WAGE FLEXIBILITY UNDER SECTORAL BARGAINING

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Abstract
Sectoral contracts in many European countries set wage floors for different occupation groups. In addition, employers often pay a wage premium (or *wage cushion*) to individual workers. We use administrative data from Portugal, linked to collective bargaining agreements, to study the interactions between wage floors and wage cushions and quantify the impact of sectoral wage floors. Although wages exhibit a "spike" at the wage floor, a typical worker receives a 20% premium over the floor, with larger cushions for older- and better-educated workers and at higher-productivity firms. Cushions also allow wages to covary with firm-specific productivity, even within sectoral agreements. Contract negotiations tend to raise all wage floors proportionally, with increases that reflect average productivity growth among covered firms. As floors rise, however, cushions are compressed, leading to an average passthrough rate of about 50%. Finally, we use a series of counterfactual simulations to show that real wage reductions during the recent financial crisis arose through reductions in real wage floors, reductions in real cushions, and a re-allocation of workers to lower wage floors. Offsetting these effects was a rapid rise in education of new cohorts, which in the absence of other factors would have led to rising real wages. (JEL: J31, J41, J51)

Teaching Slides
A set of Teaching Slides to accompany this article are available online as Supplementary Data.

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1. Introduction

How does collective bargaining affect wages? Much of the existing research on this issue focuses on the United States (e.g. Freeman and Medoff 1984; Lewis 1986; Farber et al. 2021), where union contracts set wages for *jobs*. In this setting, an increase in negotiated wage rates translates directly into an increase in wages for workers who remain in the same job. Collective bargaining agreements (CBAs) in many European countries work differently: These agreements specify a set of *wage floors* for different occupation groups. Employers can (and often do) pay idiosyncratic wage premiums on top of the floors.[1] These premiums—which Cardoso and Portugal (2005) labeled "wage cushions"—partly undo the wage-standardizing features of US-style collective bargaining, contributing to within- and between-group pay inequality.[2] Premiums can also adjust when floors change (or are frozen), providing a degree of wage flexibility that is absent in the US setting.[3]

In this paper, we explore the relationship between collectively bargained wage floors and actual wages in Portugal, using individual wage records linked to CBAs from 2008 to 2016. The Portuguese system of sectoral bargaining is broadly similar to the systems in Spain, Italy, Belgium, the Netherlands, and France (see Schulten 2016). Moreover, the prevalence of pay rates in excess of negotiated wage floors parallels the situation in other countries. Thus, we believe there are general lessons to be drawn from a study of Portugal. The setting is also interesting because, as part of a 2011 debt relief package, a *Troika* of international agencies pushed for legislative changes that would reduce the coverage of sectoral bargains.[4] This effort largely failed. Nevertheless, as we will show, significant downward real wage adjustments occurred within the framework of the existing bargaining system.

The key to our analysis is the ability to link individual workers in the annual census of employees in Portugal—known as Quadros de Pessoal (QP)—to the CBAs and wage floors that apply to their jobs. This is made possible by two institutional facts. First, the QP identifies the CBA for each worker covered by a union contract, as

1. See Holden (1989, 1998) for the case of Norway; Calmfors and Nymoen (1990) for a broader discussion of the Nordic countries; Hübler and Jirjahn (2003) and Jung and Schnabel (2011) for the case of Germany; Ordine (1995) for Italy; Dolado et al. (1997) for Spain; Butter and Eppink (2003) for the Netherlands; and Cardoso and Portugal (2005) and Bastos, Monteiro, and Straume (2009) for earlier analyses of Portugal.

2. Freeman (1980) noted that the variance of wages is lower in the union sector than in the non-union sector and credited this in part to the elimination of idiosyncratic wage variation within jobs. Similarly, Ashenfelter (1972) noted that unions raise wages of Black workers relative to Whites and suggested that this arose in part because of standardization policies that reduce racial wage gaps within jobs. See Card, Lemieux, and Riddell (2004) for more discussion.

3. This fact is widely recognized in the literature on "wage drift"—Phelps Brown (1962) presents an early, informative analysis. See also Calmfors (1993) and Schlicht (1992).

4. See Blanchard, Jaumotte, and Loungani (2014) for a discussion of the IMF's recommendations, which appear to have been adopted in a memorandum of understanding (MoU) between the Government of Portugal and the European Commission (EC), the European Central Bank (ECB), and the International Monetary Fund (IMF).

well as a job title that in principle specifies their wage floor. Second, information on all newly negotiated CBAs, including tables of wage floors for different occupational groups, is published by the Ministry of Labor. Setting aside difficulties in matching, it is therefore possible to assign wage floors to covered workers observed in the QP in October of each year. While many previous studies have attempted to link *subsets* of workers to their associated wage floors (e.g. Cardoso and Portugal (2005) for Portugal; Card, Devicienti, and Maida (2014) for Italy; Deelen and Euwals (2014) for the Netherlands; and Diez-Catalan and Villanueva 2014 for Spain), we believe this is the most comprehensive panel data set assembled to date that combines information on collectively bargained wage floors and actual wages.[5]

Two initial questions, highlighted by the goals of the Troika, are: How did the share of workers covered by CBAs change between 2008 and 2016? And how do uncovered workers compare to covered workers? Consistent with other recent studies (e.g. Addison, Portugal, and Vilares 2017), we show that the fraction of full-time workers in QP covered by CBAs fell only slightly, from 90% in 2008 to 87% in 2016. We also show that uncovered workers in Portugal earn significantly *higher* wages than covered workers, contrary to the situation in countries such as the United States or the United Kingdom.

We then present a descriptive analysis of the role of wage floors in between- and within-group wage variation. We show that the log of an individual's total monthly wage can be decomposed into four components: (i) the minimum wage; (ii) the worker's *relative wage floor* (i.e. the floor relative to the minimum wage); (iii) the gap between the base wage and the wage floor (i.e. the wage cushion); and (iv) regular supplementary payments (including meal subsidies and shift premiums).[6] As was documented by Cardoso and Portugal (2005) using the QP data for 1999, we find that differences in relative wage floors and differences in mean wage cushions both contribute to inter-group wage differences. For example, about 30% of the wage gap between men and women is attributable to higher wage floors for men, and 60% to higher mean wage cushions for men. We also show that wage inequality *within* skill groups reflects variation in both wage floors and wage cushions, as well as the covariance between them.

Within a given sectoral agreement, firms have some latitude in assigning workers to different floor categories and even more latitude in determining wage cushions. Both factors contribute to the cross-sectional variation in wages *within CBAs*. Classifying firms into deciles of average value added per worker, we show that mean base wages at top decile firms are over 40 log points higher than mean base wages at bottom decile firms *in the same sectoral agreement*. Around 10% of this effect is attributable

5. Gautier et al. (2021) have a rich database that combines individual wages and collectively bargained wage floors for France, but cannot directly link workers to specific wage floors within the relevant agreement. Fougère et al. (2018) use similar data on wage floors to study the adjustment of floors to inflation and changes in the minimum wage.

6. Meal allowances are widespread in Portugal, in part because they are tax exempt up to a fairly generous level (currently up to 7.63 euros per day).

to the assignment of workers to higher wage floors at top decile firms, while 90% is attributable to higher wage cushions. Thus, wage cushions play a particularly large role in within-CBA wage flexibility.

Next, we study the renegotiation process for wage floors. We show that all the floors in a given CBA adjust by virtually the same percentage when the contract is renegotiated. We then relate this average floor adjustment factor to measures of productivity growth among firms covered by the contract. We focus on two closely related questions: (1) Are wage outcomes driven by *average* productivity growth of covered firms or by the high- or low-performers in the covered set? (2) How sensitive are negotiated wage floors to productivity growth of covered firms? We find that floor adjustments respond to the central tendency of growth in value added per worker among covered firms, rather than to upper- or lower-tail growth, with an elasticity of around 0.10—as big or bigger than the typical elasticities estimated in the micro rent sharing literature (see Card et al. 2018).

The net effect of wage floor adjustments depends on how wage cushions respond to these adjustments. If employers react to keep the same wage cushion as a floor is raised, then wage floor increases will pass through fully to actual wages. If cushions are compressed as floors rise, however, then the passthrough rate will be lower. To estimate passthrough rates, we calculate the change in base wages that would occur if each worker maintained the same gap between their wage and the wage floor as floors are changed. We then regress actual wage increases on these simulated increases, using both OLS and instrumental variables (IVs) approach that takes the average simulated increase in wages for all workers at the same firm as an instrument for the worker-specific effect of the floor increase.

We find that the average passthrough rate of floor increases is around 50%, with a higher passthrough rate for workers with smaller wage cushions. This pattern is similar to the spillover effect of a minimum wage increase (e.g. Cengiz et al. 2019; Fortin, Lemieux, and Lloyd 2021), though we find that the impact of wage floor increases extends further up the distribution. We also test for but find no evidence of asymmetry of responses to real wage floor changes arising from new contract negotiations versus those attributable to inflation.

We also examine the effect of wage floor adjustments on employment. Specifically, using the same IV we use to study the passthrough of floors to individual wages, we relate firm-wide employment changes to the simulated increase in base wages of its employees caused by changes in wage floors. Our estimates suggest that employment is largely unaffected by higher wage floors, though we cannot reject small negative impacts.

In the final section of the paper, we conduct a simulation analysis to understand how changes in wage floors and wage cushions, as well as movements of workers between jobs with different wage floor categories, contributed to the adjustment of real wages between 2010 and 2016, as Portugal suffered through a prolonged recession. We begin by computing mean real wages for workers in different gender–education–age groups in 2010. We then increment all wage floors to incorporate renegotiations between 2010 and 2016, but keep each worker in the same floor category and hold constant their wage cushions and supplementary payments. The comparison between this counterfactual and the 2010 baseline summarizes the net effect of wage floor adjustments, and shows a 2.5 ppt *reduction* in average real wages attributable to the erosion in real floors over the 6 years. Next, we reweight skill groups in 2010 to their 2016 shares to measure the effects of demographic change. Driven by a rapid rise in shares of better-educated workers, this yields a 7.4 ppt *increase* in mean real wages in the economy as a whole that would have occurred if wage floors, wage cushions, and the assignment of workers to floors had remained constant.

We then consider a counterfactual based on workers observed in 2016, using their actual wage floors as of 2016 but simulating the wage cushion each worker would have earned in 2010 (by drawing from the distribution of cushions in 2010). Relative to the previous simulation, this counterfactual reveals the net effect of the re-allocation of workers across wage floor groups that occurred between 2010 and 2016, and yields a 4.8 ppt *reduction* in mean real wages for workers as a whole. Finally, we give each worker their actual wage cushion in 2016 (rather than a simulated 2010 wage cushion). This final step shows that changes in wage cushions within wage floor categories led to a further 2.5 ppt *reduction* in mean real wages.

Despite concerns that sectoral bargaining limits the responsiveness of real wages to negative shocks, our simulations suggest that real wages fell substantially during the debt crisis. The declines were particularly large for university-educated workers, whose mean real wages fell by 16 ppt between 2010 and 2016, reflecting a combination of declining real wage floors (−4.4 ppt), declining real cushions (−6.2 ppt), and a re-allocation of jobs toward lower wage floor categories (−8.4 ppt). Real wage cuts for lower-paid workers were smaller but still significant: young high school-educated females and males, for example, experienced declines of 4.8 ppt and 5.6 ppt, respectively.

Our findings contribute to three separate strands of research. First, we contribute to a macro-oriented literature that compares different collective bargaining systems (e.g. Calmfors and Driffill 1988; Calmfors 1993; Nickell and Layard 1999). This literature often assumes that sectoral agreements set wages for covered firms, ignoring employer-determined wage cushions—a simplification that overstates the rigidity of Portuguese wage setting.

Second, we contribute to the micro-oriented literature linking union-wage setting to wage inequality (Freeman 1980; Card 1992; DiNardo, Fortin, and Lemieux 1996; Farber et al. 2021). Building on Cardoso and Portugal (2005), we show that in a European setting, idiosyncratic wage premiums are important determinants of within-group and between-group inequality. The size and distribution of these premiums help explain why, despite high CBA coverage rates, Portugal also has relatively high wage inequality.

Finally, we contribute to the "micro Phillips curve" literature (e.g. Riddell 1979; Card 1990; Christofides and Oswald 1992) that examines the determinants of negotiated wage outcomes using union contract data. Our data allow us to examine the full set of wage floors within a contract, rather than just the "base wage" for lower skilled workers that is usually analyzed in this literature. We also study how multi-employer agreements are impacted by the distribution of firm-specific productivity growth among covered firms. Finally, we show how collectively bargained wage floors affect individual wage outcomes as well as within- and between-group wage inequality.

2. Setting and Conceptual Framework

Sectoral Bargaining in Portugal and Reforms During the Debt Crisis. In the system established in Portugal in the 1970s and still in place today, employer associations representing firms in a particular industry (and in some cases, region) sign CBAs with one or more trade unions.[7] Although these agreements technically cover only union members, in practice, employers extend the agreements to their entire workforce, regardless of membership status.[8] Under the laws and practices that were largely in place in 2010, the bargaining parties would often file a request with the Directorate-General for Employment and Labor Relations to extend the agreement to other firms in the same sector—a request that was normally granted (see Naumann 2018). Contract provisions could also be voluntarily adopted by employers in the industry.

Each CBA contains a variety of clauses prescribing work rules and practices, as well as a set of wage floors that prevail during the term of the contract. Figure 1 presents an example of the table of wage floors from a typical agreement—in this case, a 2016 agreement between the Association of Hotel and Restaurant Employers and the Union of Service Workers. This wage table distinguishes between two subgroups of employers (groups A and B) and twelve different wage floors, ranging from 440 to 960 euros per month.[9]

Collectively bargained wage clauses almost always have a nominal duration of 1 year. In case a new agreement has not been negotiated, however, the old agreement remains in force, and in the early years of our sample (2008–2009), a typical new agreement was updating a contract that was negotiated about 2 years earlier (see Section 3.1). Prior to 2003, the Labor Code required that any new agreement be at least as favorable to workers as the old agreement and also prevented firms from withdrawing from a CBA. These rules were relaxed by amendments in 2003 and 2009

7. There are two main union confederations in Portugal—the União Geral de Trabalhadores (UGT) and the more radical Confederação Geral dos Trabalhadores Portugueses (CGTP). Often, an employer association will sign separate *but identical* agreements with different unions—typically, one affiliated with UGT and another affiliated with CGTP. In our analysis, we consolidate such duplicate agreements and treat such parallel agreements as a single one.

8. We verified this directly by looking at the distribution of the fraction of employees within each firm classified as covered by a CBA in the QP. This distribution is effectively comprised of a mass at 100% and a smaller mass at 0%.

9. These are monthly salaries for full-time workers, net of payroll taxes. By law, workers receive 14 monthly salaries. As of 2016 (for which the floors apply), the minimum wage was 530 euros, so group III has a floor at the national minimum wage. The two bottom groups are apprentices, who face a minimum of 80% of the regular minimum wage.

