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# 30,000 minimum wages:

## The economic effects of collective bargaining extensions \*

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### Abstract

Many governments extend the coverage of collective agreements to workers and employers that were not involved in their bargaining. These extensions may address coordination issues but may also distort competition by imposing sector-specific minimum wages and other work conditions that are not suitable for some firms and workers. In this paper, we analyse the impact of such extensions along several economic margins. Drawing on worker- and firm-level monthly data for Portugal, a country where extensions have been widespread, and the scattered timing of the extensions, we find that, while continuing workers experience wage increases following an extension, formal employment and wage bills in the relevant sectors fall, on average, by 2%. These results increase by about 25% across small firms and are driven by reduced hirings. In contrast, the employment and wage bills of independent contractors, who are not subject to labour law or collective bargaining, increases by over 1% following an extension.

*Keywords:* Collective agreements, Worker flows, Labour law.

*JEL Codes:* J52, K31, J23.

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# 1 Introduction

The minimum wage is typically regarded as a policy variable that exhibits relatively little variability within countries. Indeed, a national minimum wage, if applicable, tends to be revised at a relatively low time frequency, possibly with further (upward) adjustments at sub-national levels. However, in many countries, the number of minimum wages is actually very large - and their values can vary frequently, not only over time and space but also along other dimensions. This situation arises from minimum wages set by collective bargaining agreements, in particular when such agreements are extended by a country's authorities beyond their subscribing employer associations and unions - and their affiliated workers (Traxler & Behrens 2002, OECD 2017). Since collective agreements typically establish minimum wages for the most common job types and job levels in those firms, their extensions are equivalent to the setting of fully-binding minimum wages, even if only for specific economic sectors or sector-region pairs.

This paper examines empirically the effects of such extensions of collective bargaining agreements - and the resulting large number of binding minimum wages - along several key economic dimensions. To the best of our knowledge, our paper is one of the first to address this important question, in particular from a quasi-experimental perspective, and focusing on employment, wage bills (payroll) and contract work (service provision) effects.<sup>1</sup>

These extensions - which also explain the large wedges between union density and union coverage in many countries (that can reach over 80 percentage points, as in France) - may promote a greater leveling of the playing field in working conditions (including minimum wages). Indeed, extensions require all firms in a sector to comply with the same set of minimum standards determined jointly by a subset of firms and workers in that sector. Any 'unfair' competition that may follow from firms undercutting the collective agreements' wage levels is automatically ruled out from a legal perspective once that agreement's extension comes into force.

On the other hand, the combination of minimum pay levels and other working conditions

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<sup>1</sup>See Margolis (1994) for an earlier paper focusing on theoretical aspects of collective agreement extensions, Magruder (2012) for an earlier analysis of a developing country, Murin et al. (2014) for a cross-country panel analysis, and Diez-Catalan & Villanueva (2015) and Guimaraes et al. (2017) for two recent studies of Spain and Portugal, respectively. On the other hand, the (national- or state-level) minimum wage literature is very large. Recent contributions include Dube et al. (2016), Neumark et al. (2014), Portugal & Cardoso (2006), Allegretto et al. (2017), Jardim et al. (2018), and Harasztsi & Lindner (2019). See also Avouyi-Dovi et al. (2013) for a related study of collective bargaining.

that follows from a collective agreement, namely because of rent sharing (Abowd & Lemieux 1993, Martins 2009*b*, Card et al. 2014) may not be appropriate for all firms and workers in a sector. In particular, smaller or new firms may target different segments of the product market, which imply different optimal settings in working conditions, especially in a context of globalisation, technological change and business cycles. In this context, extensions may be a tool to raise rivals' costs, therefore distorting competition (Haucap et al. 2001). Moreover, labour market clearing during downturns may require greater flexibility in working conditions than the one set in collective agreements, in particular in low inflation environments (European Commission 2011).

Our study also contributes to a more comprehensive understanding of minimum wages, whose potential role is much wider than that stemming from a single value over a period of time, typically affecting only a relatively small subset of low-skilled workers. As indicated above, the many countries that extend collective agreements, including France, Germany, Italy and Spain (du Caju et al. 2008, Visser 2013), are in fact subject to many thousands of minimum wages, potentially binding most workers, and evolving in a partly decentralised way, with yearly adjustments in many instances. These circumstances also magnify the scope for downward nominal wage rigidity effects (Steinar & Fredrik 2008, Martins et al. 2010, Dias et al. 2013).

Our empirical analysis is based on particularly rich data from Portugal, a country where extension mechanisms have been used widely (Cardoso & Portugal 2005, IMF 2011, Martins 2014), leading to nearly 30,000 *de facto* minimum wages, as indicated in the title of the paper. The number corresponds to the sum of different job titles across sectoral collective agreements that are subject to specific minimum wages. More than 90% of the sectoral collective agreements issued in the period under analysis (2007-2011) were subject to extensions, even if in most cases the representativeness of the bargaining partners was poor. This period is also characterised by increasing unemployment in the country, as it includes the recession of 2008-09 and part of the Eurozone debt crisis in 2010-11.

Our data lists all extensions implemented over the period under analysis, including information on their timings and relevant applicable industries. Furthermore, we also draw on monthly records of formal employment, hirings, separations, salaries, and other variables of each worker and firm in the country over the same period. Interestingly, we also examine

data on informal service providers - contractors or self-employed workers. The latter may correspond to ‘disguised’ employment relationships and represent an important margin of adjustment in segmented labour markets, despite the greater focus of the literature on the different case fixed-term employment contracts (Blanchard & Landier 2002). We then employ a difference-in-differences approach, where we track industry-specific time series of employment or the other variables mentioned above or their corresponding worker-level variables, and examine if there are systematic changes following the issuing of the extensions.

Our results, robust to several checks, including a falsification exercise, indicate a strong negative effect of extensions upon both employment and wage bills (payroll). For instance, over the four months after an extension comes into force, the average employment levels in the sectors affected drop by 2%. This employment effect dominates the wage increases for continuing workers, leading to a significant decline in wage bills. These changes are driven by a strong negative effect of extensions upon hirings while firm closures are also found to increase immediately following an extension. On the other hand, the employment of independent contractors (who can be dismissed at will and be paid below minimum wage rates) increases, potentially leading to further segmentation between protected jobs and non-standard work.

The next section discusses in greater detail the economics of the extensions of collective agreements and also presents the specific case of Portugal, including its labour market and industrial relations. Section 3 describes the data sets used in this paper and some descriptive statistics. Section 4 presents the main sector-level results. Section 5 presents robustness checks and extensions, including an analysis of firm entry and exit effects, and Section 6 describes our worker-level findings. Finally, Section 7 concludes.

## **2 Collective agreements extensions**

Collective agreements are an important building block of industrial and employment relations across many countries, in particular in continental Europe. These agreements establish a large number of working conditions that apply to the signatory parties, including minimum wages by job types and seniority but also many other issues such as working time, overtime premiums, fringe benefits, training, health and safety, promotions, contract types, severance pay, bonuses, grievances, etc. These agreements can therefore upgrade (or downgrade) the conditions set by the country’s statutory employment legislation.

Depending on the level of collective bargaining centralisation (Calmfors & Driffill 1988), the signatory parties will typically be an employer association - representing a number of firms, typically in the same sector, sometimes also from a specific region - and one or more unions - representing a number of affiliated workers, most of whom employed in the firms represented by the employer association. Moreover, the scope of collective agreements will at first correspond to the workers subject to the double affiliation principle: workers that are simultaneously members of the subscribing union(s) and that are employed by firms affiliated with the subscribing employer associations. The intersection of these two dimensions implies that, across several European countries, with the notable exception of the Nordic region, the direct coverage of collective agreements is relatively small, given the typically low union and employer association density rates, especially in Southern Europe. In contrast, non-covered workers have their employment relationships determined by individual contracts bargained directly with their employers, subject only to statutory law.

However, collective agreements may also be extended by a country's authorities, typically the labour ministry, either by discretion or if some criteria are met. In any case, once an extension is in force, the terms of the original collective agreement will apply not only to workers subject to the double affiliation principle described above but also to all workers employed in the industry in which the extension is applicable.<sup>2</sup> Collective agreement extensions explain the large gap between union density and union coverage that can be observed in many countries. Once an extension is in force, the affected workers and employers have to follow the terms and conditions of the underpinning collective agreement, in particular their higher minimum wages, in comparison to the terms and conditions applicable to individual contracts (which will typically be subject to a single, statutory minimum wage).

