No Extension without Representation?

Evidence from a Natural Experiment in Collective Bargaining[1]

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Abstract

In many countries, collective bargaining coverage is enhanced by government-issued extensions that widen the reach of collective agreements beyond their signatory parties to all firms and workers in the sector. This paper analyses the causal impact of extensions using a natural experiment in Portugal that resulted in a sharp and unanticipated decline in the extension probability of agreements. Our results, based on a regression discontinuity design, indicate that extensions had a negative impact on employment growth. This effect is concentrated amongst non-affiliated firms, which may reflect the limited representativeness of employer associations.

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I. INTRODUCTION

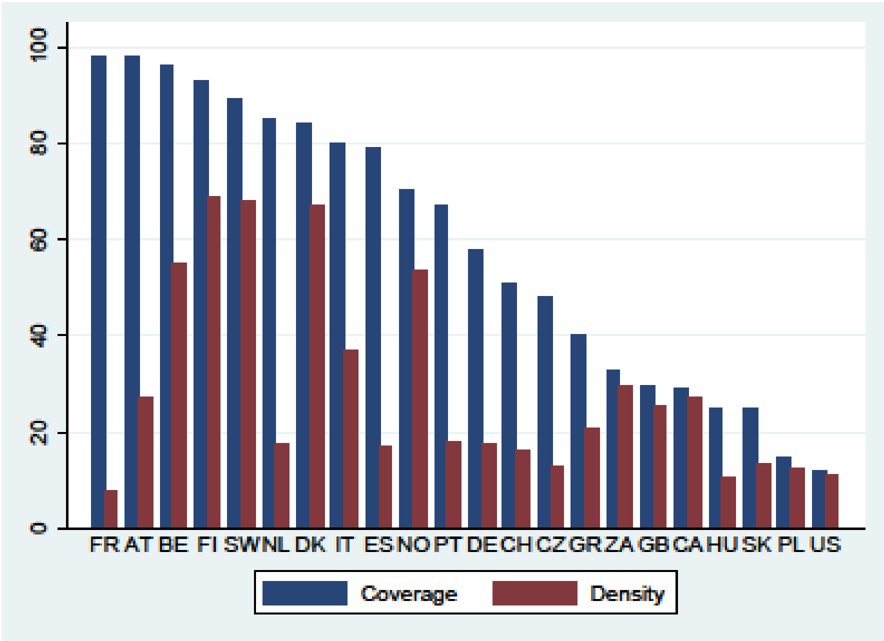
Following the global financial crisis and the large increase in unemployment that ensued, there has been a renewed interest in the role of collective bargaining for economic performance. This interest may increase again during the ongoing pandemic crisis. Well-functioning collective bargaining systems can promote high and stable employment by increasing the responsiveness of working conditions - such as wages and working hours - to economic shocks. This is achieved either through effective coordination that allows working conditions to be aligned with macro-economic conditions or through decentralization which facilitates greater consistency between wages and firm-level conditions (Blanchard et al., 2014; Dustmann et al., 2014; OECD, 2018).

One may also argue that, when fostering wage growth, collective bargaining has the potential of promoting consumption and hence aggregate demand. This may be of particular relevance where there is economic slack, inflation is low and the room for macroeconomic policy stimulus is limited. However, if not functioning optimally, collective bargaining systems run the risk of reducing the responsiveness of working conditions to negative aggregate shocks. As a result, collective bargaining may increase the social costs of economic downturns by increasing the reliance on labor shedding and slowing down labor market adjustment.

In many countries, particularly but not only in Europe, collective bargaining coverage is determined to an important extent by extensions issued by governments. These administrative extensions widen the reach of collective agreements over and beyond their signatory parties - typically firms affiliated with subscribing employer associations and workers affiliated with subscribing trade unions - to *all* firms and workers in the sector where the agreement was signed. Extensions have been motivated by the goal of creating a level-playing field and, in doing so, limiting the scope of competition on the basis of less generous working conditions while enhancing inclusiveness and reducing wage inequality. Extensions can also reduce the transaction costs of setting working conditions, which may be particularly important for small firms that lack the resources to engage in firm-level bargaining and in contexts in which general labour law is limited.

A first quantitative indication of the importance of extensions for collective bargaining coverage can be obtained by contrasting trade union density - the share of workers affiliated to a union - with collective bargaining coverage – the percentage of workers whose working conditions are subject to a collective agreement (Figure 1). In countries where the difference between union density and bargaining coverage is large, such as France, extensions tend to be important.[2] Moreover, extensions have allowed collective bargaining coverage to remain particularly high in several countries, even when union density has declined.

Figure 1. Collective bargaining coverage and trade union density



Source: ILO (2013)

Early theoretical work has highlighted potential concerns about the role of extensions for employment, particularly when the social partners are unrepresentative of the sector. Moll (1996) presents a model of administrative extensions with heterogeneous firms in which only some fraction of the largest and most productive firms is engaged in collective

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[2] However, (administrative) extensions are not the only factor behind the difference. In many countries, agreements apply *erga omnes* to all workers in a firm that co-signs a collective agreement irrespective of their union membership status. This extends the coverage of the agreement within the firm. However, this is not the case in Portugal, the country examined here, where in the absence of extensions, agreements only apply to unionized workers in affiliated firms (the 'double affiliation' principle). Of course, this does not prevent (affiliated) firms from unilaterally offering more generous conditions to non-unionised workers as well, which can also reduce the incentives for workers to unionise.

bargaining. He shows that these firms benefit from extensions because it reduces competition from less productive firms and allows paying lower wages. By contrast, non-unionized low productivity firms need to pay higher wages, reducing the scope for low-wage competition. In the same spirit, Haucap et al. (2001) show that employer associations can use extensions as an anti-competitive device by raising the labor cost of potential entrants. For both reasons, non-representative extensions could reduce employment opportunities for low-productivity workers.

Empirical papers that analyse the role of administrative extensions do not yield a uniform picture. Murtin et al. (2014) show, using country-level panel data, that excess coverage – the difference between bargaining coverage and union density – is not systematically correlated with (higher) unemployment.[3] Magruder (2012) finds, in the case of South Africa, that extensions in industries within specific districts reduce employment by around 10% compared with uncovered neighbouring districts in the same industry. Martins (2019) analyses the effects of extensions in Portugal during the period 2008-2011. Drawing on the scattered timing of extensions, social security data, and a difference-in-differences approach, he finds that extensions tend to decrease sectoral employment by 2% over the four months following an extension, particularly among smaller firms. By contrast, Hartog et al. (2002) do not find much of a role for extensions in the Netherlands.[4]

Taking these findings at face value, one possible explanation is that the results differ because extensions operate differently in different countries. Indeed, the Netherlands subjects extension requests to strict criteria to ensure that collective agreements are representative of the entire sector (Hijzen et al, 2019), while this is not the case or applies only to a more limited extent in most other countries where extensions are important. If more representative employer associations negotiate more representative agreements that suit most firms in their sector, this could explain the absence of adverse employment effects in the Netherlands. Importantly, no previous studies have directly examined the role of

[3] However, they also present evidence that suggests that the effects are more adverse in countries where the tax wedge – the difference between labor costs and take-home pay for employees - is higher.

[4] See also Martins et al (2017), who also consider the case of Portugal, and Diéz-Catalán and Villanueva (2015), who explore the contrast in collective bargaining in Spain before and after the emergence of the 2008 financial crisis and find evidence of increased job loss from more generous contracts. De Ridder and Euwals (2016) provide suggestive evidence that extensions increase wages in the Netherlands, but do not consider the role of extensions for employment.

representativeness by taking account of the membership of firms to employer associations. This paper contributes to the literature on collective bargaining by providing new insights on the causal impact of extensions. More specifically, this paper analyses the impact of administrative extensions of collective agreements on employment in affiliated versus non-affiliated firms. Importantly, it sheds light on the extent to which concerns about representativeness are warranted in practice. Our paper is also related to a broader literature about the role of firms in explaining wage inequality and the contrasting outlooks across countries, possibly depending on their evolving collective bargaining models (Card et al., 2013; Devicienti et al., 2019).

The analysis is based on a natural experiment that resulted from the immediate suspension of extensions by the new government in Portugal that took office in June 2011. We employ a regression discontinuity design (RDD) that exploits the standard administrative delay between the time an agreement is concluded until the time it is extended in combination with the suspension of extensions in June 2011. Importantly, this resulted in a sharp and unanticipated decline in the probability that an extension was issued several months prior to the change in government and the change in policy regarding extensions. This approach offers important advantages over the difference-in-differences method adopted in Martins (2019), which also draws on a different data set and covers a different period.

Our main result is that extensions had a negative effect on employment growth during the period 2010-2011, amounting to five percentage points or more depending on the specification. Moreover, the negative impact of extensions tends to be concentrated among non-affiliated firms. This suggests that the limited representativeness of employer associations is a potentially important factor behind the adverse effect of extensions on employment. These large effects are likely to reflect to some extent the specific context of recession during which the natural experiment took place. The adverse effects of extensions are also larger the longer the administrative delay in processing extensions. The latter reflects the role of retro-activity, which refers to the requirement in place until the 2012 labour reform for non-affiliated firms to pay wage arrears over the period from the entry-into-force of the original collective agreement to the time when the extension is issued. Finally, we present evidence that suggests that the adverse effect of extensions on employment growth comes about through their impact on wages in the bottom part of the distribution. This hints at a potential trade-off between the wage and employment effects of extensions.

The remainder of this paper is structured as follows. Section 2 provides the economic and institutional context at the time of the experiment. Section 3 describes the experiment, explains how this is exploited using a regression discontinuity design and discusses the validity of this approach in the present context. Section 4 discusses our matched employer-employee dataset complemented with information on collective agreements and extensions (including their timings). Section 5 presents the evidence on the impact of administrative extensions across the board as well as separately for firms that are affiliated to an employer association and those that are not. It also analyses how the impact of extensions depends on the degree of representativeness of employer associations and the role of retro-activity in combination with the administrative delay in processing extensions. Section 6 presents some additional results in relation to wage inequality. Finally, Section 7 concludes.

II. ECONOMIC AND INSTITUTIONAL CONTEXT

During the 2000s, Portugal experienced low growth, declining international competitiveness and deepening macroeconomic imbalances (Blanchard, 2007). As a result, the country had to face up to the global financial crisis in an already fragile situation. The global financial crisis prompted large increases in public deficits and loss of market access amid a sudden stop in capital flows, leading to a request for financial assistance, in April 2011, directed towards the European Union, the European Central Bank and the International Monetary Fund. Financial support was made available, conditional on several structural reforms and adjustment measures. Given the potential importance of real wage adjustment to minimize employment losses and concerns about the role of collective bargaining, structural reforms also included measures on the extension of collective agreement to non-affiliated firms.