Níveis	Grupo A	Grupo B
XII	960,0 €	930,0 €
XI	895,0 €	887,0 €
X	770,0 €	735,0 €
IX	700,0 €	670,0 €
VIII	630,0 €	610,0 €
VII	585,0 €	575,0 €
VI	540,0 €	540,0 €
V	532,0 €	532,0 €
IV	531,0 €	531,0 €
III	530,0 €	530,0 €
II	450,0 €	450,0 €
I	440,0 €	440,0 €

FIGURE 1. Example of wage table from BTE. "Contrato coletivo entre a Associação da Hotelaria, Restauração e Similares de Portugal (AHRESP) e o Sindicato dos Trabalhadores e Técnicos de Serviços—SITESE—Alteração salarial e outras".

that allowed new agreements to loosen work rules and lower wage floors. The 2003 and 2009 amendments also created a process for CBAs to expire, though procedures governing the granting of extensions were unchanged.[10]

At the peak of the financial crises in 2011, the Portuguese government signed a MoU with the EC, the ECB, and the IMF—the so-called *Troika*—committing to a wide range of policy reforms, including revisions of the contract extension framework that were intended to reduce the coverage of sectoral agreements and encourage firm-level bargaining.[11] Ultimately, these reforms ran into legal challenges, as well as opposition from employer associations, many of which supported the existing extension framework (see Naumann 2018). After Portugal exited the financial aid program in 2014, the new center-left government adopted a series of revisions that more or less restored the pre-crisis bargaining framework.

Wage Setting Under Sectoral Bargaining. The wage floors in the Portuguese CBAs set a lower bound on basic pay for workers in each occupational category. As in other

10. The number of CBAs that was determined to have expired under these rules is low: A total of 15 expiration notices were published in 2009; and over the period from 2010 to 2016, and another 17 expiration notices were published (Portugal, CRL, 2020: 58; Portugal, MTSS, 2016: 374). The number of agreements that actually expired was somewhat smaller because of subsequent court decisions.

11. A key goal for the Troika was to reduce the coverage of sectoral agreements and encourage firm-specific agreements negotiated by works councils (see European Commission 2011, p. 54). The Troika agreement ignored the fact that the Portuguese Constitution gives trade unions the exclusive right to bargain for workers. Two other practical problems were that there were less than 200 work councils in the entire country (Portugal, MTSS 2006) and that the vast majority of Portuguese firms are very small and have limited capacity for bargaining on their own.

European countries, however, firms can and do offer many workers a wage that is higher than the minimum for their category. This differs from the typical situation in the United States, where a union contract specifies a grid of wages for different jobs, and *all workers in the same job receive the same pay*—a wage standardizing property that is arguably a defining feature of unionized wage setting in a US-style system (Ashenfelter 1972; Freeman 1980). In addition, most workers in Portugal receive regular "supplementary" payments, including tax-free meal subsidies, which are the same from month to month and may be impacted by collective negotiations.

To clarify the role of these various components, let W_{it} represent the net monthly base wage for worker i in year t, and let $F_{i,t}$ represent the wage floor that applies to that worker. Let $H_{it} = W_{it} - F_{it}$ represent the absolute gap between the base wage and the wage floor, and let S_{it} represent the regular monthly supplemental payments received by worker i in year t.[12] Then, we can decompose the base monthly wage and the corresponding total monthly wage (W_{it}^T):

$$W_{it} = F_{it} + H_{it}$$
$$W_{it}^T = F_{it} + H_{it} + S_{it}.$$

For most of our analysis below, we work with logarithms of wages rather than levels. Letting $w_{it} \equiv \ln W_{it}$ represent the log of the monthly base wage, and $w_{it}^T \equiv \ln W_{it}^T$ represent the log of the monthly total wage, we can write

$$w_{it}^T = f_{it} + h_{it} + s_{it}, \tag{1}$$

where $f_{it} \equiv \ln F_{it}$ is the log of the wage floor for worker i in year t,

$$h_{it} \equiv \ln \frac{W_{it}}{F_{it}}$$

is the proportional wage premium received by the worker over his or her wage floor (which we refer to as the worker's "wage cushion"), and

$$s_{it} \equiv \ln \frac{W_{it} + S_{it}}{W_{it}}$$

represents his or her regular supplementary payments, expressed as a share of the base wage.

In the presence of a national minimum wage, it is helpful to decompose the log wage floor into the sum of the log of the minimum wage ($m_t \equiv \ln M_t$) and the gap between the floor and the minimum wage

$$f_{it} = m_t + rf_{it},$$

12. In Portugal (as elsewhere in Continental Europe), wages are normally expressed as monthly full-time rates, net of any employee payroll taxes. Wage floors in CBAs and the national minimum wage are similarly expressed. Moreover, workers receive 14 monthly salaries per year.

where rf_{it} is the wage floor relative to the minimum wage:

$$rf_{it} = \ln \frac{F_{it}}{M_t}.$$

Substituting into equation (1), we get a simple four component model of log wages

$$w_{it}^T = m_t + rf_{it} + h_{it} + s_{it} \qquad (2)$$

that expresses the log total wage for individual *i* in year *t* as the *sum* of the minimum wage, the relative wage floor for the worker's job, her wage cushion, and her regular supplementary payments. This additive structure is very convenient for decomposing the variance of the log of total wages (see Section 4), for addressing the causal question of how actual wages respond to adjustments in wage floors (see Section 6), and for considering counterfactual scenarios, such as one in which floors are raised and all wage cushions remain constant, so each worker maintains a fixed (proportional) pay premium over his or her floor (see Section 8).

3. Assigning Wage Floors to Workers

In this section, we describe our database of workers with assigned wage floors. We begin with an overview of our database of CBAs. We then discuss the QP and our procedure for assigning wage floors to workers in QP.

3.1. Data on Collective Agreements—BTE

All newly negotiated CBAs in Portugal are published in the Labor Bulletin (*Boletim do Trabalho e Emprego*, BTE) and are available in an online archive (http://bte.gep.msess.gov.pt). We began our data assembly process by extracting information for agreements published between 2008 and 2016 that included a salary clause or wage table. For each agreement we extracted:

- the names of the union(s), employer association(s), and other information that formally identifies the contract;
- the type of agreement (sectoral agreement, company agreement, multi-company agreement, government directive)[13]; and
- the starting date; expiration date; and reference information on the preceding agreement.

We also collected information on the categories and wage floors in the wage tables. The system for designating floor categories varies widely across contracts, but in most

13. Multi-company agreements (*acordo coletivo)* are legally distinct from sectoral agreements (*contrato coletivo*) and are particularly common in the finance and utility sectors. Government directives are mandated agreements imposed in the absence of any other collective agreement (*portaria de condições de trabalho)* or in the case of an unresolved dispute *(decisão arbitral*).

cases, we are able to devise a list of job titles/occupations included in each category and construct a longitudinal database of wage floors for each CBA and floor category.

There are a number of issues that have to be addressed in constructing an accurate panel of wage floors. One is that we only observe wage floors when a contract is actually updated. Thus, the first observation for each CBA/floor category occurs at the time of the first contract renegotiation after 1 January 2008. A second issue is that floor increases are sometimes back-dated. Since our interest is in the effect of wage floors on the current (flow) cost of labor, we measure the *prevailing* wage floor as of October of each year (the reference date of the QP survey), ignoring any back payments awarded by subsequent agreements. A third issue is that increases in the national minimum wage can over-ride wage floors for lower-paid workers, particularly, if the contract has not been renegotiated recently. In accordance with the labor law, we update all wage floors to meet the minimum wage as of the reference date of the QP. A fourth complication is that some agreements (such as the one underlying the wage table in Figure 1) specify separate wage floors for subgroups of firms (e.g. based on revenues) or workers (e.g. based on tenure). We keep track of the subgroup classification system and attempt to assign the correct floor to a worker, though that is not always possible.

A final issue is that an employer or employer association will often sign separate *but identical* agreements with different unions—typically, one affiliated with UGT and another affiliated with CGTP. We consolidate such duplicate agreements, reducing the total number of agreements over the 2008–2016 period from 1,467 to 1,061 (see Online Appendix A and Table A.1). We also drop agreements covering firms in agriculture or fisheries, or those in Madeira or the Azores. We are left with 988 new consolidated agreements that form our basic CBA data set. Around 50% of these are sector-wide contracts, just over 10% are multi-company contracts, and the remaining 38% are CBAs covering a single firm (see Online Appendix Tables A.2 and A.3).

Column (1) of Table 1 shows the number of unduplicated new agreements in our basic CBA data set by year of renegotiation, while column (2) shows the share of those agreements that were sectoral CBAs.[14] Close to 200 (consolidated) agreements were reached in 2008. The number then began to fall off, reflecting the tendency for renegotiations to slow down in the face of worsening economic conditions. In 2012 and 2013, the number was particularly low, driven by the severe recession and uncertainty over collective bargaining institutions in the aftermath of the MoU with the Troika. Following the nascent recovery and legislative changes in 2014 that re-established the framework for contract extensions, the number of new agreements rose to around 90 per year in 2014–2016.

Although nearly all CBAs in Portugal (97% in our sample) have a nominal 1-year duration, an existing CBA remains in force until a new one is negotiated (or in very rare cases, when an employer exits the agreement).

14. We emphasize that the numbers of agreements shown represent counts after consolidating duplicated agreements. The numbers of agreements prior to this adjustment are shown in Online Appendix Table A.3. A typical sectoral agreement covers firms in multiple regions: weighting by employment, 86% of sectoral agreements include workers in all five NUTS2 regions of Portugal.

TABLE 1. Characteristics of new contracts in BTE, workers in QP, and merged BTE-QP sample.

Year	Renegotiated contracts reported in BTE (non-duplicates, in scope)			Full time workers in QP (private sector age 18-64)			Matched BTE-QP sample		
	Number contracts in BTE (1)	Percent sectoral (2)	Mean number months since last contract (3)	Number of workers in QP (4)	Percent covered by CBA (5)	Percent covered by sectoral CBA (6)	Number of workers with assigned floors in QP (7)	Percent of all workers (7)/(4) (8)	Number of floors (9)
2008	192	58.9	20	1,966,522	90.0	75.0	634,300	32.3	1,935
2009	165	53.4	18	1,893,484	89.9	74.2	804,653	42.5	2,211
2010	140	61.4	20	1,897,345	91.4	75.2	835,011	44.0	2,357
2011	111	52.3	20	1,868,715	90.9	74.7	817,703	43.8	2,461
2012	50	38.0	25	1,768,599	89.1	71.6	832,861	47.1	2,566
2013	54	31.5	22	1,748,831	88.6	70.8	815,606	46.6	2,585
2014	83	34.9	26	1,778,271	88.4	70.6	825,698	46.4	2,619
2015	90	47.8	37	1,831,708	88.0	70.3	844,830	46.1	2,603
2016	103	46.6	29	1,884,758	87.0	69.6	855,602	45.4	2,641
All	988	50.7	23	16,638,233	89.3	72.5	7,266,264	43.7	21,978

Notes: CBA denotes a collective bargaining agreement. Columns (1)–(3) are based on non-duplicated counts of contracts for workers in mainland Portugal excluding agriculture and fisheries. Workers in QP include full-time private sector employees age 18–64. We also exclude apprentices, employees in agriculture and fisheries, employees of accounting firms, and individuals with missing data on monthly wages, education, or date of hire. Workers assigned a wage floor exclude those whose wage floor depends on firm or worker characteristics that we cannot trace, workers covered by contracts that were not renegotiated over the analysis period, and other groups (see text).

As shown in column (3) of Table 1, in 2008, the mean elapsed time since the publication date of the previous agreement was 20 months—implying a delay of about 8 months between the expiration of the old contract and the publication date of the new one. By 2015 the time since the last agreement had risen to 37 months, implying a delay of over 2 years between the expiration date and the renegotiation date. The increase in delay time was particularly pronounced for sectoral contracts, driven by the near-collapse in renegotiation of these agreements in 2012 and 2013. As a consequence of these long delays, by 2014 many workers were covered by floors that were 2–3 years old, a situation that was only partly remedied by the upswing in negotiations in 2015 and 2016.

3.2. Quadros de Pessoal

The QP is an annual census of employers conducted by the Ministry of Employment. Firms with at least one wage earner are required to submit their full roster of employees as of the reference week in October, as well as a variety of other information (including annual sales). They are also required to post their employee roster (with names, job titles, and monthly pay) inside their premises, reducing the likelihood of misreporting or under-reporting. The Ministry distributes an electronic version of the data set that has longitudinal identifiers for each firm and each worker. We use QP data for the period from 2008 to 2016.

The data for each worker include gender, age, education, occupation, date of hire, nationality, monthly earnings (split into several components), hours of work (normal and overtime), as well as the name of the CBA that the worker is covered by (if any). Unfortunately, the QP does *not* report the actual wage floor for the worker or the name of the floor category as used in the BTE. Instead, it reports the *job title* or *professional category* of the worker, which in many cases can be matched to the list of job titles or occupations reported for the floor categories in BTE.

In addition to the information collected by the QP itself, we also have access to matched income statement/balance sheet information for most employers, linked to the QP by the National Statistical Office.[15] Relevantly for our purpose, these business statistics report the yearly gross value added at the firm level. We do not construct this variable. It is directly supplied by the firm. It is defined as the production value minus the purchase of goods and services, after all production taxes have been paid and all production subsidies have been received (GVA at "factor cost" or "basic prices"), that is, what remains to be distributed among the production factors.

Starting from the universe of observations in QP, we exclude workers under the age of 18 or over 64, those in Madeira and the Azores, and those employed in agriculture and fisheries (see Online Appendix A). We also exclude apprentices (3.5% of the relevant sample), workers who are not employed full time (15.1%), and those with missing

15. The Integrated Business Accounts System—IBAS (Sistema de Contas Integradas das Empresas—SCIE) covers the non-financial business sector. The linked QP data are distributed in an anonymized format.

information on wages (8.9%, including unpaid family members and firm owners) or education/date of hire (0.1%). Columns (4)–(6) of Table 1 report the resulting number of workers in our QP sample each year, the fraction that is reported as covered by a CBA, and the fraction covered by sectoral agreements. On average, we have about 1.85 million workers per year, with a dip during the most severe recessionary years and a partial recovery by 2016. The collective bargaining coverage rate starts at 90% in 2008, remains relatively steady until 2011, then declines slightly each year thereafter, ending at 87%. On average, 81% of covered workers are covered by a sectoral agreement, a fraction that fell slightly over our sample period, from 83% to 80%.

In Online Appendix B, we use a simple dynamic model to decompose the year-to-year changes in collective bargaining coverage in our QP sample, focusing on worker-level transitions between three states: employed and covered by a CBA, employed and not covered by a CBA, and not employed (i.e. not included in QP in a given year). There are no major changes over time in the probability that people retain their coverage status or in transition rates into or out of jobs covered by a CBA (see Online Appendix Table B.1). There was a slowdown in the probability that people entered the workforce in 2011–2014, and a slight reduction in the fraction of new entrants starting a job covered by a CBA. Together, these factors account for most of the (relatively modest) losses in coverage after 2011.

