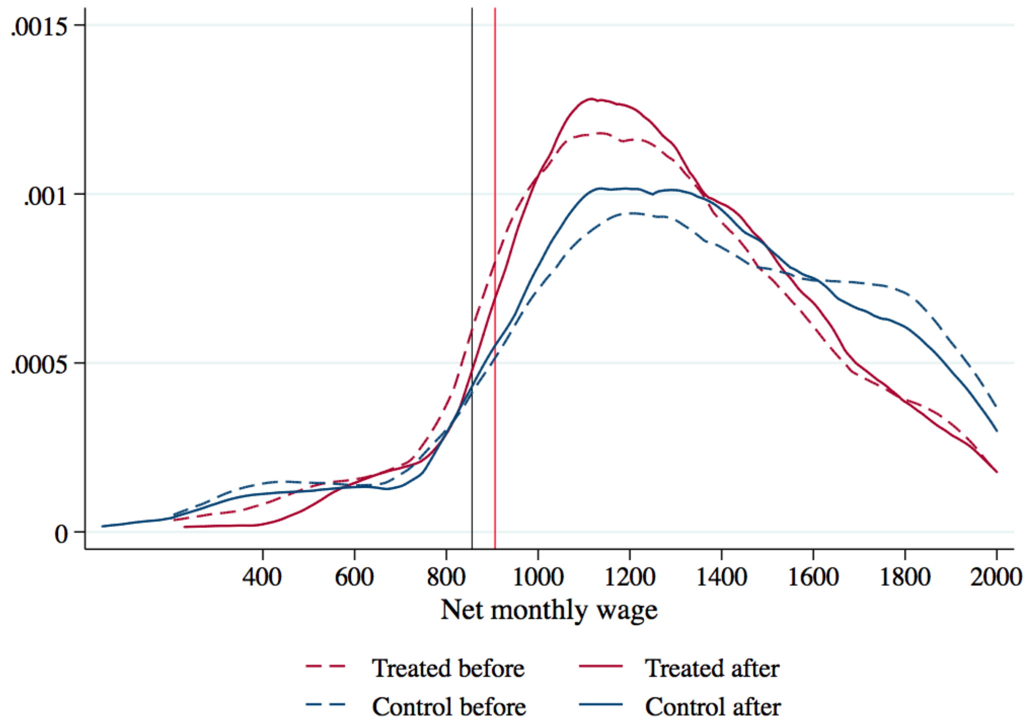
where $\hat{f}_k$ is the Kernel density estimate based on a random sample $X_1, ..., X_n$ of size n; h is the bandwidth and $K(.)$ is the kernel function, which determines the weights. This estimator is very sensible to the choice of the bandwidth applied[77], which, in some cases, could smooth exactly the cliff where the NMW is binding (Maloney and Mendez, 2004).

[75] This model was estimated twice, first restricting the sample to "treated" only and then restricting the sample to "control".

[76] See DiNardo et al. (1996) and Maloney and Mendez (2004) for a thorough treatment of kernel density estimation.

[77] Stata default has been applied.

Figure 11: Distribution of wages before and after the increase in "treated" and "control"



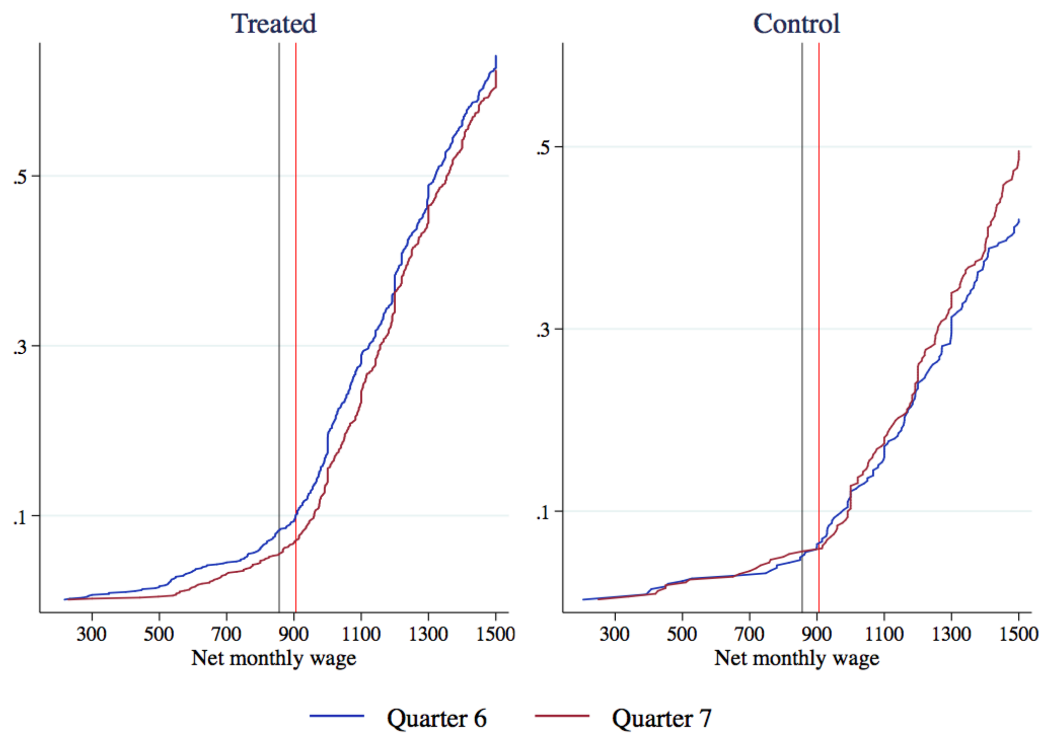
Notes: net monthly wage restricted to €2,000. Sample restricted to those workers having a probability of at least 60%. Excluded individuals below the NMW. For "treated", group $N = 797$. For "control", group $N = 248$. The black vertical line indicates the NMW before the increase, while the red vertical line reveals the new NMW. *Source:* own elaboration based on complete Labour Force Survey.

Figure 11 suggests that the policy had, in effect, forced a change in the distribution for the "treated" group. Workers directly or indirectly affected seem to have moved up intensifying the mode at a net monthly wage of around €1,200. On the other hand, the distribution in the "control" group does not seem to be altered due to the NMW increase, as we would expect[78]. If any, the changes that we observe appear to be coming from the upper tail of the distribution. As a sort of robustness check, I showed in Figure A.7 in the Appendix the evolution of the wage distributions for both groups one quarter before and one quarter after the NMW increase. As no visible movement is detectable for either "control" or "treated" groups in periods before and after the policy change, these plots suggest that the change in the distribution of the "treated" group is due to the NMW increase.

[78]Note, however, that the choice of the probability to apply is determinant for what we observe for the "control" group. Any probability level above 60% brings about the same results as for Figure 11. However, a probability of 50% shows a different, puzzling scenario (Figure A.6 in the Appendix). This may probably be the result of the contamination between "treated" and "control" groups due to the measurement error.

Cumulative wage distributions represent another method to assess the impact of the NMW on "treated" and "control". Their attractiveness with respect to the Kernel distribution relies on the fact that they do not require any judgment about the bandwidths.

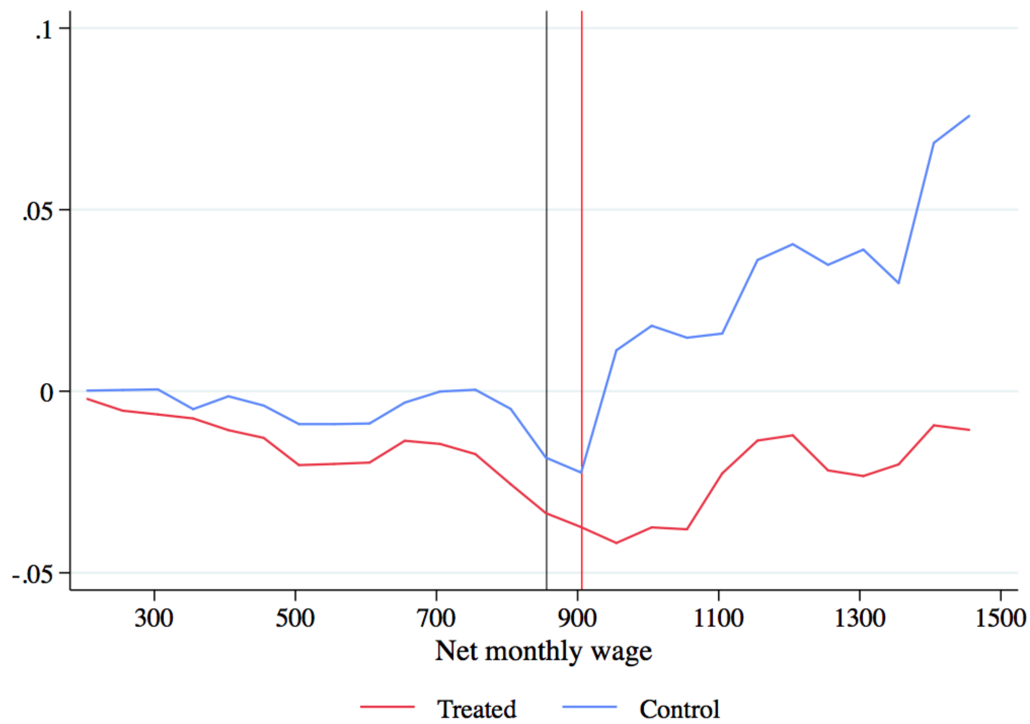
Figure 12: Cumulative wage distributions for "treated" and "control"



Notes: sample restricted to those workers having a probability of at least 60%. Excluded individuals below the NMW and with less than four months of experience in the current job. The black vertical line indicates the NMW before the increase, while the red vertical line reveals the new NMW. *Source:* own elaboration based on complete Labour Force Survey.