Until mid-2011, collective bargaining in Portugal – as in many other European countries – took place almost exclusively at the sectoral level, being driven by negotiations between employer associations and trade unions. Firm-level bargaining was essentially limited to state-owned and former state-owned companies and a small number of other very large firms or holdings. Moreover, coverage of (sectoral) collective agreements was very high, despite the low union and employer association density rate (estimated at 11% and 25% in 2009 of workers and firms, respectively), due to administrative extensions which widened the reach of collective agreements beyond the signatory parties to all firms and workers in each sector.[5]

For an extension of a collective agreement to be issued a request needed to be made to the government by either the subscribing employer association or the trade union (or both), with respect to a new or a revised collective agreement (both of which were typically about the updating of the minimum wages for workers of different occupations and job levels – Martins (2019)). The government would then assess the economic and social desirability of a potential extension, partly based on an empirical analysis of the number of workers potentially affected (in terms of increased salaries), while allowing other firms or unions to present arguments against the potential extension. While this administrative procedure would delay the issuance of extensions by several months, the positive outcome of this procedure was virtually never in doubt, resulting in the extension of almost all collective agreements. To fully promote a level-playing field, extensions entered into force retro-actively, so that they would have legal effect at the same time as the underlying collective agreements for the signatory parties, even if the extensions were issued (much) later. This forced firms to pay wage increases to workers not initially covered in the collective agreements, i.e. workers employed by non-affiliated firms and non-unionised workers in affiliated firms (the latter if their firms had not already extended internally and unilaterally the collective agreement). These wage increases had to be paid not only from the time the extension was issued but also from the time the underpinning agreement was signed.

Due to the low levels of membership of the social partners (in particular the unions), extensions played a key role in supporting high and stable collective bargaining coverage in Portugal and effectively removed the scope for low-wage competition between affiliated and non-affiliated firms in each sector. Given the high level of unemployment and the need for restoring international competitiveness, extensions were increasingly seen as a source of downward wage rigidity, particularly in smaller, younger, and typically less productive firms, who were generally not affiliated and not represented in the collective bargaining process. The low affiliation rates of the social partners thus not only increased the economic

[5] In the absence of extensions, the “double affiliation principle” applies. See footnote 2 for details. Collective agreements and their revisions were also focused on the increase of the minimum wages per occupation, as most non-wage provisions simply replicated the contents of the Labour Code (Martins and Saraiva, 2020).

importance of coverage extensions, but also raised concerns that collectively agreed wage floors did not reflect appropriate working conditions for non-affiliated firms.

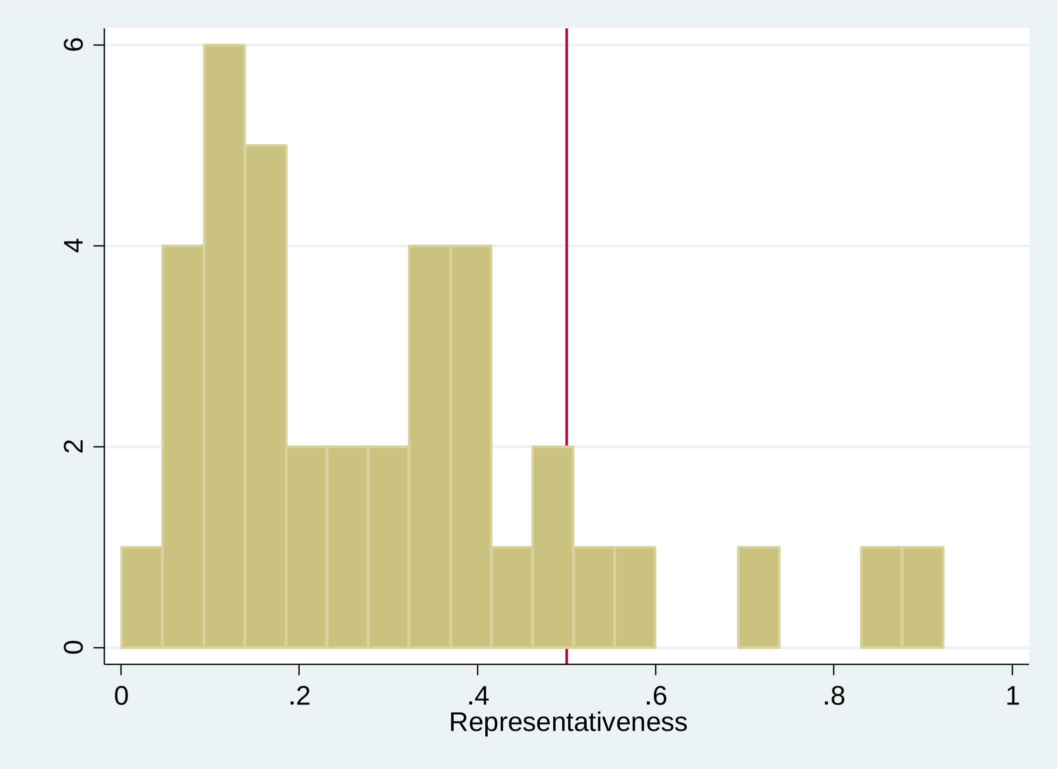
To address questions about the role of administrative extensions for wage adjustment, the government that took office in June 2011 temporarily suspended the issuance of extensions with immediate effect while preparing a reform about the procedures governing extensions. This decision was made in the context of the structural reform package agreed between Portugal and the 'troika' (EU, ECB and IMF). This package included a provision about collective bargaining ("define clear criteria to be followed for the extension of collective agreements", including "the representativeness of the negotiating organisations and the implications of the extension for the competitive position of non-affiliated firms").[6] The ensuing labor law reform of 2012 re-introduced extension procedures, while revising them in two important ways. The first was to subject extensions to representativeness criteria. This entailed that extensions were made conditional on the employer association representing firms that together accounted for 50% of the workforce of the relevant sector. The second change was that extensions entered into force at the date of the administrative decision on the extension rather than the entry-into-force date of the collective agreement itself—in other words, retroactivity was abandoned.

Given the low density of employer association membership, this led to concerns that the 50% representativeness criteria were too strict. Figure 2 documents the degree of representativeness of sectoral collective agreements signed between September 2010 and August 2011. Representativity is measured by share of the workforce in firms that are affiliated to an employer association with respect to the total workforce of the sector. The figure shows that, in most collective agreements, such share in affiliated firms fell well short of the 50% threshold adopted.

[6] The full passage reads "Define clear criteria to be followed for the extension of collective agreements and commit to them. The representativeness of the negotiating organisations and the implications of the extension for the competitive position of nonaffiliated firms will have to be among these criteria. The representativeness of negotiating organisations will be assessed on the basis of both quantitative and qualitative indicators. To that purpose, the Government will charge the national statistical authority to do a survey to collect data on the representativeness of social partners on both sides of industry. Draft legislation defining criteria for extension and modalities for their implementation will be prepared by Q2-2012". See the full text of the memorandum in http://ec.europa.eu/economy_finance/eu_borrower/mou/2011-05-18-mou-portugal_en.pdf (page 24).

Figure 2. The representativeness of collective agreements

Number of sectoral collective agreements signed between September 2010 and August 2011by the share of workers in firms affiliated to an employer association



Source: Ministry of Labour (DGERT) and *Quadros de Pessoal*, authors' calculations. Note: The vertical axis indicates the number of agreements of each level of representativeness

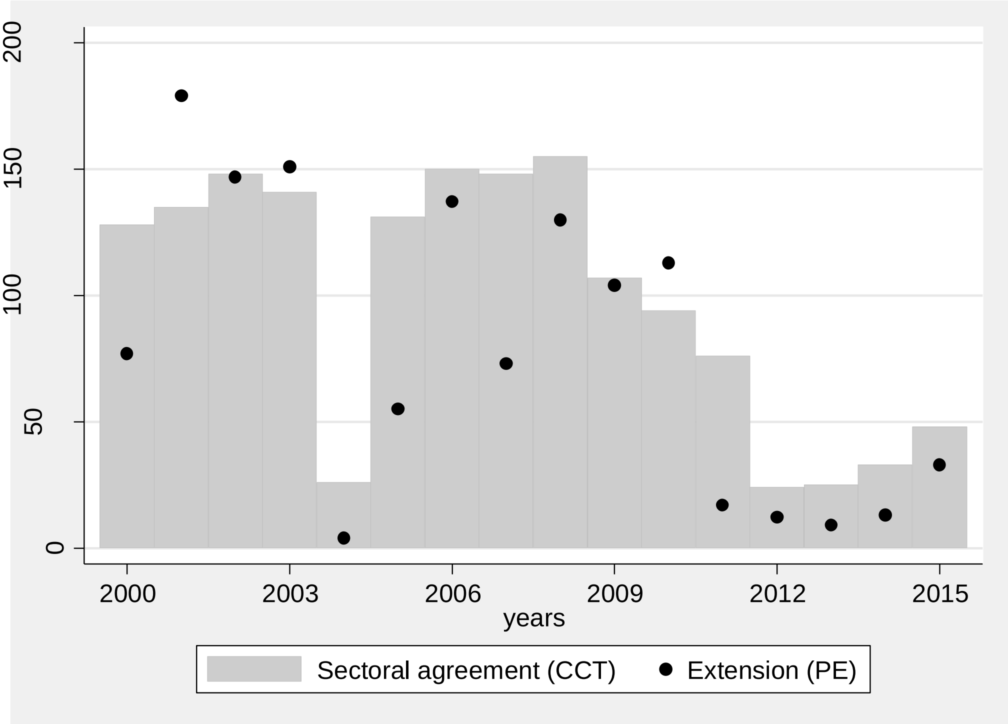
Consequently, the re-introduction of extensions in 2012 only led to a modest pick-up in the number of administrative extensions as shown in Figure 3. This largely reflected the persistently low number of new or revised collective agreements concluded at the sectoral level.[7]

Lingering concerns about the stringency of representativeness criteria in relation to extensions resulted in another reform in extension criteria in July 2014, after the end of the adjustment program, which introduced an alternative representativeness criterion, met if more than 30% of firms affiliated to a signatory employer association consisted of small or medium-sized enterprises (firms employing less than 250 employees). Since this is likely

[7] Apart from between 2011 and 2012, the figure also reveals a sharp but temporary decline in the number agreements signed and extended in 2004. This reflects the role of the 2003 labour reform that delayed the conclusion of new or revised agreements until the reform was completed. This change therefore does not reflect an unanticipated change in the extension regime as was the case in the period under study in this paper and is therefore not amenable to the same evaluation as the one conducted in the present paper.

to be the case for the large majority of employer associations, this largely represented a return to the situation pre-2011. This has resulted in a modest pick-up of the number of extensions issued in 2015 (Figure 3). More recently, in June 2017, representativeness criteria were dropped altogether, making extensions virtually automatic again, even in the current context of the Covid19 pandemic and its very negative labour market effects.