3.3. Assigning Wage Floors to Workers in QP

We used a two-step process to assign wage floors to workers in QP. We first matched contracts in QP to those in our BTE database. We then attempted to match the wage floor groups within a contract in BTE to the job category codes reported in QP.

The matching of contracts was done by hand since the CBA names in the two data sources can differ and the QP often uses outdated names. Broadly, the steps included: inspection of the text of each agreement to identify likely matches; construction of consistent longitudinal information on the renegotiation dates of each agreement and on the reported numbers of covered workers and firms to confirm matches; inspection of longitudinal information on workers in QP to identify likely CBA name changes; searches on the web pages of trade unions or employer associations; and telephone or email contacts with trade unions. With these steps, we were able to match nearly all contracts mentioned in QP to an agreement in BTE.

Matching of the wage floors in BTE to the worker categories in QP was also done by hand, and was more difficult. We began by inspecting the text of each agreement in BTE to find a list of all jobs/job titles in each floor group.[16] Next, we matched the BTE *floor groups* in a given CBA to the QP worker *categories* for the same CBA, again by direct inspection of the possible $m \rightarrow n$ matches for each CBA. In agreements setting different wage floors for workers, depending on their date of hire, tenure, or skill,

16. We often had to track past agreements to find the full list of job titles in each floor group because in some cases—such as that shown in Figure 1—only a group code is reported.

we attempted to use information in QP to assign workers to the correct wage floors. Likewise, whenever the applicable wage floor depended on firm attributes reported in QP, such as the firm's industry or employment, we matched the worker category in QP to its wage floor accordingly. Online Appendix A provides further details on the process of matching.

Despite our best efforts, we were only able to match slightly over half the workers in QP covered by a CBA to their wage floor (see Online Appendix Table A.2). The main obstacles were: (1) lack of information on the variables needed to assign workers to specific floors within a CBA; (2) too many sub-floors for each occupational category; and (3) lack of obvious matches between the occupations or job types specified in BTE and the job titles used in QP. Columns (7)–(9) of Table 1 present some information on the subset of workers in QP that were successfully assigned a floor. The fraction of matched workers rises from 32% in 2008 to 44% in 2010 and is more or less stable thereafter. These shares refer to the full QP data set reported in Column (4). If, instead, we consider only those workers actually covered by collective bargaining with a job category reported, then the shares with assigned floors rise to 39% in 2008, 52% in 2010, and 53% over the whole period under analysis. The lower rate at the start of our sample is due to the fact that many workers in QP in October 2008 or 2009 were covered by floors that were last renegotiated in 2006 or 2007, prior to the start of our BTE database. By the time of the 2010 QP, most workers in QP were covered by an agreement that was updated between January 2008 and September 2010. We note that in a typical year after 2010, our matched database includes about 2,500 separate wage floors.[17]

3.4. *Comparisons of Workers by Coverage and Floor Assignment Status*

Before proceeding with an analysis based on the subset of workers with matched floors, we examine two questions: How do covered workers with a matched floor compare to those for whom we were unable to assign a floor? And how do workers who are uncovered by CBAs compare to covered workers?

Table 2 presents some simple data that address these questions: We show characteristics and wage outcomes for all workers, for those who are covered and uncovered by a CBA, and for covered workers with and without a matched wage floor. Focusing first on the data in columns (4) and (5), we conclude that covered workers who can be assigned a wage floor are broadly similar to those who cannot. In particular, their gender, education, experience, job tenure, and mean log wages are quite similar. Importantly, this similarity is also true year-by-year (see Online Appendix Table C.1), suggesting that we can draw broader conclusions from an analysis of data for workers with assigned wage floors.

17. Martins (2021) claims that there are 30,000 minimum wage floors in Portugal. His analysis counts all job categories within the CBAs identified in QP, without taking into account the duplication of CBAs or the fact that, on average, a wage floor group in BTE actually incorporates roughly four job categories in QP. Together, these corrections imply that there are only about 5,000 separate wage floors at any point in time, roughly half of which we are able to match to a wage floor published in BTE.

TABLE 2. Comparisons of workers by CBA coverage, and of covered workers by assigned floor status.

		By CBA coverage		By floor assignment (if covered)	
	All (1)	Covered (2)	Not covered (3)	Floor assigned (4)	No floor (5)
Fraction female	0.452	0.448	0.476	0.420	0.476
Fraction with high school	0.242	0.240	0.256	0.233	0.248
Fraction with university	0.191	0.169	0.376	0.156	0.181
Mean years experience	23.85	24.32	19.98	24.50	24.14
Mean tenure current job	8.34	8.59	6.31	8.69	8.48
Mean log monthly base wage	6.696	6.675	6.858	6.664	6.686
(standard deviation)	(0.509)	(0.495)	(0.590)	(0.491)	(0.499)
Mean log monthly total wage	6.856	6.837	7.014	6.837	6.838
(standard deviation)	(0.532)	(0.522)	(0.586)	(0.517)	(0.528)
Number person-years	16,638,233	14,852,805	1,785,428	7,266,264	7,586,541

Source: Quadros de Pessoal. Sample includes wage earners age 18–64 with non-missing base wage, education, and date of hire, working under full-time contract. Apprentices, workers in Madeira and the Azores, and those in agriculture and fishing are excluded.

On the other hand, comparisons between columns (2) and (3) show that workers with and without CBA coverage are substantially different. Uncovered workers are much more likely to have a university-level education (38% vs. about 17% for covered workers), have somewhat fewer years of experience and job tenure than covered workers, and have about 20% higher wages. This wage advantage appears to be driven by worker skills: When we fit a model of wages that includes the coverage status of the current job and worker fixed effects it falls to essentially 0. Not surprisingly, wages are also more variable among uncovered workers, with $\sigma(w_{it}^T)$ about 12% higher than for covered workers.

An examination of coverage patterns within firms reveals that nearly all firms either have no covered workers or have 100% union coverage. Firms with no coverage tend to be larger than covered firms (mean employment is 8.4 workers vs. 6.1 for covered firms) and have substantially higher annual sales per worker (∼74,800 vs. ∼39,900 for covered firms). They are also more likely to be located in Lisbon and in the non-financial services sector (42% vs. 20% for uncovered firms).

The positive wage advantage for *uncovered* workers in Portugal stands in sharp contrast to the patterns in the United States, the United Kingdom, and Canada, where union coverage is positively correlated with wages. For example, data presented by Card, Lemieux, and Riddell (2004) show that the difference in mean log hourly wages between workers who are covered by collective agreements and those who are not is between 15% and 30% in all three countries (and between 5% and 25% controlling for gender, education, and experience).

4. Proximate Analysis of the Components of Wages

We now turn to a descriptive analysis of the role of the wage floors, wage cushions, and supplementary payments in determining wage differentials between groups and overall wage inequality. For this analysis (and all the analysis in the remainder of the paper), we focus on the sample of person-year observations in QP with assigned wage floors, described in columns (7)–(9) of Table 1.

Figure 2 shows the distributions of relative wage floors and wage cushions by gender, pooling across all years of our sample. Panel (a) shows that many wage floors (especially for female workers) are within 5 percentage points of the minimum wage, though there is a long upper tail of floors. Therefore, throughout our analysis, we will explore the connections between wage floors and the national minimum wage, specifically when analyzing the determinants of negotiated wage floors, the effect of wage floors on actual wages, and decomposing the changes in real wages in Sections 5.2, 6, and 8, respectively. A similar pattern holds for the distribution of wage cushions in panel (b). Approximately 27% of males and 39% of females earn a base wage that is within 5 percentage points of their respective floor.[18]

Figure 3–6 show how the mean values of the three individual-specific components of wages highlighted in equation (2) vary over time for different groups of workers. Figure 3 presents mean relative floors, cushions, and supplements by gender; Figure 4 presents similar data by education level; and Figure 5 presents mean floors and cushions for workers employed at firms in different quartiles of the distribution of value added per worker. Finally, Figure 6 shows the mean values of wage floors, cushions, and supplements by age for female and male workers (pooling across all years in our sample).

In interpreting these figures, it is helpful to keep two points in mind. First, our samples in 2008 and 2009 are slightly less representative than in later years due to the lack of data on wage floors that were renegotiated 1 or 2 years ago prior to 2010. Second, the real value of the minimum wage rose relatively sharply between 2008 and 2010 (with a +6% adjustment in 2009 and a +4% adjustment in 2010). Thereafter, the real minimum drifted downward for 3 years before raises of +4% in 2014 and +4% in 2016. Consequently, between 2010 and 2016, the real minimum was relatively stable, ending up only 3 log points higher in 2016 than in 2010. Given these two factors, we focus most of our attention on changes from 2010 to 2016 throughout this paper.

Examination of Figures 3–5 shows that across most subgroups, mean relative wage floors fell during our sample period. The declines were partly driven by increases in the minimum wage, particularly in 2014 and 2016 (see Figure 3). The declines were

18. Note that, legally, all wage floors are required to comply with the national minimum wage, apart from those for apprentices (and handicapped workers), who are excluded from our sample. Actual base wages should also comply with the respective wage floors, though in our data set there can be negative wage cushions, arising if the base wage is misreported to QP, or in certain cases like an extended sick leave. In our analysis, we keep the small fraction of workers with negative wage cushions, except as noted in Section 6.

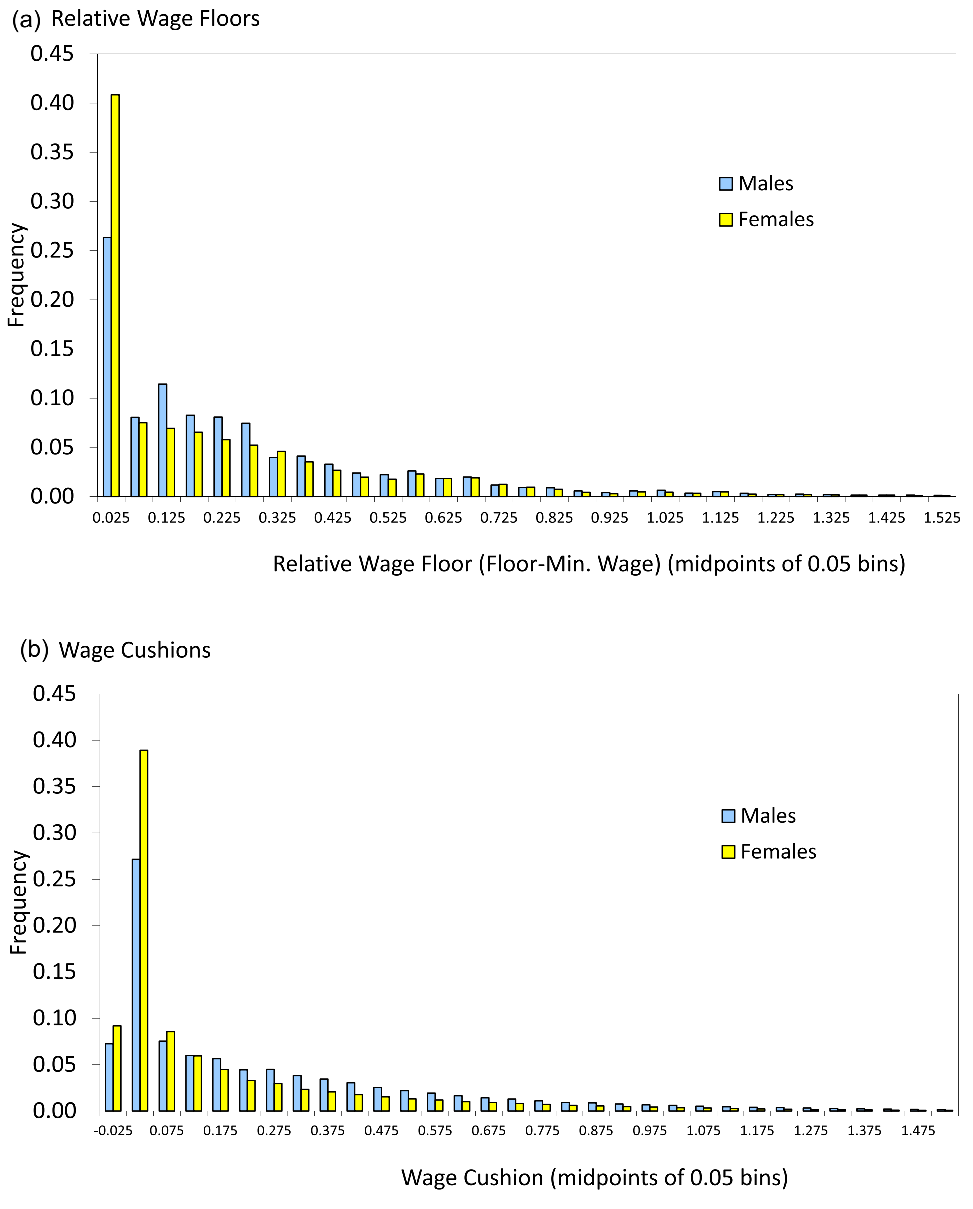

FIGURE 2. Distributions of relative wage floors and wage cushions by gender.

much larger for better-educated groups, but even for those with less than a high school education, the mean gap between their wage floor and the minimum wage fell from about 15 log points in 2010 to 10 log points in 2016. As we show in Section 8, a lot of the decline in relative wage floors for highly educated workers arose through a re-allocation of workers to jobs with lower wage floors. Such re-allocations were less

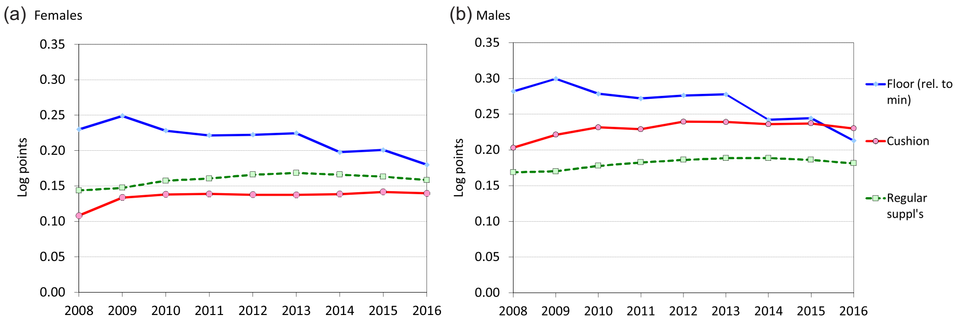

FIGURE 3. Components of mean wages (relative to minimum wage) for females and males.