Figure 12 provides information that is in line with what we have observed in Figure 10 and 11. On the one hand, it confirms the capacity of the NMW to alter wages of those workers that are in the "treated" group and have wage levels close or at the minimum. Indeed, at any given wage there is a lower mass of workers after the NMW increase; and the gap between the mass of workers before and the mass of workers after the NMW increase marginally decreases the further away the wage is from the NMW level. On the other hand, the "control" group presents an opposite effect. Following the policy change, it seems that there is a higher mass of workers at a given wage level. As I have discussed before, there can be at least three reasons for this "rebound effect".

Figure 13: Difference cumulative wage distribution for "treated" and "control"



Notes: sample restricted to those workers having a probability of at least 60%. Excluded individuals below the NMW and with less than four months of experience in the current job. The black vertical line indicates the NMW before the increase, while the red vertical line reveals the new NMW. *Source:* own elaboration based on complete Labour Force Survey.

Figure 13 graphically shows the vertical difference between the mass of workers before and after the increase for both "treated" and "control" groups. The cliff that is formed to the right of the red line for the "treated" group can be directly linked to the NMW increase. However, we would like to observe a different pattern for the difference of the "control" group. Ideally, its trend should gravitate around the zero. Conversely, this trend suggests that at a given wage, following the NMW increase, there is a greater mass of workers that belong to an affected group of industries. This is somewhat puzzling as we would expect a similar fact to occur as a result of change in worker composition, which is accounted for by excluding those workers that have less than four months of experience on their current job.

Besides, it represents a threat to our identification if it does not mimic the counterfactual evolution of the "treated" group. Regrettably, this may easily be the case. Indeed, these puzzling results are likely to be caused by either measurement error in the treatment assignment, or a small sample size for the control group, which spoils the validity of the findings. With these considerations in mind, we estimate model 3.

Table 4: NMW impact on actual wages (first strategy)

	(I)	(II)	(III)
Interaction (β)	0.075*** (0.0146)	0.055*** (0.0112)	0.051*** (0.01)
Controls	No	Yes	Yes
Time FE	Yes	Yes	Yes
Industry FE (84 categories)	No	No	Yes
Adjusted R-squ	0.0067	0.4371	0.4652

Notes: clustered standard errors at the industry level in parenthesis; * p < .1, ** p < .05, *** p < .001. Probability weights applied. Controls include dummies for highest degree obtained, dummies for region of residence, dummy for being a male, a continuous variable for years of tenure and a quadratic term for age. The dependent variable is the logarithm of net monthly earnings.

Table 4 summarises the results obtained running model 3. Column I reports the results on the raw specification, i.e. without controls and industry fixed effects. Column II adds controls and column III introduces industry fixed effects. If the common trend assumption holds, column I suggests that the policy change brought about an average increase in actual wages in the "treated" group of 7.5 percent. The coefficients remain positive and significant at the one percent level also after we include basic controls and industries' fixed effects (columns II and III). The final specification, that includes all the controls, quarter fixed effects and industry fixed effects, show a coefficient for β_1 (the interaction term) equal to 0.05. In other words, the NMW increase occurred in 2004 raised, on average, actual wages in the affected industries by 5 percent, once time invariant characteristics and other forms of heterogeneity are taken into account. This can be quantified as follows: a real increase in the NMW of about 4.5 percent results in about 5 percent nominal increase in actual wages for the affected industries, on average.

8.1.2 Robustness check

There are a number of simple checks that can be performed to assess the validity of the DiD strategy. Among others, using data for prior periods (say period -1) and perform again the DiD comparing year 0 and year -1, if there was no policy change between year 0 and year -1. If this placebo DiD is non-zero, there are good chances that the estimate comparing quarter 0 and quarter 1 is biased too. One can also use an alternative control group C'. If the DiD with the alternative control is different from the DiD with the original control C, then the original DiD is likely to be biased (Gruber, 1996). Lastly, it is possible to replace the dependent variable Y by another outcome Y' that is not supposed to be affected by the reform. If the DiD using Y' is non-zero, then it is likely that the DiD for Y is biased too.

In our setting it is possible to exploit the time dimension. In fact, during quarters 5 and 6 there has not been implemented any NMW reform. Hence, I compared the gap between actual wages for "treated" and "control" in quarter 6 to the gap in quarter 5, as if the NMW increase occurred between these two quarters. To perform this robustness check, we run model 2. As Table A.5 in the Appendix shows, the interaction term (which corresponds to β_3 in model 2) is small and insignificant for all the three different specifications, whereas the coefficients for the control do not change. This serves as a formal validity of the common trend assumption before the increase in the NMW and corroborates the fact that the detected effect in actual wages for the "treated" group in model 3 is due to the institutional raise of the NMW.

However, as discussed before, it is unlikely that the common trend assumption holds as a counterfactual. The interaction coefficients may be the result of the large fall in average wages that we observed in Figure 10 and throughout the other graphical representations. Hence, if average wages in the "control" group were to follow the pattern shown in Figure 9, the interaction coefficient would probably be smaller in magnitude. That is to say, assuming that average wages kept growing at the same pace after the rise in the NMW, findings in Table 4 would be upwardly biased.

8.1.3 Analysis on employment

Assuming that the findings above were correctly estimated, we proceed to examine the effect of the NMW increase on employment. The dependent variable is employment level in an industry s in quarter t. It has been computed as follows:

$$\text{employment rate in quarter } t \;=\; \left(\frac{\#employed\ in\ industry\ s}{\#tot\ employed\ +\ \#tot\ unemployed}\right) \cdot 100$$

Table 5: NMW impact on employment (first strategy)

Model 4	Employment rate		
	(I)	(II)	(III)
Interaction (beta)	-0.001 (0.0079)	-0.02 (0.0184)	-0.002 (0.0092)
Controls	No	Yes	Yes
Time FE	Yes	Yes	Yes
Industry FE (84 categories)	No	No	Yes
Adjusted R-squ	0.033	0.063	0.996
Observations	340	340	340

Notes: clustered standard errors at the industry level in parenthesis; * $p < .1$, ** $p < .05$, *** $p < .001$. Probability weights applied. Controls include share of male in the industry, a continuous variable for years of tenure and a quadratic term for age. The dependent variable is the employment rate (%).

Table 5 summarises the results from model 4, the final specification of the first strategy. Column I provides the findings for the specification without controls and industry fixed effects, which are in turn introduced in column II and III. The results for employment are highly insignificant, meaning that a real increase in the NMW of about 4.5% has a null impact on employment, on average. Such a finding would have many implications for policy-making as it suggests that social welfare could be improved and economic inequality eased without causing negative repercussion on employment.

However, we need to be very careful in drawing any conclusion, as there are many limitations in this study[79]. First, there is measurement error in forming "treated" and "control" that generates an attenuation bias. Second, the common trend assumption is likely to be violated in the first stage, meaning that the "control" group may not be correct to mimic the "treated" group trend in the absence of the policy change. Third, the common trend assumption is also very likely not to hold for employment, as can be seen Figure A.8 in the Appendix. In addition, even in the absence of these three threats to identification, as previously emphasised, these results may correspond to a LATE.

[79]The following particularly applies to the first strategy.

8.2 Second strategy

8.2.1 First stage: NMW impact on actual wages

Given the several complications that we encountered applying the first strategy, in this subsection we turn to the second strategy. Firstly, we estimate models 5 and 6[80] to assess the impact of the NMW and the binding wages increase on actual wages.

Table 6: NMW impact on actual wages (second strategy)

Model 5	**Panel A:** Log NMW			**Panel B:** Log BW		
	(I)	(II)	(III)	(I)	(II)	(III)
Log actual wage	0.76*** (0.046)	0.51*** (0.034)	0.55*** (0.029)	0.89* (0.491)	0.35** (0.160)	0.12** (0.051)
Controls	No	Yes	Yes	No	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (264 categories)	No	No	Yes	No	No	Yes
Adjusted R-squared	0.024	0.443	0.476	0.031	0.444	0.476
Observations	481,015	458,723	458,723	481,015	458,723	458,723

Notes: this table presents the dependent variable on the left, for the sake of tidiness. Clustered standard errors at the industry level in parenthesis; * $p < .1$, ** $p < .05$, *** $p < .001$. Probability weights applied. Controls include dummies for highest degree obtained, dummies for region of residence, dummy for being a male, a continuous variable for years of tenure and a quadratic term for age. Log wage is the logarithm of net monthly earnings.

Table 6 reports the main findings of model 5. The NMW has a strong positive impact on actual wages, which is robust to the inclusion of basic controls, quarter fixed effects and industry fixed effects (Panel A). The coefficient of interest in column III Panel A suggests that a ten percent increase in the NMW brings about 5.5 percent increase in actual wages, on average. This is a high estimate as it implies that almost half of the growth in the nominal NMW is reflected in actual wages. Results in Panel A are also robust to the inclusion of the lagged NMW (see Table A.6 in the Appendix for model 6), suggesting a low, if not null, anticipation effect. This robustness check waves away another potential threat to this identification: reverse causality. The magnitude and

[80]Note that the sample does not include those industries that did not match with the BMB. Including them, however, does not change the results.

even the timing of a NMW increase are determined by the inflation level and productivity. So, higher actual wages may be the cause rather than the consequence of a NMW increase. If this were the case, however, we would expect to find the lagged variable for the NMW significant the quarter before.

Panel B displays a different scenario. Although the coefficients remain positive and significant after the inclusion of the basic controls, quarter fixed effects and industry fixed effects, their magnitude substantially shrinks. Column III tells us that a ten percent increase in an industry's binding wage leads to 1.2 percent increase in actual wages, on average. This is somehow unexpected since the continuous variable BW includes the minimum binding wages for each industry and, contrarily to the NMW, should capture the impact of any wage adjustment occurred in a given quarter for the industry that experienced it. Furthermore, when the lagged BW is included in the analysis, the BW coefficient turns insignificant (Table A.6 in the Appendix). This is probably due to the noise present in the BW variable: the few industry level adjustments taking place each quarter combined with the measurement error in linking workers with their industry generates a fuzzy design that hinders to detect the true impact.

The choice of the variable to use to indicate the increase in minimum wages hinges on the findings from Table 6. Due to the limitations of my dataset, it is better to use the NMW, rather than a more precise variable such as the BW. We therefore estimate model 7 using the logarithm of the NMW over the period.