Figure 3. Number of sectoral collective agreements and extensions, 2000-2015



Source: Ministry of Labour (DGERT), authors' calculations. Note: The vertical axis indicates the number of agreements in each year considered.

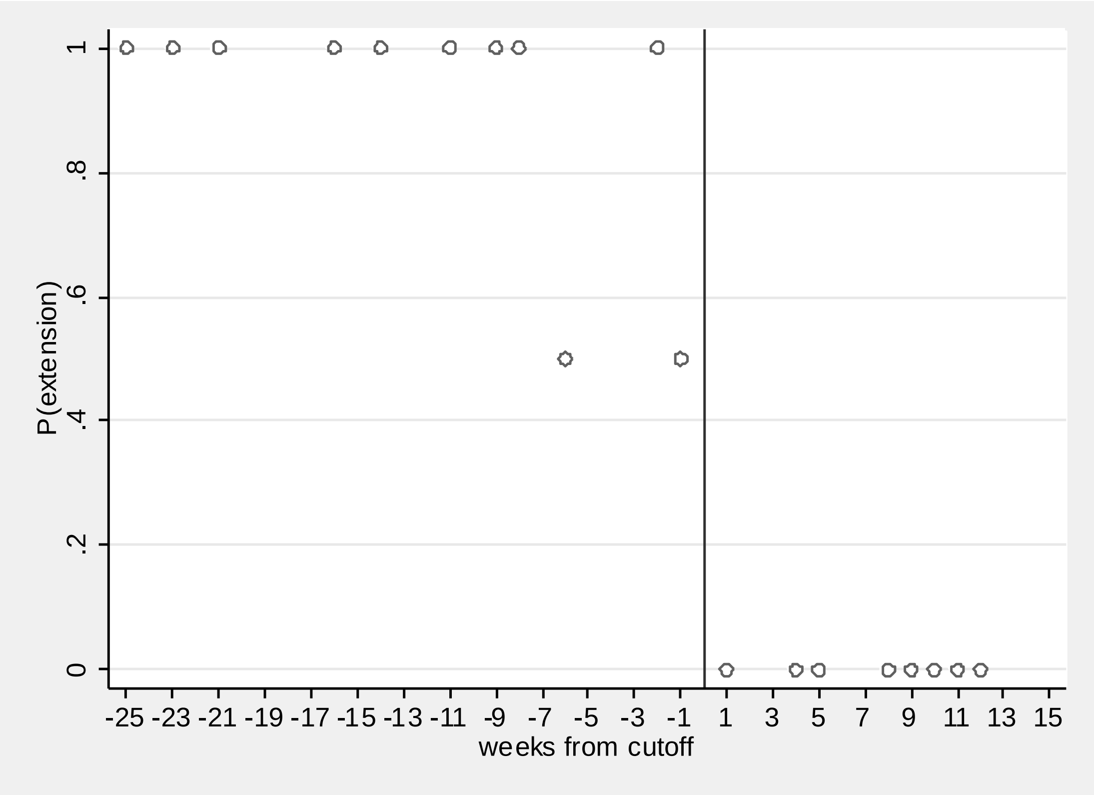
III. Methodology

A. The "natural" experiment

In order to analyse the causal effects of coverage extensions, this paper makes use of the natural experiment that resulted from the decision of the government that took office in 2011 to immediately suspend the extension of collective agreements, as described above. Importantly, due to the usual administrative delay associated with the extension of collective agreements, a substantial number of collective agreements had been signed before the new government took office, on 21 June 2011, but were not extended or, in a limited number of cases, had their extension considerably delayed (to the second half of 2012 or later).

As shown in Figure 4, this created a sharp discontinuity around February/March 2011 in the probability that a collective agreement was extended in the 12 months following the conclusion of a collective agreement. More specifically, the figure displays the probability that collective agreements *published* in each of the weeks before or after 1 March 2011 are extended in 2011. Until 24 January 2011, about five weeks before the cutoff date, all collective agreements were extended during the subsequent twelve months. In the period from 24 January 2011 to 28 February 2011 some collective agreements were extended, but not all as was the case before, presumably because no decision had been reached by the time the new government took office. From 1 March 2011 until 20 June 2011, no collective agreements were extended during the subsequent twelve months.

Figure 4. The probability that a collective agreement is extended



Source: Ministry of Labour (DGERT), authors' calculations. Weeks from 1 March 2011 (September 2010 – June 2011)

Moreover, the decision of the new government to suspend the extension of collective agreements was unexpected and hence could not be anticipated by trade unions and employer associations conducting the collective bargaining (a process that can last six months or more). In fact, in February or even in March 2011, there was no public information regarding the April 6th bailout request, the May 17th bailout package, the June 5th elections (unexpected as a full legislature would come to an end only in 2013) nor the new policy on extensions introduced by the government that took office in June 21st.

This means that insofar as one focuses on a short window around 1 March 2011, it is unlikely that there are systematic differences in the characteristics of workers and firms covered by collective agreements in the period just before the new government took office - and that were extended - and the characteristics of workers and firms covered by collective agreements in the period just after the new government took office - and that were not extended. While this is uncontroversial in Portugal, it is important for the present paper as it determines the validity of the “natural” experiment for analyzing the causal impact of extensions.

B. Regression discontinuity design

The sharp and unanticipated decline in the probability that a collective agreement is extended in early 2011 is used to analyse the impact of extensions by means of a regression discontinuity design (RDD). The intuition of RDD is that the outcomes of firms and workers covered by collective agreements signed just before 1 March 2011 provide a good counterfactual for those of firms and workers where a collective agreement had been signed just after 1 March but was not extended. The main advantage of RDD compared with other quasi-experimental estimators is that it relies on relatively weak assumptions and that these are testable in the same way as in a randomised experiment (Hahn et al., 2001; Lee and Lemieux, 2010).

Since there is some variation in the administrative delay associated with the extension of agreements, the probability of extension does not fall from one to zero from one week to the next (see Figure 4). In order to use a strict sharp design, we drop the two agreements signed between 24 January and 28 February of 2011 that were not extended. In this case, all agreements signed before 1 March 2011 were extended and all those signed after were not.[8]

Formally, the sharp RDD can be described by the following model:

(1) $y_i = \alpha + \delta D_i + \gamma f(t_i - T) + \theta X_i + v_i$,

where y_i refers to the outcome variable of interest, e.g. the growth rate of employment between 2010 and 2011 across all firms subject to collective agreement i, D_i is a treatment dummy that is equal to one if an agreement i is extended in 2011 and zero otherwise (in practice, this means that the dummy takes value zero from 1 March 2011 onwards), $f(.)$ is a function that controls for the independent effect of relative time t on both sides of the threshold T (i.e. the number of weeks until or since the time threshold), and X_i is a set of controls (discussed in Section C below).[9]

An alternative possibility is to make use of a fuzzy RDD that explicitly takes account of the non-fully sharp decline in the probability of extension during the period 24 January 2011 to 28 February 2011, given the two data points mentioned above. Formally, the fuzzy RDD can be described by an outcome equation and a treatment equation. The treatment equation models the probability that a collective agreement is extended conditional on relative time as a function of a constant (α), a dummy (T_i) that is equal to one from the date after which the probability of an extension is zero and a function that controls for the independent effect of relative time on both sides of the threshold ($f(.)$):

(2a) $D_i = \alpha + \delta T_i + \gamma f(t_i - T) + \theta X_i + \varepsilon_i$

[8] An alternative option would be to focus on agreements signed just before 24 January 2011 and those signed just after 28 February 2011. However, this would unduly restrict the size of the sample.

[9] In other words, in our RDD we make use of a cross-sectional comparison of annual outcomes within the same year between firms in sectors covered by collective agreements that were extended and firms in sectors covered by collective agreements that were not extended. Therefore, we do not conduct an RDD over time: our outcome variables focus on changes between October of one year (say 2011) and October of the previous year (2010).

The outcome equation in turn models the outcome variable of interest (y) as a function of a constant (α), the predicted probability that the agreement is extended $\widehat{D}$ and a function that controls for the independent effect of time on both sides of the threshold ($f(.)$), as in (2a):

(2b) $y_i = \alpha + \tau\widehat{D}_i + \gamma f(t_i - T) + \theta X_i + v_i$

The outcome and treatment equations are estimated with 2SLS using the same estimation sample for the treatment and outcome equations.

In practice, we pool the data (described below) across affiliated and non-affiliated firms, that is, we stack them up for each collective agreement. We then perform two exercises. First, we estimate the impact of extensions on total employment growth between October 2010 and October 2011 across firms while controlling for affiliation status. Second, in order to analyze the differential effects of extensions on firms that are affiliated to an employer association versus those that are not, we add an interaction between the treatment dummy and the dummy for affiliation status, while also allowing for different relative time effects across affiliation-status groups, and, as before, controlling for the independent effect of affiliation status. Standard errors correct for heteroskedasticity and are clustered by agreement date and agreement to take account of the discrete nature of our data as suggested by Card and Lee (2008) as well as the fact that we split and then pool the data between affiliated and non-affiliated firms. Note that we draw on a limited number of agreements and that it would be desirable to have a larger sample size, particularly around the cutoff.

Controlling for relative time effects is key in the present context for two main reasons. First, since the dependent variables are measured in October 2010 and October 2011, the part of the year to which firms are exposed to extensions depends on the timing of the extensions. An agreement that is extended later necessarily has a smaller time period to generate effects than one that is extended earlier. Second, economic conditions may affect the timing of agreements as well as their actual contents. For these reasons, outcomes are likely to depend on relative time. In our analysis, relative time effects are assumed to be either linear or quadratic and are allowed to differ between the two sides of the threshold.[10] Regressions are weighted (by the number of employees in 2010) in order to obtain the average effect of extensions on total employment (rather than the average effect of extensions across collective agreements).