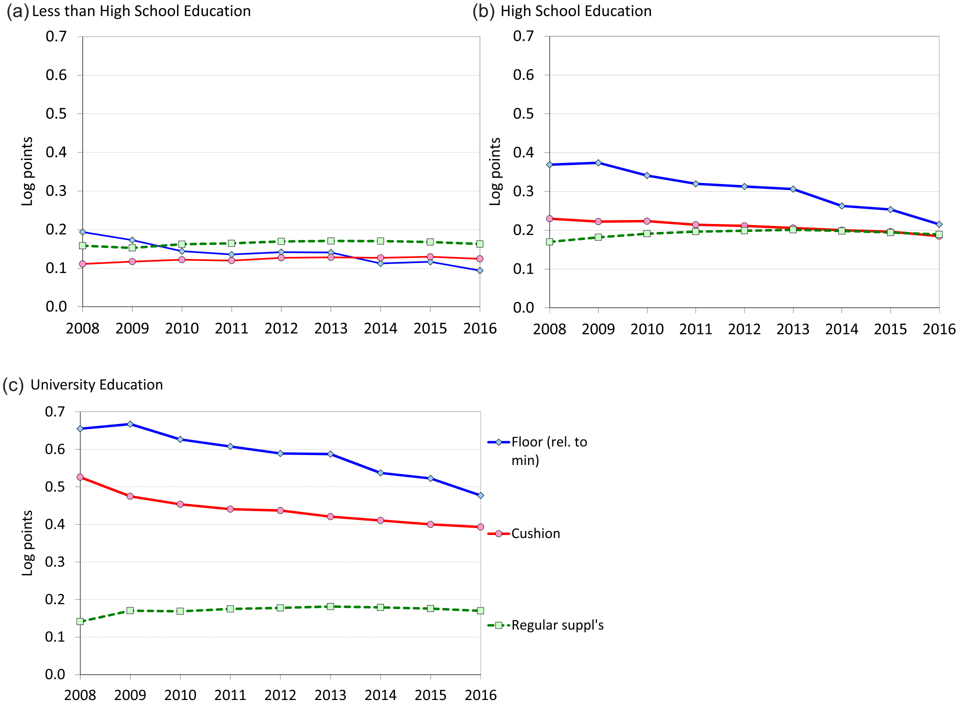

FIGURE 4. Components of mean wages (relative to minimum wage) by education group.

important for lower-educated groups, in part because their floors were clustered closer to the minimum wage even in 2010.

In contrast to the erosion in relative floors, the mean values of cushions and supplements were more stable, though there was a clear decline in mean cushions for workers with a university-level education. Figure 5 also shows that mean floors and cushions declined for workers at firms in the top quartile of value added per worker, leading to some narrowing of between-firm wage differentials—the opposite of the

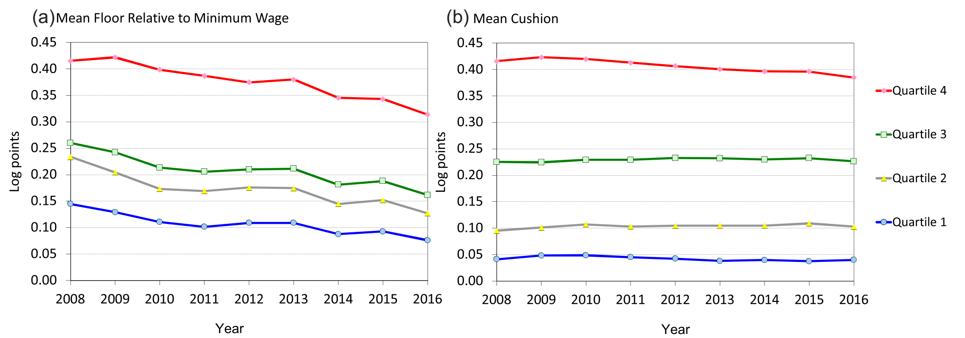

FIGURE 5. Mean floor (relative to minimum wage) and mean cushion by quartile of firm value added/worker.

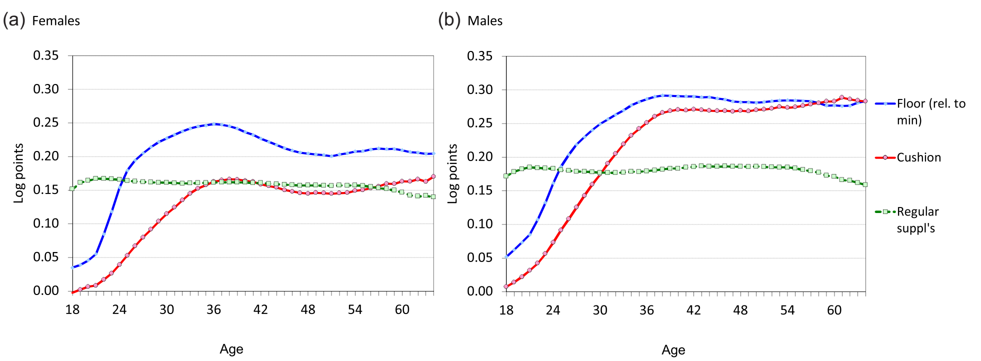

FIGURE 6. Components of age profile of mean wages (relative to minimum wage) for females and males.

pattern documented for Germany (Card et al. 2013) and the United States (Song et al. 2019).

The age profiles in Figure 6 reveal that young workers tend to be employed in jobs with very low wage floors and to receive small wage cushions. By age 25 or so, however, mean floors are in the 15% range and mean wage cushions are 5%–8%, and by age 40, a typical female has a wage floor of around 25 log points and a cushion of 15 points, while a typical male has a wage floor of nearly 30 points and a cushion of over 25 points. Thus, both floors and cushions contribute to the life cycle profile of wages.

Table 3 presents a more systematic summary of the net contributions of relative wage floors, cushions, and supplements to the levels and variances of wages for all workers and by gender, education, and firm value-added quartile. The first 5 columns decompose the means of log salary, while columns (6)–(10) pertain to variances. In the first row, for example, we show the mean log real monthly wage for all workers, the mean wage differential relative to the minimum wage (61 log points), and the mean contribution of relative wage floors (24 log points), wage cushions (19 log points), and

TABLE 3. Proximate contributions of wage floors, cushions, and supplements to level and variance of wages.

	Decomposition of means:					Decomposition of variances:				
	Mean log total wage (1)	Mean relative to min. (2)	Relative wage floor (3)	Mean wage cushion (4)	Mean supplements (5)	Variance log total wage (6)	Variance relative wage floor (7)	Variance Cushion (8)	Variance Supplements (9)	2 × Cov [relative floor & cushion] (10)
All workers	6.84	0.61	0.24	0.19	0.17	0.267	0.086	0.112	0.024	0.042
(Percent of total)			(40.2)	(31.4)	(28.4)		(32.3)	(42.1)	(9.1)	(15.6)
By gender:										
Males	6.91	0.68	0.26	0.23	0.18	0.286	0.091	0.129	0.030	0.039
(58.0% of observations)			(39.1)	(34.0)	(26.8)		(31.9)	(45.2)	(10.3)	(13.5)
Females	6.74	0.51	0.22	0.14	0.16	0.226	0.078	0.084	0.017	0.040
(42.0% of observations)			(42.3)	(26.5)	(31.2)		(34.7)	(37.3)	(7.6)	(17.8)
Gender gap	0.16	0.16	0.05	0.09	0.02	0.060	0.013	0.045	0.012	−0.010
(Percent of gap)			(29.7)	(57.9)	(13.3)		(21.8)	(74.7)	(20.6)	(−16.6)
By education:										
<High school	6.65	0.43	0.14	0.12	0.16	0.123	0.031	0.061	0.021	0.010
(61.1% of observations)			(32.7)	(28.8)	(38.5)		(25.2)	(49.4)	(16.7)	(8.2)
High school	6.93	0.70	0.30	0.21	0.19	0.256	0.091	0.113	0.027	0.023
(23.3% of observations)			(42.7)	(29.7)	(27.6)		(35.4)	(43.9)	(10.5)	(8.8)
University	7.41	1.18	0.57	0.43	0.17	0.372	0.141	0.239	0.035	−0.023
(15.6% of observations)			(48.7)	(36.6)	(14.7)		(37.8)	(64.3)	(9.4)	(−6.3)
HS/<HS gap	0.28	0.27	0.16	0.08	0.03	0.134	0.060	0.052	0.006	0.012
(Percent of gap)			(58.3)	(31.2)	(10.5)		(44.8)	(38.9)	(4.8)	(9.3)
University/HS gap	0.48	0.48	0.28	0.22	−0.02	0.115	0.050	0.126	0.008	−0.046
(Percent of gap)			(57.4)	(46.3)	(−4.0)		(43.1)	(109.6)	(7.1)	(−39.7)

TABLE 3. Continued.

	Decomposition of means:					Decomposition of variances:				
	Mean log total wage (1)	Mean relative to min. (2)	Relative wage floor (3)	Mean wage cushion (4)	Mean sup-plements (5)	Variance log total wage (6)	Variance relative wage floor (7)	Variance Cushion (8)	Variance Supple-ments (9)	$2 \times$ Cov [relative floor & cushion] (10)
By quartile of firm VA/worker:										
First quartile	6.51	0.28	0.11	0.04	0.13	0.076	0.027	0.044	0.011	−0.002
(25.0% of observations)			(38.4)	(15.3)	(46.3)		(35.1)	(57.5)	(13.9)	(−2.3)
Fourth quartile	7.19	0.96	0.37	0.41	0.18	0.321	0.114	0.183	0.034	0.043
(25.0% of observations)			(38.7)	(42.2)	(19.1)		(35.6)	(57.2)	(10.5)	(13.4)
Fourth-first quartile	0.69	0.68	0.27	0.36	0.05	0.245	0.088	0.140	0.023	0.045
(Percent of gap)			(38.9)	(53.1)	(8.0)		(35.7)	(57.1)	(9.4)	(18.3)

Notes: See note to Table 2. Information on value added per worker is only available for non-financial firms. See text.

supplements (17 log points). As shown by the numbers in parentheses just below the row entries, these three terms contribute 40.2%, 31.4%, and 28.4%, respectively, to the mean log gap between monthly salaries and the national minimum wage.

For the decomposition of variances, we show var[rf_{it}], var[h_{it}], var[s_{it}], and 2 cov[rf_{it}, h_{it}] (two times the covariance of relative floors and wage cushions), which is nearly all cases the largest of the covariance terms arising from a decomposition of var[w_{it}] based on equation (2). For the workforce as a whole, relative floors contribute 32.3% of the overall variance in log total salaries, cushions contribute 42.1%, supplements contribute 9.1%, and the positive covariance of floors and cushions across workers contributes 15.6%. Together, these four terms account for 99.2% of the total variance.

The next set of rows in Table 3 show similar statistics for males, females, and for the gender gaps in mean log wages and the variance of wages. Males have higher and more variable wages than females, differences that are attributable to both higher and more variable floors (30% of the gender gap in mean wages and 22% of the gap in variance of wages) and higher and more variable cushions (60% of the gender gap in mean wages and 75% of the gap in variance of wages). Similar conclusions apply to the wages of more- versus less-educated workers. For example, 58% of the 27 log point gap in mean wages between high school graduates and those with less than a high school education is attributable to higher floors, while 31% is attributable to higher cushions. In their analysis of base wages, floors and cushions in the 1999 QP, Cardoso and Portugal (2005) likewise found that cushions contributed about 40% of the overall return to each additional year of education.

As shown in the bottom three rows of the table, differences in mean wage cushions also play a significant role in explaining the mean wage gap between firms in the top and bottom quartile of value added per worker: just over one half of the 68 log point gap in mean wages is explained by higher average cushions at more productive firms. Again, this is consistent with findings by Cardoso and Portugal (2005) on the effects of firm-specific productivity on the floor and cushion components of wages. These patterns suggest that the wage cushions at a firm are correlated with the firm's ability to pay, consistent with models of firm-specific wage setting (e.g. Card et al. 2018).

To investigate this more fully, we conducted a simple analysis of wage floors, wage cushions, and log base wages, controlling for the specific CBA covering each worker. Specifically, we fit models of the form

$$y_{it} = \alpha_0 + \alpha_x X_{it} + \sum_{d=2}^{10} \alpha_d I_{d(i,t)} + \psi_{CBA(i,t)} + u_{it}, \tag{3}$$

where y_{it} represents either the wage floor, wage cushion, or log base wage of worker i in year t, X_{it} is a set of worker characteristics (gender, education, and age), $d(i,t)$ is an index function that maps the worker to the value-added deciles of his/her employer in year t, I_d is a dummy for the dth decile of value added, $CBA(i,t)$ is another index function that maps the worker to the specific CBA, he or she is covered by in year t, and ψ_C represents a set of CBA fixed effects. The results are presented in Table 4. For

TABLE 4. Estimated models for wage floor, cushion, and base wage within CBAs.

	Log floor		Wage cushion		Log base wage		Cushion share*	
CBA fixed effects	No (1)	Yes (2)	No (3)	Yes (4)	No (5)	Yes (6)	No (7)	Yes (8)
Female	−0.054 (0.003)	−0.053 (0.001)	−0.086 (0.003)	−0.094 (0.002)	−0.140 (0.003)	−0.147 (0.003)	0.615	0.641
Education (years)	0.031 (0.001)	0.024 (0.000)	0.027 (0.001)	0.030 (0.001)	0.058 (0.001)	0.054 (0.001)	0.457	0.560
Age	0.014 (0.000)	0.013 (0.000)	0.024 (0.000)	0.023 (0.000)	0.037 (0.000)	0.036 (0.000)	0.627	0.635
Age ^{2}/100	−0.010 (0.001)	−0.010 (0.001)	−0.019 (0.001)	−0.018 (0.001)	−0.029 (0.001)	−0.028 (0.001)	0.650	0.631
Indicators for decile of mean log value added per worker at firm:								
Decile 2	0.001 (0.004)	−0.004 (0.002)	0.016 (0.003)	0.014 (0.003)	0.017 (0.005)	0.010 (0.004)	0.934	1.434
Decile 3	0.031 (0.008)	−0.001 (0.002)	0.021 (0.006)	0.044 (0.003)	0.052 (0.005)	0.043 (0.004)	0.399	1.016
Decile 4	0.047 (0.012)	0.000 (0.004)	0.034 (0.012)	0.073 (0.005)	0.081 (0.011)	0.074 (0.008)	0.416	0.994

TABLE 4. Continued.

	Log floor		Wage cushion		Log base wage		Cushion share*	
CBA fixed effects	No (1)	Yes (2)	No (3)	Yes (4)	No (5)	Yes (6)	No (7)	Yes (8)
Decile 5	0.030	0.009	0.077	0.097	0.107	0.106	0.718	0.913
	(0.008)	(0.003)	(0.006)	(0.004)	(0.005)	(0.005)		
Decile 6	0.037	0.006	0.115	0.137	0.152	0.143	0.754	0.955
	(0.007)	(0.003)	(0.005)	(0.005)	(0.007)	(0.005)		
Decile 7	0.042	0.016	0.155	0.174	0.197	0.191	0.787	0.915
	(0.004)	(0.002)	(0.005)	(0.005)	(0.006)	(0.006)		
Decile 8	0.079	0.028	0.188	0.223	0.268	0.251	0.704	0.887
	(0.007)	(0.003)	(0.009)	(0.008)	(0.009)	(0.009)		
Decile 9	0.114	0.039	0.246	0.290	0.360	0.329	0.684	0.882
	(0.009)	(0.007)	(0.010)	(0.010)	(0.012)	(0.014)		
Decile 10	0.234	0.052	0.336	0.414	0.570	0.466	0.589	0.888
	(0.023)	(0.004)	(0.016)	(0.011)	(0.019)	(0.012)		
R-squared	0.387	0.656	0.294	0.353	0.499	0.536	–	–

Notes: Models are estimated on 6,518,290 person-year observations for workers covered by CBAs at firms with non-missing value-added data. All models also include year effects. Standard errors, clustered by firm, in parentheses.