Table 7: Minimum wage impact on average wages (second strategy)

	Average log wage			
	Model 7		Robustness check	
	(I)	(II)	(III)	(IV)
Interaction	0.18***	0.16***		
(β_3)	(0.060)	(0.054)		
Interaction lagged			0.025	-0.05
(α_3)			(0.049)	(0.044)
Controls	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE (264 categories)	Yes	Yes	Yes	Yes
R-squared	0.209	0.307	0.21	0.311
Observations	9,882	9,868	9,519	9,508

Notes: clustered standard errors at the industry level in parenthesis; * p < .1, ** p < .05, *** p < .001. Probability weights applied. Controls include share of male in the industry, a continuous variable for years of tenure and a quadratic term for age. Log wage is the logarithm of net monthly earnings.

The main findings are summarised in Table 7. Column I presents the result with quarter and industry fixed effects, while column II introduces some basic controls. The interaction coefficient is positive and significant in both specifications. The validity of these results is further corroborated by a modification of model 7, which includes the lagged variable for the NMW. In this case the coefficients are negative and insignificant in both specifications (columns III and IV). These findings suggest that, once controlling for time-invariant characteristics and time-variant characteristics at the industry level (such as average age in the industry), industries that raise their minimum binding wage in the quarter of the NMW increase by, say, 10 percent experience a 1.6 percent higher increase in average actual wages than industries whose wages remained unaltered.

8.2.2 Analysis on employment

Following the same rationale we now turn to investigating the impact of a minimum wage increase on the employment of those industries that experienced it. In every quarter in which the policy change took place, employment rates of those industries directly affected by an increase in their minimum binding wage were compared to those industries that did not alter their minimums.

Table 8: Minimum wage impact on average employment rate (second strategy)

	Employment rate			
	Model 7		Robustness check	
	(I)	(II)	(III)	(IV)
Interaction (β_3)	-0.039 (0.024)	-0.041* (0.024)		
Interaction lagged (α_3)			-0.026 (0.019)	-0.029 (0.019)
Controls	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE (264 categories)	Yes	Yes	Yes	Yes
R-squared	0.001	0.002	0.001	0.002
Observations	9,882	9,868	9,519	9,508

Notes: clustered standard errors at the industry level in parenthesis; * p < .1, ** p < .05, *** p < .001. Probability weights applied. Controls include share of male in the industry, a continuous variable for years of tenure and a quadratic term for age. The dependent variable is employment rate in industry s in quarter t.

Table 8 reports the main findings obtained running model 7 on employment. Column I presents the results with quarter and industry fixed effects, while column II introduces some basic controls. Model 7 is further estimated replacing the continuous NMW variable with its lagged counterpart (columns III and IV), as robustness check. The effects on employment are small but significant at the 10 percent level for the specification including the controls (column II). The coefficient of interest is negative, implying that an increase of the lowest coefficient for an industry is associated, on average, with a decrease in employment. So, an increase in the binding wage of say 10 percent leads to a decrease in employment rate of 0.4 percentage points, on average. When the lagged variables are included in the model, their interaction coefficients are both negative but insignificant at standard levels (columns III and IV).

As for the first strategy, it is important to be careful when interpreting these results. The second strategy has shown that indeed industries that raised their minimum binding wage, concurrently with the NMW increase, experience a larger growth in average actual wages than those industries whose lowest coefficient remain unaltered. It further suggests that these same industries that were affected by the increase in wages come across an average decrease in employment. However, many limitations hinder a proper construal of the findings. First, the second strategy still suffers from the measurement error in forming "treated" and "control" that generates an attenuation bias. Second, model 7 controls for time-invariant characteristics (industries fixed effects) and events that are common to every industry (quarter fixed effects). This means that any time-variant occurrence, not accounted for by the basic controls, poses a threat to the identification. Across the years of the analysis, it is highly possible, for instance, that the financial crisis had a heterogeneous impact on industries, or new technology generated exogenous increases in productivity in some sectors.

9 Conclusion

This paper has attempted to contribute to the wide literature on the economic effects of the NMW exploiting a novel setting. It takes advantage of the coexistence of two institutional forces for wage determination in France to compare industries that are directly affected by an increase in the minimum wage coefficient to those that are not. An explicit advancement with respect to previous studies relies, for example, in the possibility of comparing entire industries among each other, rather than individuals at the NMW with individuals higher up the wage distribution. Furthermore, thanks to the several statuses that an industry can have with reference to the NMW[81], there is ample room for different identification strategies. This study pursued two main strategies. First, it applies a quasi-experimental design (DiD) following industries directly affected by an increase in the NMW ("treated") and industries whose minimum wages have remain unaltered ("control") for two quarters before and two quarters after the policy change occurred. Second, it uses all the years available to identify the average effects of the NMW increases on actual wages and employment for the affected industries over the period.

The main findings suggest that an increase in the NMW and, more generally, an increase in the minimum binding wage is associated with an average growth in actual wages. This is supported both graphically, thanks to Kernel and cumulative distributions of wages and plots of average and median wages, and formally, by the models applied in both strategies. They both report, in fact, significant and positive effects of NMW on actual wages. However, results are far from being conclusive for employment. While the first strategy points to a zero impact of a rise in the NMW on employment, the second strategy shows a small negative estimate[82].

Due to the several limitations of the study, it is difficult to draw definitive conclusions from these findings. The final sample is the result of a troublesome double merging that produces measurement error in the variable that determines the workers' belonging to a given industry. Although it is arguably a classical measurement error, it still generates an attenuation bias in the coefficient

[81] See categories in section 7 for examples.
[82] Note that it is significant only at the 10% level.

estimate (Griliches, 1986). Also, the common trend assumption, which is crucial for the validity of the first strategy, is likely to be violated for both the study on wages and employment. The first strategy also lacks of statistical power, as by construction, it performs the analysis on a restricted number of industries and quarters. In addition, both strategies may suffer from any time-variant occurrence, not accounted for by the basic controls. Even assuming that the results are internally valid, there could be a problem of external validity. France falls, in fact, in a very specific category of countries that still have the existence of various minimum wage floors and a very high share of the working population relying on them. Hence, the results may be the by-product of such a particular setting.

In spite of the limitations and caveats, this paper can serve as starting point for future research aiming at comparing industries with different wage floors. There are at least three elements that can be improved. First, on a technical level, those studies that will focus on outcomes that can be explored with the datasets used in this analysis, such as wages, employment, or hours worked, will need to find TDPs (or equivalent) that cover all the period under consideration. With this information, the possibility of non-classical measurement error would be waved away and classical error would be limited. Besides, they should try to improve the accuracy of the merging between the LFS and the BMB by exploiting the variables present in the LFS that precisely inform on the job sector of the individual so to reduce to the minimum the number of duplicates in the analysis. Turning to the identification strategy, it may be interesting to focus on those NMW increases that have been somehow unexpected either because inflation triggered an early increase, or because the "expected" change resulted higher due to the governmental *coup de pouce*. If one can convincingly argue that such policy changes were exogenous, then industries that were following the NMW and those lying just above it but that were reached and surpassed by the sudden increase may form highly credible "treated" groups. Third, such setting allows for many other outcomes to be addressed. Equipped with data at the firm or industry level on exports, for instance, it would be possible to investigate the effects of NMW increases on international competitiveness. There are still several unanswered questions regarding the economic effects of the NMW, such as whether it alleviates poverty, reduces inequality, increases productivity and so forth. Even though this paper does not provide any conclusive evidence, it may set the path for forthcoming research that may contribute to the understanding of the economic dynamics surrounding the MW making it a better policy tool.

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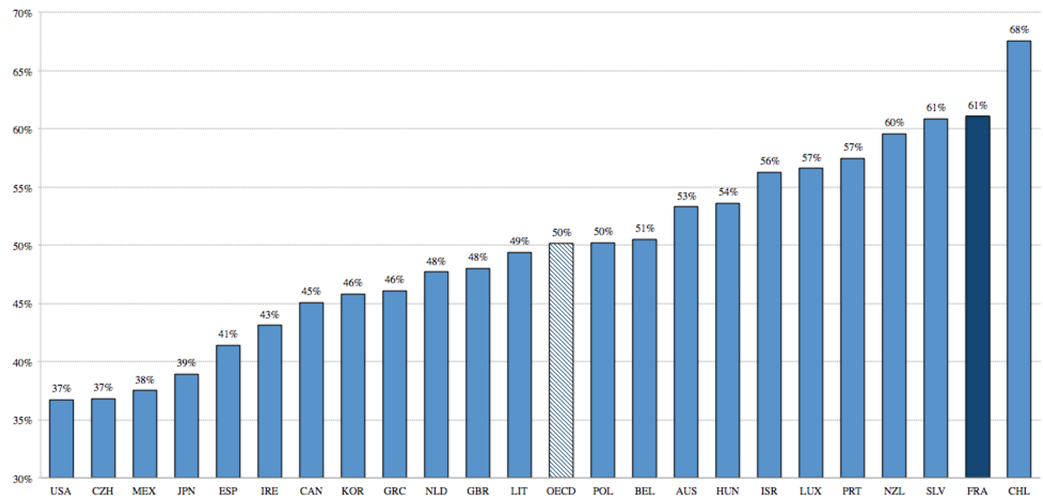
10 Appendix

A Figures

Figure A.1: Cross-section of NMW-median wage ration for OECD countries in 2014



Source: own elaboration based on OECD statistics.