Many arguments have been put forward in support for extensions, including those similar to the case of minimum wages. For instance, extensions lead to the establishment of minimum wages for more qualified workers that do not meet the double affiliation criterion and which otherwise would only be subject to a likely non-binding statutory minimum wage. The additional minimum wages from extensions can therefore increase living standards, promote a more balanced income distribution and foster firm-specific training. Furthermore, monopsony arguments in support of minimum wages (Azar et al. 2018, Martins 2018) may be even more

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<sup>2</sup>The domain of collective agreements can also originally be restricted to a given region (and or occupation), in which case the extension is also likely to be restricted to that same region (and or occupation). For simplicity, we will henceforth only refer to extensions within industries.

relevant in the case of workers that are placed at an intermediate or higher level in firm's hierarchy, given the potentially greater role of firm-specific skills and information asymmetries in the case of more skilled workers, compared to less skilled workers. More generally, collective bargaining and the resulting social dialogue can be boosted by extensions, thus fostering higher levels of productivity and living standards.

However, as well known in the minimum wage literature, higher minimum wages following from extensions may hurt employment levels or growth rates, in particular amongst less skilled workers, reducing labour market efficiency and leading to segmentation and (youth) unemployment. Such higher minimum wages may also lead to higher prices. Moreover, extensions may magnify the scope for downward nominal wage rigidity effects, particularly in times of low inflation and economic downturns (Steinar & Fredrik 2008, Martins et al. 2010, Dias et al. 2013, Jimeno & Thomas 2013), even if real wages are responsive when inflation is higher (Martins et al. 2012). Extensions could therefore diminish significantly the ability of the labour market to accommodate negative shocks.

From an industrial organisation perspective, the absence of extensions may promote 'unfair competition' from firms outside employer associations that pay lower wages than those set in collective agreements. These firms can therefore charge lower prices for their products, undercutting their competitors that are subject to the typically more generous working conditions that follow from collective bargaining. In the absence of extensions, such competition will exert a downward pressure on wages. On the other hand, as OECD (2012) put it, '[...] dominant firms impose wage and working conditions on others via the administrative extension of collective agreements, reducing competition and entry, thereby hurting competitiveness', echoing the views in Haucap et al. (2001). This may be consistent with the finding that statutory minimum wages established or influenced greatly by social partners (the national-level representatives of employer associations and unions) tend to be higher than those without social partner intervention (Boeri 2012).<sup>3</sup>

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<sup>3</sup>It is also important to note that the incentives for employer association membership are positively affected by the probability of extensions being issued. If extensions are more likely, membership would be more advantageous as it would allow a firm to influence the outcome of a collective agreement that will bind it eventually. In contrast, if extensions are not likely, then non-members would benefit from the greater flexibility of individual bargaining or firm-specific collective agreements. On the other hand, the incentives for workers to become union members may be diminished by the likelihood of an extension given the resulting scope for free-riding: benefiting from the more generous conditions of a collective agreement negotiated by unions without paying union fees to the trade union that conducts such bargaining.

## 2.1 The case of Portugal

The Labour Code of Portugal establishes that '[a] collective agreement [...] in force can be applied, entirely or partly, by an extension to employers and employees in the economic activity and profession considered in the collective agreement' (article 514). Furthermore, '[t]he extension is possible after [the government] weighing the social and economic circumstances that may justify it, in particular the identity or economic or social similarity of the cases in the extension and the underlying collective agreement.'

Until early 2011, sectoral collective agreements, the most predominant type in the country, were virtually always extended by the Government, following a request from one (or both) of its subscribers (the union or the employers' association). An extension would then widen substantially the domain of the underlying collective agreement. This domain is originally limited to those workers simultaneously affiliated with the trade union that participated in the bargaining and employed by a firm affiliated with the employers' association. Following the extension, the collective agreement will cover all workers employed in the relevant industry. This original domain, before extension, would typically be particularly small, as in many other (Southern) European countries: for instance, Addison et al. (2015) reports that only 11% of the private sector employees in Portugal in 2010 was unionised. However, firms that are affiliated with employer associations would tend to extend the collective agreement to their own non-unionised employees, in order to apply the same wage table to all workers and not to encourage trade union affiliation (Martins 2019).

Another important aspect, possibly more specific to the case of Portugal, is that statutory employment law, which is applicable to all employees, is considerably comprehensive in its 560 articles. The country's Labour Code therefore already establishes potentially relatively high floors in most working conditions that can also be subsequently regulated in collective agreements. For instance, the OECD Employment Protection Legislation comparisons typically place Portugal in the top positions across most of its different dimensions during the period covered in our empirical analysis (OECD 2012). This implies that the most important provisions that are actually introduced by collective agreements (and extensions, in a second stage) concern the minimum wages per job type - and not other aspects of the employment relationship, as shown in Martins & Saraiva (2019). Even if the labour law allows that some provisions are negotiated in a downward sense (i.e. become less generous to employees) pro-



vided that, from a global perspective, working conditions are improved, this possibility is rarely accepted by unions. This context also indicates that, in general, new agreements or revisions of existing agreements do not introduce major changes to working conditions other than higher minimum wages.

Another related point that follows from the relatively restrictive employment law of Portugal and that we also examine below is that the usage of contractors or service providers may become relatively more interesting when compared to employees from the firm's point of view. While employees are subject to employment law (including collective bargaining), contractors are not. This implies that the wages and all other working conditions of contractors are far more flexible than those of employees: the former can be terminated at will and their cost can be renegotiated freely. To prevent the reclassification of employees as contractors by firms, the Labour Code lays out a number of criteria to distinguish between the two types of work relationships, such as whether the worker can choose by herself when to conduct the work and where to work from, if the tools used by the worker are the property of the firm, and if the worker has to follow the direct supervision from a manager of the firm. If a court of law decides that these criteria are not properly met, then the worker is considered as an employee (under an open-ended contract) and the firm is liable to pay fines.

Table 1 presents statistics about new and revised collective agreements and their extensions in Portugal in each year over the period 2005-2012. Collective agreements are divided into their three main categories, which depend on the employer side of the bargaining: an employers' association, a group of firms, or a single firm. Extensions are almost always only issued in the first case only but are also almost always required in that case.<sup>4</sup> The figures suggest a very high level of sectoral centralisation of bargaining in Portugal, as in other European countries. Indeed, sectoral agreements are clearly the most predominant form of collective agreements, at about 150 per year up to 2010. This figure compares to about only 25 multi-firm agreements and 70 firm-specific agreements, in striking contrast to a number of more than 300,000 firms with employees in the country. More importantly, a large percentage (about 90%) of sectoral agreements are subject to extensions.<sup>5</sup> The figures also indicate that, in 2011 and, in particular, in 2012 there is a steep decline in collective agreements (except in the case of firm-specific ones) and extensions. This development follows from the adjustment programme

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<sup>4</sup>See Hartog et al. (2002) for a comparison of wage levels under different collective agreement types.

<sup>5</sup>The number of extensions exceeds that of agreements in some years because some extensions concern agreements signed in the previous year.

adopted in Portugal discussed below and used in one of our robustness checks (Subsection 5.5).

Another important component of Table 1 concerns the number of workers subject to the new and revised collective agreements of each type and in each year, including the effect of the extension.<sup>6</sup> Between 2005 and 2010, in the case of sectoral agreements, they average 1.4 million workers per year - or approximately half the total private sector employment level. In contrast, multi-firm and firm-specific agreements have a much smaller scope, of about 50,000 workers each per year. Also noteworthy is the large decline in the sectoral agreement coverage of new or revised agreements in 2012, when both the number of agreements and extensions is substantially smaller, although many more workers remain covered by earlier agreements.

As to the timing of the legal effects of the extension, it generally coincides with the effects of its underpinning collective agreement, in several cases the month of January of the year in which the bargaining was concluded. This practice ensures that the collective agreement can be applied to all workers from the same time, regardless of their union affiliation or the employer association affiliation of their firms. However, this practice also forced employers bound by the extension to pay immediately or over a short period after any resulting wage arrears - driven by the difference between the minimum wages under the old and new version of the agreement. These backpay-related wagebill increases may exacerbate any negative effects of extensions on employment, particularly for liquidity constrained firms.<sup>7</sup>

### 3 Data

This empirical study draws on two novel data sets. The first one is obtained from the monthly social security records of the entire population of firms in Portugal between January 2007 and June 2012. This is a firm-level data set of high quality as it based on the administrative records of social security contributions (typically 34.75% of the gross salary of each employee). The records available, made available by the Ministry of Labour, indicate the number of formal employees as well as the number of independent contractors (service providers) paid by each

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<sup>6</sup>Given that there is no information about workers subject to the double affiliation at least until 2010, it is not possible to obtain statistics about the number of workers affected before the extension.

<sup>7</sup>A new framework in force between 2012 and 2015, a period beyond the scope of this paper, restricted extensions to cases in which subscribing employers employ at least 50% of the workers in the sector in which the extension is to be issued, following similar models in countries like Germany and the Netherlands (Hijzen et al. 2019).

firm in each month.<sup>8</sup> The records also indicate additional variables such as the number of new hires and leavers (only in the case of workers, not service providers) and the total wage bills (separately of workers and service providers) of each firm in each month. An additional variable is the industry affiliation of the firm, according to an ISIC rev. 4 five-digit code.