*Share of the effect of covariate in row heading on base wage that is attributable to effect on wage cushion.

reference, we show models for the three outcomes with and without CBA effects. A comparison of the estimated α_x and α_d coefficients between these models allows us to assess how much of the overall variation in each outcome across workers and firms is preserved within collective agreements.

The wage floor models in columns (1) and (2) of Table 4 summarize the *assignment process* between workers and floors in the labor market as a whole (column 1) and within CBAs (column 2). The coefficients of the worker-specific characteristics suggest that women are assigned to lower floors, while better-educated and older workers are assigned to higher floors: These matching effects are only somewhat attenuated within CBAs. The value added decile effects show that wage floors are higher at firms with higher value added per worker. Unlike the pattern for worker characteristics, however, the cross-firm gradient in floors is substantially flatter within CBAs than in the market as a whole. As emphasized by Boeri et al. (2021), this could be a cause for concern if less profitable firms are covered by collective agreements with relatively high floors.

The wage cushion models in columns (3) and (4) suggest that mean cushions vary across gender, education, and age groups more or less the same within CBAs as they do in the labor market as a whole. In contrast, mean cushions are *more responsive* to firm profitability within CBAs, partly undoing the relatively flattening of differences in wage floors within agreements.

As a consequence, looking at the models for log base wages in columns (5) and (6), we see that about 85% of the market-level variation in mean base wages across value-added deciles is preserved *within CBAs*.[19] In other words, sectoral agreements appear to only modestly dampen the sensitivity of wages to firm profitability.

The importance of cushions as a source of flexibility is summarized in columns (7) and (8). Since the base wage is just the sum of the wage floor and the wage cushion, we can calculate the share of the α coefficients reported in columns (5) and (6) that is attributable to the variation in cushions. This is around 60% for gender, education, and age, regardless of whether we condition on CBA effects or not. It is closer to 70% for the coefficients associated with firm profitability deciles when we do not condition on CBA effects, but rises to around 90% when we look at wages within CBAs. Descriptively, then, wage cushions play a relatively large role in maintaining wage flexibility in the presence of sectorally bargained wage floors.

5. Determinants of Negotiated Wage Floors

In this section, we turn to an analysis of the determinants of negotiated wage floor increases. Our first goal is to try to understand how the wage floors within a given CBA move relative to each other. To foreshadow our results, we find that in nearly all cases the floors are adjusted proportionally, so there is a single number—representing

19. This estimate comes from regressing the estimated decile effects in column (6) on those in column (5).

the mean increment in wage floors—that fully summarizes the negotiation results. Our second goal is to study how the rate of adjustment of wage floors responds to demand conditions at the firms covered by the CBA. We note that all our models include year effects, so we are not able to address the question of how collectively bargained wage floors adjust to changes in the minimum wage (as in Fougère et al. 2018).

5.1. Simple Models of Wage Floor Adjustment

As a starting point, consider a series of increasingly rich models for the change in the real wage floor of group g when CBA c is renegotiated in year t:

$$\Delta f_{cgt} = \delta_t + \varepsilon_{cgt}, \tag{4a}$$

$$= \delta_t + Z_{ct}\delta_Z + \varepsilon_{cgt}, \tag{4b}$$

$$= \delta_{ct} + \varepsilon_{cgt}, \tag{4c}$$

$$= \delta_{ct} + R_{cgt}\delta_R + \varepsilon_{cgt}. \tag{4d}$$

(Note that Δf_{cgt} involves a change over different numbers of years, depending on when contract c was last negotiated.) Model (4a) includes only year effects: The fit of such a model allows us to assess how far are CBA renegotiations in Portugal from the "fully centralized" benchmark that is often taken as a normative ideal by macroeconomists (e.g. Calmfors and Driffill 1988). Model (4b) adds some contract-specific characteristics Z_{ct}—most importantly, the duration of time since the last negotiation, which can range from 1 to 3 years, or even longer in a few cases. Model (4c) includes a set of contract-specific fixed effects, which fully absorb *any* CBA×year factors, like industry-wide demand shocks or changes in local labor market conditions that affect workers in the contract. The fit of this model allows us to assess the extent to which all floors within a given CBA move together. Finally, model (4d) adds a set of characteristics R_{cgt} of the workers covered by wage floor c, g, and asks whether there is any evidence that floors within the same CBA are adjusted to reflect the characteristics of the covered workers in different floor groups, controlling for the mean contract-level change in floors (captured by δ_{ct}).

Table 5 presents adjusted R^2 statistics for variants of these four sets of models, estimated using the changes in mean real wage floors for workers in our matched QP-BTE database.[20] We estimate the models by weighted OLS, using as weights the number of workers in floor group g of CBA c.

The fit of the fully centralized model (4a) (row 1) is surprisingly good, with an adjusted R^2 statistic of close to 80%, suggesting that most of the variation in wage floor adjustments is explained by just 7 year effects. Adding controls for industry, worker

20. Note that we exclude wage floors that are set at exactly the minimum wage, since adjustments for such floors are presumably insensitive to firm, industry, or worker characteristics.

TABLE 5. Alternative models for renegotiated wage floors.

	Adjusted R-squared
Explanatory variables (degrees of freedom)	
1. Year effects (7)	0.787
2. Year effects (7) and modal industry effects (15)	0.834
3. Year effects (7), modal industry effects (15), and worker characteristics (3)	0.838
4. Year effects (7), modal industry effects (15), worker characteristics (3), and dummies for elapsed time since the last renegotiation (6)	0.853
5. Year × modal industry effects (71), worker characteristics (3), and dummies for elapsed time since last renegotiation (6)	0.898
6. CBA effects (454)	0.981
7. Contract effects (454) and worker characteristics (3)	0.982

Notes: Dependent variable is change in real wage floor for a specific floor group within a CBA. Estimates are weighted by the number of workers covered by the floor. Sample includes 5,358 renegotiated floors that were set strictly above the minimum wage at the current and most recent previous renegotiation.

characteristics, and time since last negotiation (row 4) raises the adjusted R^2 to 85%; adding industry×year effects (row 5) raises it to nearly 90%.

The specification in row 6 adds contract-year effects (i.e. model (4c)). These increase the adjusted R^2 to 98%, leaving almost no unexplained within-contract variation in wage floors. Adding controls for the mean fraction of females, mean age, and mean education of workers covered by each floor (model (4d)) increases the fit only very slightly. These variables have very small but statistically significant effects, showing slightly faster growth in floors that cover a higher share of women, older workers, and less-educated workers.

Overall, we reach three conclusions from this simple analysis. First, nearly 80% of the variation in average negotiated wage floors across our sample is explained by year effects. Second, about one half of the remaining variation is explained by industry-specific shocks, workforce demographics, and the lag since the last negotiation. Third, nearly all the remaining variation is explained by CBA-specific effects, meaning that in a typical negotiation, all the floors are adjusted by the same percentage.

5.2. *Modeling Contract-Wide Mean Changes in Floors*

Building on the findings in Table 5, we turn to an analysis of the determinants of the mean wage floor adjustment, δ_{ct} in a given sectoral contract negotiation (estimated from a model like equation (4c)). Our main focus is on the question of how wage floor increases are affected by changes in productivity/profitability of firms covered by the CBA. In particular, we are interested in whether firms with faster productivity growth exert a stronger influence on negotiations than less profitable firms (Fanfani 2020; Boeri et al. 2021), potentially threatening the survival of the latter firms. We proceed by examining the effects of changes in the mean and various quantiles of the distribution of value added per worker among the firms affected by the contract.

Let $m(VA_{ct})$ represent the mean of log value added per worker in year t for firms covered by sectoral CBA c, and let $q(VA_{ct})$ represent the employment-weighted qth quantile (e.g. the 25th or 50th) of the distribution of log value added among these firms. Assume that contract c is renegotiated in year t and was last renegotiated in $t - \ell$ (so $\ell = 1, 2, 3$ is the years since the last renegotiation). Then the changes in productivity relevant for the renegotiation can be summarized by

$$DmVA_{ct} = m(VA_{jct-1}) - m(VA_{jct-\ell-1})$$

$$DqVA_{ct} = q(VA_{jct-1}) - q(VA_{jct-\ell-1}).$$

Note that we lag the financial information by a year, reflecting the fact that a contract that is updated in year t will have been negotiated before financial information from the current year is realized.[21]

Our first set of models for floor adjustments in contract c, presented in Table 6, take the form

$$\delta_{ct} = \beta_0 + \beta_1 DxVA_{ct} + \beta_2 Z_{ct} + e_{ct}, \quad (5)$$

where $DxVA_{ct}$ is the change in the mean ($x = m$) or some quantile ($x = q$) of the distribution of value added per worker among relevant firms, and Z_{ct} is a set of contract-specific covariates, including time effects, dummies for the number of years since the last renegotiation, a measure of cumulative inflation since the last negotiation, and measures of the share of females, the share of university graduates, and the mean age of workers covered by the contract. Column (1) shows a model using $DmeanVA_{ct}$ as the measure of demand-side factors, while columns (2)–(6) replace this with $DqVA_{ct}$ based on the 10th, 25th, 50th, 75th, and 90th quantiles.

Judging by the adjusted R-squared statistic, the change in the *mean* of value added among firms covered by a CBA is the best predictor of negotiated wage floor changes, though the median is a close second. The magnitude of the estimated β_1 coefficient suggests that wage floors are relatively responsive to the central tendency in industry-wide productivity growth, with an elasticity of wages to mean or median changes in value added of around 0.06–0.07. There also appears to be some limited "catch up" for past inflation: the model in column (1), for example, implies that real wages recover about one fifth of their lost value arising from inflation since the last negotiation.[22] Finally, consistent with the evidence in Figures 3 and 4, floors covering a higher share

21. Note that we use the change in the qth quantile of VA_{jct}, rather than the qth quantile of the change in VA_{jct}, to summarize the distribution of demand shocks among firms covered by a given contract. Under the rank invariance assumption that is widely used in the quantile treatment effects literature (e.g. Firpo 2007), these are the same.

22. Since all the models in Table 6 include year effects and dummies for the number of years since the last negotiation, the identification of the lagged inflation effect relies on differences in inflation over different time windows in our sample period, similar to Card (1990).

TABLE 6. Models for change in average wage floor in renegotiated CBAs.

	Measure of distribution of real value added per worker used:					
	Mean (1)	10th percentile (2)	25th percentile (3)	50th percentile (4)	75th percentile (5)	90th percentile (6)
Change in real value added/worker	0.068	0.007	0.037	0.067	0.034	0.032
	(0.014)	(0.005)	(0.011)	(0.015)	(0.011)	(0.007)
Other controls:						
Cumulative inflation since last renegotiation	0.202	0.032	0.034	0.142	0.115	0.040
	(0.086)	(0.089)	(0.074)	(0.068)	(0.071)	(0.071)
Share of females covered by CBA	0.015	0.017	0.015	0.014	0.016	0.015
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Share of university graduates covered by CBA	−0.002	−0.008	−0.008	−0.007	−0.006	−0.005
	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
Mean age of workers covered by CBA	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year effects and dummies for time since the last renegotiation	yes	yes	yes	yes	yes	yes
R-squared	0.946	0.931	0.933	0.943	0.934	0.934

Notes: Dependent variable is change in real average wage floor in a renegotiated CBA. Estimates are weighted by the number of workers covered by the CBA. Standard errors (in parentheses) are clustered by CBA. Models in different columns use different summary statistics—as indicated in the column heading—for the distribution of changes in real value added per worker among firms covered by the CBA.

TABLE 7. Models for 2010–2016 change in average wage floor—renegotiated CBAs.

	Measure of distribution of real value added per worker used:					
	Mean (1)	10th percentile (2)	25th percentile (3)	50th percentile (4)	75th percentile (5)	90th percentile (6)
No other control variables:						
Change in real value added/worker (2009–2015)	0.134 (0.025)	0.105 (0.027)	0.115 (0.037)	0.131 (0.025)	0.078 (0.023)	0.055 (0.026)
R-squared	0.475	0.383	0.421	0.510	0.284	0.178
With controls for industry:						
Change in real value added/worker (2009–2015)	0.093 (0.040)	0.068 (0.029)	0.074 (0.042)	0.094 (0.036)	0.040 (0.027)	0.033 (0.025)
R-squared	0.570	0.551	0.557	0.574	0.472	0.475

Notes: Dependent variable is change in real average wage floor from 2010 to 2016 in CBAs that were renegotiated at least once. Estimates are weighted by the number of workers in the agreement. Robust standard errors in parentheses. Models in different columns use different summary statistics—as indicated in the column heading—for the distribution of changes in real value added per worker among firms covered by the CBA over the 2009–2015 interval.

of female workers tended to rise more quickly in the 2010–2016 period, while floors covering a higher share of university-educated workers tended to fall slightly.

A potential concern with the models in Table 6 is that changes in firm-specific value added contain transitory fluctuations (and/or measurement errors) that are not completely eliminated by using the industry-wide means or medians. Such fluctuations/errors may play an outsize role in driving measured changes in the upper and lower quantiles. To address this issue, we conducted a complementary analysis of longer-run changes in contractual wage floors. Specifically, for all CBAs that were renegotiated at least once between 2010 and 2016, we constructed an average change in wage floors from 2010 to 2016, then fit a series of models relating this longer-run change to corresponding changes in the mean and quantiles of value added per worker for firms covered by the CBA. The results are presented in Table 7. We present specifications with no other controls in the upper panel and models that control for the modal industry of the covered firms (with a total of seven dummies) in the lower panel.

As expected, these models show a somewhat higher elasticity of wage floors with respect to productivity changes among covered firms, with a point estimate of 0.134 for the effect of the change in mean log value added when major industry dummies are excluded from the model and 0.093 when they are included. As in Table 6, the best fitting models are those that relate changes in wage floors to changes in the central tendency of productivity change among covered firms. Moreover, if we estimate models that include *both* the median (or mean) change in value added and one of the other quantiles (see Online Appendix Table D.1), then we find that all the explanatory power comes from the median or mean change.

The magnitudes of the estimated elasticity of wage floors with respect to mean or median changes in value-added in Table 7 are comparable or larger than typical estimates in the rent sharing literature (see Card et al. 2018). We note, however, that if changes in wage floors lead to some compression of wage cushions (as we find to be the case in the next section), then the impact of value added changes on average *wages* will be smaller than the impact on wage floors. In fact, we find that only about one half of a rise in wage floors is passed on to wages—the other half is absorbed by reductions in wage cushions. Assuming a 50% passthrough, the implied elasticity of workers' wages with respect to rises in productivity among firms covered by the relevant CBA is between 0.046 and 0.067—closer to the middle of the range of estimates in the rent-sharing literature.