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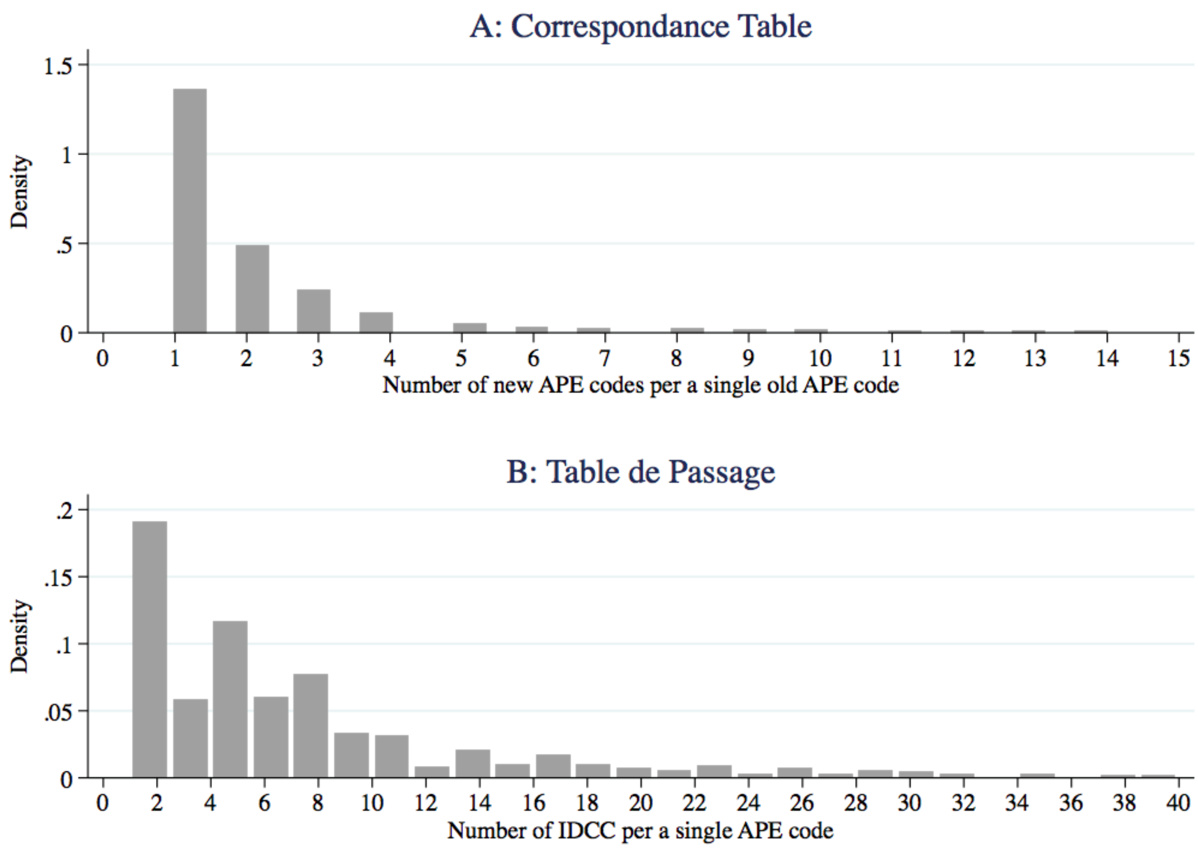
Figure A.2: Density of possibilities for TC and TDP



Source: own elaboration based on Table de Correspondence 2008 and Table de Passage 2009.

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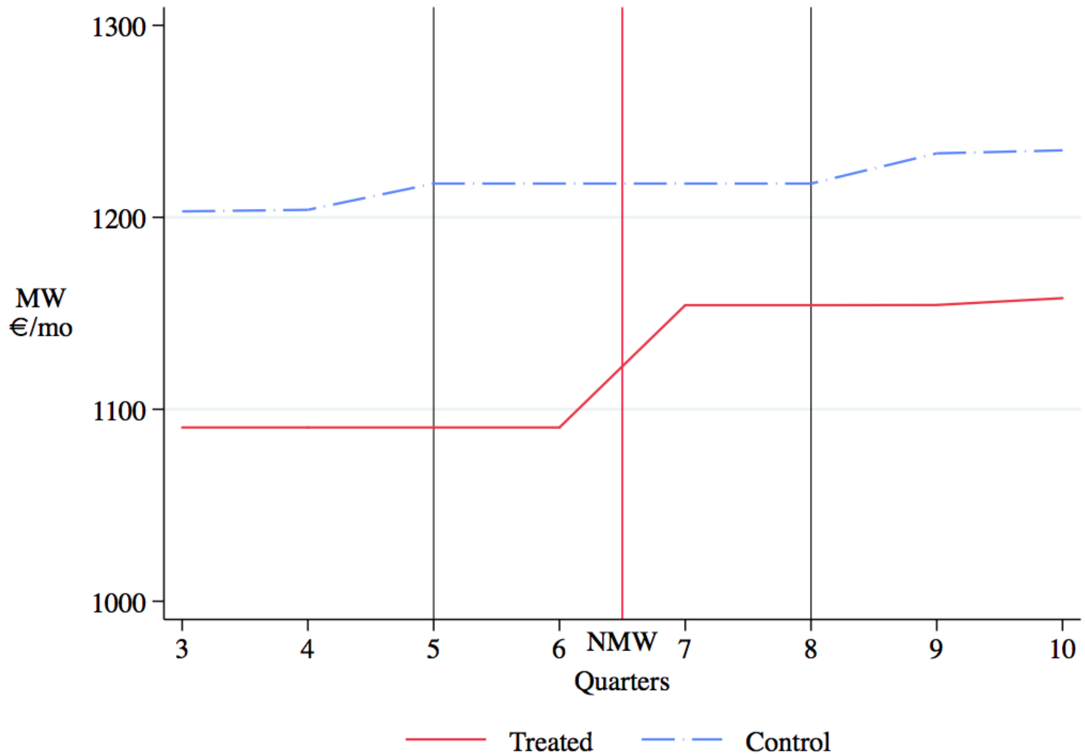
Figure A.3: Evolution minimum wage for "treated" and "control"



Notes: quarters start in July 2003. Therefore in this figures the 10 quarters cover from part of 2003 to part of 2005 included. The black vertical lines delineate the period under study for the first strategy, while the red vertical line indicates the NMW increase. *Source:* own elaboration based on Base des Minima de Branches.

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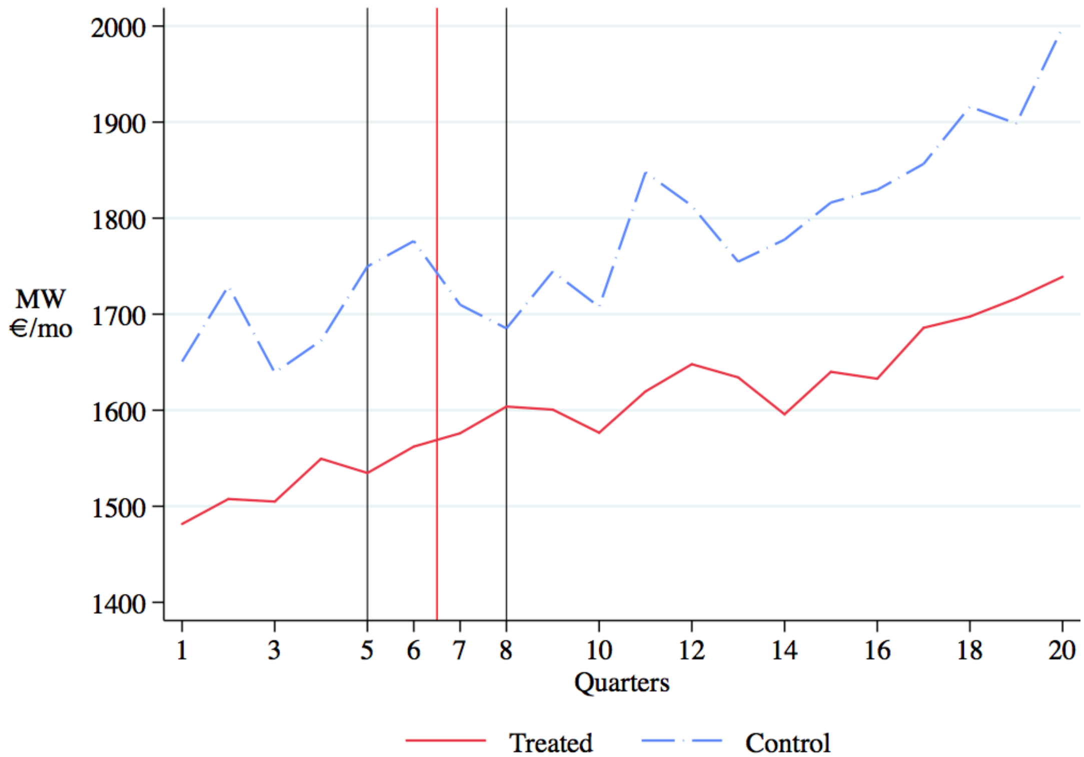
Figure A.4: Average net monthly wages in "treated" and "control" without just hired workers



Notes: quarters start in January 2003, which is the first year available. Therefore in this figures the 20 quarters cover from 2003 to 2007 included. The black vertical lines delineate the period under study for the first strategy, while the red vertical line indicates the NMW increase. Averages weighted using the probability assigned to each worker; individuals below the binding NMW are excluded; workers with less than 4 months experience in his or her present job were excluded. *Source:* own elaboration based on complete Labour Force Survey.