An important feature of RDD is that, as long as the treatment is randomized around the threshold, controlling for any characteristics should not affect the estimated size of the discontinuity at the threshold. Controlling for observed or unobserved characteristics in our context may nevertheless be helpful, to take account of the potential effects of any other differences across sectors that concluded their bargaining just before or after the threshold for extension.

C. Validity

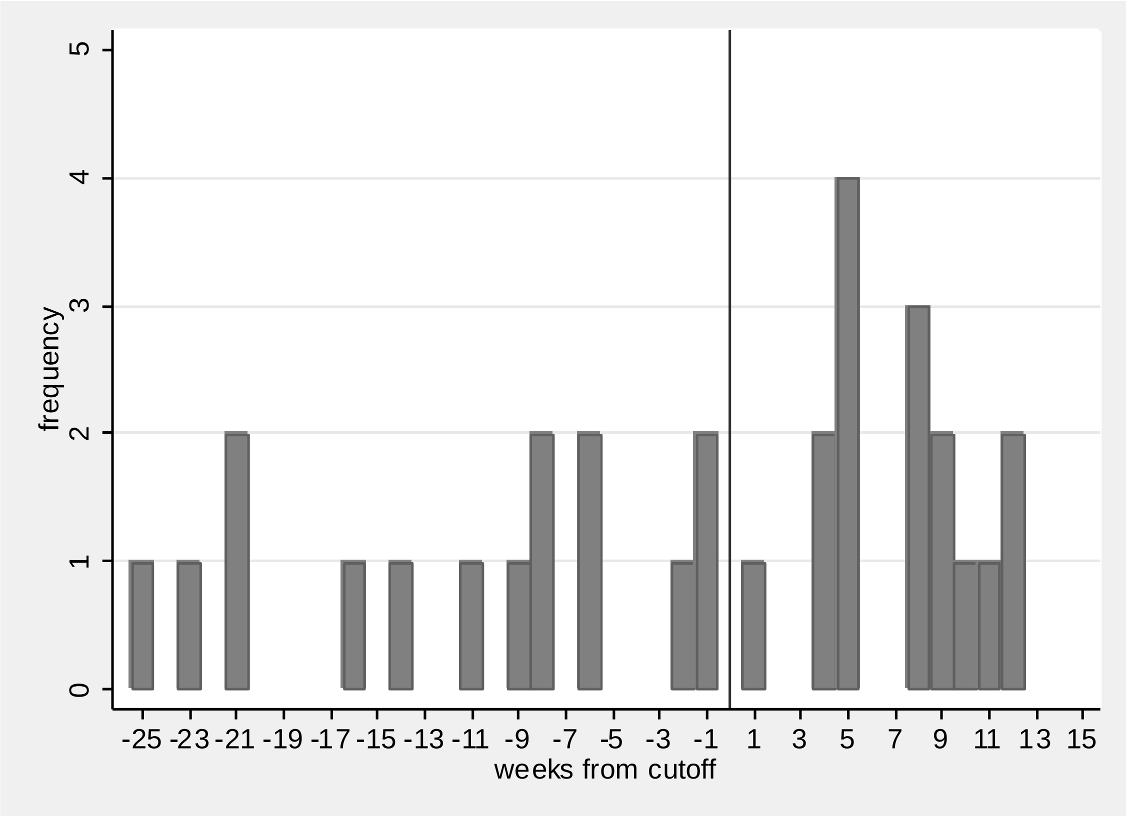
The validity of our natural experiment hinges on the assumption that the decision of the new government to suspend the extension of collective agreements was unexpected and hence could not be anticipated by the social partners. If, on the contrary, the suspension of extensions had been anticipated, this could have affected the incentives for concluding an agreement and hence the frequency of observing new or revised collective agreements. Figure 5 plots the number of agreements in each week during the period September 2010 to June 2011. It does not indicate that the average number of collective agreements published in each week declined after 1 March 2011. The average number of agreements per week is even slightly higher after 1 March 2011 than in the period that preceded it.[11]

Figure 5. The number of collective agreements over time

Weeks from 1 March 2011 (September 2010 – June 2011)

[10] Given the small number of collective agreements, we will mainly focus on (potentially asymmetric) linear relative time effects.

[11] Given the limited number of collective agreements in each week, a formal test on the continuity of the density of agreements around the threshold would not be informative.



Source: Ministry of Labour (DGERT), author's calculations.

Anticipation effects may also be reflected in the contents of the agreements and their composition across different types of firms and workers. In order to check whether there are any systematic differences between agreements signed just before and after 1 March 2011, we conducted a series of balancing tests which assess whether there are discontinuities along a variety of different dimensions across the threshold (Table 1). In practical terms, this involves estimating equations (1) and (2) using several pre-determined variables in our dataset as the dependent variable (see the next section for more information on our data). These are respectively the degree of representativeness (the share of the sector workforce in affiliated firms), pre-reform employment growth (2009-2010), the size of the agreement in terms of the log number of employees potentially covered, log average firm size (in terms of employment), log average hourly wage, log median hourly wage, export intensity (exports as share of total revenue), and log labor productivity (revenue per worker). The balancing tests control for linear or quadratic relative time effects and are conducted for the pooled sample as well as separately by affiliation status.

Table 1. Balancing tests

	All		Non-affiliated		Affiliated	
	(1)	(2)	(3)	(4)	(3)	(4)
Balancing variables	linear	quadratic	linear	quadratic	linear	quadratic
Representativeness, 2010	0.0815	-0.0080	0.0923	-0.0183	0.0220	0.0303
- share of workforce in afffiliated firms	(0.1143)	(0.0574)	(0.1422)	(0.0617)	(0.0695)	(0.0510)
Employment growth, 2009-2010	0.0865	0.0975	0.0953	0.1045	0.0813	0.0844
	(0.0155)	(0.0080)	(0.0195)	(0.0106)	(0.0099)	(0.0076)
	***	***	***	***	***	***
Log employment, 2010	-0.2195	-0.1584	-0.4327	-0.3626	0.5093	0.1547
	(0.6026)	(0.6073)	(0.6009)	(0.6273)	(0.7890)	(0.5082)
Log average firm size, 2010	-1.2418	-1.4115	-1.2439	-1.3098	-1.5805	-1.6135
- number of workers per firm	(0.3004)	(0.1840)	(0.2429)	(0.2017)	(0.2265)	(0.1765)
	***	***	***	***	***	***
Log average wage, 2010	-0.0510	-0.0812	-0.0530	-0.0603	-0.1147	-0.1120
- within job title and year	(0.0870)	(0.0956)	(0.0852)	(0.0875)	(0.0901)	(0.0758)
Log median wage, 2010	0.0040	-0.0092	-0.0127	-0.0059	-0.0243	-0.0035
- within job title and year	(0.0764)	(0.0828)	(0.0775)	(0.0781)	(0.0824)	(0.0605)
Export intensity, 2010	-0.4642	-0.4553	-0.4072	-0.3808	-0.5818	-0.6151
	(0.0523)	(0.0494)	(0.0579)	(0.0583)	(0.0466)	(0.0264)
	***	***	***	***	***	***
Log labour productivity, 2010	0.1753	-0.0735	0.2313	0.0320	-0.2217	-0.1636
	(0.4604)	(0.5252)	(0.4727)	(0.5360)	(0.3814)	(0.2627)

Notes: The estimates reported correspond to the coefficients of the extension dummy in equation (1). Regressions are weighted by the number of employees in 2010. Standard errors are robust and clustered by collective agreement and signature date. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively.

The results in Table 1 suggest that for some variables there are statistically significant differences between the treatment and control groups. Given the relatively small number of agreements used in the present context and the large number of variables considered in these balancing tests, this should not be surprising. It suggests, however, that the natural experiment that we are exploiting does not provide the full equivalent of a randomized experiment. To address this, we make the treatment and control groups more comparable by explicitly allowing for differential pre-treatment trends in employment (in practical terms this is done by transforming the dependent variable into the change in the growth rate) as well as by including all variables as controls for which systematic differences are observed in our balancing analysis (average firm size and average export intensity).

IV. DATA AND IMPLEMENTATION

A. Administrative personnel records (Quadros de Pessoal)

The main data for this paper are Personnel Records (*Quadros de Pessoal, henceforth QP*), an administrative matched employer-employee panel that covers the universe of firms and workers in the private sector in Portugal (see Cardoso and Portugal, 2005, and Martins et al, 2012, for earlier papers using *QP*). Given our goals, we make use of annual information for the period 2009-2011. The information on workers (e.g. employment, earnings) refers to October of each year, while that on firms (e.g. sales) refers to the full year. Particularly important for the present purposes is the information on the applicable collective agreement for each worker and on employer association membership of firms, i.e. whether they are a member and, if so, of which association (see Addison et al (2017) and Martins (2020) for analyses of trade unions and employers' associations in Portugal). Since the latter is available only for 2010, we analyse the role of affiliation status for the impact of extensions based on their affiliation in the year before the reform. This implies that we focus on firms that were present in 2010, as well as all their workers, but do not consider new firms. For the present purposes, we do not require any information on employer association membership in the subsequent years. Firms that are covered by firm- and holding-level collective agreements are excluded from the analysis.

B. Information on collective agreements and extensions (DGERT)

Information on collective agreements and their possible extensions is publicly available from the Ministry of Labour (DGERT) website.[12] The resulting dataset includes information on the publication dates of collective agreements and, if applicable, of their extensions, the signatory employer associations and trade unions as well as the economic activities (and regions and or occupations, if applicable) covered by the agreement (see the appendix for more detail on the timelines of publications and extensions).

The dataset used for the empirical analysis consists of 36 collective agreements signed over the period October 2010 to August 2011 (see the Annex for details). Together these account for approximately 20% of the workforce in the private sector. In the empirical analysis, we mainly focus on the 31 agreements that were signed between 8 October 2010 and 20 June 2011. The reason for limiting the scope to agreements signed before 20 June 2011 is that the extension procedure was suspended when the new government took office in 21 June 2011. While this decision was not publicized and is unlikely to have had a major impact on

[12] http://bte.gep.msess.gov.pt/

collective bargaining in the following weeks, we feel it is more prudent to limit ourselves to agreements that were signed before the new government took place.[13]

The period from 8 October 2010 and 20 June 2011 can be divided into three sub-periods. First, a 25-week period from September 2010 to 24 January 2011 during which all nine new or revised agreements were subsequently extended. Second, a 5-week transition period during which three agreements were signed and two were not (24 January 2011 – 28 February 2011). Third, a 15-week period from 1 March 2011 to 20 June 2011 during which 18 new or revised agreements were signed that were not extended during the subsequent 12 months.