6. Effect of Wage Floors on Wages

In this section, we turn to an analysis of the effect of changes in wage floors on the actual wages of workers. Conceptually, our approach builds on standard techniques for studying the passthrough of an increase in sales taxes to the final price paid by consumers. Specifically, we relate actual wage changes for workers at a firm to the simulated changes that would occur if floors were adjusted but all other components of wages remained fixed.

Consider the set of employees at a given firm j in year $t-1$. Let Δf_{it} represent the percentage change in the real wage floor between $t-1$ and t for worker i in this set. If the CBA covering the worker was renegotiated in the past year, then Δf_{it} is just the negotiated floor adjustment in that contract (adjusted for inflation). If the CBA was not renegotiated, then Δf_{it} is minus the percentage change in the price deflator between $t-1$ and t. Using the notation introduced in Section 3, let W_{it-1} represent the level of the monthly base wage of the worker in year $t-1$, and let F_{it-1} represent the level of her wage floor. We define

$$\begin{aligned}\Delta w^*_{it} &\equiv \ln(W_{it-1} + F_{it-1}\Delta f_{it}) - \ln(W_{it-1})\\ &\approx (F_{it-1}/W_{it-1})\Delta f_{it}, \end{aligned} \tag{6}$$

which is just the simulated increase in the log base wage of worker i if her wage floor were increased by the proportion Δf_{it}, and there was no change in the gap between her base wage and her wage floor.

The actual change in the worker's base wage includes the change in H_{it}, the gap (in euros) between her base wage and her floor

$$\begin{aligned}\Delta w_{it} &= \ln(W_{it-1} + F_{it-1}\Delta f_{it} + \Delta H_{it}) - \ln(W_{it-1})\\ &\approx (F_{it-1}/W_{it-1})(\Delta f_{it} + \Delta H_{it}/F_{it-1})\\ &= \Delta w^*_{it}(1+\gamma_{it}),\end{aligned} \tag{7}$$

where $\gamma_{it} = \Delta H_{it}/\Delta F_{it}$ is the ratio of the change in the absolute cushion component for worker i to the absolute change in her wage floor. To illustrate the implications of this equation, consider two limiting cases. At one extreme, suppose that H_{it} remains constant as the wage floor changes (as is assumed in the construction of Δw^*_{it}). Under this scenario $\gamma_{it} = 0$, and (7) implies that $\Delta w_{it} = \Delta w^*_{it}$. At the opposite extreme, suppose that the base wage W_{it} remains constant as the wage floor is raised (a situation that can only happen if $W_{it-1} > F_{it}$, that is, the initial base wage is above the new floor). Under this scenario, $\gamma_{it} = -1$ and $\Delta w_{it} = 0$, that is, an increase in floors is fully offset by a reduction in the worker's wage cushion.[23] Note that we can also construct a parallel measure of the effect of floor increases on a worker's *total wage* under the assumption that the gap between the worker's total wage and her floor stays constant, and compare that to the change in her total wage, Δw^T_{it}.

To proceed, consider a simple regression model relating Δw_{it} to Δw^*_{it} and a set of controls (X_{it}):

$$\Delta w_{it} = \theta_0 + \theta_1 \Delta w^*_{it} + \theta_x X_{it} + \xi_{it}. \tag{8}$$

We focus on estimating this model for the set of workers who remain at the firm between $t-1$ and t and stay in the same wage floor group—a group we refer to as the "firm stayers". The coefficient θ_1 provides a measure of the effect of wage floors on the base wage of stayers. A salient null hypothesis is $\theta_1 = 1$, which corresponds to the hypothesis that increases in wage floors are passed through fully to workers. If rising wage floors are partially absorbed by a reduction in wage cushions, however, then $\theta_1 < 1$, and in the limiting case in which floor increases have no effect on wages, $\theta_1 = 0$.

A potential concern in estimating a model of wage changes for stayers is that workers who remain with the firm and in the same floor category may be selected in a way that is correlated with their potential wage increase, leading to selection bias in the error term ξ_{it}. To address this, and to set the stage for the employment growth models we present in the next section, we present IV estimates that use $\Delta\overline{w}^*_{jt}$ (the mean of Δw^*_{it} across all N_{jt-1} employees of the worker's firm in year $t-1$, including stayers and non-stayers) as an instrument for the worker-specific simulated wage increase.

Estimation results for a variety of specifications of equation (8) are presented in Table 8. Columns (1)–(4) present models for the effect of floors on base wages, while columns (5)–(8) present a parallel set of models for total wages. As a point of departure, columns (1) and (5) present simple OLS models based on equation (8). The control variables include year effects and dummies for female gender and university education, as well as a linear term in the worker's age. We also add the change in log real value-added per worker at the employer. This is meant to control for firm-specific demand shocks that may be jointly correlated with the unexplained component of base wage increases (i.e. ξ_{it}) and the increase in wage floors affecting the firm.

23. A third scenario is one in which each worker's *proportional* cushion h_{it} remains constant as floors change. In this case, $\Delta w_{it} = \Delta f_{it}$.

TABLE 8. Models for effect of changes in wage floors on changes in real wages of stayers.

	Models for change in log base wage of stayers				Models for change in log total wage of stayers			
	Individual-level wages		Firm-wide average wages		Individual-level wages		Firm-wide average wages	
	OLS (1)	IV** (2)	OLS (3)	OLS (4)	OLS (5)	IV** (6)	OLS (7)	OLS (8)
Simulated change in base or Total wage*	0.458 (0.016)	0.530 (0.020)	0.550 (0.021)	0.546 (0.031)	0.446 (0.034)	0.536 (0.044)	0.555 (0.045)	0.521 (0.049)
Change in real value-added per worker at firm (coeff × 10)	0.021 (0.005)	0.021 (0.005)	0.021 (0.005)	0.021 (0.005)	0.017 (0.008)	0.017 (0.008)	0.017 (0.008)	0.017 (0.008)
Share of workers with reneg-otiated floor (coeff × 10)	–	–	–	0.000 (0.005)	–	–	–	0.000 (0.012)
Share with renegotiated floor × Mean simulated change	–	–	–	0.008 (0.031)	–	–	–	0.068 (0.050)
Demograhic controls and year effects	yes	yes	yes	yes	yes	yes	yes	yes
First stage coefficient (instrument = mean simulated change for all workers present in previous year)	–	1.029 (0.004)	–	–	–	1.027 (0.004)	–	–
R-squared	0.092	0.092	0.229	0.229	0.030	0.030	0.078	0.078

Notes: Standard errors (clustered by firm) in parentheses. Dependent variable is change in individual base wages (columns 1–2), individual total wages (columns 5–6), firm-wide average change in base wages (columns 3–4), or firm-wide average change in total wages (columns 7–8) for workers who remain at the firm from previous to current year. Models are estimated on worker-level data for 2,785,220 workers who remain at the same firm from previous to current year, but in columns (3)–(4) and (7)–(8) dependent variable and all covariates are firm-wide averages (so estimates are identical to estimates based on firm-wide average wage changes, weighing by the number of stayers). Demographic controls are shares of females and university-educated workers and mean age of workers at the firm as of the previous year.

*In columns (1)–(2), this variable is the simulated change in the individual's base wage, based on the actual change in the real wage floor for the individual and assuming that (absolute) gap between the wage floor and the base wage remains constant. In columns (5)–(6), this variable is the simulated change in the individual's total wage, based on the actual change in the real wage floor for the individual and assuming that (absolute) gap between the wage floor and the total wage remains constant. In columns (3)–(4) (7–8), this variable is the mean simulated change in base wages (total wages) for all workers who were present in the previous year.

**Model estimated by IVs, treating the simulated change in the individual's base wage (column 2) or total wage (column 6) as endogenous and using as an instrument the mean simulated change in base wages (column 2) or total wages (column 6) for all workers who were present in the previous year.

The models for base wages (column 1) and total wages (column 5) yield estimates of $\theta_1 \approx 0.45$; in both cases the estimates are relatively precise. Corresponding IV models that use $\Delta\overline{w}^*_{jt}$ as an IV for Δw^*_{it} are presented in columns (2) and (6). The estimated first stage effects of $\Delta\overline{w}^*_{jt}$ on Δw^*_{it} are reported in the second last row of the table: In both cases, the first stage coefficients are close to 1.0 in magnitude and highly significant. Interestingly, the IV estimates of θ_1 are about 15% larger in magnitude than the OLS estimates, suggesting that ξ_{it} is negatively correlated with Δw^*_{it} (perhaps reflecting the omission from the sample of workers who get promoted to a higher floor group).

Given that the first stage coefficient of $\Delta\overline{w}^*_{jt}$ is close to 1, the IV estimates of θ_1 in columns (2) and (5) are (approximately) equal to the reduced form effects of $\Delta\overline{w}^*_{jt}$ on Δw_{it} or Δw^T_{it}. Moreover, in the absence of individual-level covariates these reduced-form effects would be numerically equivalent to the effects obtained from a *firm-level* regression model relating the average wage increase for all stayers at the firm ($\Delta\overline{w}_{jt}$) to $\Delta\overline{w}^*_{jt}$ and controls

$$\Delta\overline{w}_{jt} = \rho_0 + \rho_1\Delta\overline{w}^*_{jt} + \rho_x X_{jt} + \bar{\xi}_{jt}. \tag{9}$$

Our individual-level models include individual-specific gender, education, and age controls, so we cannot quite reproduce the micro-level estimates from the firm level regression. However, as shown in columns (3) and (7), when we estimate equation (9) using firm-wide averages of the covariates as controls we find, as expected, that the estimates of ρ_1 are approximately equal to the corresponding IV estimates of θ_1.[24]

Finally, the specifications in columns (4) and (8) interact $\Delta\overline{w}^*_{jt}$ with a variable indicating the fraction of all workers at the firm whose wage floor was renegotiated between $t-1$ and t. (For the 90% of firms in which all workers are covered by a single CBA, this fraction is either 0 or 1, depending on whether the CBA was recently renegotiated or not, but for firms where different occupation groups are covered by different CBAs, it can be strictly between 0 and 1.) This interaction term allows us to check whether the responsiveness of wages to floor changes is the same when wage floors are explicitly adjusted upward by a contract renegotiation as when they are passively adjusted (typically downward) by inflation.[25] The estimated interaction effects are statistically indistinguishable from 0, providing no evidence of asymmetry in the passthrough of wage floor changes.

The estimates in Table 8 suggest that, on average, only about one half of the implied increases in wages arising from changes in wage floors are passed through to workers. The balance is offset by reductions in wage cushions, with relatively small effects on supplementary wage payments given the similarity of the passthrough effects on base wages and total wages. The ability of any particular worker's wage cushion to absorb an increase in wage floors, however, depends on the size of their wage cushion. In

24. The standard errors are about the same too, which is expected given that we cluster the standard errors by firm.

25. During our sample period, there were 2 years with negative inflation in Portugal.

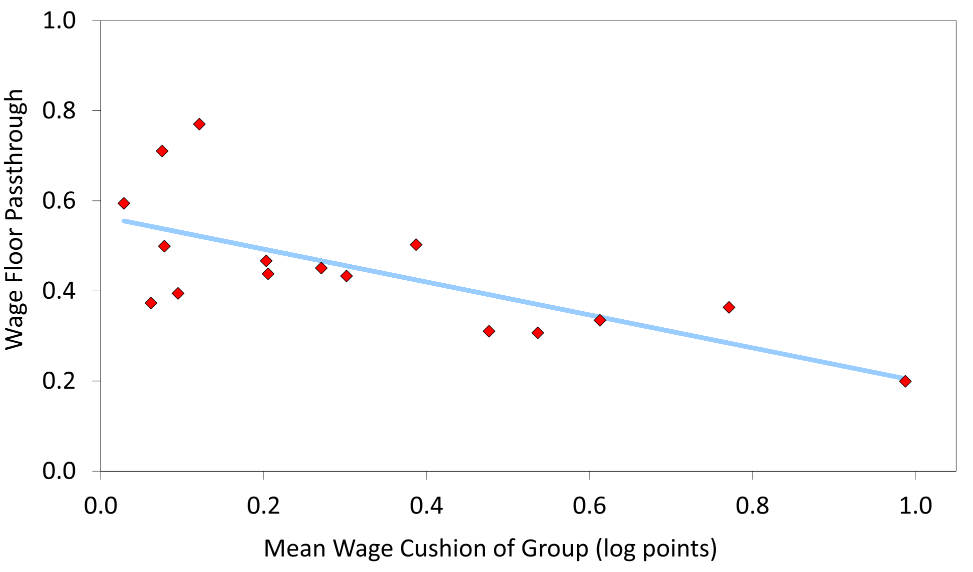

FIGURE 7. Estimated passthrough rates of floor increases to base wages, by group. Based on estimates in Table 8. Fitted OLS line shown, R-squared = 0.49.

the minimum wage literature, for example, a typical finding is increases in minimum wages have large effects on workers whose wage is below or close to the new minimum but smaller or even zero effects on those earning substantially above the minimum.[26]

We explore the heterogeneity in the degree of passthrough of wage floor increases in Table 9. Each row represents a different skill group (classified by gender, education, and age). For each group, we report the share of all firm stayers in the group, the mean relative wage floor, mean wage cushion, and mean supplements for members of the group (as measured in year $t-1$), and group-specific estimates of the passthrough effect based on the aggregated reduced form model of equation (9). We exclude results for people aged 18–24 with a university education because, for both genders, this group is extremely small.

As expected, the estimated passthrough rates tend to be larger for groups with lower wage cushions. For example, females with less than a high school education who are between 25 and 44 have an average wage cushion of roughly 8 log points and estimated passthrough rates of 0.71 (st. err = 0.02) using base wages or 0.63 (st. err = 0.07) using total monthly wages. By comparison, females in the same age range with a university education have an average wage cushion of roughly 48 log points and estimated passthrough rates of 0.31 (st. err = 0.05) and 0.37 (st. err = 0.08).

This relationship is illustrated in Figure 7, where we plot the estimated passthrough rate for each demographic group (estimated using base wages) against the mean wage

26. See Fortin, Lemieux, and Lloyd (2021) and Cengiz et al. (2019) for overviews of the existing literature and evidence on spillover effects of minimum wages in the US labor market.

TABLE 9. Estimated passthrough rates for floor increases, by subgroup.