The data set originally comprises over 26.1 million firm-month observations, which refer to 646,788 different firm identifiers. Each firm appears in the data set a mean of 40.4 times over the 66 months covered. The average firm size, in terms of the number of employees, is 7.5. Moreover, the total number of workers per month varies from a peak of 3.09 million (in July 2008) to a trough of 2.82 million (in April 2012), consistent with the business cycle over the period covered.

We also draw on a worker-level version of this data set, including information on all individual wages paid by each firm to each employee in each month, over the slightly shorter period from January 2007 to October 2010. There is no information on hours worked but this is not very relevant in the context of Portugal in which part-time work is relatively rare. The data set also includes the identifier of each worker and of each firm as well as the industry of the firm. Hirings and separations variables are constructed from the spells of worker-firm matches. In contrast to the firm-level data set, information on contractors and the industry in which they work is not available in the worker-level data. Given the large size of the resulting data set, we restricted our analysis to a sample of workers that are employed in sectors that are subject to an extension at least once over the full period covered in our study, corresponding to more than 75 million observations.<sup>9</sup>

The second key data set used lists all extensions of collective agreements issued between January 2008 until December 2011. The data set also includes information about the relevant industry affected by the extension, in terms of a two- to five-digit ISIC rev. 4 code, depending on the width of the underlying collective agreement. This information is obtained by the Ministry of Employment from the legal documents of each individual collective agreement and extension order.

According to this second data set, a total of 375 extensions were issued between 2008 and

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<sup>8</sup>These service provider contractors are informally know as 'recibos verdes' ('green receipts', by the colour of the original documents where payments were recorded, before moving to electronic statements. These contractors also pay social security contributions and are entitled to some social protection but are not bound by employment law or collective agreements. More specifically, their salaries can be lower than minimum wages.

<sup>9</sup>See Goncalves & Martins (2018) for a study of self-employment (contractors) using a smaller version of this worker-level data set.

2011: 136 in 2008, 106 in 2009, 116 in 2010 and 17 in 2011.<sup>10</sup> The extensions have been published relatively uniformly across all months over the four years analysed, with a peak of 69 in February and a trough of 17 in June. 2011 is an exception, as all 17 extensions in that year were published until May only, given the political context described above.

As to the timing of when extensions enter into force (in many cases in a date earlier than when the extension is issued, as discussed above), this is heavily concentrated in January (276 extensions across the different years, out of the total of 375). This in order to coincide with the typical month when the collective agreement underpinning the extension enters into force. Indeed, the average difference between the two dates (publication and production of effects) over the 372 extensions for which both dates are available is significant, at 10.4 months. This implies that firms directly affected by the extension (i.e. that paid salaries in between the minimum levels determined by the previous and the new versions of the relevant collective agreement) will be forced to back-date possibly substantial wage increments, at least for continuing workers.

Given that each extension may cover more than one industry, and each industry may be defined at a different level of aggregation, we reshape the original list of extensions in terms of specific industry/extension combinations. Furthermore, each extension industry code was also matched to all corresponding relevant five-digit industry codes.<sup>11</sup> This resulted in a set of 963 five-digit-industry/extension pairs, scattered across 51 two-digit industries. The largest number of extensions are found in manufacture of food products (ISIC 10, 100 extensions), crop and animal production (01, 72), retail trade (47, 78), wholesale trade (46, 67), and manufacture of computer, electronic and optical products (26, 56).

As mentioned before, occasionally collective agreements and their extensions apply only to specific occupations or to specific regions. However, given that we do not have information about the occupation profile of the workers of each firm in our data set, we assume that all extensions cover all workers of each firm in the relevant industry. While this is likely to

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<sup>10</sup>These figures do not correspond exactly to those of Table 1 since they refer to extension orders while those in the table refer to the number of collective agreements that were subject to extensions. Moreover, occasionally one extension covers more than one agreement, particularly when an employers' association celebrates the same agreement with two different unions.

<sup>11</sup>This relevance criterion is met when there is at least one firm under such industry code in the social security data set. As an example, consider the extension of a collective agreement in the metal manufacturing sector involved both ISIC codes 25 ('Manufacture of fabricated metal products, except machinery and equipment') and 265 ('Manufacture of measuring, testing, navigating and control equipment'). These two codes corresponded to 29 five-digit industries under sector 25, from 25110 to 25992, and six five-digit industries under sector 265, from 26510 to 26530.

be the case in the majority of extensions, this assumption will introduce some measurement error in our analysis that will bias downward our estimates. As to the regional dimension, we disregard in our main results the 71 extensions of collective agreements that we considered of a subnational scope.

Finally, the two main data sets are merged in order to obtain combined information about employment levels across industries (industry-level data) or across workers (worker-level data) and the timing of the publication of the collective bargaining extensions. Table 2 - top panel - presents descriptive statistics of the resulting data set, covering nearly 84,000 observations. The mean (formal) employment level across those industry-month observations is 2,095 (with a standard deviation of 6,903), while the mean wage bill is 2.07 million euros (with a standard deviation of 6.3). Mean monthly hires are 60.1 individuals while the corresponding figure for separations is 62.2 (both hirings and separations refer only to formal employees, not contractors). The table also describes a key dummy variable used below, which flags the industry-month observations in the four months after the publication of a relevant extension, and corresponds to 3.2% of all observations. The bottom panel of Table 2 presents descriptive statistics of the worker-level data set, including an average log wage of 6.54, mean worker separation and entry rates of 5.6%,<sup>12</sup> and that 13.7% of the worker-month observations correspond to the four-month window following an extension.<sup>13</sup>

### 3.1 Case study: the 2010 construction sector agreement

In order to facilitate a better understanding of the process surrounding the extension of collective agreements, we provide an illustration based on a specific case study: the construction sector collective agreement signed between the AECOPS employer association (and other employer associations) and the SETACCOP and SITESC trade unions. This agreement applied to a sector with 19,000 employers and 300,000 workers, according to its text.

The collective agreement text listed over 180 clauses, including provisions on minimum wages by worker category, description of main job titles, health and safety, fixed term con-

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<sup>12</sup>These rates are based on separation (entry) indicators, defined as one when the individual was not employed in the following (previous) two months in the same or a different firm. By construction all individuals must be hired and separate at least once, which explains why the descriptive statistics are equal. However, the timing of each separation or entry does not have to be and is not equal across individuals, in particular those exposed to extensions at different times.

<sup>13</sup>The large increase in the last figure compared to its sector-level equivalent (3.2%) reflects the facts that the worker-level data set excludes observations from sectors that are never subject to an extension and from the year of 2011, when the number of extensions was much reduced, and that sectors that never have their agreements extended tend to be smaller than those that have at least one extension over the period covered.

tracts, trial periods, holidays, overtime, and replacement of workers. Many of these clauses repeated or changed only marginally the provisions already in place in the general labour law and in the original 2005 version of the collective agreement (see Martins & Saraiva (2019) for a detailed analysis of this and other agreements). The agreement was published in late March 2010, establishing wage floors that came into effect from January 1st. The monthly base wage floors ranged from 380 euros (apprentices) to 841 euros (directors and managers) per month, which compare to a statutory minimum wage in that year of 475 euros (380 for apprentices) in that same year.

The agreement was subject to an extension issued in July 2010 so to bind all workers not directly covered, except those affiliated with different unions, given the opposition expressed by some of the latter following the preliminary publication of the extension. The extension also determined that the new wage floors had retroactive effect, from January 1st, 2010 (the date when the agreement came into force), but the overdue wage increments could be paid monthly in up to four installments from September.

The extension order also provided some additional information about its predicted impact, following a background study conducted by the Ministry of Employment. In particular, the study estimated that 60,000 workers would be affected by the new minimum wage floors set by the collective agreement for 2010. Of those 60,000 workers, 25,000 were estimated to be earning 6% or more below the new minimum levels, indicating a considerable 'bite' of the extension. These figures were based on the latest data available at the time, 2008, which were then extrapolated to 2010 using an estimate of the inflation level over the period. Moreover, the extension text also indicated that most firms that employed workers that were paid below the new wage floors following the extension employed nine or fewer workers.

## 4 Sector-Level Results

Given our theoretical discussion, there are a number of economic variables that may be affected by the extension of collective agreements. We focus on employment and wage related variables, along both standard, labour-law-regulated dimensions and more flexible, service-provision aspects. In the case of the former, we also investigate the impact of extensions upon worker flows, namely hirings and separations.

Our identification approach is based on the scattered timing of extensions: as indicated in

Section 3, extensions are issued virtually uniformly along the twelve months of the year over the four year period under study. They can be argued to be exogenous from the perspective in typically smaller firms that are not affiliated in employer associations. Even in the case of affiliated firms, the timing of extensions is subject to considerable noise, including the completion of negotiations with trade unions and the approval process by the government. In this context, we employ a standard difference-in-differences estimator, in which we contrast the development over time of each variable of interest in industries subject to extensions and in other industries where extensions are not issued (or not issued at the same time). The high-frequency nature of the data, by microeconomic standards, with up to 60 observations per each one of the 1,500 five-digit industries under study, allows us to control for time-invariant heterogeneity in a detailed way. Overall, our rich data structure allows us to construct credible counterfactuals of the variables of interest at the time when extensions are more likely to bite.