C. Combining the information into a semi-aggregated dataset

For the present purposes, we construct a dataset with information on employment and wages (as well as sales, exports and productivity) by agreement, year, and membership status. This data set follows from combining information from the *QP* matched employer-employee panel with information from collective agreements from the Ministry of Labour (DGERT). Unfortunately, linking the two sources of information is not straightforward in practice since the agreement codes in *QP* do not necessarily correspond to those used by DGERT. We proceeded as follows. First, since workers in each firm may be covered by more than one agreement, we focus on the agreement that represents the largest share of workers in the firm. Second, we use the name of the employer associations to which affiliated firms are affiliated to link collective agreements in *QP* and DGERT**.** Third, we extend the linking between the two collective agreement codes (QP and DGERT) to non-affiliated firms. In other words, we establish the domain of each collective agreement in terms of firm identifiers, first using 2010 data (for which we have employer affiliation information) and then the remaining years too. Fourth, we collapse the firm-level data by year, agreement and employer association membership status. This yields a dataset with 62 observations (31 agreements for each affiliation status group) for each year.

In an extension, we also create a different version of our data set, for analysis not at the collective-agreement and affiliation-status level but instead at the more detailed

[13] However, as will be shown below, the analysis is robust to adding these agreements to the analysis. Note also that two agreements had unclear information regarding affiliation membership and are dropped in some analysis.

occupation/agreement/affiliation level. In this second case, each observation corresponds to the number of workers in an occupation of an agreement in (October) of a year. The advantage of this approach is that the same occupation can be present in different agreements, including agreements that were subject to extensions and agreements that were not subject to extensions. However, since there is a change in the occupational classification codes between 2009 and 2010, the occupation-level analysis can be conducted for employment growth (between 2011 and 2010) but not for the change in employment growth (which would also require data for 2009).

V. Results on employment growth

A. Overall effects of extensions

The results on the impact of extensions on employment growth (ΔE_{2011}) and the change in employment growth (ΔE_{2011}-ΔE_{2010}) on all covered firms, irrespective of membership status, are reported in Table 2. It shows the results based on both the fuzzy and the sharp RDD under the assumption of either linear or quadratic relative time effects. The results based on the sharp RDD using a linear specification are visualized in Figure 6.[14]

According to the results, extensions have an adverse and statistically significant impact on employment growth. The results tend to be qualitatively similar whether a fuzzy or a sharp set-up is used, whether linear or quadratic controls for relative time are included and whether the pre-reform trend is controlled for or not.[15] The impact of extensions on employment growth is potentially large, ranging from 5 to 10 percentage points depending on the specification. These large effects are likely to reflect at least in part the specific context during which the natural experiment took place, namely that of the 2011-2013 recession hitting firms still recovering from the global financial crisis. Because the need for adjusting working conditions was presumably greater under such circumstances, extensions may have had a larger adverse impact on employment than they would have had in normal

[14] For ease of presentation, the results presented in Figure 6 do not include any controls apart from affiliation status and relative time effects.

[15] Note, however, that the coefficients lose statistical significance when controlling for quadratic relative time effects and focusing on employment growth but remain largely unchanged when focusing on the change in employment growth.

times. Furthermore, as discussed above, the retro-active entry-into-force of extensions in a context where many firms are liquidity constrained is found to have exacerbated the negative employment effects found here.

Table 2. The effects of extensions, all firms

Panel A. Employment growth, p.p., (ΔE_{2011})

	Fuzzy		Sharp	
	(1)	(2)	(1)	(2)
Treatment dummy	-0.0953	-0.0500	-0.0578	-0.0420
	(0.0446)	(0.0591)	(0.0262)	(0.0262)
	**		**	
Constant	-0.1042	-0.1358	-0.1226	-0.1293
	(0.0388)	(0.0274)	(0.0313)	(0.0236)
	**	***	***	***
Relative time effects	Linear	Quadratic	Linear	Quadratic
Observations	62	62	58	58
R-squared	0.3976	0.4037	0.4290	0.4359

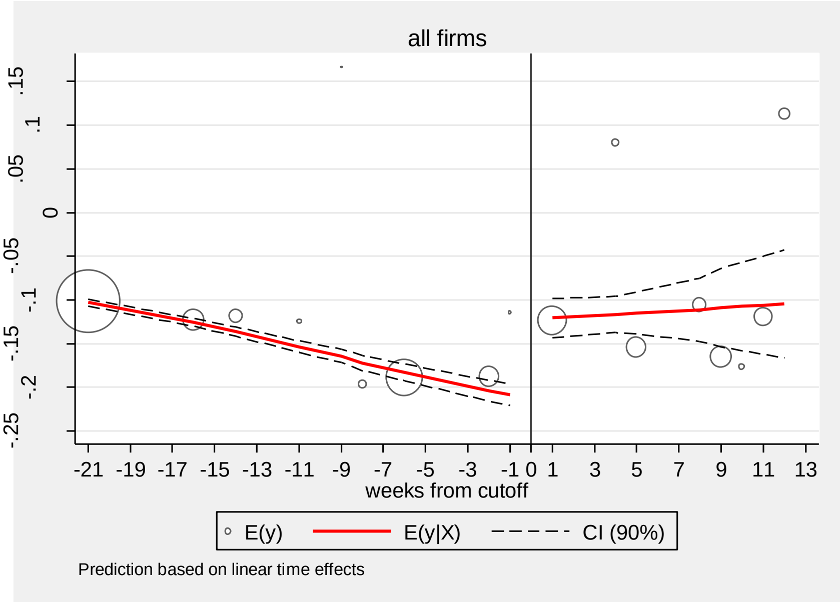
Panel B. Change in employment growth, p.p., (ΔE_{2011}- ΔE_{2010})

	Fuzzy		Sharp	
	(1)	(2)	(1)	(2)
Treatment dummy	-0.1574	-0.1472	-0.1022	-0.1035
	(0.0548)	(0.0709)	(0.0301)	(0.0295)
	***	**	***	***
Constant	-0.1950	-0.1985	-0.2238	-0.1926
	(0.0380)	(0.0360)	(0.0279)	(0.0247)
	***	***	***	***
Relative time effects	Linear	Quadratic	Linear	Quadratic
Observations	62	62	58	58
R-squared	0.6579	0.6581	0.6809	0.6958

Notes: Regressions are weighted by the number of employees in 2010. They include controls for log average firm size, export intensity and affiliation status. Standard errors are robust and clustered by collective agreement and signature date. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively. Authors' calculations based on QP and DGERT data.

Figure 6. The overall effects of extensions on employment growth

Change in employment growth, p.p., (ΔE_{2011}- ΔE_{2010}), sharp RDD



Notes: The figure presents the estimated change in the growth rate of employment by collective agreement publication week, as measured from the threshold date of 1 March 2011 (week 0). The results are estimated from a sharp RDD using employment weights. The size of the circles is proportional to the employment of the corresponding collective agreement(s). Standard errors are robust and clustered by collective agreement and signature date. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively. Authors' calculations based on QP and DGERT data.

The results are robust to a variety of different specifications (Table 3). Focusing on the specification with linear relative time effects, we now compare the baseline results based on the fuzzy RDD and the sharp RDD with alternative specifications or samples (Panel A and Panel B), in the following order. "Controls" includes only affiliation status as control and not average firm size and export intensity as in the baseline specification. "Bandwidth" extends the observation window by including agreements signed after the new government took office in June 2011 to the end of August 2011. "Falsification" assesses whether there is a discontinuity when using a placebo date that evenly splits the number of agreements in the post-reform period (March – August) on each side of a fictional threshold (around the middle of May). The results indicate that excluding the controls and extending the bandwidth does not qualitatively change the results relative to the baseline. The falsification test does not point to any discontinuities in employment growth around the fictional reform date.

Table 3. Sensitivity analysis, all firms

Panel A. Fuzzy RDD[1]

	Employment growth				Change in employment growth			
	Baseline	Controls	Bandwidth	Falsification	Baseline	Controls	Bandwidth	Falsification
Treatment dummy	-0.0953	-0.0511	-0.0976	-0.0345	-0.1574	-0.1217	-0.1540	-0.0345
	(0.0446)	(0.0163)	(0.0566)	(0.0556)	(0.0548)	(0.0189)	(0.0523)	(0.0556)
	**	***	*		***	***	***	
Constant	-0.1042	-0.0963	-0.1078	-0.2129	-0.1950	-0.1350	-0.1701	-0.2129
	(0.0388)	(0.0053)	(0.0523)	(0.0541)	(0.0380)	(0.0123)	(0.0327)	(0.0541)
	**	***	**	***	***	***	***	***
Relative time effects	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	62	62	76	46	62	62	76	46
R-squared	0.3976	0.3472	0.4975	0.5902	0.6579	0.5452	0.6898	0.5902

Panel B. Sharp RDD[1]

	Employment growth				Change in employment growth			
	Baseline	Controls	Bandwidth	Falsification	Baseline	Controls	Bandwidth	Falsification
Treatment dummy	-0.0578	-0.0603	-0.0164	-0.0345	-0.1022	-0.1169	-0.0898	-0.0345
	(0.0262)	(0.0203)	(0.0264)	(0.0556)	(0.0301)	(0.0245)	(0.0210)	(0.0556)
	**	***			***	***	***	
Constant	-0.1226	-0.0891	-0.1570	-0.2129	-0.2238	-0.1352	-0.2017	-0.2129
	(0.0313)	(0.0115)	(0.0412)	(0.0541)	(0.0279)	(0.0143)	(0.0259)	(0.0541)
	***	***	***	***	***	***	***	***
Relative time effects	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	58	58	72	46	58	58	72	46
R-squared	0.4290	0.3917	0.5210	0.5902	0.6809	0.5650	0.7015	0.5902

Panel C. Occupation-level analysis (employment growth)[2]

	Fuzzy RDD, Employment growth				Sharp RDD, Employment growth			
	Baseline	Controls	Bandwidth	Falsification	Baseline	Controls	Bandwidth	Falsification
Treatment dummy	-0.1282	-0.0543	-0.1292	-0.0197	-0.0558	-0.0606	-0.0462	-0.0197
	(0.0597)	(0.0209)	(0.0454)	(0.0404)	(0.0282)	(0.0304)	(0.0213)	(0.0404)
	**	***	***		**	**	**	
Constant	1.5142	-0.0956	-0.2796	-0.6206	1.4891	-0.0897	-0.2920	-0.6206
	(1.8044)	(0.0150)	(0.3563)	(0.0827)	(1.8096)	(0.0265)	(0.3562)	(0.0827)
		***		***		***		***
Relative time effects	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	3,320	3,320	4,257	2,451	3,048	3,048	3,985	2,451
R-squared	0.0774	0.0205	0.1527	0.1626	0.0821	0.0243	0.1646	0.1626

Regressions are weighted by the number of employees in 2010 and include controls for log average firm size, export intensity and affiliation status unless stated otherwise. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively. "Controls": affiliation status only; "bandwidth": October 2010-August 2011; "Falsification": placebo reform date (mid-May 2011) using agreements in post-reform period only (March 2011 – August 2011).

