Paying for others' protection: Causal evidence on wages in a two-tier system*

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October 30, 2013

Preliminary version

Abstract

Mandated employment protection reduces wages of workers by shifting both the demand and supply schedules. In a segmented labor market, theory predicts that a reform of employment protection will have an asymmetric impact on wages of entry and incumbent jobs. We explore a reform that increased the employment protection of open-ended contracts for a well-defined subset of firms (treated), while leaving it unchanged for other firms (control). The causal evidence points to a reduction in wages for new open-ended and fixed-term contracts and no impact on wages of more tenured workers. The reductions estimated for entrants are on the -0.7 to -0.5 percentage points range and represents a shift into lower wages of X% of the total expected costs from the extra protection. The impact on wages is heterogeneous across worker characteristics, such as gender, age, and skills.

Keywords: Wages; Two-tier systems; Quasi-experiment; Employment protection

JEL Codes: J31; J32; J63.

*We acknowledge the financial support provided by FCT's grant PTDC/EGE-ECO/112177/2009. We would like to thank participants in the Banco de Portugal Conference on Labor Market, the SEEK 2013, AIEL – Rome, SIE – Bologna and EALE – Turin conferences. We thank the detailed comments by XXX. Opinions expressed herein do not necessarily reflect the views of the *Banco de Portugal*. Any errors are the responsibility of the authors.

1 Introduction

Two-tier labor markets affect a large proportion of workers in European economies. Nowadays, 1 out of each 6 salaried workers in the Euro area have a temporary contract, but in Spain and Portugal the share reaches one-fourth. These contracts have a low conversion rate into a permanent position. The two-tier system affects the level and return to investment in human capital and, ultimately, economic growth. It is of utmost importance for policy makers to understand the consequences of partial reforms that led to two-tier systems. How does a two-tier reform affect wages? Who pays for the extra protection? We answer these questions in the context of a reform of the Portuguese Labor Code that generated a quasi-experiment, showing that wages respond asymmetrically to the two-tier nature of the reform.

In competitive markets there are no free lunches and workers will pay for their own protection. In competitive markets, Lazear (1990) shows that the cost of protection will be transmitted to workers in the form of lower wages. This question gains an extra degree of complexity in two-tier systems because protected open-ended contracts coexist with more flexible fixed-term arrangements (Boeri 2010). In the last decades, European increased the protection gap between incumbents (on open-ended contracts) and entry jobs (mostly on fixed-term contracts) rather than flexing the rules governing open-ended contracts. However, increasing the employment protection of open-ended contracts may generate a substantial spillover on the other segment. For instance, Boeri (2010) predicts an increase in the wage premium of incumbents over entry jobs. The insider-outsider bargaining model of Lindbeck and Snower (2001) has the same impact on wages through a different mechanism. In their model, incumbent workers enjoy a larger bargaining power, which is used to avoid the cost of the extra protection. On the contrary, the model predicts that movers, under new contracts (either open-ended or fixed-term), with lower bargaining power, will pay a large share of the total cost of the extra protection of incumbents.

Our analysis contributes to the growing literature on two-tier labor markets, which dates back to the work by Abowd, Corbel and Kramarz (1999) and has been extended by Bentolila, Cahuc, Dolado and Le Barbanchon (2010), Boeri (2010), and Cahuc, Charlot and Malherbet (2012). However, establishing a causal relationship between employment protection and wages has been a challenge for the empirical literature (Autor, Donohue III and Schwab 2006). We overcome this by exploring a reform of the Portuguese Labor Code implemented in 2004. The reform increased the protection of open-ended employment for firms with 11 to 20 workers (our treatment group), but left it unchanged for all other firms (we use firms with 21 to 50 workers as a control group). We use an administrative matched employee-employer dataset – *Quadros de Pessoal* – covering all private sector matches, for the 2002 to 2008 period. The dataset covers more than 3.1 million match $\times$ year observations.

In this setting, we ask if wages of workers in treated firms reacted to the tighter employment protection. The difference-in-differences estimates indicate that wages in treated firms fell. This is true for base wages, but also for total compensation and their hourly counterparts. However, like suggested by the theory, the impacts differ markedly between incumbents and new contracts, regardless of the type of contract. We obtain a fall in total wages of new contracts of around 0.6 percentage points, while incumbents have their wages almost unchanged. The magnitude of these wage reductions cober around two-thirds of the total expected increase in firing costs.