As to the definition of the relevant time range from the perspective of the impact of the extension, we focus on a dummy variable capturing the four months immediately subsequent to an extension in the relevant sector. This time window strikes us as an appropriate compromise between a period long enough to capture any possible immediate effects while also not too long given the yearly or nearly yearly frequency of the revision and subsequent extension of several collective agreements. There is also an inflation effect to take into account, which will prompt the wage increase to be partly canceled out over time in real terms.<sup>14</sup>

In this context, we estimate equations of the following format:

$$Employment_{it} = \beta Extension_{it} + \alpha_i + \gamma_{jt} + e_{it}, \quad (1)$$

The dependent variable in this case,  $Employment_{it}$ , corresponds to the logarithm of the employment level of industry  $i$  in month  $t$ .  $Extension_{it}$  is the key regressor of interest, a dummy variable equal to one in the first four months after the extension of the collective agreement in industry  $i$  comes into force (and zero otherwise), i.e. over periods  $t + 1$  to  $t + 4$ . Furthermore,  $\alpha_i$  refers to the (over 1,400) five-digit-industry fixed effects, one for each industry  $i$ , and  $\gamma_{jt}$  refers to the (540) time fixed effects, one for each pair of a specific month and a specific one-digit industry (denoted by subscript  $j$ ). These time-industry fixed effects allow for an extremely flexible estimation of time patterns, specific to different industries,

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<sup>14</sup>In any case, later we also consider more flexible specifications, namely by allowing for possible anticipation effects or effects stemming from the collective agreements before extensions are issued.

and also picking up inflation effects. It is from any significant systematic differences in the dependent variable of the relevant industries over the periods immediately subsequent to extensions that their effects will be documented. These fixed effects also allow for possible correlations between the timing of extensions (and their underlying collective agreements) and particular trends in the business cycle of the sector at stake. (In our robustness checks we also consider even more flexible business cycle effects, varying at the 2- or 3-digit industry level.)

Table 3 presents our main results, following the specification 1. Besides Employment, we also consider complementary dependent variables such as Hirings, Separations, and Wage bills (in the latter case both of workers and of service providers, separately and jointly).<sup>15</sup> Moreover, given the logarithmic transformation of the dependent variable, the coefficients on the  $Extension_{it}$  dummy variable reported can be interpreted as a percentage effect. The result presented in column 1, top row, indicates that the average number of total workers in an industry falls by 2% over the four months subsequent to an extension. To the extent that collective bargaining real minimum wages increase by 1% to 3% over the period, according to official statistics, the implicit elasticity of labour demand (the ratio between the percentage change in employment and the percentage change in wages) can be estimated to range between 2 and 2/3, the latter value at the top of the typical range of estimates in the literature. However, when taking into account the backpay effect (wage increases that will also have to be paid for salaries of previous months), the elasticities of labour demand will be lower and the upper bound of this range will fall more in line with existing estimates.

Additionally, when considering separately hirings and separations, the results in columns 2 and 3, top row, indicate that the industry employment effect is driven essentially by hirings, which fall by over 4%, while the effect on separations is not significant. On the other hand, service providers, not subject to the extensions' wage floors (nor to any other aspect of labour law or collective bargaining), see their number increase by 1.4%. However, since the number of service providers by industry is typically far smaller than that of formal employees (214 vs 2,096, on average), as indicated in Table 2, the increase in the former does not compensate for the decrease in the latter, leading to an overall negative effect on the number of workers (employees and contractors).

We now turn to the second main row of Table 3, which focuses on wages. The results

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<sup>15</sup>See Martins (2008) for another analysis of job flows in Portugal, based on a different data set.



indicate that the wage bill of workers, i.e. the sum of all salaries paid to formal workers, falls by 2.2% on average over the four months following an extension. This coefficient is of the same sign and virtually the same as the one obtained for the number of underlying workers (-2%). This result may suggest that the net effect on the wage bill is driven essentially by (lower) quantities (employment), a point that we revisit below and when we conduct the worker-level analysis.

The remaining results of the second main row of Table 3 indicate a wage bill effect amongst service providers similar to that of the number of service providers (1.1%). More importantly, the total wage bill effect (-2%) is shown to be very similar to the employee wage bill effect, as expected from the fact that, as indicated in Table 2, the spending on service providers by industry is typically far smaller than that on formal employees (two vs 0.2 million euros, on average). Finally, consistently with the similar effects on the number of workers and on their wage bill, the (sector) average wage is not affected significantly. While continuing workers that are initially paid below the new minimum wages before an extension will see their wages increased, other workers will potentially have their wages unchanged. Yet another group will leave the firm (and be potentially replaced by a different number and or profile of new hirings). In this context, the effect of extensions on the average wage of a changing pool of workers in a given industry is ambiguous, which is consistent with the insignificant coefficient that we obtained. Sojourner et al. (2015) find similar results in the context of nursing homes in the U.S.. Note also that the hours margin is unlikely to be relevant here, in contrast to Jardim et al. (2018), given the relatively limited use of part-time contracts in Portugal and the employment law restrictions in the country regarding unilateral, employer-led reductions of hours of work.

Overall, these results clarify the theoretical discussion in terms of the important finding that, at least in the context considered here, the extensions of collective agreements can have a detrimental effect on employment. In aggregate, industry-level terms, any gains from a level-playing field set by employer associations and unions that are, in general, not very representative, do not appear to compensate the employment losses from wage floors potentially above the marginal products of a number of workers employed by the firms bound by the extensions. We also find that the negative employment effect is driven by a reduction in hirings and not by an increase in separations. This is consistent with evidence that employment

adjustments in firms are made primarily by reducing entry and not by increasing separations (Abowd et al. 1999). This can be particularly important in settings characterised by high separation costs of permanent workers as those resulting from strict employment protection legislations, as in Portugal (Blanchard & Portugal 2001, Martins 2009*a*, OECD 2012), with severance payments of one month of salary per year of tenure and potential judicial uncertainty costs.

Moreover, the fact that the number of non-regulated service providers increases suggests that there are important substitution effects between the two forms of labour considered here (formal employment and informal work - contractors). These can be seen as a response to the increase in the minimum wage levels prompted by extensions. This result is also relevant from the perspective of the literature on segmented labour markets and the effects of partial labour market reforms (Blanchard & Landier 2002), even if it has focused on the duality between permanent- and fixed-term contracts, disregarding the further segmentation in informal work that we examine here, increasingly important in the context of the emergence of the 'platform' or 'gig' economy.

The finding of similar employment and wage bill effects is important also in terms of the debate on the potential of aggregate demand stimuli during downturns via administrative wage increases. Our case study of Portugal indicates that extensions may not only undermine employment - they can also contribute to even lower levels of demand. The aggregate demand gains from higher salaries for those that keep their jobs may not compensate the losses in salaries from those whose jobs are discontinued or who are not even hired in the first place.

## 5 Robustness

### 5.1 Timing of the effects

In this section, we present a number of robustness checks regarding our main results - Table 3. First, we assess in greater detail the timing of the effects upon the main variables drawing on our high-frequency, monthly data. We are interested in understanding the extent to which there is a close match between the month when the extension is issued (and the following months). We are also interested in the potential for anticipation effects, which cannot be entirely ruled out as the underpinning collective agreement will already be in force for several firms at the time that the extension is issued. Specifically, we estimate equations as follows:

$$Employment_{it} = \sum_{k=-3}^5 \beta_k Ext_{it}^k + \alpha_i + \gamma_{jt} + e_{it}. \quad (2)$$

All variables have the same interpretation as in equation 1.  $Ext_{it}^k$  are now a set of nine dummy variables, each equal to one in a specific month before or after the extension is issued, from the third month (where  $k=-3$ ) before to the fifth month after ( $k=5$ ), including the same month when the extension is issued ( $k=0$ ). This specification allows us to estimate a time profile of specific month effects, including also the period before the extension, in case of anticipation effects, for instance.

Table 4 presents the results for each one of the five key variables from Table 3. The first column, concerning the number of formal employees, presents a monotonic path of effects, starting at -1.4% in the same month that the extension is issued (significant only at the 10% level) and increasing gradually to -2.3% (significant at the 5% level) at the fourth month after the extension. The second column examines the effect of extensions on hirings and finds that they have their highest value on the month of the extension, at -9.4% (significant at the 1% level) and average -6% (significant at least at the 5% level) along the following four months, except in one case. There is some evidence of anticipation effects, in terms of coefficients significant at the 10% level in the two months before the extension. As to separations, the results indicate that there is not any significant effect across the nine months considered, consistently with the results of Table 3.