1. Standard errors are robust and clustered by collective agreement and signature date.

2. Standard errors are robust and clustered by collective agreement, occupation and signature date.

Authors' calculations based on QP and DGERT data.

The results are also qualitatively and quantitatively similar when conducting the analysis at the occupation/agreement/affiliation level (Panel C). The specification is similar to the one for the baseline results except for the inclusion of occupation dummies. Standard errors are clusters by occupation, agreement and signature date. The inclusion of occupation dummies ensures that the effect of extensions is identified within similar occupations and hence not driven by differences in the occupational structure of agreements that extended and those that are not extended.

B. Effects of extensions by affiliation status

In order to analyze the effects of extensions across firms that are or are not affiliated to an employer association, we now allow their effects to differ across firms that are and those that are not by adding an interaction term of the treatment dummy with affiliation status. In this case, we consider a new version of our original specification as follows:

$$(1)\ y_i = \alpha + \delta_1 D_i * A_i + \delta_2[D_i * (1 - A_i)] + \delta_3 A_i + \gamma f(t_i - T) + \theta X_i + v_i,$$

in which A_i is a dummy variable equal to one for affiliated firms (a status that is already determined at the time of the extensions reform). See also Becker et al. (2013) for an analysis of the identification of heterogeneous treatment effects with an RDD where the heterogeneity of treatment effects pertains to interactions with observable variables as in our case. The baseline results along with a number of sensitivity checks are reported in Table 4. The results based on sharp RDD with linear relative time effects are visualized in Figure 7.

The results indicate that the adverse impact of extensions on employment growth tends to be concentrated among non-affiliated firms. This is the case for the majority of specifications reported, including for our preferred specifications that control for the pre-reform growth rate in employment. The fuzzy RDD results suggest that the impact of extensions is negative and statistically significant for non-affiliated firms, but positive and statistically significant for affiliated firms. Using the sharp RDD, the results are more mixed, with the results for employment growth suggesting that the effects are concentrated among affiliated firms and those for the change in employment growth that the effects are concentrated among non-affiliated firms.[16] Since we prefer the results that control for

[16] When focusing on the change in employment growth, extensions have a significant impact on both non-affiliated firms and affiliated firms, with their impact being about twice as large among non-affiliated firms as among affiliated firms. The effects among affiliated firms may indicate that some of these firms only increase the pay of their non-unionised workers once extensions are issued. Alternatively, these effects may

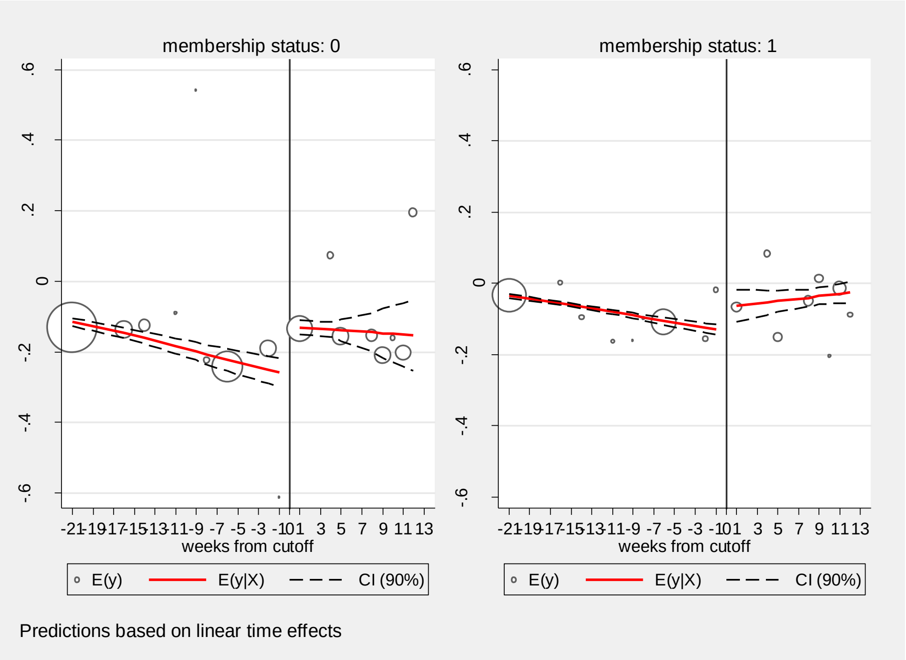
(continued…)

differences in the pre-reform growth rate in employment by focusing on the change in employment growth, we conclude that the negative effects of extensions are concentrated among non-affiliated firms.

The sensitivity checks further suggest that the baseline results do not hinge on the inclusion of controls or the definition of the observation window and systematic differences across extended and non-extended agreements in occupational structure. Moreover, the falsification test does not point at the presence of any significant discontinuities around the fictive threshold in the post-reform period.

Figure 7. The effects of extensions by affiliation status

Change in employment growth, p.p., (ΔE_{2011}- ΔE_{2010})



Notes: "Membership status: 0" refers to non-affiliated firms; "Membership status: 1" refers to affiliated firms. The figure presents the estimated change in the growth rate of employment by collective agreement publication week, as measured from the threshold date of 1 March 2011 (week 0). The results are estimated from a sharp RDD using employment weights. The size of the circles is proportional to the employment of the corresponding collective agreement(s). Standard errors are robust and clustered by collective agreement and signature date. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively.

These results are consistent with the view that affiliated firms can shape the use of extensions for their own benefit at the expense of non-affiliated outsiders. They also

reflect the increase in market power by affiliated firms and the subsequent reduction in output and employment to maximize profits.

provide a first indication that representativeness matters since the wage floors negotiated in collective agreements appear to be less appropriate for affiliated firms than for non-affiliated firms. This suggests that the lack of representativeness of employer associations is a potentially important factor behind the adverse effect of extensions.

Table 4. Results by affiliation status

Panel A. Employment growth (2010-2011)[1]

	Fuzzy RDD				Sharp RDD			
	Baseline	Controls	Bandwidth	Falsification	Baseline	Controls	Bandwidth	Falsification
Non-affiliated firms	-0.1121	-0.1395	-0.1114	0.0224	-0.0536	-0.0515	-0.0071	0.0684
* treatment dummy	(0.0470)	(0.0574)	(0.0592)	(0.0661)	(0.0329)	(0.0248)	(0.0311)	(0.1080)
	**	**	*			**		
Affiliated firms	0.1790	0.1172	0.0087	0.0631	-0.0980	-0.0982	-0.0610	-0.0280
* treatment dummy	(0.0712)	(0.0876)	(0.0962)	(0.0577)	(0.0204)	(0.0326)	(0.0292)	(0.0336)
	**				***	***	**	
Affiliated firms	-0.0989	-0.0549	-0.0200	0.0631	-0.1633	-0.0575	-0.0985	-0.0295
	(0.0497)	(0.0598)	(0.0798)	(0.0577)	(0.0481)	(0.1103)	(0.0594)	(0.0497)
	*				***			
Constant	-0.0544	-0.0587	-0.0527	-0.0670	-0.0786	-0.0975	-0.0722	-0.0978
	(0.0180)	(0.0275)	(0.0300)	(0.0664)	(0.0156)	(0.0187)	(0.0269)	(0.1082)
	***	**	*		***	***	**	
Relative time effects	linear	linear	linear	linear	linear	linear	linear	linear
Observations	62	62	76	46	58	58	72	46
R-squared	0.4157	0.3802	0.5026	0.5137	0.4579	0.4140	0.5432	0.5393

Panel B. Change in employment growth[1]

	Fuzzy RDD				Sharp RDD			
	Baseline	Controls	Bandwidth	Falsification	Baseline	Controls	Bandwidth	Falsification
Non-affiliated firms	-0.1787	-0.2446	-0.1706	-0.0370	-0.1222	-0.1309	-0.0986	-0.0337
* treatment dummy	(0.0653)	(0.1118)	(0.0650)	(0.0607)	(0.0428)	(0.0306)	(0.0309)	(0.0928)
	**	**	**		***	***	***	
Affiliated firms	0.2565	0.1080	0.1798	-0.0295	-0.0512	-0.0636	-0.0681	-0.0216
* treatment dummy	(0.0916)	(0.1877)	(0.0887)	(0.0497)	(0.0159)	(0.0317)	(0.0272)	(0.0255)
	***		*		***	*	**	
Affiliated firms	0.0779	0.1072	0.0707	0.0380	-0.0080	0.0612	0.0540	0.0634
	(0.0265)	(0.0411)	(0.0474)	(0.1109)	(0.0273)	(0.0309)	(0.0442)	(0.0984)
	***	**				*		
Constant	-0.0710	-0.0814	-0.0888	-0.1138	-0.0865	-0.1283	-0.1113	-0.1247
	(0.0163)	(0.0481)	(0.0210)	(0.0532)	(0.0164)	(0.0181)	(0.0212)	(0.0870)
	***		***	**	***	***	***	
Relative time effects	linear	linear	linear	linear	linear	linear	linear	linear
Observations	62	62	76	46	58	58	72	46
R-squared	0.6650	0.5670	0.6919	0.5906	0.6990	0.5762	0.7091	0.5963

Panel C. Occupation-level analysis (Employment growth)[2]

	Fuzzy RDD, Employment growth				Sharp RDD, Employment growth			
	Baseline	Controls	Bandwidth	Falsification	Baseline	Controls	Bandwidth	Falsification
Nnon-affiliated firms	-0.1736	-0.1388	-0.1716	-0.0339	-0.0470	-0.0524	-0.0379	-0.0007
	(0.0802)	(0.0607)	(0.0594)	(0.0420)	(0.0295)	(0.0290)	(0.0214)	(0.0494)
	**	**	***			*	*	
Affiliated firms	0.1927	0.1232	0.0834	0.0096	-0.0934	-0.0966	-0.0904	-0.0595
	(0.1271)	(0.1010)	(0.0848)	(0.0431)	(0.0522)	(0.0583)	(0.0360)	(0.0489)
					*	*	**	
Constant	1.4994	-0.0597	-0.2883	-0.6165	1.4808	-0.0978	-0.3074	-0.6435
	(1.8100)	(0.0331)	(0.3566)	(0.0821)	(1.8108)	(0.0254)	(0.3565)	(0.0887)
		*		***		***		***
Relative time effects	linear	linear	linear	linear	linear	linear	linear	linear
Observations	3,320	3,320	4,257	2,451	3,048	3,048	3,985	2,451
R-squared	0.0779	0.0235	0.1534	0.1636	0.0830	0.0256	0.1666	0.1652

Regressions are weighted by the number of employees in 2010 and include controls for log average firm size and export intensity unless stated otherwise. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively. “Controls”: affiliation status only; “bandwidth”: October 2010-August 2011; “Falsification”: placebo reform date (mid-May 2011) using agreements in post-reform period only (March 2011 – August 2011).
[1.] Standard errors are robust and clustered by collective agreement and signature date.
[2.] Standard errors are robust and clustered by collective agreement, occupation and signature date.
Authors’ calculations based on QP and DGERT data.