Increased employment protection results in a larger wage premium of incumbent open-ended contracts. In a segmented labor market, firms shift the cost of employment protection to the less protected segment, circumventing – however inefficiently – part of the effects of the extra rigidity. The new cohort of permanent jobs will pay for the extra protection and the unchanged wage level of incumbent workers can be interpreted as a sign of nominal wage rigidity typical of all incentive models of the labor market, as the ones of internal labor markets at the firm level (Doeringer, Piore et al. 1971, Lazear 2011).

The paper contributes to the policy discussion on the single contract in European countries. The empirical identification of a causal spillover on wages raises additional concerns on the optimality fo a labor market split by contract type. Our results favor the adoption of simpler contractual relationships.

2 The reform of employment protection

The Portuguese labor market is an extreme case of a two-tier system. In this section, we provide an overview of its characteristics, along with a description of a reform that increased the employment protection gap between open-ended and fixed-term contracts. This reform provides the quasi-experimental setting used to analyze the impact of employment protection on wages.

2.1 The 2004 reform: More protection for open-ended contracts

In the Portuguese labor code two types of contracts are offered concurrently: fixed-term and open ended contracts. Today, there are no major legal restrictions on temporary hiring, the labor code makes them legally (and economic) substitutes. The differences in severance payments for permanent and fixed-term contracts are minor. The largest contribution to the gap resides in the procedural costs and uncertainty to terminate a match. They are absent at the expiration of fixed-term contracts, but are rather significant for permanent positions. Firing a worker implies written procedures and witnesses interviews involving the works council and, if the worker is a union delegate, the union itself. Altogether, the procedures extend the dismissal process substantially, typically 2 months, involving legal counselors and administrative costs. Often, to avoid the costs of long and uncertain judicial processes, firms reach out-of-court agreements with the worker. Not surprisingly, these settlements typically exceed the amount legally required.

In 2004, a labor market reform increased the procedural costs for a subset of firms, generating a quasi-experimental setting. Before the reform, the law exempted firms with less than 21 workers from the legal procedures listed above. The reform of the labor code changed this threshold to 11 workers (*Decreto-Lei 99/2003*). The protection gap between open-ended and fixed-term contracts widened for a subset of firms (11 to 20 workers), but remained the same for all other firms. Even though the costs of the procedural requirements are not explicitly defined in the legislation, firms incorporate these expected costs and may adjust wages accordingly.

In the quasi-experimental setting, firms with 11 to 20 workers constitute the *treatment group*; firms with 21 to 50 workers, a subset of those not affected by the reform, constitute the *control group*. The specific choice of control firms was made to guarantee a common trend between treatment and control units in the before period. Such firm-size restrictions are common in the literature, e.g. Burgess, Lane and Stevens (2001), Kugler and Pica (2008), Martins (2009).

One problem that may arise with the introduction of thresholds in the legislation is the pooling of firms in one side of the thresholds to avoid the costs of extra protection. The histograms in Figure 1 show the distribution of firms by size. Before the reform the exemption threshold was at 20 employees (left panel) and after the reform it was at 10 employees (right panel). In neither case there are noticeable discontinuities in the distribution of firms, ruling out a strong influence of such strategic behavior by firms.

[FIGURE 1 (see page 23)]

For the identification strategy required by the difference-in-differences estimator, it is worth mentioning that the employment protection legislation reform was part of a more encompassing revision of the Labor Code, which may cast questions over the causal interpretation of the results. For instance, the maximum duration of fixed-term contracts was extended from 3 to 6 years. However, such revisions were not specific to the firm size. Following the difference-in-differences identification assumption, these changes, as well as other economy-wide changes, are assumed to affect equally treatment and control groups, canceling in the conditional differencing. Additionally, we perform several robustness checks and test extensively the sensitivity of our results to the specific choice of the treatment and control groups.

2.2 Expected impact on wages

In the competitive equilibrium, with risk neutral workers, a government-mandated transfer in the form of higher employment protection is completely offset by an initial transfer from the worker to the firm (Lazear 1990). Later, the worker receives the same amount either as higher wages or as a severance payment. However, if there are restrictions to these transfers (e.g. liquidity constraints), such Coasean solutions are not available and the overall impact on wages is negative. The increase in employment protection shifts labor demand inward and creates a deadweight loss, because part of the dismissal costs to the firms are not recouped by the workers and firms are not as profitable as before. In addition, if the extra protection is valued by workers, labor supply will shift outward and wages fall further (Summers 1989).