	Fraction of stayers in group (1)	Mean relative wage floor (2)	Mean wage cushion (3)	Mean wage supple-ments (4)	Estimated passthrough rate of wages floor changes: Base wage (5)	Total wage (6)
Males						
<High school, age 18–24	0.011	0.074	0.078	0.183	0.50 (0.06)	0.68 (0.11)
<High school, age 25–44	0.206	0.162	0.203	0.182	0.47 (0.03)	0.54 (0.09)
<High school, age 45–64	0.188	0.190	0.271	0.173	0.45 (0.03)	0.46 (0.06)
High school, age 18–24	0.007	0.108	0.095	0.201	0.39 (0.09)	0.48 (0.16)
High school, age 25–44	0.086	0.297	0.302	0.186	0.43 (0.05)	0.42 (0.12)
High school, age 45–64	0.031	0.429	0.536	0.163	0.31 (0.04)	0.23 (0.12)
University, age 25–44	0.061	0.545	0.613	0.125	0.33 (0.06)	0.29 (0.09)
University, age 45–64	0.016	0.720	0.988	0.112	0.20 (0.06)	0.24 (0.13)
Females						
<High school, age 18–24	0.005	0.028	0.028	0.153	0.59 (0.04)	0.56 (0.12)
<High school, age 25–44	0.121	0.093	0.075	0.157	0.71 (0.02)	0.63 (0.07)
<High school, age 45–64	0.099	0.120	0.121	0.152	0.77 (0.03)	0.76 (0.05)
High school, age 18–24	0.005	0.067	0.062	0.189	0.37 (0.13)	0.36 (0.15)
High school, age 25–44	0.074	0.223	0.205	0.172	0.44 (0.04)	0.53 (0.09)
High school, age 45–64	0.023	0.322	0.387	0.144	0.50 (0.04)	0.47 (0.07)
University, age 25–44	0.057	0.442	0.477	0.129	0.31 (0.05)	0.37 (0.08)
University, age 45–64	0.009	0.618	0.771	0.106	0.36 (0.07)	0.31 (0.10)

Notes: Passthrough rates are estimated from OLS models relating firm-wide average of change in mean log base wage or mean log total wage of firm stayers to average simulated change in base wages or total wages of all workers that were present in previous year. See note to Table 8, columns (3) and (7).

cushion for the group. We draw two conclusions from this graph. First, even for low-cushion groups, the passthrough rate is less than 1, suggesting that the modest floor increases typically negotiated during our sample period (in the range of 1%–3%) were partly absorbed by compressing wage cushions. Second, in contrast to the pattern found in the minimum wage literature, wage floor increases in collective bargaining agreements appear to have some positive "spillover" effect even on relatively high-cushion groups.

Robustness Checks. The passthrough models in Table 8 address concerns about the selectivity of workers who remain in the same firm and floor category. Another potential concern is the endogeneity of the wage floor itself. Unobserved demand shocks might influence both the evolution of wage floors and a firm's willingness to raise wages. To investigate this issue, we use an IV procedure that links back to Section 5.2 on the determinants of floor changes. Specifically, in the first stage, we instrument the change in wage floors (for renegotiated CBAs) using the average change in value added per worker at other firms covered by the CBA since the time of the last agreement. Given the lags in contract renegotiations, we impose the requirement that lagged information on value added per worker is available for at least 2 previous years for the firm and other firms in the CBA.

Results are reported in Online Appendix Table E.1. Importantly, we find that estimates of wage floor passthrough from specifications similar to those in Table 8 are very similar in the restricted sample with lagged value added data. When we use changes in value added per worker at other firms as an instrument for the wage floor increases, we obtain larger estimates of the passthrough effect than from our base specifications.[27] We emphasize, however, that value added changes at other firms in the same CBA may be correlated with sectoral demand shocks that positively affect wages, leading to an upward bias in this alternative IV strategy.[28]

Next, we strengthen the link of our analysis to the minimum wage literature. We complement our analysis of the impacts of wage floors on different demographic sub-groups by dividing workers into different groups based on the size of their wage cushions prior to the contract renegotiation. This approach is very similar to that used in Dustmann et al. (2022) to study the spillover effects of the newly introduced German minimum wage. Online Appendix Figure E.1 shows the effect of a rise in the wage floor on wages for workers whose base wage was closer or further from the old floor. Consistent with the patterns in Figure 7, we find larger passthrough effects for workers whose base wage was close to the floor (i.e. those who had a low "wage cushion"). But in contrast to the minimum wage literature, we find that the "spillover effect" of base wage increases extends to workers earning considerably above the old wage floor—again consistent with the patterns in Figure 7.

27. In these models, we follow the specifications in Section 5.2 and include dummies for time since the last contract renegotiation and a measure of cumulative inflation over that period.

28. This is particularly a concern because firm-specific value added measures are noisy, and value-added changes at other firms may proxy for firm-specific changes.

7. Effect of Wage Floors on Employment

Although increases in wage floors are partly absorbed by the compression of wage cushions, they still lead to some increase in workers' base wages and total salaries. This opens up the question of whether firms use reductions in employment as another channel of adjustment to higher floors. Such employment effects might be expected if employment and wage outcomes lie on a traditional downward-sloping employment demand function. To the extent that wages are endogenously set by firms with market power, however, the equilibrium relationship is less clear and may even be upward-sloping.

Building on the results in Table 8, we fit a series of simple models of the form

$$\Delta \ln E_{jt} = \tau_0 + \tau_1 \Delta \overline{w}^*_{jt} + \tau_x X_{jt} + \zeta_{jt}, \quad (10)$$

where E_{jt} is the total number of employees of firm j in year t, and $\Delta \overline{w}^*_{jt}$ is the average simulated change in total base wages of employees present at the firm in period $t-1$. We estimate this model for all firms, and separately for the subset where the modal worker is covered by a *sectoral* CBA.[29] For the latter set of firms, wage floors are arguably more exogenous to firm-specific conditions.

The results are presented in Table 10. We show a specification with only year effects and a control for the increase in real value added per worker at the firm in columns (1) and (4), a second model with controls for gender, fraction of university-educated workers, and mean age of workers in columns (2) and (5), and a specification that allows for an interaction between $\Delta \overline{w}^*_{jt}$ and the share of workers with a renegotiated wage floor in columns (3) and (6).

The estimation results are quite similar for the models estimated on all firms and on the subset covered by sectoral contracts, and point to three main conclusions. First, increases in firm-specific productivity (as measured by the change in real value added per worker) have a significant positive effect on employment growth, about ten times larger in magnitude than the effect on wages.

Second, none of the models shows a negative effect of floor increases on employment growth. The model in column (2), for example, yields an estimate of $\hat{\tau}_1 = 0.165$ for the effect of floor-induced base wage increases on employment. If one assumes that firms set employment taking wages as exogenous, then one could convert this into an estimated demand elasticity by dividing by the estimate of ρ_1 from equation (9). Using the estimate of ρ_1 from column (2) of Table 8, yields an estimate of the elasticity of employment with respect to base wages of 0.30, with a standard error of approximately 0.33. While this point estimate is positive, a 95% confidence interval ranges from −0.36 to 0.96, so we cannot rule out small negative employment responses.

29. As noted, about 90% of firms have only a single CBA, but for firms, where workers are covered by two or more different CBAs, we assign sectoral coverage status based on the characteristics of the agreement that covers the largest number of workers.

TABLE 10. Models for effect of changes in wage floors on change in firm-wide employment.

	Dependent variable = change in log employment					
	All firms			Firms covered by sectoral CBAs		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of simulated change in Total wage of employees (using actual floor changes)	0.432 (0.168)	0.165 (0.177)	0.373 (0.186)	0.412 (0.179)	0.104 (0.190)	0.317 (0.198)
Change in real value-added per worker at firm	0.027 (0.003)	0.026 (0.003)	0.026 (0.003)	0.029 (0.003)	0.028 (0.003)	0.028 (0.003)
Share of workers with renegotiated floors (coeff × 10)	–	–	−0.030 (0.037)	–	–	−0.027 (0.040)
Share with renegotiated floor × mean simulated change	–	–	−0.303 (0.305)	–	–	−0.316 (0.316)
Demograhic controls	no	yes	yes	no	yes	yes
Year effects	yes	yes	yes	yes	yes	yes
R-squared	0.014	0.027	0.027	0.015	0.029	0.029

Notes: Standard errors (clustered by firm) in parentheses. Dependent variable is change in log of employment at the firm, including all workers counted in our main QP data set. Models are weighted OLS estimates, weighted by the number of employees. Mean of simulated change in total wage (covariate in first row) is calculated by incrementing real wage floors of employees in previous year but assuming no change in other wage components. Demographic controls are shares of females and university-educated workers and mean age of workers at the firm as of the previous year. In columns (4)–(6), sample is restricted to firms where the modal CBA covering workers at the firm is a sectoral contract.

A third finding, consistent with the results in Table 8, is that there is no evidence of asymmetry in reactions to actively renegotiated wage floor changes versus changes in real wage floors arising from inflation.

The models in Table 10 describe employment outcomes for all workers. The results in Table 9, however, suggest that the wage impacts of wage floor increases vary across groups. To check whether there is similar heterogeneity in the employment impacts, we estimated models like (10) by gender, education, and age group. The results are presented in Online Appendix Table E.2, alongside the corresponding estimates of the wage effects for each group from Table 9. Ten of the 16 estimated employment effects are positive, while six are negative. Only one is significantly negative ($t = 2.02$); three are significantly positive ($t = 2.43$, 2.97, and 4.19). Moreover, the estimated employment effects for each group are positively correlated with the corresponding wage passthrough effects—the opposite of what would be expected if wage floors have larger negative effects on the employment of groups whose wages are most responsive to floor increases.[30]

30. The correlation of $\hat{\tau}_1$ and $\hat{\rho}_1$ across the 16 groups is 0.21.

TABLE 11. Summary of counterfactual scenarios.

A.	All workers in 2010, with actual 2010 floors, cushions, and supplements
B.	Start with A, increment each floor by actual change 2010–2016 *B minus A captures floor adjustments, holding constant cushions and supplements*
C.	Start with B, reweight skill groups to 2016 shares *C minus B captures demographic changes*
D.	All workers in 2016, with 2016 floors, but 2010 cushions and supplements *D minus C captures the re-allocation of workers across floor groups, holding constant floors, cushions, and supplements*
E.	Start with D but update to actual 2016 cushions *E minus D captures adjustment of cushions within wage floor groups*
F.	Start with E but update to actual 2016 supplements (=All workers in 2016 with 2016 floors, cushions and supplements) *F minus E captures adjustment of supplements within floor-groups*

Since young workers have small average wage cushions (see Figure 6), the findings in Table 9 suggest that their wages may have been pushed up relatively more by increases in wage floors over the past decade, preventing firms from hiring them in the first place—an effect that may be hard to discern from models of employment growth such as equation (10). While a full analysis of this concern is beyond our scope, Online Appendix Figure E.2 shows data on the fractions of young men and young women (age 16–24) who were not in employment, education or training in Portugal, and six other countries (Italy, the United States, Spain, France, the United Kingdom, and Germany) over the 2004–2019 period. The so-called "NEET" rates for both gender groups in Portugal track the rates in other countries fairly closely: There is not much evidence of a relative rise in the post-crisis era. For example, comparing Portugal to the United States, the difference in differences of NEET rates for 2017–2019 versus 2004–2007 is −0.3% for males and +1.4% for females. Parallel difference in differences relative to the United Kingdom are −4.4% for young men and −3.7% for young women. The only country that did appreciably better than Portugal (and virtually all other countries) was Germany.

8. Decomposing Changes in Real Wages, 2010–2016

In this section, we combine the insights from the previous sections and document how the various components of wages contributed to overall changes in wages for the economy as a whole and for different groups over the 2010–2016 period. Our approach builds on the methodology developed by DiNardo, Fortin, and Lemieux (1996)—hereafter DFL—for analyzing the effects of trade unions and minimum wages on trends in US wage inequality. Specifically, we conduct a series of counterfactual simulations—summarized in Table 11—that provide a step-by-step decomposition of the changes in the mean total monthly real wages for different groups of workers.

We start with scenario A, which takes all workers in our matched QP-BTE sample in 2010. The outcomes in this sample represent the actual distribution of wages in 2010. Next, in scenario B, we increment the wage floor that applies to each worker in 2010 by the percentage change of that floor between 2010 and 2016, holding constant the worker's (proportional) wage cushions and (proportional) wage supplements. A comparison of outcomes between scenario B and the baseline scenario A allows us to assess what would have happened if floors adjusted as they did between 2010 and 2016, but all workers remained in their same floor categories and received the same cushions and supplements as they did in 2010.

In scenario C, we reweight the observations in scenario B by the relative probability that workers in a given gender/education/age cell were present in the labor market in 2016 versus 2010. Following the logic of DFL, this reweighting allows us to assess how the changing demographic composition of the workforce would have affected wage outcomes, holding constant the assignment of workers to their 2010 wage floor groups, with their 2010 cushions and supplements, but with 2016 floors.

In scenario D, we take all workers in our matched QP-BTE sample in 2016, but assign each worker in a given wage floor group a randomly drawn wage cushion and wage supplement from the distributions of *the same wage floor group in 2010*.[31] Relative to scenario C (which has 2010 workers in their 2010 floor groups but assigned the 2016 floors), scenario D captures any re-allocation of workers across wage floor groups while holding constant wage floors at their 2016 values, and the distributions of wage cushions and supplements for workers in a given floor group at their 2010 distributions.

We note that this re-allocation effect reflects a combination of within-job effects, between-job effects, and entry effects. Within jobs, a change in the rate at which workers are promoted to higher wage floor categories will lead workers in a given age range in 2016 to be assigned to better or worse wage floor groups than they would have been assigned to in 2010. For job changers, any shift in the probabilities of moving up or down the "job ladder" (as measured by the level of the wage floors at the origin and destination job) will likewise lead to a change in the assignment of workers to wage floors. Finally, any change in the assignment of labor market entrants (or re-entrants) to wage floor groups will contribute to the overall re-allocation effect.

In scenario E, we adjust scenario D by assigning each worker his or her *actual* wage cushion in 2016. A comparison with scenario D allows us to assess the impact of changes in the distribution of wage cushions within a given wage floor group. Finally, scenario F just takes the distribution of workers in 2016 with their 2016 floors, cushions, and supplements. This differs from scenario E by the updating of the distribution of wage supplements from 2010 to 2016, allowing us to quantify the impact of changing wage supplements.

Table 12 summarizes the comparisons across these different scenarios for the overall population of workers and various subgroups. We begin by showing the mean

31. This re-assignment approach builds on DFL, who assessed the effect of a national changing minimum wage by assigning the lower tail of wages from one year to the distribution of wages in another year.

TABLE 12. Components of adjustment of real wages, 2010–2016.