C. The role of representativeness and retroactivity

We now consider the role of representativeness and retro-activity for the impact of extensions on the employment performance of non-affiliated and affiliated firms. The degree of representativeness of employer associations is measured by the share of the workforce in affiliated firms in the total employment of the relevant sector. This definition is the same as the representativeness criterion that were introduced as part of the 2012 labor market reform.

The results indicate that degree of representativeness does not appear to matter significantly for the impact of extensions for either affiliated or non-affiliated firms (Table 5a). In fact, almost all coefficients of the interactions between extension and representativeness are insignificant. Moreover, in additional robustness checks (available upon request), we find similar results when considering dummy variables defined at different representativeness thresholds (30% and 50%, for instance).

This may be surprising given the systematic differences in the impact of extensions between affiliated and non-affiliated firms. The absence of an apparent role for representativeness here may be due to a number of factors. A technical explanation could be that it is not the variation in representativeness *per se* that matters but whether a majority of employees is represented or not. While we have looked at this as well, identification is not straightforward due to the very limited number of agreements with representativeness levels above 50%. A substantive explanation could be that representativeness criteria based on a majority rule – as was introduced during the 2012 reform – are not sufficient for ensuring that the interests of non-affiliated firms are fully taken into account. This may be either because a 50% threshold is not high enough or because, in practice, bargaining is largely driven by market leaders in a sector with smaller affiliated firms in the same sector having little or no influence over the outcome of the negotiations. This suggests that representativeness criteria may need to be fine-tuned further or be complemented with a test of public interest as is the case in, for example, in the Netherlands.

All in all, the results do not allow drawing strong conclusions about the effectiveness of representativeness criteria in mitigating the adverse impact of extensions. However, even if representativeness criteria do not effectively ensure that the interests of non-affiliated firms are taken into account, representativeness criteria may still play a useful role in the longer-term by promoting the degree of organization among employers, particularly if implemented gradually over time. This may be valuable per se as it is may help to improve the quality of industrial relations as well as the degree of trust between social partners (see also Box 3.2 in IMF, 2016).

Our next topic concerns the requirement for non-affiliated firms to retro-actively pay wage increases over the period from the entry-into-force date of collective agreements among affiliated firms to the publication date of the extension to non-affiliated firms. The rationale of applying retro-activity to extensions is to ensure that a level playing field between signatory and non-signatory firms is preserved, consistent with the spirit of sector-level bargaining and the logic behind extensions (Hijzen et al., 2019). However, their potential bite is considerable, particularly for liquidity-constrained firms: in fact, the typical time for processing an extension in Portugal in the period from late 2010 to early 2011 was about 180 days (about six months).

Table 5a. Results on the role of representativeness by affiliation status

	Fuzzy		Sharp	
	(1)	(2)	(1)	(2)
	ΔE	ΔΔE	ΔE	ΔΔE
Treatment effect * non-affiliated firms	-0.1278	-0.1757	-0.0822	-0.0974
	(0.0424)	(0.0621)	(0.0354)	(0.0386)
	***	***	**	**
Treatment effect * affiliated firms	0.3415	0.3383	-0.0780	-0.0531
	(0.1412)	(0.1542)	(0.0427)	(0.0454)
	**	**	*	
Representativeness * non-affiliated firms	-0.2729	-0.1284	-0.3197	-0.0297
	(0.1469)	(0.1888)	(0.1690)	(0.1893)
	*		*	
Representativeness * affiliated firms	0.1770	0.0388	0.0805	-0.0691
	(0.0797)	(0.1047)	(0.0594)	(0.0769)
	**			
Treatment dummy * non-affiliated firms * representativeness	0.3024	0.1296	0.2443	-0.1178
	(0.1603)	(0.2129)	(0.1706)	(0.2044)
	*			
Treatment dummy * affiliated firms * representativeness	-0.5177	-0.2431	-0.1288	-0.0233
	(0.2553)	(0.2887)	(0.1087)	(0.0989)
	*			
Affiliated firms	-0.2098	-0.2005	0.0079	-0.0016
	(0.0866)	(0.0741)	(0.0428)	(0.0406)
	**	**		
Constant	-0.0025	-0.0467	-0.0265	-0.0740
	(0.0390)	(0.0341)	(0.0332)	(0.0304)
				**
Relative time effects	linear	linear	linear	linear
Observations	62	62	58	58
R-squared	0.5042	0.6747	0.5587	0.7248

Regressions are weighted by the number of employees in 2010. Standard errors are robust and clustered by collective agreement and signature date. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively. "Representativeness" is measured by the share of the workforce in affiliated firms in the total employment of the relevant sector.

We find that retro-activity plays a potentially important role in explaining the adverse impact of extensions on employment among non-affiliated firms (Table 5b). The degree of retro-activity is measured by the number of days between the entry-into-force date of the collective agreement among affiliated firms and the publication date of the extension to non-affiliated firms. The negative average treatment effect reflects the impact of extensions on the change in employment growth for the typical administrative delay (180 days). The interaction of the treatment effect with the administrative delay gives the impact of a one-day increase in the administrative delay on the change in the growth rate of employment following an extension. This is negative for non-affiliated firms, while it is insignificant or even positive for affiliated firms. The difference between affiliated and non-affiliated is consistent with our discussion above since retro-activity should hit non-affiliated firms directly, while it may indirectly benefit affiliated firms by reducing competition from non-affiliated firms.

One can obtain an indication of the impact of extensions in the absence of retro-activity by considering their effect when there is no administrative delay in processing extensions, i.e. when the administrative delay is zero. This is done by re-estimating our model while defining the administrative delay in absolute value rather than as the difference from 180. Doing so implies that the treatment dummy now captures the impact of extensions in the absence of any administrative delay, rather than its impact for an average administrative delay shown in Table 5b (results not reported). Under this specification, the overall impact of extensions on employment growth is reduced, and so is the extent to which the effects are concentrated among non-affiliated firms. In the case of the fuzzy RDD, the results indicate that retro-activity accounts for a substantial part of the adverse impact of extensions, with the negative effect on employment growth in non-affiliated firms falling by approximately 40%, but their effects remain sizeable and concentrated among non-affiliated firms. By contrast, the results based on the sharp RDD suggest that extensions have no effect on employment growth in either non-affiliated or affiliated firms in the absence of any administrative delay. All in all, the results suggest that retro-activity explains a significant part of the negative effect of extensions on employment growth among non-affiliated firms.[17]

[17] These results should be interpreted with some caution since the discussion relies heavily on the assumed linear relationship between the length of the administrative delay and the impact of extensions and employment growth.

Table 5b. Results on the role of retroactivity by affiliation status

	Fuzzy		Sharp	
	(1)	(2)	(1)	(2)
	ΔE	ΔΔE	ΔE	ΔΔE
Treatment effect * non-affiliated firms	-0.1026	-0.1688	-0.0459	-0.1100
	(0.0406)	(0.0510)	(0.0300)	(0.0344)
	**	***		***
Treatment effect * affiliated firms	0.1708	0.2452	-0.0919	-0.0498
	(0.0692)	(0.0817)	(0.0237)	(0.0200)
	**	***	***	**
Treatment effect * non-affiliated firms * administrative delay	-0.0003	-0.0006	-0.0003	-0.0005
	(0.0001)	(0.0002)	(0.0002)	(0.0003)
	**	***		*
Treatment effect * affiliated firms * administrative delay	-0.0001	0.0003	-0.0001	0.0001
	(0.0002)	(0.0003)	(0.0002)	(0.0002)
Affiliated firms	-0.0926	-0.1715	0.0763	-0.0111
	(0.0543)	(0.0493)	(0.0270)	(0.0282)
	*	***	***	
Constant	-0.0549	-0.0682	-0.0785	-0.0859
	(0.0191)	(0.0176)	(0.0161)	(0.0169)
	***	***	***	***
Relative time effects	linear	linear	linear	linear
Observations	62	62	58	58
R-squared	0.4425	0.7089	0.4730	0.7176

Regressions are weighted by the number of employees in 2010. Standard errors are robust and clustered by collective agreement. *, **, *** refer to statistical significance levels of 10%, 5% and 1% respectively. "Administrative delay" is defined in terms of the number of days since the entry-into-force date of the collective agreement among affiliated firms and the publication date of the extension to non-affiliated firms.

At least in part, these results are likely to reflect the specific context during which the reform took place. Since collective agreements are public documents and there was little uncertainty as to whether or not a collective agreement would eventually be extended, retro-activity should not pose any problem as long as firms act rationally and there are no financial frictions. However, in a context where economic conditions were deteriorating rapidly and many (non-affiliated) firms were liquidity-constrained, the requirement to retro-actively pay wage increases seems to have slowed the growth rate of employment considerably (i.e. it would have made employment growth even more negative in most cases). It is important to bear in mind that in periods of growth these effects would most likely be smaller. These findings are also consistent with the large job losses found in

Martins (2019), also considering a period of economic downturn (including the great recession).

VI. RESULTS ON WAGE INEQUALITY

Collective bargaining has been regarded as an important mechanism towards reducing wage inequality in a labor market (e.g. Blanchard et al, 2014). Moreover, this effect can be strengthened by extensions, as these can widen dramatically the coverage of binding minimum wages for different occupations and job levels in multiple industries in a given country. In this section, we examine empirically the effects of extensions on wages and wage inequality.