In terms of wage bills, the effects are significant in the case of formal employees on the first, third and fifth months after the extension, at about -2.5%. In the case of service providers, the effects are significant over the months before the extension, the month of the extension itself and two months after (the first and the fourth), at about 2% to 3%. This case - service providers - is the only across the five variables in which there is evidence of effects before the extension is issued, while in the case of the number of workers and hirings there is also evidence of effects in the actual month of the extension.

Overall, this month-by-month analysis supports our previous results based on a four-month time window. More important, we find clear spikes in the effects (both in terms of point estimates and statistical significance) across different outcome variables once the extension is in force and little evidence of effects before the extension is issued. These then tend to die out at the end of our preferred time window. In the case of separations, for which we find no

evidence of effects in our main results, the detailed month-by-month analysis also indicates no impact of extensions in any of the nine-month period considered.

## 5.2 Firm types

In our second set of robustness checks, we start by focusing on the effects of extensions upon smaller firms. From our theoretical discussion, any negative effects of extensions on employment are likely to be stronger amongst them. Smaller firms will tend to have lower marginal products of labour, which will make them more sensitive to increases in wage floors, especially when facing liquidity constraints (Cabral & Mata 2003). Moreover, small firms are also less likely to be affiliated with employers' associations that are conducting the collective bargaining. In that sense, the extensions can also be regarded as more exogenous in the case of these smaller firms. We conduct our analysis of this question by constructing a new time series of employment and other variables by industry using only information from firms whose average size over the 2007-2011 period does not exceed nine employees.

The results, presented on the first row of Table 5, indicate that the negative effect on employment documented in Section 4 is not only still present and significant in the case of small firms but also increases, by approximately 50%: it is now -2.6%, compared to -2% for the full population. Hirings are also negatively affected - by -2.2% - while, as before, separations do not change systematically in the months just after extensions. Also consistently with the main results, the wage bill of formal workers falls (-2.2%), by slightly less than the effect on the number of workers. The only exception in terms of the similarity of the two sets of results is that, unlike before, there is no positive effect on the wage bill of service providers amongst small firms.

Table 5 also presents estimates obtained separately for the manufacturing and services sectors. The results again prove robust, in particular the key finding of negative effects on employment, at -1.9% and -1.4% respectively. Separations are not affected, as before, while hirings fall, albeit only significantly in the case of manufacturing. The total wage bills also respond in the same direction as in the main results. This is also the case for the contractors' wage bill in the manufacturing sector but not the services sector.

### 5.3 Common support

An important additional area for robustness purposes concerns the extent to which different sectors are comparable. Given our identification approach based on a difference-in-differences estimator, it is important to draw on sectors without extensions (over time or in particular periods) that can provide credible counterfactuals. Table 6 presents a number of additional analyses under this approach, complementing those by major economic sector conducted in the previous subsection.

The first analysis considers only sectors where extensions are effectively carried out. This choice follows from the fact that, as indicated above, a large number of sectors do not exhibit any extensions over the period concerned, largely because they did not establish (or renew) collective agreements over those years. We examine only the three-digit industries that report at least one extension over the period under analysis and all observations of their corresponding five-digit industries. In this way, we can focus on an even more comparable control group of industries that are not subject to extensions, or not at the same time, in the spirit of a (difference-in-differences) matching approach, although at a potential cost in terms of the representativeness of the sample and the precision of our estimates.

Reassuringly, the results under this analysis are particularly similar to those of Table 3, even if the number of observations falls by approximately half. According to the results presented in the first panel of Table 6, the effect of extensions on the number of workers and on their wage bill is of -1.5% and -1.7% (significant at the 1% level), the effect on hirings is -3.5% (significant at the 1% level), the effect on separations is -2.3% (significant at the 5% level) while the effect on service providers' wage bill (0.9%) is again significant even if only at the 10% level.

The second panel of Table 6 takes a different perspective on the common support criterion by considering only those sectors that consistently report hirings and separations in every month over the 2007-2001 period. This criterion implies that, unlike in previous estimations, the number of observations is always the same for the four main variables (34,260). However, the comparability of treated and control groups may be less adequate than in the approach of the first row given that, for instance, those sectors can belong to very different industries. In any case, once again the same qualitative results are found, namely of decreases in the number of workers and hirings and no effects on separations, despite declines in terms of

significance and even magnitude of the coefficients. The negative effect on the main wage bill is not significant, unlike the positive effect of the service providers wage bill.

The third and fourth panels of Table 6 are concerned with the sensitivity of the results to different approaches in controlling for time effects. Instead of the already very flexible set of controls adopted in all results, based on a dummy variable for each month of each year in each one-digit industry, we consider here a specification based on quadratic time trends by two-digit industries or time effects specific to each pair between a month, year and a 3-digit industry. The results are again consistent with the previous evidence, in particular in the negative effects on workers and hirings and the positive effect on the service providers' wage bill (third panel).

#### **5.4 Time differences**

The discussion above underlines the potential importance of specific time periods in explaining the effects documented. In fact, as the years 2008, 2009 and 2011 were of recession in Portugal while 2007 and 2010 were of moderate growth, these differences in the business cycle may interact in some way with the extensions. Moreover, while extensions were issued across each year, their production of effects tended to be concentrated in January in order to coincide with the typical production of effects of the underlying collective agreement. This may potentially generate interactions between the extensions and other variables that confound the effect of the former.

In order to assess the potential role of these issues, we split our observations into different smaller periods. In particular, we split the five-year period considered in two ways: first, from January 2007 until June 2009 and from July 2009 until December 2011 (Table 7, first and second panels); and second, January to June and July to December of all years (2007 to 2011, third and fourth panels). The results are again very similar to the benchmark findings of Table 3. The main effect, on the number of workers, is always significantly negative, ranging between -1.3% and -2.9%. Similarly, the effect of extensions on hirings is always significantly negative, in this case ranging from -3.5% and -4%, as in the cases of the effects on the workers' wage bill (-1.5% to -2.4%). Finally, the effects on separations and the service providers' wage bill are always insignificant, with the exception of the months of July to December, when they are negative (-3.8%) and positive (3.6%), respectively. The fact that the employment

and wages effects are larger for the July-December period (compared to January-June) may be related to the role of backpay: extensions issued later in the year may still produce effects from January or soon after in that same year, which will imply a larger wagebill burden for the affected employers (Hijzen & Martins 2016).

## 5.5 Falsification exercise

The agreement signed between Portugal and the European Union and the International Monetary Fund in May 2011 (IMF 2011) led to a suspension of the extension of collective agreements by the new government that took office in June. This suspension was in force until early 2012, when new criteria for extensions were under preparation. However, a number of collective agreements had their administrative process of extension started by June 2011, namely through the publication of notices by the Ministry of Labour indicating the intention of extending such agreements following the request submitted by the collective agreement subscribers.

However, as those extensions did not come into force, they can be used to provide further evidence on the causal interpretation of our results. If, as we argue, following the evidence presented above, the increase in minimum wages determined by extensions can hurt employment in the sectors affected, then extensions that are announced but do not actually come into full force should not have such negative effects. This is in contrast to an alternative interpretation that we test here that the negative effects documented so far are driven not so much by the extension itself but instead by the coming into force of the collective agreement subject to the extension.<sup>16</sup>

We conduct this additional check on the validity of our results under a falsification approach: based on a set of 15 such potential extensions which had been subject to a preliminary public notice and that also meet the condition of being linkable to collective agreements that were subject to extensions in previous years. These agreements cover a wide set of industries, such as Security services, Chemical and pharmaceutical products, Textiles, Food distribution, Furniture, Merchandising, Meat distribution, Bread, and Fish processing. As explained above,

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<sup>16</sup>This concern is not supported from the results regarding the timing of the effects (Table 4), which tend to coincide with the actual timing of the extension, although in a few cases they start one or two months before. However, even such few potential suggestions of ‘anticipation’ are reconcilable with the fact that extensions are announced before they are issued and come into effect. Furthermore, collective agreements are also disclosed before the extension, implying that firms outside employer associations may respond even before the extension is issued.

collective agreements tend to be revised regularly, on a yearly or lower frequency. Such revisions tend to be focused on the wage floors set for each job type and job category although they occasionally also include other provisions (Martins & Saraiva 2019). This time linkage allows us to use the information collected by the Ministry of Employment regarding the agreements - and their potential extensions - namely along the range of industries potentially affected.

We then create a data set with exactly the same characteristics as the one used for our main analysis, except that the only extensions considered are those from the specific set of 15 discussed here, and use it for the same type of analysis presented above, namely those based on equation 1. Given that these specific extensions were not issued and the dates when extensions are issued are used for the key right-hand-side four-month dummy variable, we imputed the likely date of issuance based on previous extensions of previous versions of the same reference collective agreements and historical information on the typical time gap until the publication of the extension (two months). In our final data set, only the 15 extensions have the ‘Four months post extension’ dummies switched on. These are also the only ones whose potential impacts on the dependent variables are considered in the table. Moreover, given that these potential extensions would be issued only between May and September of 2011, we restricted the sample used here to the second half of our period, i.e. to 2009:07-2011:12 (as in the second main row of Table 7), in order to draw on a more comparable distribution of observations between the ‘before’ and ‘after’ periods.