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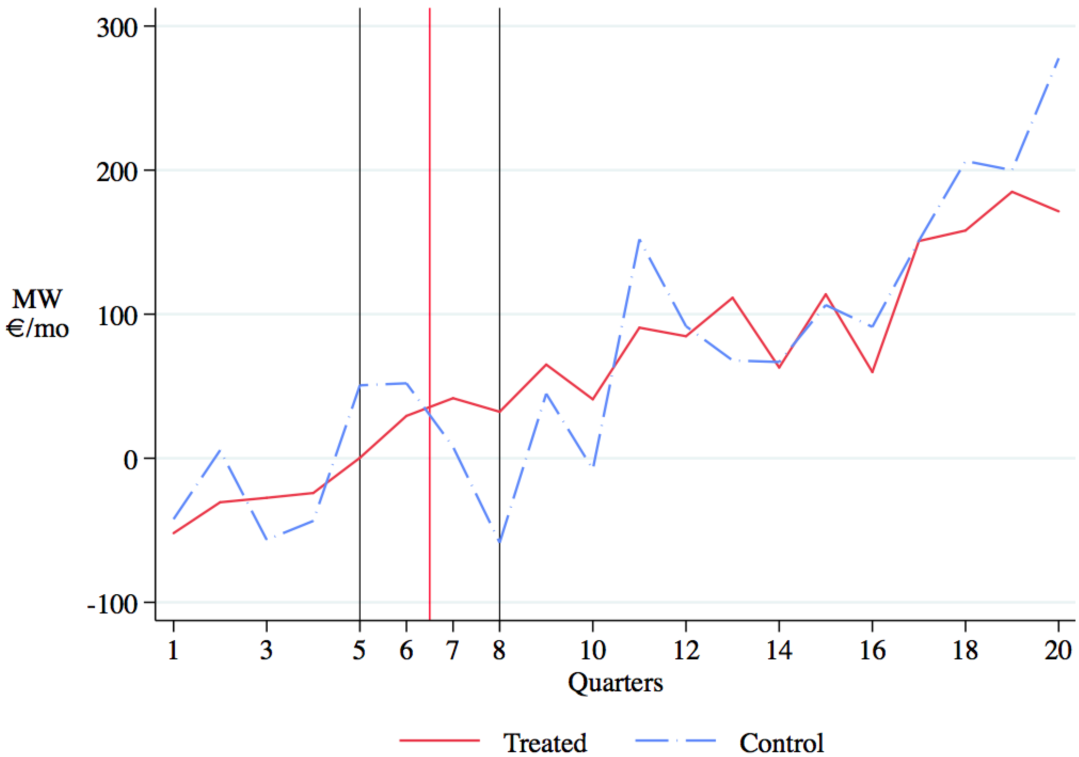
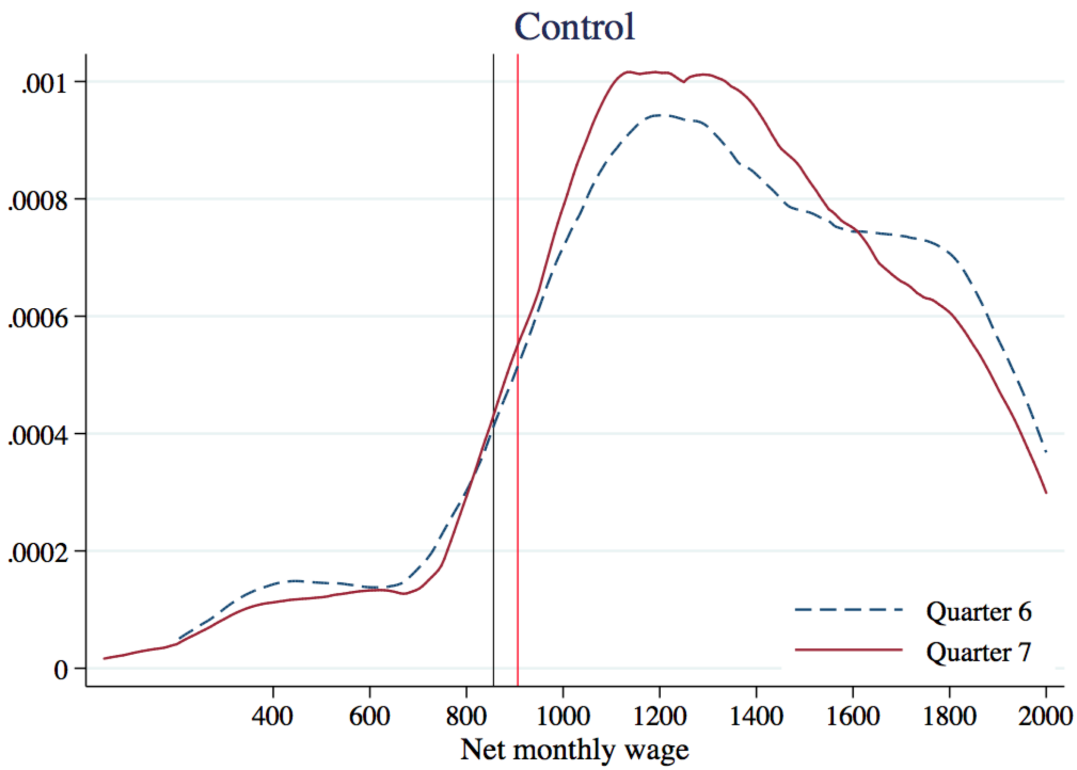
Figure A.5: Net monthly wageâs residuals in “treated” and “control”



Notes: quarters start in January 2003, which is the first year available. Therefore in this figures the 20 quarters cover from 2003 to 2007 included. The black vertical lines delineate the period under study for the first strategy, while the red vertical line indicates the NMW increase. Averages weighted using the probability assigned to each worker; individuals below the binding NMW are excluded. *Source:* own elaboration based on complete Labour Force Survey. Click <u>here</u> to go back to the main text.

Figure A.6: Kernel wage distribution for “control” group



Notes: net monthly wage restricted to €2,000. Sample restricted to those workers having a probability of at least 50%. Excluded individuals below the NMW. The black vertical line indicates the NMW before the increase, while the red vertical line reveals the new NMW. *Source:* own elaboration based on complete Labour Force Survey.

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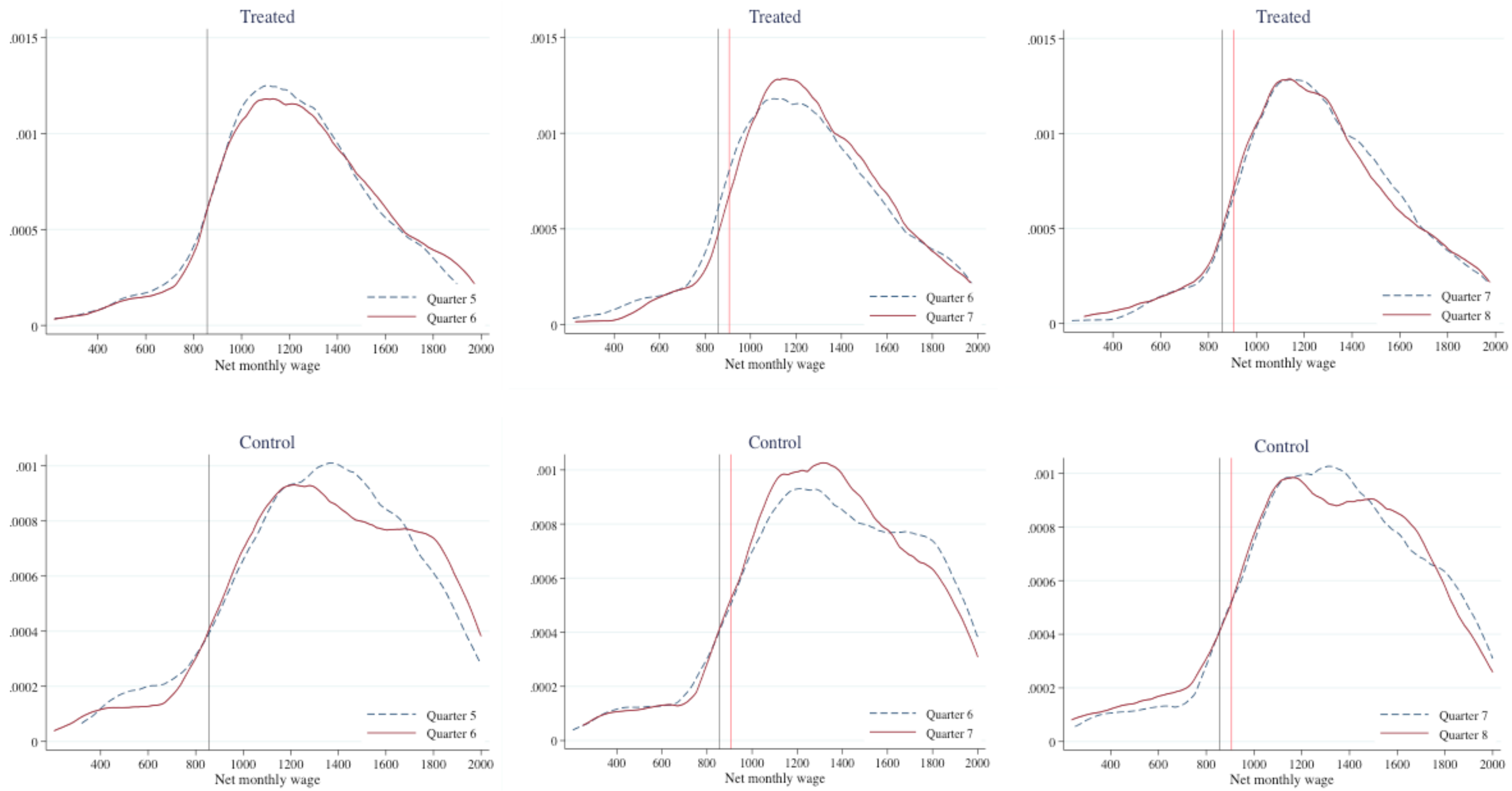
Figure A.7: Kernel wage distributions for quarters 5, 6, 7 and 8 by group.