In order to take account of the fact that wages are set separately for different job categories within agreements, we focus on changes in residual wages and wage inequality within job categories, agreements, membership status and years. In an effort to control for the confounding role of composition effects when examining the impact of extensions on residual wages over time, we restrict the focus to workers who are employed in both 2010 and 2011. The analysis is conducted by examining the change between 2010 and 2011 in the 5^{th}, 10^{th}, 15^{th} and 20^{th} percentile of the residual wage distribution as well as the changes in each of these moments relative to the median. Apart from the new dependent variable, the econometric model is identical to that used for the analysis of employment growth in the previous section.

The results, presented in Table 6, show that extensions have a tendency to increase wages (only) in the bottom of the distribution and therefore also to reduce inequality. More specifically, extensions tend to increase wage growth by about 5 percentage points for workers in the fifth percentile of the residual wage distribution (column 1 of Panel B) and tend to reduce the growth rate of the P50/P5 percentile ratio by a similar amount. Moreover, the effects of extensions on wages and inequality become smaller when moving up the residual wage distribution. Its effects on the wages of workers in the 20^{th} percentile of the residual wage distribution are insignificant in all specifications.[18]

[18] We also find similar insignificant results for higher percentiles (available upon request). Martins (2019) when considering continuing workers also finds that wages increase following extensions.

All in all, these findings support a binding interpretation of the extension mechanism, in the sense that they in fact push upwards the wages of the bottom tail of the distribution, which is in the background of the entire paper. The findings also suggest the existence of a trade-off between the adverse effects on employment documented in the previous section and the beneficial effects on the wages of low-wage workers and inequality documented here.

Table 6. Results on inequality

Panel A. Fuzzy RDD

	(1) Δp5	(2) Δ(p50-p5)	(3) Δp10	(4) Δ(p50-p10)	(5) Δp15	(6) Δ(p50-p15)	(7) Δp20	(8) Δ(p50-p20)
Treatment effect	0.0805	-0.0865	0.0695	-0.0755	0.0710	-0.0770	0.0289	-0.0349
	(0.0373) **	(0.0481) *	(0.0400) *	(0.0532)	(0.0566)	(0.0721)	(0.0285)	(0.0453)
Constant	-0.0286	0.0501	-0.0082	0.0296	-0.0248	0.0463	-0.0109	0.0324
	(0.0221)	(0.0296)	(0.0205)	(0.0297)	(0.0327)	(0.0419)	(0.0165)	(0.0259)
Relative time effects	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	62	62	62	62	62	62	62	62
R-squared	0.0875	0.1217	0.2290	0.2353	0.1727	0.1858	0.0644	0.1265

Panel B. Sharp RDD

	(1) Δp5	(2) Δ(p50-p5)	(3) Δp10	(4) Δ(p50-p10)	(5) Δp15	(6) Δ(p50-p15)	(7) Δp20	(8) Δ(p50-p20)
Treatment effect	0.0493	-0.0476	0.0494	-0.0477	0.0519	-0.0502	0.0184	-0.0166
	(0.0232) **	(0.0294)	(0.0213) **	(0.0306)	(0.0296) *	(0.0392)	(0.0147)	(0.0242)
Constant	-0.0310	0.0522	-0.0087	0.0300	-0.0266	0.0478	-0.0109	0.0321
	(0.0213)	(0.0282) *	(0.0185)	(0.0275)	(0.0298)	(0.0389)	(0.0155)	(0.0247)
Relative time effects	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	58	58	58	58	58	58	58	58
R-squared	0.1019	0.1442	0.2675	0.2510	0.2170	0.2120	0.0756	0.1275

Results based on residuals from individual-level log base wage regression on job category dummies, collapsed by firm type (affiliated vs non-affiliated), collective agreement and year. Δp5 denotes the change in the 5th percentile (of the cell's log base wage residual) between 2011 and 2010, Δ(p50-p5) denotes the difference between the median and the 5th percentile in 2011, and similarly for the remaining dependent variables.

VII. CONCLUDING REMARKS

In many countries, collective bargaining coverage is supported by administrative extensions that widen the reach of collective agreements beyond their signatory parties to all firms and workers in their reference sectors. Because of their potential roles in stimulating wage growth and reinforcing downward wage rigidity, extensions have become the focus of an increasingly intense policy debate in recent years. However, given the lack of hard evidence on the effects of extensions, and of collective bargaining more generally, the debate has largely tended to be based on subjective priors rather than factual arguments. By exploiting a natural experiment on collective bargaining in Portugal, this paper seeks to contribute to the ongoing discussions by providing new insights on the causal impact of extensions.

More specifically, this paper analyzed the causal impact of administrative extensions on employment growth using a natural experiment that resulted from the immediate suspension of extensions by the government that took office in Portugal in June 2011. Our analysis employs a regression discontinuity design that exploits the administrative delay in issuing extensions in combination with their suspension in June 2011 and novel matched employer-employee-agreement panel data. Importantly, this suspension resulted in a sharp and unanticipated decline in the probability that an extension was issued, several months prior to the change in government.

The results in the paper provide important insights for the debate on the role of extensions in countries undergoing adjustment periods – or, more generally, across countries facing the labour market effects of the Covid19 pandemic –, but also on collective bargaining more generally:

> First, our evidence indicates that extensions played an adverse role for employment growth during the period covered and, thereby, are likely to have amplified the unemployment response to the global financial crisis until they were suspended. However, we caution that the estimated adverse impact on employment growth may not necessarily generalize to periods with different economic conditions—in particular in periods of growth, as opposed to the recession period covered here—or countries with different institutional settings (including with respect to retro-activity and representativeness of unions and employer associations).
>
> Second, the adverse effects of extensions on employment growth mainly concern firms that are not affiliated with an employer association, i.e. those that do not participate or are not represented in the bargaining of collective agreements. The concentration of adverse employment effects among non-affiliated firms suggests

that extensions suit the interests of affiliated firms better than those of unaffiliated firms. This may imply that the lack of representativeness of employer associations is a potentially important factor behind the adverse effect of extensions.

Third, however, the degree of representativeness of employer associations is not found to matter significantly for employment growth. This may reflect the low levels of, and limited variation in, representativeness in our data, or that representativeness criteria (such as those implemented in Portugal in 2012) are not sufficient to ensure effectively that agreements are in the public interest. However, even if the introduction of the strict representativeness criteria in 2012 did not have a direct impact on employment growth, they are likely to have had a major impact on wage adjustment by greatly reducing the number of extensions issued, and therefore, indirectly, contributed positively to employment growth. Over the longer term, they also may help to promote employer organization, particularly when representativeness criteria are introduced gradually, and contribute to the quality of industrial relations as well as trust between social partners

Fourth, the retro-activity with which extensions entered into force until the 2012 reform appears to be harmful for employment among non-affiliated firms. This has two important implications. It suggests that our results are to some extent specific to the weak economic conditions under which the "natural" experiment took place. If there were no uncertainty about the extension of agreements and firms were not liquidity-constrained, then retro-activity would not be expected to slow down employment growth. It also suggests that the 2012 reform may have helped to reduce the adverse effect of extensions by removing their retro-activity. Concerns that this undermines the spirit of sectoral bargaining and extensions can partly be addressed by shortening the administrative delay associated with issuing extensions.

Fifth, there appears to be a trade-off between the adverse effects of extensions on employment growth and their beneficial effects on low wages and in promoting lower wage inequality.

All in all, this paper considers many key features of sectoral bargaining, most of which for the very first time, using a novel type of matched data, and does so in a causal framework.

Hopefully further research for other countries and time periods will complement our findings and also contribute towards the establishment of a sounder international evidence base of the effects of collective bargaining.

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Appendix

Additional background information

Agreements and extensions timeline: The timeline of an agreement is as follows: 1. signed by the two parties (employers’ and workers’ representatives); 2. submitted to the Ministry of Labour; 3. published by the Ministry of Labour (after a brief analysis by Ministry officials); 4. comes into force (at the same time as publication in step 3, although it may produce effects at that time, earlier or later, depending on what it is stated in the agreement itself).

The timeline of an extension is as follows: 1. One or both subscribers request the extension of the agreement (once it is signed and submitted to the Ministry); 2. The Ministry conducts an analysis of the likely impact of the extension (based on the number of workers that would be subject to the extension, using the same QP data); 3. The Labour Minister publishes the extension, establishing its production of effects (typically backdated to the production of effects of the underpinning agreement).

The interruption that is exploited in this paper concerns agreements that were published and in force and which had an extension pending (timeline point 2, when the analysis is being conducted) but which were not extended.

Table A1. Collective agreements and extensions

Agreement sector	Date agreement	Date extension
Metallurgic industry	08-09-2010	22-12-2010
Agriculture, fishing and forestry	22-09-2010	10-01-2011
Car sale	08-10-2010	10-01-2011
Viana do Castelo retail	08-10-2010	29-12-2010
Clinical analysis labs	08-11-2010	28-02-2011
Wine trade sector	22-11-2010	28-02-2011
Football clubs (players)	15-12-2010	22-03-2011
Cork industry, North, Office workers	29-12-2010	26-04-2011
Wine industry, cellars	10-01-2011	26-04-2011
Textile industry	24-01-2011	
Hotels and restaurants, Centre and South	24-01-2011	23-05-2011
Aveiro retail	22-02-2011	23-05-2011
Ropes industry	28-02-2011	
Chemical and pharmaceutical retail	28-02-2011	30-05-2011
Wood	09-03-2011	
Pharmaceutical products retail	29-03-2011	
Merchandising firms	29-03-2011	
Viseu retail	08-04-2011	
Wheat	08-04-2011	
Coffee	08-04-2011	
Driving schools	08-04-2011	
Fish preserve industry	26-04-2011	
Bread manufacturing, Lisbon	26-04-2011	
Guarda, retail	29-04-2011	
Poultry, slaughter	09-05-2011	
Farming	09-05-2011	
Meat, retail, South	16-05-2011	
Retail storehouses	23-05-2011	
Bread manufacturing	23-05-2011	
Fishing	30-05-2011	
Non-alcoholic beverages	30-05-2011	
Cement	08-06-2011	
Shoe manufacturing	15-07-2011	
Farming, Abrantes	15-07-2011	
Farming, Beja	22-07-2011	
Construction sector	08-08-2011	
Private schools	16-08-2011	
Clothing	16-08-2011	
Textile industry	16-08-2011	