Table 8 presents the results. Unlike in our main findings (Table 3) on the results specific to the subperiod considered, we now find that all main coefficients are insignificant, in particular in the case of the number of workers (1.2%, with a standard error of 1.1%). The only exception is the wage bill of service providers but that has the ‘wrong’ sign (-2.7%) and is significant only at the 10% level. Other restrictions on the data set, similar to previous robustness checks, generate similar results, in particular involving no effects on employment or hirings (results available upon request). These findings are consistent with those of Hijzen & Martins (2016), which examines this period from a different, regression-discontinuity perspective and with a different data set.

We interpret the results from this falsification exercise as additional evidence of a causal relationship between extensions and the different economic variables studied here, as well



as providing a more precise perspective of the actual mechanism underpinning the results. Specifically, we find that only those extensions that become legally binding have negative effects on employment and other economics variables. This result also indicates that, in the context of our analysis, collective agreements by themselves do not necessarily have negative effects, provided their scope is not extended beyond bargaining parties.

## 5.6 Firm entry and exit effects

Given the important effects documented above regarding employment flows, it is of interest to investigate their underpinnings also in terms of the firm entry and exit margins. If the minimum wages set by extensions are not affordable by some firms, then an effect along these margins could be anticipated. For instance, entrepreneurs may be discouraged from launching new businesses when they face additional restrictions in the wage conditions that may negotiate with their workers. This view is relevant also given the evidence presented before of stronger negative employment effects amongst smaller firms.

We examine these questions by constructing a new data set derived from the original monthly information on all firms. Specifically, first we identify new firms as those whose individual identifiers appear for the first time in the data in a given month, while old firms are those whose individual identifiers appear for the last time in the data in a given month. We then create a count of their numbers per month and industry, as well as of these firms' numbers of formal employees and service providers. The latter two variables indicate the flows of workers and service providers that join (leave) the industry at the time the firm enters (exits) the market, as in the context of the literature on job and worker flows (Abowd et al. 1999). We then employ the same estimation approach as that of equation 1, considering the log of the number of firms, formal employees and service providers as alternative dependent variables, separately for firm entry and firm exit.

Table 9 presents the results. The first panel considers firm entry effects, where all coefficients are statistically insignificant. This indicates that the emergence of new firms is not affected by the extension of collective agreements. The same applies to the time series of formal employees and service providers hired by such new firms, which are not significantly different at the months immediately following the extensions. However, when turning to the second panel of Table 9, which considers firm exit effects, we find the number of firms that

leaves an industry increases significantly - by 4% - over the four months subsequent to an extension. The effect on service providers is similar (4.3%), while the coefficient on the workers specification is also equivalent in magnitude (5%) but, unlike before, not significant.

We take these results - in particular, the increased firm exit - to add further support to the view that extensions can create difficulties for some firms. The randomness inherent in firm-level monthly data on entry and exit can explain the absence of significant effects on firm entry, although noncompliance may also be significant amongst new firms, to the extent that they are not fully aware of all applicable regulations and they are less likely to be unionised.

The fact that the exit effect is significant as far as the number of service providers is concerned but not in terms of formal workers can be potentially explained by a greater reliance upon the former by firms most affected by the extensions. Such firms may have a preference towards informal workers which makes the higher minimum wages imposed on their formal labour force - typically representing a greater share of the total wage bill - more difficult to withstand, prompting their closure. Additionally, the difference in the statistical significance between the firm number and workers effects may be explained by a smaller average firm size of firms that exit following an extension. As indicated before, smaller firms tend to not be affiliated with employer associations and are thus less likely to see their interests regarding working conditions fully taken into account during collective bargaining.

## 6 Worker-Level Results

Having established our main results, we now revisit them at the worker-level, with a view to providing additional robustness checks. We adopt a similar model than that of equation 1, except that each observation corresponds now to a given individual employed in a given firm and observed in a given month. We also control for spell fixed effects, i.e. we have a dummy variable for each match between a given worker and a given firm, which allows us to understand if the outcomes of interest exhibit particular patterns at the period during the match when the worker's firm is subject to extensions. The latter are matched to the data in the same way as in the case of our main industry-level analysis.

Unlike in our main analysis, here we do not analyse direct employment effects as all individual observations correspond to cases in which individuals are employed. We could add observations regarding periods in which individuals are not employed but it would be unclear

how to define the extension variable in those instances. However, we do focus on worker flows, hirings and separations, by considering dichotomous dependent variables that are switched on in the first month just after the worker joins the firm or in the last month before the worker leaves the firm. Moreover, we also consider the wage dimension, by considering the log of total monthly pay as an additional dependent variable. As to our data set, as indicated above, we consider only industries that are subject to extensions.

Table 10 (top panel) presents the main results, based on a data set with over 75 million observations. These indicate a significant and large negative effect of extensions on hirings (-3.9%), a significant and positive effect of extensions on separations (0.6%), and positive effects of extensions on individual wages (1.1% or 1.5%, depending on the usage of spell or individual fixed effects).

Overall, these findings are very much in line with those from the industry-level analysis, except that these coefficients tend to be larger and more precise. In the case of separations, the effects are always significantly positive, when before they tended to be always insignificant. However, the previous point that the hirings margin is more active than its separations counterpart still holds.

An additional important aspect concerns the positive effects we document here, in the case of individual workers. Regardless of the type of fixed effect (individual or spell, which in case tend to overlap significantly), we find positive effects of extensions on the wages of workers (conditional on remaining employed following the extension). This is as expected given the nature of the extension mechanism, which requires firms to comply with the higher minimum wages from new or revised collective agreements in their industry. This is however in contrast to the industry-level analysis, which documented negative total wage bill effects and insignificant average wage effects. The contrast between the two approaches highlights the relevance of the composition effects that are present in the industry-level analysis, involving a comparison of average industry wages over time that are shaped by changing shares of hirings, separations, wage rigidity and wage increases (Sojourner et al. 2015). This is a methodological point that may also be of interest in other studies.

We conduct two additional robustness checks around the main worker-level results presented here. These involve restricting the time period range examined here while at the same time centering it around each extension. More specifically, we consider only the observations

regarding the last four (two) months before an extension, the month of the extension, and the first four (two) months following the extension. This ensures that we are not mixing in the control group (before-extension period) observations regarding the after-extension months, even if the latter may correspond to the before-extension period of the next extension.

Table 10 (bottom panels) presents the results, regarding the four- and two-month windows, respectively. In both cases, we find once again results consistent with our main industry-level analyses and, particularly, with our novel worker-level findings (for a larger period): negative effects on hirings and positive effects on separations and on the wages of continuing workers. Moreover, all coefficients are larger (in absolute terms) compared to the benchmark case of the top panel.<sup>17</sup>

We conduct one final robustness check, in which we aggregate the worker-level data used in this section to the firm- and industry-level and redo our main analysis. Table 11 presents the resulting findings, indicating that they are entirely consistent with our benchmark industry-level findings (Table 3), including negative effects on employees, hirings and total wage bill, and insignificant effects in the case of separations and the average wage. In results not presented, we also find that within-firm wage inequality (measured through the ratio of the 80th and 20th percentiles or the coefficient of variation) tends to fall in the months following an extension.

## 7 Conclusions

Several countries extend the coverage of collective bargaining agreements to entire sectors, therefore binding non-subscriber workers and employers. This process increases substantially the number of widely applicable minimum wages. From a theoretical perspective, extensions can have many different types of effects. Extensions may address coordination issues and reduce transaction costs, promote greater equity in pay, foster investments in human capital, expand aggregate demand, and possibly nudge firms towards high-wage/high-productivity equilibria. On the other hand, extensions may impose sector- and occupation-specific minimum wages (and other work conditions) that are not appropriate for many firms and workers. This may be the case especially if the unions or employer associations that subscribed the orig-

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<sup>17</sup>When using sector-level data the results are not statistically precise enough, given the much smaller number of observations (less than 5% of the original sample size, depending on the window range adopted, as both the number of industries and the number of time periods of each industry is significantly restricted).

inal collective agreement are not representative. Moreover, extensions increase the scope for downward nominal wage rigidity, with potential negative effects on employment, particularly during downturns in low-inflation environments as in the Eurozone recently.

In this paper, we are one of the first to analyse empirically the impact of extensions along several relevant economic margins. We draw on firm- and worker-level monthly data for Portugal, a country where such extensions have been widespread, leading to nearly 30,000 *de facto* minimum wages (one for each main job in each industry). Our results, based on a difference-in-differences approach and a detailed discussion of the institutional context of collective bargaining, indicate that extensions have a significantly negative causal effect on employment. For instance, in the four months after an extension is issued, formal employment falls on average by 2%. Also, this effect is 25% larger across small firms, which are less likely to be represented in collective agreements negotiations. Firm closures are also found to increase significantly in the months after an extension is issued in the respective sector. Given the increasing rate of unemployment (and particularly youth unemployment) over the period, such employment losses are not likely to have been absorbed by other sectors. Furthermore, the effect of extensions on sectoral wage bills is also negative. Indeed, their employment impact outweighs the wage increase for continuing workers resulting from extensions, which we observe in our worker-level analysis. On the other hand, the number of contractors (service providers), which are not subject to collective agreements nor to labour law, tends to increase with extensions, contributing towards greater labour market segmentation.