Notes: net monthly wage restricted to €2,000. Sample restricted to those workers having a probability of at least 60%. Excluded individuals below the NMW. The black vertical line indicates the NMW before the increase, while the red vertical line reveals the new NMW. *Source:* own elaboration based on complete Labour Force Survey

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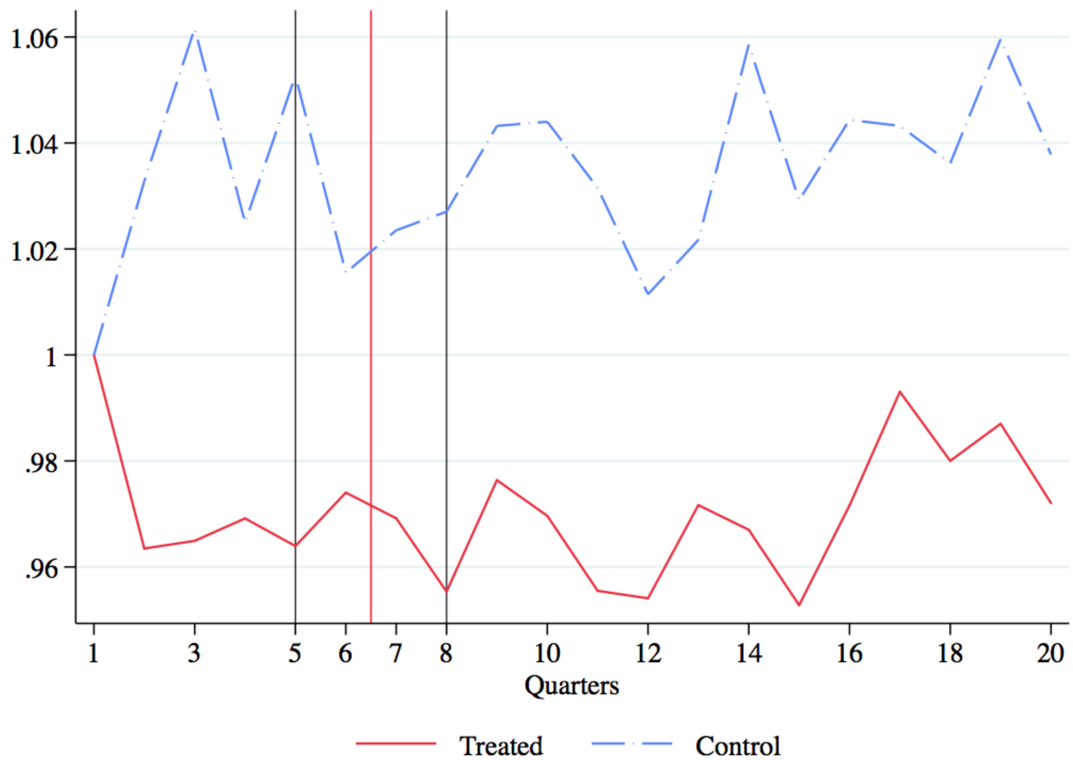
Figure A.8: Employment rate evolution over the period for "treated" and "control"



Notes: quarters start in January 2003, which is the first year available. Therefore in this figures the 20 quarters cover from 2003 to 2007 included. The black vertical lines delineate the period under study for the first strategy, while the red vertical line indicates the NMW increase. Averages weighted using the probability assigned to each worker; individuals below the binding NMW are excluded. Trend normalised in quarter 1. *Source:* own elaboration based on complete Labour Force Survey.

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A Tables

Table A.1: Summary statistics across contractual industries (years 2003 to 2012) - part 1/6

Title	Quarters above NMW	Covered workers	Covered workers (%)	Binding wage growth (%)	Average binding wage
Presse Quotidienne Regionale Employes	40	36	0.03	1.1	1,377
Papiers Cartons Production cadres	40	45	0.03	5.2	1,608
Papiers Cartons Transformation cadres	40	32	0.02	5.2	1,608
Boucherie Commerce	40	391	0.29	1.3	1,321
Tuiles Briques Industrie	40	60	0.04	4.4	1,637
Confiserie Chocolaterie Biscuiterie detaillants	40	101	0.07	2.3	1,311
Petrolie Industrie	40	325	0.24	3.2	1,331
Poissonnerie	40	108	0.08	2.5	1,358
Batiment ouvriers Franche Comte	40	158	0.12	1.4	1,324
Assurances Societes	40	1,411	1.04	2.1	1,324
Travaux Publics ouvriers Aquitaine	40	92	0.07	2.8	1,351
Travaux Publics ouvriers Bretagne	40	78	0.06	2.8	1,357
Travaux Publics ouvriers Idf	40	355	0.26	2.6	1,389
Travaux Publics ouvriers Lorraine	40	57	0.04	3.1	1,359
Travaux Publics ouvriers Midi Pyrenees	40	75	0.06	2.5	1,355
Travaux Publics ouvriers Nord Pas de Calais	40	111	0.08	2.6	1,363
Travaux Publics ouvriers Paca	40	161	0.12	3.0	1,377
Travaux Publics ouvriers Pays de la Loire	40	134	0.10	2.7	1,355
Travaux Publics ouvriers Picardie	40	29	0.02	2.4	1,363
Travaux Publics ouvriers Poitou Charente	40	34	0.02	2.5	1,343
Travaux Publics ouvriers Rhone Alpes	40	242	0.18	2.2	1,384
Banques	40	2,659	1.97	4.2	1,357
Eau et Assainissement	40	390	0.29	2.8	1,339
Assurances Courtage	40	343	0.25	1.7	1,371
Architectes	40	359	0.27	2.8	1,385
Travaux Publics Ingenieurs cadres	40	509	0.38	2.9	1,956
Batiment Ingenieurs cadres	40	507	0.38	2.2	1,424
Travaux Publics etam Idf	40	157	0.12	2.6	1,389
Travaux Publics etam Rhone Alpes	40	84	0.06	2.6	1,380
Navigation Personnel Sedentaire	40	99	0.07	4.7	1,331
Societes Financieres	38	357	0.26	6.0	1,343
Produits du Sol Engrais	38	190	0.14	2.6	1,268
Patisserie	38	160	0.12	1.8	1,300
Produits Alimentaires Elabores Indu	38	439	0.32	1.5	1,277
Agences de Voyages	38	315	0.23	1.8	1,300
Mutualite	38	581	0.43	1.5	1,345
Telecommunications	38	871	0.64	2.3	1,299
Siderurgie	38	198	0.15	2.4	1,298
Metallurgie Meurthe et Moselle	37	123	0.09	3.0	1,297
Tracteurs Materiel Agricole	37	759	0.56	1.8	1,276
Viandes Industrie Commerce de Gros	37	494	0.37	1.2	1,333
Organismes de Tourisme A But Non Lucratif	37	136	0.10	1.5	1,303
Cuirs et Peaux Industrie	36	32	0.02	1.5	1,284
Manutention Ferroviaire	36	77	0.06	1.7	1,275
Boulangerie Patisserie Artisanale	36	1,269	0.94	1.8	1,288
Laboratoires dentaires Protheses	36	143	0.11	2.7	1,300
Travaux Publics ouvriers Normandie	36	81	0.06	2.4	1,324
Assurances Agences Generales	36	235	0.17	1.9	1,302

Note: the table is sorted according to 'quarters above the NMW', which refers to the number of quarters a given contractual industry has had the minimum coefficient above the NMW. 'Covered workers' are the workers covered by the bargained wage at the industry level, the figure is in hundreds. 'Binding wages' are those wages that the industry has to comply with: it can be the industry bargained wage or the NMW, depending on whether the former falls above or below the latter. *Source:* own elaboration based on the Base des Minima de Branches.

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Table A.1: Summary statistics across contractual industries (years 2003 to 2012) - part 2/6

Batiment etam Nord Pas de Calais	36	98	0.07	2.3	1,321
Bourse	36	87	0.06	3.2	1,335
Entreprises de Proprete	36	3,611	2.67	2.0	1,286
Batiment ouvriers Basse Normandie	35	257	0.19	1.0	1,289
Pharmacie Repartition	35	147	0.11	1.4	1,295
Analyses Medicales Laboratoire	34	431	0.32	1.6	1,276
Charcuterie Industrie	34	375	0.28	1.0	1,282
Casinos	34	167	0.12	2.0	1,289
Import Export	33	422	0.31	1.5	1,284
Publicite	33	776	0.57	1.4	1,303
Aerauliques	33	197	0.15	1.6	1,292
Transports Publics Urbains	33	528	0.39	2.5	1,295
Mareyeurs	33	83	0.06	1.9	1,275
Remontees Mecaniques et Domaines Skiables	32	128	0.09	0.9	1,288
Charcuterie Commerce	32	163	0.12	3.6	1,277
Cabinets Medicaux	32	829	0.61	2.3	1,295
Centres Socioculturels	32	344	0.25	1.8	1,293
Distributeurs Conseil Hors Domicile	32	124	0.09	1.3	1,288
Recuperation Industrie et Commerce	31	323	0.24	2.1	1,279
Batiment ouvriers Midi Pyrenees	31	379	0.28	2.4	1,283
Bois d'Oeuvre et derives Negoce	31	122	0.09	1.7	1,272
Pharmacie Industrie	30	1,296	0.96	2.8	1,284
Habillement Commerce Succursales	30	1,031	0.76	1.5	1,374
Prevention Securite	30	1,479	1.09	2.0	1,294
Travail Temporaire Permanents	30	369	0.27	2.0	1,279
Eaux Boissons Sans Alcool	30	172	0.13	1.0	1,286
Metallurgie Eure	29	120	0.09	1.4	1,283
Vins de Champagne	29	48	0.04	3.0	1,323
Animation	29	1,226	0.91	1.6	1,289
Batiment ouvriers Nord Pas de Calais	29	516	0.38	2.4	1,285
Bricolage	29	742	0.55	1.4	1,287
Boulangerie Patisserie Industrielle	29	429	0.32	1.4	1,276
Huissiers Justice	29	102	0.08	1.6	1,299
Golf	29	52	0.04	1.3	1,284
Notariat	29	503	0.37	2.5	1,281
Avocats Personnel Salarie	28	342	0.25	1.7	1,284
Fruits Legumes Epicerie	28	667	0.49	1.2	1,288
Promotion Construction	28	233	0.17	1.7	1,266
Batiment ouvriers Champagne Ardennes	28	192	0.14	2.7	1,269
Pharmacie Officine	28	1,177	0.87	1.4	1,281
dechets Activites	28	543	0.40	3.0	1,275
Foyers Jeunes Travailleurs	28	58	0.04	1.8	1,293
Serigraphie	27	53	0.04	1.4	1,278
Automobile Services	27	4,322	3.20	2.0	1,266
Pharmacie Usage Veterinaire	27	275	0.20	1.6	1,278
Navigation de Plaisance	26	141	0.10	1.1	1,282
Metallurgie Saone et Loire	26	169	0.13	2.8	1,286
Batiment ouvriers Rhone Alpes	26	876	0.65	1.8	1,271
Tissus Tapis Linge de Maison	26	54	0.04	1.5	1,277
Metallurgie Nord Maubeuge	26	82	0.06	1.8	1,276
Grands Magasins	26	400	0.30	1.6	1,348
Batiment etam Paca	26	141	0.10	3.1	1,284

Note: the table is sorted according to ‘quarters above the NMW’, which refers to the number of quarters a given contractual industry has had the minimum coefficient above the NMW. ‘Covered workers’ are the workers covered by the bargained wage at the industry level, the figure is in hundreds. ‘Binding wages’ are those wages that the industry has to comply with: it can be the industry bargained wage or the NMW, depending on whether the former falls above or below the latter. *Source:* own elaboration based on the Base des Minima de Branches.