In a two-tier labor market, Boeri (2010) predicts that an increase in the employment protection for open-ended workers will increase their wage premium over fixed-term workers. The impact on wages reflects the reduction in the conversion of temporary matches into permanent ones, the increase in the destruction rate of fixed-term contracts and the reduction in the job loss rate of permanent contracts. The model does not offer a clear prediction about the impact on the wage level of permanent and temporary matches separately, but only on the widening of the wage gap. The result rests on the assumption of substitutability between the two contracts. If, instead, these matches are poor substitutes, the impact may be tamed.

The Lindbeck and Snower (2001) bargaining model predicts also a larger wage gap. With dismissal threats more costly, firms lose bargaining power over incumbent workers, leading to higher wages for open-ended contracts. New entrants, the outsiders, stand to lose, in the form of lower wages.

3 Data

We use an annual administrative employer-employee dataset, *Quadros de Pessoal*, which reports, with respect to October of each year, all private sector employment in Portugal. Our analysis starts in 2002, the first year for which the information on the type of contract is available, and ends in 2008, to avoid the influence of the 2009 Labor Code revision. This also avoids the onset of the financial crisis started in 2009. We have two years prior to the reform, 2002 and 2003, and five years after the reform, 2004-2008. *Quadros de Pessoal* have very detailed firm, worker and match data and been extensively used in the microeconomic analysis in Portugal (e.g. Cabral and Mata 2003, Martins, Solon and Thomas 2012).

The sample includes matches in firms with 11-50 workers. Additionally, we restrict it to workers aged between 15 and 65 years old. We also dropped matches with less than 85 hours or more than 215 hours of work per month, because a standard full-time job has a regular 8-hour working day, 5 days a week. Matches with wages below the minimum wage and above the 99th percentile of the wage distribution were excluded. All observations were checked for longitudinal consistency of time invariant information and have valid information for the variables included in the estimation.

Table 1 presents summary statistics for the sample of treatment and control workers. There are a total of 1,405,800 matches (worker $\times$ firm pairs), resulting in an unbalanced panel with 3,581,305 observations (match $\times$ year pairs). These matches are spread over 56,680 firms and 1,302,865 workers. In the before period, there are 372,770 treatment observations and 513,638 control observations. In the after period, there are 1,128,155 treatment observations and 1,566,742 control observations.

Fixed-term contracts were introduced in 1976, but only since 1995 they constitute the major source of employment growth. In 2002, they represented almost 20% of salaried employment, increasing to 27% in 2008. The sample average share of fixed-term contracts is 25.8%, a figure similar to the average for the total private sector. However, there is a large dispersion of the share of fixed-term contracts at the firm level, with some firms relying heavily in this types of contracts, and also at the industry level. Average tenure is 84 months, but with a large standard deviation, 89 months. These features hint at the two-tier characteristic of the labor market, with long spells of highly protected employment coexisting with an increasing share of short-term matches. Another characteristic of the Portuguese labor market is the low level of schooling, close to 50% of workers have 6 or less years of education. Consequently, the average nominal base wage is close to 657 euros (the average minimum wage in this period is 347 euros), below the average for the economy, but conforming with the positive firm-size wage premium.

[TABLE 1 (see page 24)]

4 Difference-in-differences estimator

Unconditional estimator

To identify the causal treatment effect, we use a standard difference-in-differences model (Meyer 1995). Let Y_{it}^{Treat} be the outcome of interest for individual i at time t in state $Treat$, where $Treat = 1$ if in a treated firm, and 0 otherwise. Due to the fact that, at time t, individual i cannot be in both states, the individual treatment effect, $Y_{it}^1 - Y_{it}^0$ cannot be computed. However, with an appropriate control group, the difference-in-differences overcomes this limitation by comparing the average behavior before and after the legislative change for the treatment group with the before and after outcomes for the control group.