It is important to note, however, that at least part of the negative effects documented here may be influenced by the low or even negative growth rates that characterise the economy and time period under study. For instance, Addison et al. (2013) presents evidence that the employment effects of (statutory) minimum wage increases can be more severe during downturns. On the other hand, Harasztosi & Lindner (2019) and other recent studies find that firms can adjust to higher (statutory) minimum wages through reductions in profits and higher prices and little employment effects, at least in sectors not significantly exposed to international trade. While we show that extensions can harm employment and other economic variables, further research covering different business cycle circumstances and different extension criteria may provide welcome complementary evidence on this important but understudied labour market institution.

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## Tables

Table 1: **Number of collective agreements and extensions, 2005-2012**

Type	Sectoral agreements	Multi-firm agreements	Firm-specific agreements	Extensions
Year	Number of agreements			
2005	151	28	73	56
2006	153	26	65	192
2007	160	27	64	104
2008	172	27	97	178
2009	142	22	87	128
2010	141	25	64	140
2011	93	22	55	24
2012	36	9	40	13
	Number of workers			
2005	1,015,976	68,748	36,748	
2006	1,343,643	73,390	37,267	
2007	1,430,660	58,233	32,384	
2008	1,778,216	47,232	69,398	
2009	1,299,371	59,902	37,952	
2010	1,309,267	64,455	33,344	
2011	1,160,080	52,737	24,102	
2012	291,068	26,645	9,909	

**Notes:** Sectoral agreements ('Contratos coletivos de trabalho') are established between one or more employer associations and one or more unions. Multi-firm agreements ('Acordos coletivos de trabalho') are established between a group of firms and one or more unions. Firm-specific agreements ('Acordos de empresa') are established between one firm and one or more unions. Extensions are issued by the government and widen a collective agreement (typically a sectoral agreement) to the entire sector beyond the workers subject to double affiliation. The number of workers indicated correspond to those bound by a collective agreement after the latter is extended. The number of extensions can exceed that of agreements because extensions can be issued with respect to agreements signed in the previous year.

Table 2: Descriptive statistics - main data sets

Variable	Obs	Mean	Std. Dev.	Min	Max
Industry-level data					
Formal employees	83,896	2095.9	6903.3	0	172165
Service providers	83,896	214.1	912.7	0	27134
Wage bill (employees)	83,896	2065801	6323416	0	1.93x10 <sup>8</sup>
Wage bill (service providers)	83,896	221225	841931	0	3.07x10 <sup>7</sup>
Job creation	76,794	14.1	98.4	0	5585
Job destruction	83,936	10.9	89.6	0	6898
Hirings	83,896	60.1	356.3	0	25609
Separations	83,896	62.2	361.6	0	16273
Firm entry	82,556	2.3	8.7	0	355
Workers (New firms)	82,556	3.7	31.6	0	4936
Firm exit	83,936	2.5	10.5	0	693
Workers (Exiting firms)	83,936	5.1	39.0	0	4877
Month	83,896	6.5	3.4	1	12
Year	83,896	2009.0	1.4	2007	2011
Four months post extension	83,936	.0318	.1757	0	1
Worker-level data					
Log wage	75,322,248	6.54	.667	-36.04	13.33
Month	75,369,983	22.77	12.91	1	45
Worker separation	75,369,983	.056	.231	0	1
Worker entry	75,369,983	.056	.231	0	1
Industry code	75,369,983	44.62	24.81	1	96
Four months post extension	75,369,983	.137	.343	0	1

**Notes:** Worker-level data: 'Formal employees' denotes the number of formal workers employed across five-digit sectors over the 2007-2011 monthly period. 'Service providers' corresponds to the number of individuals (self-employed contractors) that provide services to firms across the different sector-month observations (outside the scope of a formal labour contract). The two 'Wage bill' variables denote the total pay of workers or service providers in nominal euros. 'Job creation (destruction)' is the increase (decrease) in total (formal) employment in a sector compared to the prior month, if positive (zero otherwise). 'Hirings' and 'Separations' are the number of new social security identifiers reported by firms in each month compared to the previous month or that are not reported in the following month, respectively. 'Month' and 'Year' indicate the relevant cell date. 'Four months post extension' is a dummy variable equal to one over the four months immediately after an extension is issued in the respective five-digit sector. Firm-level data: 'Log wage' denotes the logarithm of the monthly wage of each worker over the 2007:1-2010:9 period (variable 'Month', ranging from 1 to 45). 'Separation' is a dummy variable equal to one if the worker is not employed over the following two months. 'Entry' is a dummy variable equal to one if the worker was not employed over the previous two months. 'Industry' indicates the industry code of the firm of the worker. 'Four months post extension' is a dummy variable equal to one over the four months following an extension.

Table 3: Main results - Employment and Wages

	(1)	(2)	(3)	(4)
	Employees	Hirings	Separations	ServiceProviders
Four months post ext.	-.020 (.004)***	-.041 (.013)***	-.014 (.012)	.014 (.003)***
Obs.	83358	60179	62457	67911
$R^2$	.988	.887	.907	.983

  

	Wagebill	WagebillSP	TotalWagebill	AvgWage
Four months post ext.	-.022 (.005)***	.011 (.005)**	-.020 (.005)***	-.002 (.003)
Obs.	83358	67911	83896	83358
$R^2$	.984	.975	.984	.875

**Notes:** Each coefficient reported is obtained from a separate regression based on the dependent variable indicated at the heading of each one of the eight groups of statistics. ‘Employees’ indicates the total number of employees per sector-month. ‘ServiceProviders’ corresponds to the number of individual service providers working for each firm in each sector. ‘WageBill’ (‘WageBillSP’) corresponds to the total nominal pay of employees (service providers) in each sector. ‘TotalWagebill’ corresponds to the sum of ‘WageBill’ and ‘WageBillSP’. ‘AvgWage’ corresponds to the average wage per employee (i.e. the ratio between ‘WageBillEe’ and ‘Workers’). All dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects and (540) month/year/one-digit-industry fixed effects. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.

Table 4: **Time profile effects**

	(1)	(2)	(3)	(4)	(5)
	Employees	Hirings	Separations	Wagebill	WagebillSP
m(-3)	-.006 (.008)	-.004 (.022)	-.019 (.020)	-.017 (.013)	.031 (.009)***
m(-2)	-.007 (.008)	-.038 (.023)*	-.028 (.021)	-.010 (.010)	.033 (.010)***
m(-1)	-.009 (.007)	-.041 (.024)*	.021 (.021)	.003 (.010)	.029 (.009)***
m(0)	-.014 (.007)*	-.095 (.025)***	.013 (.023)	-.014 (.009)	.030 (.009)***
m(1)	-.017 (.007)**	-.069 (.026)***	-.009 (.021)	-.027 (.009)***	.020 (.008)**
m(2)	-.018 (.007)***	-.026 (.024)	-.018 (.021)	-.012 (.009)	.010 (.008)
m(3)	-.020 (.007)***	-.043 (.024)*	-.002 (.021)	-.027 (.009)***	-.001 (.009)
m(4)	-.023 (.007)***	-.046 (.024)*	-.030 (.021)	-.017 (.009)*	.021 (.008)**
m(5)	-.014 (.007)**	.020 (.025)	-.012 (.022)	-.024 (.008)***	.004 (.009)
Obs.	83358	60179	62457	83358	67911
$R^2$	.988	.887	.907	.984	.975

**Notes:** The coefficients reported in each column are obtained from separate regressions based on the dependent variable indicated at the heading of the column. ‘m(t)’ refers to ‘t’ months after the extension is issued (e.g. m(0) refers to the month when the extension is issued and m(-1) to the month before the extension is issued). Dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects and (540) month-year-one-digit-industry fixed effects. See notes to Table 3 for more information. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.

Table 5: **Robustness checks I - Firm size and sectors**

	(1)	(2)	(3)	(4)	(5)
	Employees	Hirings	Separations	Wagebill	WagebillSP
Small firms only					
Four months post ext.	-.026 (.004)***	-.022 (.013)*	-.011 (.012)	-.024 (.005)***	.005 (.006)
Obs.	81434	50728	53078	81434	65305
$R^2$	.985	.879	.89	.978	.972
Manufacturing only					
Four months post ext.	-.019 (.006)***	-.057 (.021)***	-.007 (.018)	-.020 (.007)***	.016 (.007)**
Obs.	31007	19516	21103	31007	23097
$R^2$	.989	.819	.861	.986	.975
Services only					
Four months post ext.	-.014 (.005)***	.008 (.018)	.007 (.016)	-.016 (.007)**	.005 (.008)
Obs.	42172	33636	34179	42172	36737
$R^2$	.986	.904	.919	.982	.976

**Notes:** ‘Small firms’ refers to results based on a sector-level data set constructed only from firms that employ a mean of employees below 10 over the period available. ‘Manufacturing (services) only’ refers to results based on a sector-level data set constructed only from ISIC rev 4 sectors 10 to 33 (45 to 98) firms. Dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects and (540) month-year-one-digit-industry fixed effects. See notes to Table 3 for more information. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.