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Table A.1: Summary statistics across contractual industries (years 2003 to 2012) - part 3/6

Sucrerie Distillerie	26	76	0.06	3.9	1,315
Chaussures Commerce detaillant	25	104	0.08	1.2	1,296
Bureaux etudes Techniques	25	7,571	5.60	1.8	1,286
Hotellerie Plein Air	25	94	0.07	2.4	1,274
Verre Fabrication Main	25	46	0.03	1.3	1,289
Metallurgie Bas Rhin	25	307	0.23	1.8	1,274
Metallurgie Isere Hautes Alpes	25	297	0.22	1.8	1,275
Industrie Laitiere	24	467	0.35	1.7	1,277
Metallurgie Ingenieurs et cadres	24	4,136	3.06	2.7	1,312
Metallurgie Savoie	24	100	0.07	1.6	1,278
Manutention Nettoyage Aeroport	24	57	0.04	2.6	1,297
Optique Lunetterie	24	334	0.25	1.6	1,276
Immobilier	24	1,482	1.10	3.6	1,326
Batiment ouvriers Paca	24	595	0.44	3.0	1,273
Hlm	24	360	0.27	1.8	1,276
Geometres	24	116	0.09	2.7	1,288
Chimie	23	2,255	1.67	2.2	1,270
Metallurgie Ardennes	23	129	0.10	1.2	1,285
Batiment ouvriers Aquitaine	23	495	0.37	1.9	1,271
Batiment ouvriers Limousin	23	106	0.08	1.5	1,273
Cabinets dentaires	23	372	0.28	1.8	1,278
Alimentaires Diverses Industries	23	183	0.14	1.4	1,280
Menuiserie Charpentes	22	192	0.14	1.5	1,279
Habillement Industries	22	355	0.26	1.2	1,275
Verre Fabrication Mecanique	22	243	0.18	1.4	1,286
Metallurgie Seine et Marne	22	137	0.10	1.7	1,274
Librairie Commerce detail Papeterie	22	645	0.48	2.2	1,272
Chaussures Industrie	22	89	0.07	1.3	1,289
Batiment ouvriers Alsace	22	260	0.19	2.3	1,273
Batiment ouvriers Pays de La Loire	22	611	0.45	1.9	1,268
Batiment ouvriers Idf	22	1,090	0.81	1.7	1,271
Metallurgie Cote d'Or	22	105	0.08	2.4	1,269
Batiment etam Haute Normandie	22	65	0.05	2.8	1,275
Batiment etam Rhone Alpes	22	227	0.17	2.4	1,282
Photographie	22	82	0.06	1.4	1,292
Exploitations Frigorifiques	21	58	0.04	2.3	1,264
Commerces de Gros	21	3,315	2.45	1.6	1,273
Gardiens Concierges	21	708	0.52	2.4	1,274
Batiment ouvriers Poitou Charente	21	285	0.21	2.7	1,263
Metallurgie Seine Maritime Rouen Dieppe	21	189	0.14	2.2	1,273
Hotels Cafes Restaurants	21	5,936	4.39	1.3	1,306
Cooperatives Consommation	21	.	.	1.5	1,316
Carrieres et Materiaux etam	20	164	0.12	1.5	1,274
Plasturgie	20	1,242	0.92	1.8	1,268
Vins Cidres Jus de Fruits	20	413	0.31	1.7	1,265
Quincaillerie Commerce Interregionale	20	179	0.13	1.1	1,296
Combustibles Liquides Gaz Commerce	20	160	0.12	2.0	1,275
Batiment ouvriers Centre	20	392	0.29	1.6	1,266
Maroquinerie	20	173	0.13	1.3	1,293
Metallurgie Aisne	20	92	0.07	1.5	1,276
Batiment etam Aquitaine	20	113	0.08	3.1	1,292
Batiment etam Bretagne	20	115	0.09	3.4	1,291
Batiment etam Centre	20	92	0.07	3.4	1,282

Note: the table is sorted according to 'quarters above the NMW', which refers to the number of quarters a given contractual industry has had the minimum coefficient above the NMW. 'Covered workers' are the workers covered by the bargained wage at the industry level, the figure is in hundreds. 'Binding wages' are those wages that the industry has to comply with: it can be the industry bargained wage or the NMW, depending on whether the former falls above or below the latter. *Source:* own elaboration based on the Base des Minima de Branches.

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Table A.1: Summary statistics across contractual industries (years 2003 to 2012) - part 4/6

Batiment etam Lorraine	20	68	0.05	3.5	1,293
Batiment etam Midi Pyrenees	20	89	0.07	3.3	1,302
Batiment etam Pays de La Loire	20	153	0.11	3.4	1,284
Batiment etam Idf	20	322	0.24	3.3	1,281
Imprimerie et Industries Graphiques	18	608	0.45	1.8	1,271
Transports Aeriens Personnel au Sol	18	901	0.67	1.6	1,267
Metallurgie Finistere	18	98	0.07	1.8	1,268
Metallurgie Ain	18	165	0.12	1.7	1,272
Tourisme Social et Familial	18	128	0.09	1.8	1,274
Papiers Cartons Production ouvriers etam	18	205	0.15	1.8	1,267
Papiers Cartons Transformation ouvriers etam	18	207	0.15	1.8	1,267
Batiment ouvriers Bretagne	18	531	0.39	2.1	1,272
Batiment ouvriers Languedoc Roussillon	18	357	0.26	1.5	1,270
Batiment ouvriers Picardie	18	259	0.19	1.7	1,265
Jeux Jouets Industrie	18	68	0.05	1.6	1,276
Metallurgie Drome Ardeche	18	169	0.13	2.0	1,269
Meunerie	18	211	0.16	1.2	1,270
Prestataires de Services du Secteur Tertiaire	18	1,247	0.92	1.7	1,276
Particulier Employeur	18	7,750	5.74	2.0	1,268
Metallurgie Gard Lozere	18	52	0.04	1.6	1,299
Vente A Distance	18	272	0.20	2.3	1,266
Chaussures Commerce Succursales	17	211	0.16	1.3	1,304
Commerces detail Non Alimentaire	17	1,053	0.78	1.7	1,273
Batiment ouvriers Haute Normandie	17	290	0.21	1.6	1,268
Jardineries	17	182	0.13	1.9	1,264
Ameublement Negoce	17	663	0.49	1.9	1,267
Fleuristes Animaux Familiers	17	232	0.17	1.3	1,274
Metallurgie Vosges	17	74	0.05	2.0	1,270
Caoutchouc	16	579	0.43	1.6	1,279
Bois et Industries	16	450	0.33	2.3	1,267
Pompes Funebres	16	169	0.13	2.1	1,266
Metallurgie Haute Savoie	16	249	0.18	2.0	1,269
Metallurgie Morbihan Ille et Vilaine	16	241	0.18	1.8	1,267
Equipements Thermiques	16	308	0.23	2.2	1,278
Metallurgie Midi Pyrenees	16	480	0.36	1.7	1,267
Metallurgie Loire Atlantique	16	285	0.21	1.9	1,270
Batiment ouvriers Lorraine	16	288	0.21	2.3	1,266
Metallurgie Jura	16	61	0.05	1.4	1,272
Veterinaires Cabinets	16	140	0.10	2.2	1,266
Metallurgie Loir et Cher	16	81	0.06	1.6	1,288
Coiffure	16	1,036	0.77	1.9	1,279
Bijouterie Joaillerie	15	177	0.13	1.7	1,275
Metallurgie Marne Haute Meuse	15	115	0.09	1.9	1,268
Fruits Legumes Expedition Exportation	15	83	0.06	1.7	1,272
Habillement Commerce detail	15	694	0.51	1.8	1,281
Metallurgie Oise	15	158	0.12	1.9	1,266
Transports Routiers	14	6,556	4.85	2.4	1,271
Cartonnage Industrie	14	128	0.09	1.6	1,266
Metallurgie Manche	14	78	0.06	1.7	1,266
Metallurgie Calvados	14	137	0.10	1.8	1,265
Organismes Formation	14	755	0.56	1.8	1,279
Publicite Logistique	14	77	0.06	1.8	1,271

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Table A.1: Summary statistics across contractual industries (years 2003 to 2012) - part 5/6