The identification requires that the average outcomes for treated and controls would have followed parallel paths over time in the absence of the treatment; this assumption is known as the common trend:

$$E[Y_{it}^0 - Y_{it'}^0 \mid Treat = 1] = E[Y_{it}^0 - Y_{it'}^0 \mid Treat = 0], \tag{1}$$

where t' is a time period before the new legislation.

If the assumption expressed in equation (1) holds, the average treatment effect on the treated can be estimated by the sample analogues of

$$\{E[Y_{it} \, | Treat = 1] - E[Y_{it} \, | Treat = 0]\} - \{E[Y_{it'} \, | Treat = 1] - E[Y_{it'} \, | Treat = 0]\}\,, \tag{2}$$

where Y_{it} is the observed outcome for individual i at time t. If treated and control groups are not balanced in covariates, which may occur often in quasi-experimental settings, the difference-in-differences set-up can be extended to accommodate a set of covariates, and the average impact estimated with a linear regression model (Angrist and Pischke 2009).

Conditional estimator

In our empirical setting, we estimate the following conditional difference-in-differences model:

$$\log(y_{it}) = \psi_1 Treat_{it} + \psi_2 After_{it} + \psi_3 After_{it} \times Treat_{it} + X_{it}\beta + \varepsilon_{it}, \tag{3}$$

where y_{it} is one of four possible outcomes for the wage of worker i at time t: (a) monthly base wage; (b) hourly base wage; (c) monthly total wage; or (d) hourly total wage. $After_{it}$ is a dummy variable taking value one for the period after the reform, 2004 to 2008, and zero for the period before the reform, 2002 and 2003. In this first estimation, the treatment indicator, $Treat_{it}$, is defined for each period t, and equals 1 for the *treatment group* (matches in firms with 11-20 workers) and 0 for the *control group* (matches in firms with 21-50 workers). Later, we will consider different definitions of the treatment and control groups. Consequently, the coefficient on the interaction term, $After_{it} \times Treat_{it}$, identifies the causal average treatment effect on the treated due to the policy change.

Despite limiting our study to firms with 11 to 50 workers, there are elements of heterogeneity that we control for with a set of firm, worker and match characteristics. The firm characteristics included in matrix X_{it} are: (i) the logarithm of the number of workers as a proxy for firm size, (ii) the firm age (indicator variables: 1, 2, . . . , 10, 11-15, 16-20, and more than 20 years), (iii) the sector of activity (at 2-digits), (iv) the region (the 23 Portuguese districts), and (v) an indicator of foreign ownership majority. On the worker side, we control for: (vi) gender, (vii) nationality, (viii) age, entering as a quadratic polynomial, and also for (ix) five levels of education (4 or less years; 6 years; 9 years; high school; and college degree). In terms of match characteristics, we control for: (x) white and blue collar positions, (xi) workers on a (regulated) minimum wage, with an indicator variable, and for (xii) tenure, entering as a quadratic polynomial.

The definition of treatment and control units based on the size of the firm opens the possibility for firms and workers to self-select into the treatment and control groups in response to the policy. The fixed effects estimator is designed to address issues of endogeneity in the regressors (Lee 2005). Given that the wage is a match-specific outcome – the result of the joint characteristics of workers and firms – we decided to use a match fixed effect in the estimation of equation (3) (we test the robustness of the results to the inclusion of firm and worker fixed effects). This estimator assumes that the error term $\varepsilon_{it} = \alpha_i + u_{it}$, where the match unobserved component α_i is orthogonal to X_{it} and u_{it} is the idiosyncratic error. Reported standard errors are corrected for clustering.

5 Wages and employment protection: Quasi-experimental evidence

We start by showing that log-wages of treatment and control groups follow a common trend in the before period, validating a key identifying assumption. Then we show that wages of matches in treated firms decrease due to the tighter employment protection. However, we also show that the impact is stronger among new open-ended and fixed-term contracts. More tenured open-ended workers, who benefited directly from the extra protection, pay little to nothing in terms of their labor income.

5.1 Common trend

As discussed, a key identifying assumption in the difference-in-differences estimator is the existence of a common trend between treatment and control units in the period before the reform, which is assumed to continue in the absence of the reform. Therefore, before presenting our estimates of the causal impact of employment protection on wages, we test this hypothesis.