Table 6: **Robustness checks II - Common support and time controls**

	(1)	(2)	(3)	(4)	(5)
	Employees	Hirings	Separations	Wagebill	WagebillSP
Sectors with extensions					
Four months	-.015	-.035	-.023	-.017	.009
post ext.	(.004)***	(.014)***	(.012)**	(.005)***	(.005)*
Obs.	43909	34250	35378	43909	37775
$R^2$	.992	.897	.915	.989	.984
Sectors with constant flows					
Four months	-.004	-.014	.00003	-.003	.014
post ext.	(.002)*	(.014)	(.011)	(.003)	(.004)***
Obs.	34260	34260	34260	34260	33910
$R^2$	.992	.876	.905	.989	.978
Alternative time controls (2-digit industry trends)					
Four months	-.014	-.008	.001	-.019	.008
post ext.	(.004)***	(.014)	(.012)	(.005)***	(.005)*
Obs.	83358	60179	62457	83358	67911
$R^2$	.988	.878	.9	.983	.974
Alternative time controls (3-digit industry time effects)					
Four months	-.018	-.029	-.022	-.018	-.007
post ext.	(.006)***	(.021)	(.018)	(.007)**	(.007)
Obs.	75974	53078	55250	75974	60571
$R^2$	.991	.918	.93	.988	.985

**Notes:** ‘Sectors with constant flows’ are the five-digit industries that always report both positive hirings and positive separations over the observations available. ‘Sectors with extensions’ are the five-digit industries under three-digit industries that report at least one extension over the period under analysis. ‘Alternative time controls (quadratic trends)’ controls for quadratic time trends by 2-digit industries (instead of one-digit-industry month/year dummies). ‘Alternative time controls (3-digit industry time effects)’ controls for 14,770 3-digit industry times month-year fixed effects. Dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects. The first two specifications also include (540) month-year-one-digit-industry fixed effects. See notes to Table 3 for more information. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.



Table 7: **Robustness checks III - Different time periods**

	(1)	(2)	(3)	(4)	(5)
	Employees	Hirings	Separations	Wagebill	WagebillSP
2007:01-2009:06 only					
Four months	-.029	-.040	-.004	-.024	-.003
post ext.	(.006)***	(.021)*	(.018)	(.008)***	(.009)
Obs.	41470	30956	31482	41470	33744
$R^2$	.99	.901	.916	.986	.975
2009:07-2011:12 only					
Four months	-.013	-.035	-.022	-.021	-.007
post ext.	(.004)***	(.018)*	(.015)	(.005)***	(.005)
Obs.	41888	29223	30975	41888	34167
$R^2$	.995	.894	.915	.993	.989
January-June, all years					
Four months	-.014	-.040	.007	-.015	-.000
post ext.	(.005)***	(.018)**	(.016)	(.007)**	(.007)
Obs.	48654	34585	36647	48654	39610
$R^2$	.988	.886	.909	.984	.977
July-December, all years					
Four months	-.024	-.037	-.038	-.021	.036
post ext.	(.007)***	(.021)*	(.017)**	(.008)***	(.009)***
Obs.	34704	25594	25810	34704	28301
$R^2$	.987	.9	.916	.985	.975

**Notes:** ‘2007:01-2009:06’ (‘2009:07-2011:12’) corresponds to observations from January 2007 until June 2009 (July 2009 until December 2011). ‘January-June’ (‘July-December’) corresponds to observations from January to June (July to December) in each year. Dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects and (540) month-year-one-digit-industry fixed effects. See notes to Table 3 for more information. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.

Table 8: **Falsification exercise - Extensions planned but not issued**

	(1)	(2)	(3)	(4)	(5)
	Employees	Hirings	Separations	Wagebill	WagebillSP
Four months post ext.	.012 (.011)	-.055 (.049)	-.010 (.040)	.004 (.015)	-.027 (.015)*
Obs.	41888	29223	30975	41888	34167
$R^2$	.995	.894	.915	.993	.989

**Notes:** These results are based on a subset of 15 potential extensions that were considered publicly by the Labour Ministry in 2011 but were not issued. The results presented are based on an imputation of the likely date of issuance and information on the industry impact of the extensions based on previous extensions of previous versions of the same reference collective agreements and historical information on the time gap until the publication of the extension. Only the 15 extensions switch on the 'Four months post extension' dummies whose potential impact on the dependent variables is considered in the table. The analysis covers the period 2009:07-2011:12. Dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects and month-year-one-digit-industry fixed effects. See notes to Table 3 for more information. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.

Table 9: Firm entry and exit effects

	(1)	(2)	(3)
	Log Firms	Log Workers	Log ServiceProviders
Firm entry			
Four months post ext.	-.008 (.018)	-.003 (.032)	-.012 (.022)
Obs.	29969	25471	19006
$R^2$	.763	.58	.72
Firm exit			
Four months post ext.	.040 (.015)***	.050 (.033)	.043 (.021)**
Obs.	33581	29479	19372
$R^2$	.806	.59	.728

**Notes:** Firm entry and exit is identified from the first and last occurrence of firm identifiers in the data. ‘Firms’ refers to the number of new or dying firms in each five-industry and month. ‘Workers’ (‘Service providers’) refers to the number of workers (service providers) in such two types of firms over the first or last month of the firm. Dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects and (540) month-year-one-digit-industry fixed effects. See notes to Table 3 for more information. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.

Table 10: **Worker-level results - Job flows and Wages**

	(1)	(2)	(3)	(4)
	Hirings	Separations	WagesWFE	WagesSFE
Main results - full sample				
Four months post ext.	-.039 (.00006)***	.006 (.00007)***	.015 (.0001)***	.011 (.0001)***
Obs.	7.51e+07	7.51e+07	7.52e+07	7.50e+07
$R^2$	.491	.477	.767	.794
Four-month window around extension only				
Four months post ext.	-.036 (.00009)***	.025 (.00008)***	.019 (.0002)***	.018 (.0002)***
Obs.	2.35e+07	2.35e+07	2.35e+07	2.35e+07
$R^2$	.187	.627	.811	.819
Two-month window around extension only				
Four months post ext.	-.040 (.0002)***	.020 (.0001)***	.022 (.0003)***	.022 (.0003)***
Obs.	1.46e+07	1.46e+07	1.46e+07	1.46e+07
$R^2$	.25	.694	.813	.825

**Notes:** Each coefficient reported is obtained from a separate regression based on the dependent variable indicated at the heading. ‘Hirings’ (‘Separations’) is a dummy variable equal to one if the worker joined (left) the firm in that month (in the following month). ‘WagesWFE’ (‘WagesSFE’) is the log of the total monthly pay of the worker from their firm in each month, in a specification with worker (match worker-firm spell) fixed effects. All wages dependent variables are measured in logs. All specifications include (over 3.3 million) firm-worker spell fixed effects (or 2.6 million worker fixed effects) and (450) month/year/one-digit-industry fixed effects. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.

Table 11: Aggregation from worker- to firm- or industry-level

	(1)	(2)	(3)	(4)	(5)
	Employees	Hirings	Separations	Wagebill	AverageWage
Firm-level analysis					
Four months	-.014	-.168	-.030	-.006	.007
post ext.	(.0004)***	(.003)***	(.002)***	(.0005)***	(.0003)***
Obs.	7390097	1127099	1195381	7387345	7387345
$R^2$	.937	.602	.592	.933	.807
Industry-level analysis					
Four months	-.022	-.047	-.014	-.020	.002
post ext.	(.004)***	(.013)***	(.014)	(.005)***	(.003)
Obs.	20583	17271	17402	20583	20583
$R^2$	.995	.942	.939	.992	.909

**Notes:** Each coefficient reported is obtained from a separate regression based on the dependent variable indicated at the heading of each one of the eight groups of statistics. ‘Employees’ (‘Hirings’, ‘Separations’) indicates the total number of employees (hiring, separations) per sector-month. ‘Wagebill’ corresponds to the sum of all individual salaries per firm-month or industry-month. ‘AvgWage’ corresponds to the average wage per employee (i.e. the ratio between ‘WageBill’ and ‘Workers’). All dependent variables are measured in logs. All specifications include (over 1,400) five-digit-sector fixed effects and (540) month/year/one-digit-industry fixed effects. Robust standard errors. Significance levels: \*: 0.10; \*\*: 0.05; \*\*\*: 0.01.