Metallurgie Haut Rhin	14	299	0.22	1.8	1,272
Metallurgie Vendee	14	149	0.11	2.2	1,265
Metallurgie Haute Saone	14	97	0.07	1.7	1,272
Horlogerie Bijouterie Commerce	13	194	0.14	2.0	1,272
Batiment ouvriers Bourgogne	13	237	0.18	2.1	1,265
Audiovisuel Electronique Equipement Menage	13	762	0.56	1.7	1,267
Metallurgie Mayenne	13	88	0.07	2.2	1,264
Metallurgie Belfort	13	236	0.17	1.9	1,268
Textile Industrie	12	691	0.51	1.7	1,268
Reprographie	12	43	0.03	2.7	1,267
Metallurgie Moselle	12	264	0.20	2.0	1,267
Experts Comptables	12	1,297	0.96	2.1	1,268
Metallurgie Doubs	12	119	0.09	2.0	1,266
Blanchisserie Interregionale	12	318	0.24	1.8	1,266
Metallurgie Aube	12	47	0.03	2.0	1,265
Metallurgie Pyrenees Atlantiques	12	106	0.08	1.6	1,272
Metallurgie Orne	11	80	0.06	2.0	1,273
Metallurgie Seine Maritime Le Havre	11	101	0.07	2.0	1,267
Metallurgie Eure et Loir	11	95	0.07	2.3	1,265
Restaurants de Collectivites	11	942	0.70	1.9	1,269
Metallurgie Herault Aude	11	81	0.06	1.9	1,266
Metallurgie Nord Valenciennes	11	203	0.15	2.0	1,266
Espaces de Loisirs	11	346	0.26	2.3	1,268
Volailles Industrie Commerce de Gros	11	281	0.21	1.6	1,270
Panneaux Bois	11	59	0.04	2.2	1,265
Metallurgie Region Parisienne	10	2,731	2.02	2.2	1,267
Bonneterie	10	190	0.14	2.2	1,264
Ameublement Fabrication	10	487	0.36	2.0	1,263
Metallurgie Nord dunkerque	10	61	0.05	1.9	1,267
Batiment ouvriers Auvergne	10	217	0.16	2.5	1,265
Metallurgie Bouches du Rhone	10	231	0.17	2.1	1,267
Metallurgie Charente Maritime	9	59	0.04	2.0	1,265
Metallurgie Creuse Haute Vienne	9	85	0.06	2.5	1,264
Ceramique Industrie	9	102	0.08	1.9	1,266
Metallurgie Alpes Maritimes	9	65	0.05	2.5	1,264
Metallurgie Cher	9	74	0.05	2.2	1,266
Metallurgie deux Sevres	9	89	0.07	2.2	1,266
Metallurgie Maine et Loire	9	169	0.13	2.3	1,265
Alimentation	9	6,601	4.89	1.9	1,266
Metallurgie Somme	9	91	0.07	2.0	1,267
Metallurgie Allier	8	74	0.05	1.9	1,268
Metallurgie Marne	8	88	0.07	2.0	1,265
Metallurgie Vienne	8	86	0.06	2.3	1,263
Cinema Exploitation	8	97	0.07	1.8	1,265
Metallurgie Loire Yssingeaux	7	239	0.18	2.2	1,265
Parfumerie Esthetique	7	491	0.36	1.9	1,273
Materiaux Construction ouvriers	6	253	0.19	2.7	1,263
Materiaux Construction etam	6	400	0.30	2.7	1,263
Metallurgie Rhone	6	440	0.33	2.3	1,264
Metallurgie Sarthe	6	169	0.13	2.3	1,265
Restauration Rapide	6	1,460	1.08	2.6	1,264
Ciments Industrie ouvriers	5	21	0.02	2.2	1,263
Metallurgie Nievre	5	58	0.04	2.3	1,265

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Table A.1: Summary statistics across contractual industries (years 2003 to 2012) - part 6/6

Metallurgie Flandres Douaisis	5	234	0.17	2.2	1,266
Metallurgie Puy de Dome Clermont Ferrand	5	115	0.09	2.2	1,264
Metallurgie Girondes Landes	5	180	0.13	2.4	1,265
Metallurgie Yonne	5	80	0.06	2.5	1,263
Ciments Industrie etam	4	34	0.03	2.3	1,263
Sport Equipement Loisirs Commerce	4	603	0.45	2.7	1,263
Metallurgie Cote D Armor	4	54	0.04	2.3	1,264
Edition	4	210	0.16	2.6	1,264
Metallurgie Indre et Loir	4	81	0.06	2.2	1,264
Metallurgie Puy de Dome Thiers	3	32	0.02	2.2	1,265
Metallurgie Loiret	3	135	0.10	2.3	1,264
Metallurgie Pas de Calais	2	191	0.14	2.5	1,263
Medecine du Travail	0	159	0.12	2.7	1,263
Miroiterie	0	119	0.09	2.7	1,263
Cafeterias	0	200	0.15	2.7	1,263
Ports Autonomes	0	135	0.10	2.7	1,263
Average	**22**	**495**	**0.4**	**2.1**	**1292**

Note: the table is sorted according to 'quarters above the NMW', which refers to the number of quarters a given contractual industry has had the minimum coefficient above the NMW. 'Covered workers' are the workers covered by the bargained wage at the industry level, the figure is in hundreds. 'Binding wages' are those wages that the industry has to comply with: it can be the industry bargained wage or the NMW, depending on whether the former falls above or below the latter. *Source:* own elaboration based on the Base des Minima de Branches.

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Table A.2: Descriptive statistics on the two groups of contractual industries

Variable	Only LFS Mean	Only LFS Median	Full set of info Mean	Full set of info Median	Mean Difference	P-value
Panel A						
Monthly wage	1,680	1,500	1,716	1,457	36	0.000
Probability	0.51	0.36	0.36	0.20	-	-
Years of tenure	13	10	10	7	-3	0.000
Age	41.5	42	38.6	37	-2.88	0.000
Panel B						
Region prevalence	IDP 18%		IDP 18%			
Male prevalence	51%		60%			
University or higher	25.13%		19.56%			
Number of observations	403,561		520,163			
Number of individuals	242,595		182,240			

Note: sample restricted to the working population. "Only LFS" refers to the 144 contractual industries without information on wage adjustments. Statistics in Panel A are calculated using regressions weighted with probabilities. Figures in Panel B are computed restricting the sample to "only LFS" and "full set of info" respectively. *Source:* own elaboration based on complete Labour Force Survey.

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Table A.3: Share of industries in a given category in t

Quarter NMW	Category 1.	2.	3.	4.	5.
3	0.46	0.1	0.21	0.01	0.16
7	0.29	0.09	0.29	0.03	0.23
11	0.18	0.07	0.27	0.02	0.29
15	0.16	0.08	0.32	0.05	0.26
19	0.19	0.12	0.25	0.02	0.29
22	0.21	0.08	0.28	0.01	0.27
23	0.23	0.02	0.09	0.003	0.39
27	0.32	0.12	0.22	0.01	0.2
29	0.18	0.23	0.12	0.003	0.28
33	0.12	0.36	0.3	0.01	0.1
37	0.06	0.41	0.32	0.02	0.1
39	0.21	0.05	0.36	0.03	0.21
Average	0.22	0.15	0.26	0.02	0.23

Note: figures presented in percentage. Category 1 means that the industry's collective wage lied above the NMW in $t-1$; in t it does not adjust its minimum wage floor; the industry's collective wage is still above the NMW. Category 2 that the industry lied above the NMW in $t-1$; in t it adjusts its minimum wage floor; the industry's collective wage is still above the NMW. Category 3 that the industryâs collective wage lied above the NMW in t-1; in t it does not adjust its minimum wage floor; hence, following an increase in the NMW, the industry's collective wage falls below the NMW. Category 4 that the industry's collective wage lied below the NMW in $t-1$; in t it adjusts its minimum wage floor; the industry's collective wage moves above the NMW. Category 5 that the industry's collective wage lied below the NMW in $t-1$; in t it does not adjust its minimum wage floor; the industry's collective wage remains below the NMW. *Source:* own elaboration based on Base des Minima de Branche.

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Table A.4: Descriptive statistics for "treated" and "control"

Variable	Treated Mean	Treated Median	Control Mean	Control Median	Mean difference	P-value
Panel A						
Monthly wage	1,503	1,303	1,654	1,438	-151	0.000
Probability	0.0807	0.013	0.087	0.015	-0.002	0.000
Years of tenure	10.25	6.2	12.1	8	-1.85	0.000
Age	38.28	38	38.5	39	-0.22	0.004
Industry size	60,705	18,200	40,184	24,300	20,521	0.000
Panel B						
Region prevalence	IDP 14%		IDP 23%			
Male prevalence	61%		48%			
University or higher	16%		23%			
Number of observations	63,697		25,077			
Number of individuals	5,143		2,183			

Note: sample restricted to the quarters of the analysis, i.e. quarters from 5 to 8. Figures in Panel A obtained through weighted regressions. "treated" are those industries that experienced a rise in their minimum wages, "control" are industries that left their minimum unaltered throughout the period. *Source:* own elaboration based on complete Labour Force Survey.

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Table A.5: Robustness check wages (first strategy)

	(I)	(II)	(III)
Interaction (β)	0.005	0.001	0.006
	(0.0239)	(0.0163)	(0.0166)
Age		0.058***	0.054***
		(0.0034)	(0.004)
Age^2		-0.001***	-0.0005***
		(0.0000)	(0.0000)
Being a male		0.281***	0.247***
		(0.0139)	(0.0106)
Years of tenure		0.008***	0.007***
		(0.0008)	(0.0009)
Controls	No	Yes	Yes
Time FE	Yes	Yes	Yes
Industry FE (84 categories)	No	No	Yes
Adjusted R-squarec	0.01	0.4482	0.4724
Observations	45,333	42,978	42,978

Note: clustered standard errors at the industry level in parenthesis; * $p < .1$, ** $p < .05$, *** $p < .001$. Probability weights applied. Controls include dummies for highest degree obtained, dummies for region of residence, dummy for being a male, a continuous variable for years of tenure and a quadratic term for age. The dependent variable is the logarithm of net monthly earnings. Sample restricted to quarter 5 and 6 only.

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Table A.6: Robustness check on actual wages (second strategy)

Model 6	Log actual wage					
	(I)	(II)	(III)	(I)	(II)	(III)
Log NMW	0.76***	0.51***	0.55***			
	(0.046)	(0.034)	(0.029)			
Log lagged NMW	-0.013	0.007	0.008			
	(0.028)	(0.021)	(0.018)			
Log BW				0.31	0.20	0.10
				(0.437)	(0.156)	(0.07)
Log lagged BW				0.61	0.16	0.01
				(0.374)	(0.122)	(0.048)
Controls	No	Yes	Yes	No	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (264 categories)	No	No	Yes	No	No	Yes
Adjusted R-squared	0.024	0.443	0.476	0.031	0.444	0.476
Observations	481,015	458,723	458,723	480,992	458,702	458,702

Note: clustered standard errors at the industry level in parenthesis; * $p < .1$, ** $p < .05$, *** $p < .001$. Probability weights applied. Controls include dummies for highest degree obtained, dummies for region of residence, dummy for being a male, a continuous variable for years of tenure and a quadratic term for age.

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