The existence of a common trend in (log)wages prior to the reform can be formally tested with the following specification:

$$y_{it} = \theta_1 Treat_{it} + \theta_2 Time_t + \theta_3 Treat_{it} Time_t + X_{it}\Phi + \varepsilon_{it}, \tag{4}$$

where $Time_t$ is a linear time trend and the remaining variables are defined as in equation (3). The coefficient of the interaction term, $Treat_{it} \times Time_t$, identifies the change in the difference of log-wages over time between treatment and control matches. If the common trend assumption holds, then θ_3 should be statistically non-significant.

The estimates of $\theta_1 - \theta_3$ are presented in Table 2. The before period is extended to include the five years period prior to the reform: 1999 – 2003. We reject the existence of a different growth path of log-wages across treatment and control firms. For the four measures of wages – base and total wages, in monthly and hourly terms – the coefficients on $Treat_{it} \times Time_t$ are all statistically non-significant and quite small to be economically meaningful (between -0.03 and 0.03 percentage points). These results are reassuring for our identification strategy. The choice of the firm size in the control group – firms with 21 to 50 workers – was made to guarantee a common trend. For some of the log-wage measures, expanding the firm size in the control group lead to reject a common trend across treatment and control units.

[TABLE 2 (see page 25)]

5.2 Average treatment effect on the wages of the treated

Are the mandate benefits paid with lower wages?

Following Lazear (1990), we expect that firms will pass on to workers in the form of lower wages, (part of) the higher firing costs. Unfortunately, the exact (average) increase in costs is not known, otherwise we could test for the complete pass-through to wages. Nonetheless, we will discuss this issue based on a proxied value for this cost. We test the impact on wages in the quasi-experimental setting, expecting ψ_3 in equation (3) to be negative. The first panel of Table 3 presents the results for all contracts. The impact on wages is rather uniform across the different wage measures – a decrease of around 0.30 percentage points – although slightly larger for hourly measures (base and total wages per hour). The new legislation caused treated firms to decrease wages relatively to what would have been their behavior had there not been an increase in firing costs. These results are compatible with an inward shift in labor demand, eventually reinforced with an outward shift in labor supply.

[TABLE 3 (see page 26)]

Martins (2009) studies a similar reform, that took place in Portugal in 1989. This reform exempted smaller firms, with less than 21 workers, from the job protection procedural rules listed before. Martins (2009), using firm-level data, finds that small-firm average wages decreased relatively to larger firms, with higher levels of protection, and rationalizes the result with a fall in bargaining power of workers in smaller firms. In our case, firms bargaining power seems to outweight any increase in the worker's bargaining power due to the higher protection. In fact, our result is perfectly coherent with the predictions of Lazear (1990). A possible explanation for the difference in results with Martins (2009) rests on the relative importance of fixed-term contracts. Whereas during our sample period there is a largely segmented labor market, in which the marginal worker is on a temporary contract, in the 80's and early 90's the share of fixed-term contracts was very low. Nowadays, the worker bargaining power is much lower.

But who pays for the protection?

Although the legislation applied exclusively to workers on open-ended contracts, it is plausible that firms may have spread the costs among incumbent and new open-ended contracts, but also to unprotected fixed-term contracts (Boeri 2010). The latter effect is the result of a negative spillover to the wages of fixed-term contracts, whose protection remained unchanged. Incumbent open-ended contracts, with more bargaining power, may be shielded from a direct impact of the reform, in line with insider-outsider theories (Lindbeck and Snower 2001). In these models, employment protection strengthens the workers' bargaining position and prevents wages of incumbent workers from falling. In this case, the wages of new new open-ended matches, which compete with the flows of fixed-term contracts, are expected to adjust much more strongly.

To test for differentiated impacts by contract and tenure, we split the sample into workers on open-ended contracts and workers on fixed-term contracts, and the former into low-tenured (up to 36 months) – the flow of new jobs on open-ended contracts – and high-tenured (more than 36 months). For each group of workers we estimate (3); the magnitude and statistical significance of ψ_3 gives an estimate of the burden supported by each of the three groups of workers.