	Mean log wage in 2010	Components of real wage in 2010			Change in real wage 2010–2016	Simulated components of real wage change					Share workers w/floor equal to min wage		
		Rel. floor	Cushion	Supplt's		Change in floors	Reweighing across skill groups	Re-allocation to new floor groups	Change in cushion within floor group	Change in supplt's within floor group	Actual 2010 (in column 1)	After change in floors (in column 6)	Actual 2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
All	6.863	0.250	0.195	0.166	−0.017	−0.022	0.074	−0.048	−0.025	0.005	0.126	0.187	0.205
Males	6.931	0.271	0.234	0.174	−0.022	−0.028	0.077	−0.050	−0.026	0.006	0.074	0.124	0.147
Females	6.767	0.220	0.140	0.154	−0.001	−0.015	0.079	−0.045	−0.024	0.004	0.199	0.277	0.277
Education<high school	6.677	0.142	0.122	0.160	−0.022	−0.014	0.009	−0.012	−0.007	0.002	0.165	0.233	0.259
Education = high school	6.994	0.328	0.228	0.187	−0.126	−0.033	0.025	−0.092	−0.034	0.007	0.081	0.151	0.210
Education = university	7.491	0.611	0.468	0.160	−0.161	−0.044	0.019	−0.084	−0.062	0.010	0.015	0.037	0.063
Age 18–24	6.582	0.114	0.040	0.176	−0.009	−0.004	0.040	−0.038	−0.016	0.009	0.246	0.347	0.411
Age 25–44	6.870	0.260	0.192	0.167	−0.025	−0.024	0.071	−0.046	−0.029	0.004	0.121	0.173	0.192
Age 45–64	6.907	0.259	0.234	0.163	−0.024	−0.023	0.065	−0.051	−0.020	0.006	0.109	0.183	0.197
Males by education and age													
<High school, age 18–24	6.552	0.078	0.045	0.177	−0.024	0.000	0.000	−0.018	−0.010	0.005	0.173	0.259	0.347
<High school, age 25–44	6.723	0.154	0.145	0.172	−0.032	−0.022	0.000	−0.004	−0.007	0.002	0.094	0.148	0.186
<High school, age 45–64	6.820	0.196	0.206	0.167	−0.060	−0.027	0.000	−0.023	−0.013	0.002	0.077	0.139	0.163
High school, age 18–24	6.643	0.154	0.043	0.193	−0.056	−0.012	0.000	−0.040	−0.017	0.013	0.194	0.277	0.366
High school, age 25–44	7.065	0.368	0.250	0.195	−0.177	−0.037	0.000	−0.112	−0.035	0.006	0.045	0.092	0.146
High school, age 45–64	7.476	0.557	0.463	0.204	−0.209	−0.042	0.000	−0.132	−0.050	0.015	0.017	0.041	0.071
University, age 18–24	7.043	0.490	0.154	0.146	−0.147	−0.039	0.000	−0.094	−0.041	0.027	0.037	0.076	0.114
University, age 25–44	7.541	0.624	0.507	0.158	−0.181	−0.042	0.000	−0.075	−0.073	0.009	0.008	0.019	0.040
University, age 45–64	8.058	0.840	0.794	0.171	−0.185	−0.043	0.000	−0.113	−0.047	0.017	0.003	0.008	0.017

TABLE 12. Continued.

	Mean log wage in 2010	Components of real wage in 2010			Change in real wage 2010–2016	Simulated components of real wage change					Share workers w/floor equal to min wage		
	(1)	Rel. floor (2)	Cushion (3)	Supplt's (4)	(5)	Change in floors (6)	Reweighing across skill groups (7)	Re-allocation to new floor groups (8)	Change in cushion within floor group (9)	Change in supplt's within floor group (10)	Actual 2010 (in column 1) (11)	After change in floors (in column 6) (12)	Actual 2016 (13)
Females by education and age													
<High school, age 18–24	6.465	0.045	0.011	0.158	−0.014	0.012	0.000	−0.021	−0.005	0.001	0.418	0.536	0.603
<High school, age 25–44	6.533	0.093	0.043	0.144	0.007	0.001	0.000	0.004	−0.001	0.002	0.309	0.361	0.379
<High school, age 45–64	6.601	0.128	0.081	0.140	−0.026	−0.003	0.000	−0.020	−0.006	0.003	0.233	0.360	0.372
High school, age 18–24	6.565	0.100	0.021	0.191	−0.048	−0.005	0.000	−0.035	−0.012	0.004	0.326	0.474	0.564
High school, age 25–44	6.840	0.255	0.159	0.174	−0.118	−0.031	0.000	−0.061	−0.029	0.003	0.099	0.195	0.258
High school, age 45–64	7.152	0.391	0.337	0.171	−0.185	−0.039	0.000	−0.112	−0.041	0.007	0.038	0.092	0.153
University, age 18–24	6.956	0.440	0.115	0.148	−0.156	−0.048	0.000	−0.083	−0.046	0.021	0.094	0.158	0.208
University, age 25–44	7.317	0.538	0.367	0.160	−0.175	−0.045	0.000	−0.073	−0.063	0.007	0.022	0.056	0.090
University, age 45–64	7.765	0.763	0.583	0.167	−0.203	−0.046	0.000	−0.131	−0.040	0.015	0.009	0.025	0.043

Notes: Wages refer to real monthly wages, including regular supplementary payments. Column (11) reports the actual share of workers at a floor equal to the minimum wage in 2010. Column (12) considers the updating in collective bargaining floors, ceteris paribus, reporting the share of workers who would have had a floor at the minimim wage in 2016 if they had stayed in their same floor category as in 2010. Column (13) reports the actual share of workers at a floor equal to the minimum wage in 2016. The change from columns (12) to (13) hence reflects demographic shifts in the economy and re-allocation of workers with a given age–gender–education across floors. See text for description of simulations for decomposition.

log total monthly wage in 2010 (column 1) and the components of this total, as described by equation (2) (columns 2–4). Next, we show the actual change in mean log wages between 2010 and 2016, which was −1.7% for workers as a whole, but ranged between −20% (for some university-educated groups) and +0.7% (for women with less than high school education who were between 25 and 44).

Column (6) shows the difference in mean log wages between scenario B and scenario A, and summarizes the impact of changing wage floors. On average, real wage floors declined by about 2.2%, but the mean floors affecting young- and less-educated workers actually rose slightly, reflecting the influence of the minimum wage, which increased in real value by 3.5% between 2010 and 2016, pushing up some of the lowest wage floors in the economy.[32]

Column (7) shows the effect of demographic changes captured by the difference between scenario C and scenario B. (Note that within any of the narrowly defined gender/education/age groups in the bottom panel of the table, this difference is 0.) Average education levels were rising quickly in Portugal between 2010 and 2016, a trend that would have increased wages by about 7.4% in the absence of other factors.

Offsetting the rise in education was a re-allocation of workers across floor groups, the effects of which are captured by the differences in mean wages between scenarios D and C, presented in column (8). On average, workers were being reallocated to lower-paying jobs over our sample period, leading to a nearly 5% reduction in real wages. The effects of this downgrading were particularly large for older university-educated and high-school educated workers, and were negligible for younger less-educated workers who were already working at jobs with the lowest wage floors. It is possible that this re-allocation was partly caused by high wage floors, but our interpretation is that it was more likely a reflection of the fact that job openings at higher "rungs" of the job ladder tend to disappear in recessions (e.g. Moscarini and Postel-Vinay 2018).

Next, columns (9) and (10) show the effects of changes in wage cushions and wage supplements. On average, wage cushions declined over the course of the financial crisis, with larger declines for groups that were initially earning larger average cushions. In contrast, the value of wage supplements was relatively stable, though groups with the largest declines in floors and cushions experienced small increases in the value of their supplemental payments. This reflects the fact that some components of supplementary payments are expressed in absolute terms (such as meal allowance payments), and as the base wage of a group declines, the *relative value* of their supplementary payments will rise.

The general pattern of the different components in columns (7)–(10) is illustrated in Figure 8. We plot the overall change in log wages for each of the 18 demographic groups highlighted in Table 12 against their mean log wage in 2010, along with the contributions of floor updates, changes in cushions and supplemental payments, and the effect of re-allocations across floor groups. The figure shows that the large reductions

32. The decline in wage floors was more pronounced for university-educated workers. If they had kept their job categories of 2010, their real wage floors would have declined by about 4%.

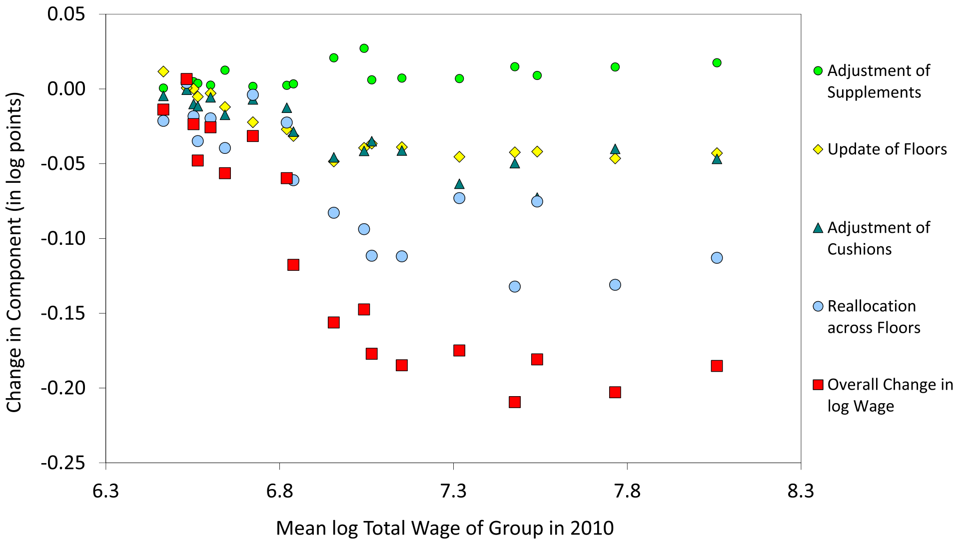

FIGURE 8. Components of change in mean log real wages across groups, 2010–2016.

in real wages for higher-paid groups in Portugal between 2010 and 2016 reflected the combined effects of falling wage floors, reduced wage cushions, and re-allocations to lower-paying floor categories.

One way to summarize the relative contributions of these different components to the between-group pattern of wage changes is to compute $\text{cov}[\Delta\overline{w}_s^T, \Delta\bar{z}_{ks}]/\,\text{var}[\Delta\overline{w}_s^T]$, where $\Delta\overline{w}_s^T$ represents the change in the real average total wage of skill group s between 2010 and 2016, and $\Delta\bar{z}_{ks}$ is the mean change in the kth component for skill group s. Since $\Delta\overline{w}_s^T = \sum_k \Delta\bar{z}_{ks}$ these terms sum to 1. Following this approach, we estimate that changes in real wage floors accounted for 24% of the between-skill group variation in real wages reductions, changes in real wage cushions accounted for 26%, re-allocation across floor group accounted for 56%, and changes in real supplemental payments accounted for −6%.

The final set of columns quantifies the impact of the national minimum wage on wage floors. Column (11) shows the fraction of workers with a wage floor equal to the minimum wage in 2010. Column (12) reports the fraction whose floor would have equaled the minimum wage in 2016 if they had remained in the same floor group, subject only to floor updating. Finally, column (13) reports the fraction of workers with a wage floor equal to the minimum in 2016. By comparing columns (11) and (12), we can characterize the degree of "encroachment" of the minimum wage on lower wage floors. In 2010, 13% of the workforce were in job categories with a wage floor exactly at the level of the minimum wage. Holding everyone in their 2010 jobs, this fraction would have risen to 19% by 2016. Thus, all else equal, about 6% of workers would have had wage floors that were overtaken by the minimum wage, leading to a roughly 50% increase in the share of workers with floors at the minimum. In addition, as shown by the comparison between columns (12) and (13), demographic changes and re-allocations of workers to jobs lead to another 1.5% increase in the fraction of workers with floors at the minimum wage. Looking across gender, age, and education groups, we see that the re-allocation effect is particularly large for younger workers in all education groups, reflecting the tendency for new entrants in all education categories to enter jobs with relatively low wage floors during the 2010–2016 period. An interesting question for future research is whether this pattern was partly reversed in the recovery that has taken place since 2016.

9. Conclusions

In this paper, we have tried to provide a simple framework for thinking about the effects of “European style” sectoral wage contracts on wage inequality and patterns of wage changes for different individuals and groups over time. Our approach builds on earlier work by Cardoso and Portugal (2005). As they (and many subsequent authors) have noted, a key feature that distinguishes European style contracts from union contracts in the United States is that most workers receive an idiosyncratic wage cushion that “tops up” their wage over the contractual wage floor. We therefore adopt some of the methods that have been developed to study the effect of minimum wages—specifically models of *wage spillovers*—to the study of sectoral wage floors. We also extend the seminal approach of DiNardo, Fortin, and Lemieux (1996) to develop a series of counterfactuals that allow us to show how changes in wage floors, changes in wage cushions, and re-allocations of workers across different floor categories all contributed to wage adjustments over the past decade in Portugal.

Since wage cushions are set by the employer, rather than by the sectoral bargain itself, they introduce an important source of wage flexibility both to the cross-sectional wage distribution at a point in time, and to changes in wages for individuals and groups over time. We show that variation in wage cushions contributes significantly to many of the standard “wage gaps” in the labor market, including differences by gender, education, age, and between more and less profitable employers. The variation in wage cushions is particularly important in allowing wages to vary between more and less profitable firms covered by the same sectoral agreement, addressing a concern about sectoral bargaining that is widely raised by policy analysts (e.g. Boeri et al. 2021).

We also show that when wage floors are renegotiated in a sectoral wage bargain, only about one half of the increase is passed through to workers’ wages. The other half is absorbed by a reduction in wage cushions. As has been well documented in the study of minimum wages, the passthrough effect of sectoral wage floors is larger for workers whose wages are closer to the floor (i.e. those with a smaller wage cushion), but in our case, we find some degree of passthrough even for workers whose wages are far above the floor for their job category.

We find little evidence that employers adjust to rising wage floors by cutting overall employment: Looking across 16 demographic subgroups, we find one significantly negative estimate of the effect of wage floors on employment, counterbalanced by three significantly positive estimates. Moreover, the estimated employment effects of wage floors for different demographic groups are positively correlated with the estimated wage effects—the opposite of the pattern expected if wage floors reduce employment. The absence of systematic employment effects may not be too surprising in a setting where the majority of workers are receiving an employer-determined wage cushion that places their wage above the floor: A growing body of evidence suggests that when wages are set by employers, the effect of minimum wage increases is small.

Our counterfactual analysis of wage changes from before to after the recent financial crisis in Portugal shows that the remarkable declines in real wages for many groups were accomplished by a combination of declining real wage floors, declining real wage cushions, and a re-allocation of workers across wage floor categories. The re-allocation effect was particularly important for higher-educated groups, who entered new jobs at lower floors than would have been expected prior to the crisis and were also promoted less quickly to higher wage floor categories. A growing body of work summarized in Moscarini and Postel-Vinay (2018) suggests that a key characteristic of recessions is the absence of job openings at higher-paying firms. The pattern of re-allocations and slower promotions to higher wage floors observed in our sample period is very consistent with that view.

An important limitation of our study is that we only have data for one country. It is possible that some of the flexibility we document in the Portuguese labor market is absent in other labor markets. Indeed, the Portuguese labor market has long been characterized by relatively high levels of wage inequality. In other countries, institutional or legal restrictions may make it impossible for firms to reduce wage cushions when sectoral wage floors are increased. Providing evidence on how floors and cushions interact in other countries would clearly be helpful for future policy-making.

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Supplementary Data

Supplementary data are available at *JEEA* online.