Overall, the legislation had a small – 0.2 p.p. – impact on the wages of workers on open-ended contracts (second panel of Table 3). However, the impact differs substantially with tenure. For existing open-ended contracts – the incumbents (third panel) – there is a small impact – 0.1 p.p. – on base wages and no impact on total wages. For new open-ended contracts the fall ranges between 0.8 and 0.9 p.p., while for fixed-term contracts wages are 0.5 to 0.7 p.p. lower. These results suggest that firms adjusted wages on the flow of entry jobs, either new permanent jobs or fixed-term contracts (note that the vast majority of new jobs are fixed-term contracts). Firms may face difficulties in adjusting the wage level of existing open-ended contracts due to explicit or even implicit contractual arrangements (Lazear 2011). However, that is not the case for new contracts, which have lower wages than they would have had in the absence of the increase in employment protection, regardless of the type of contract.

We can follow Heckman et al. (2006) and estimate the effect of expected firing costs on the expected discounted value of wages at the time the worker is hired. Using our estimates in Table 3, the wage loss for an average worker in a small firm after the reform amounts to about 2.3 euros for an open ended-contract (a -0.272 percentage points wage loss and an average monthly wage of 838 euros) and 6.5 euros for a fixed-term contract (a -0.842 percentage points wage loss and an average monthly wage of 767 euros). Per year, the figures are 32 and 91 euros, respectively.

Next, we compute the present discounted value of the loss, W, due to the reform. We use an annual discount rate of 8%, i.e. a discount rate (β) of 0.92 and match the average tenure of each group of workers with a specific annual survival probability. We use a survival probability, ρ, of 0.9 for open-ended contracts (8 years of average tenure) and 0.65 for fixed-term contracts (2 years of average tenure). Finally, we compute $W(\psi_3|\beta,\rho) = \sum_{t=0}^{\infty}[\beta\rho]^t$. This is equal to 191 euros for open-ended contracts and to 226 euros for fixed-term contracts. If we take a weighted average of the two, we obtain 200 euros as the average wage reduction.

We can now compare this figure with the expected firing cost. The increase in firing costs equals the legal expenses associated with the longer procedural costs (a two-month extension). If we do a conservative estimation this will amount to 5000 euros. Of course, these are worst-scenario costs, because not all workers are fired and even those fired may not go through this long process (for example there may be off-court arrangements). If we impute a 5% probability of the occurrence of a dismissal associated with these procedures, we get a total expected cost of 250 euros. This implies that around 80% ($200/250 = 0.8$) of the expected firing costs is translated into wages, with fixed-term workers paying one-third and open-ended contract workers paying the remaining.

Overall, these results confirm the two-tier model predictions. Incumbents are shielded from the adjustment process. They do not pay for the extra protection they receive. Thus, the wage premium of permanent employment increases relatively to temporary employment. However, this is not true for the new generation of open-ended contracts, for whom wages are lower due to the extended protection. Firms pass into lower wages most of the additional expected costs with firing.

6 Heterogeneity

Often, policies that apply equally to all workers have differentiated impacts. The margin of adjustment may vary depending on key productive characteristics such as the skill level or the sector of activity. Labor supply elasticities differ across labor market groups, for instance between male and female workers, which will generate different impacts of the policy change. Another source of heterogeneity may arise from the bargaining power of workers, arguably higher for older and white collar workers. Employment protection disproportionately protect workers with higher tenure and higher wages. These workers have a higher incentive to litigate. In this section, we explore these sources of heterogeneity. For the sake of brevity and given the similarity of the results estimated for the different wage measures, we concentrate on the total monthly wage, that captures all the wage adjustment mechanisms of firms.

Age

The results by age show that young workers pay a higher price than older workers (35 or more years) for the additional employment protection (columns (A)-(B) of Table 4). Concentrating on the estimates for the younger, the impact is larger for new open-ended contracts, a wage loss of 0.93 percentage points, which compares with a wage loss of 0.38 percentage points for fixed-term contracts. The larger wage penalty for young workers under new open-ended contracts is fully consistent with the future higher expected costs of layoffs for these workers. On the contrary, young incumbents are shielded from the costs of protection.

Wage losses are not confined to young workers. Older workers on new open-ended contracts also experience wage losses, but those do not prove to be statistical significant. The larger labor market experience prevents older worker from paying a larger share of the new protection.

[TABLE 4 (see page 27)]

Gender

The results by gender present a sharp contrast – Table 4, columns (C) and (D). The reductions in wages are an exclusive of male workers, particularly among those with new open-ended contracts and fixed-term contracts, to whom the losses are close or exceed one percentage point. The wage reduction for older open-ended contracts is much smaller, slightly above 0.25

percentage points, but is statistically significant. The results for females are quite different. There is no impact for all types of contracts and tenure.

These results are fully consistent with a larger labor supply elasticity for female workers, as in Blundell and MaCurdy (1999), and find support in Bertola, Blau and Kahn (2002) model, which shows employment protection having the largest effects on the wages of workers with inelastic supply.

Manufacturing, construction, and services

In columns (E)-(G) of Table 4, we split our sample into three sectors: manufacturing, construction, and services. The results show a substantial degree of heterogeneity across sectors. The largest impact occurs by far in the construction sector; wage losses due to the more stringent employment protection are larger than 1.5 percentage points for new contracts and 0.7 for older open-ended contracts. In the services sector, the impacts estimated are still large, but they do not exceed -0.7 percentage points for new contracts. In the manufacturing sector, the point estimates for new open-ended contracts are similar in magnitude, but only weakly statistically significant.

These results seem to reflect the varying degrees of flexibility in production technology across sectors. Centeno and Novo (2012) show that construction has the higher turnover rate, reflecting the inherent temporary characteristics of construction projects. In the manufacturing sector, contracts tend to last longer and collective bargaining is also higher due to stronger union representation. The differences in specific human capital across sectors may also explain why losses vary. We observe stronger downward adjustments in sectors where specific human capital is typically less important, say services. The high rotation of workers in the construction and services sectors allows also for a larger wage adjustment.

White- and blue-collar matches

The type of employment protection introduced in the reform studied increased the expected cost of employing high-tenured and high-wage workers – mostly white-collar workers. These workers are more likely to litigate and should suffer the largest impact of the policy. The wage loss of high-tenured white collar jobs may reflect this effect. Furthermore, the increase in employment protection for more tenured workers may have shifted the demand away of these workers into workers who are close substitutes, but have low wages and shorter tenures. This may explain the absence of wage losses for new contracts (both open-ended and fixed-term contracts) of white collar workers (columns (H) of Table 4).

Blue collar workers have lower tenure and bargaining power, and the share of fixed-term contracts in new jobs is also larger. The results in column (I) show that, consistent with their lower bargaining power, new open-ended contracts for blue collar workers have a larger wage loss. The results for blue-collar workers on fixed-term contracts are not statistically significant for total wages, but they are much larger for the base wage measures (not shown in the Table).

In general, the flow of new contracts borne the larger wage losses in all groups analysed. This result seems to reflect the highest expected costs of lay-off of the new contracts and the larger bargaining power of incumbent workers. The absence of wage falls (or smaller wage reductions) for more tenured open-ended contracts may reflect the nominal wage rigidity that is usually associated with the workings of internal labor markets in firms. Fixed-term contracts share part of the cost of the policy, without any gain in terms of protection. Note that the estimated impact is a net effect on wages for new temporary contracts. In Centeno and Novo (2012) we show that as a result of this reform firms increased the share of temporary workers and this increase in the demand should have translated into higher wages for these workers. However, as we argued in **?** and **?** put forward very clearly, the marginal worker in Portugal is a temporary worker and employment and wage adjustments operate to a large extent through this type of jobs.

7 Endogeneity of treatment responses and robustness

The thresholds included in the legislation create the possibility for firms and workers to self-select into (or out of) treatment. Whereas the inclusion of match-specific effects may go some way into solving endogeneity problems, it is, nonetheless, informative to redefine the sample under analysis to assess the sensitivity of our results to potential sources of bias.

In this section, we look at the endogeneity of treatment responses coming separately for firms and workers. Table 5 addresses these potential sources of bias. We also study the possibility of an anticipation effect during 2003 and perform a falsification exercise using a placebo treatment group, defined at a fake firm-size threshold.

[TABLE 5 (see page 28)]

Firm's self-selection

Although Figure 1 showed no evidence of firm clustering around the size thresholds, the identification of the causal effect is threatened by this possibility. The usage of match fixed effects controls for all time-invariant unobserved factors that may affect the propensity of firms and workers to self-select into (or out of) treatment. Nonetheless, because fixed-effects are no panacea, to tackle this issue, we perform four different exercises.

Column (A) in Table 5 presents the estimates of the full sample with firm fixed-effects. The impacts computed with firm fixed-effects are more negative than the ones obtained with match fixed-effects. Firm fixed-effects alone only make our results stronger.

In column (B), we set the treatment status in the before period and keep it unchanged in the after period, even if firms changed size. This sample excludes new firms from 2004 onwards, but keeps the treatment and control groups unaffected by firms' sorting decisions. The point estimates are lower than in the baseline sample for fixed-term contracts, but higher for new open-ended contracts. Qualitatively the results are the same – a wage reduction for new contracts and no impact in existing open-ended contracts.

The behavior of firms close to the size thresholds may be of concern, as they may strategically choose a smaller size to avoid additional judicial uncertainty and procedural firing costs. To control for such behavior, we remove from the data firms clustered around each period's threshold (column (C)). In particular, in the before period, with a 20-worker threshold, firms with 18-25 workers are not considered and, in the after period, with a 10-worker threshold, firms with 11 or 12 workers are excluded. Again, all point estimates are larger than in our baseline exercise.

Workers's self-selection

Workers may also non-randomly sort around the thresholds. They may be able to choose their own employment protection regime, moving between firms with a specific size. As with firms, if less productive workers apply to more protected jobs, a negative association between wages and employment protection cannot be interpreted as the causal effect of employment protection on wages.

The worker fixed effects estimator may control for these selection events. The results presented in column (D) of Table 5 are in the range discussed for match fixed-effects, although with a smaller impact for new open-ended contracts.

Workers's and firm's self-selection

Column (E) considers a sample of workers and firms who never changed treatment status. This option allows for firm and worker entry in the post-reform period. The results obtained are larger than in our baseline sample for older open-ended contracts and for fixed-term contracts. This is probably expected. Take workers that move across these firms, they probably do it to avoid wages losses. Thus, once these workers and firms are excluded from the sample, the impact of the new legislation on those less elastic to the reform is larger, specially for incumbent workers that do not change treatment status for whom we obtain a significant wage loss. These workers may be paying high quality white-collar positions in the services sector.

Anticipation effect

One common feature of the type reforms studied here is the fact that they are discussed for quite a long period of time before implementation. This translated into the possibility of firms anticipating the reform effects and starting adjusting their workforce prior to its enactment. In our case, this will translate into an anticipation of new hirings in 2003 and a larger flow of separations to avoid the extra costs of the policy.

Column (F) presents the results obtained with a sample in which we drop all new hires and separations occurred in 2003. The results do not change when compared with our baseline estimates.

Falsification test

In the final column of Table 5, we perform a placebo test using as treatment group the set of firms with 21 to 30 workers and as control group firms with 31 to 50 workers (this is done year-by-year, as in Table 3). The results are reassuring as all coefficients of interest are statistically not significant.

All alternative definitions for the sample are fraught with shortcomings, arising from the fact that they are selected samples of the targeted population. However, our point estimates of the causal effect are robust to these new definitions. Our choice of the match fixed effects estimator seems conservative in that the magnitude of the estimated impact is smaller than with other estimators.

8 Conclusions

Wages adjust downwards to more stringent mandated employment protection. The causal evidence gathered shows that workers pay the extra protection in the form of lower wages. A finding compatible with an inward shift in labor demand – higher expected costs for employers – but also with a labor supply outward shift – the value to workers of the additional protection.

The main findings in our paper are, on the one hand, the large wage loss of new contracts, and on the other hand, the absence of losses in the wages of incumbent workers. We also obtain a large spillover effect of the increased protection of open-ended contracts on wages of workers on fixed-term contracts. This result highlights the strong segmentation of the Portuguese labor market and the channels of wage flexibility introduced by new contracts. This generates a wage premium and is consistent with the reduced role of wages as an incentive for fixed-term workers, whose probability of entering a long-term relationship with the firm is quite reduced (only around 15 percent of these contracts are converted into a permanent one, Centeno and Novo 2012).

The overwhelming evidence that employment protection decreases wages and increases the wage gap between open-ended and fixed-term contracts is in line with most search and flow models (Boeri 2010). However, these results are in contradiction with the argument in Blanchard and Portugal (2001). Their flow model implies that employment protection raises wages by increasing workers' bargaining power. But they fail because their model ignores the nature of flows in two-tier labor markets. As the results in Centeno and Novo (2012) show, segmented labor markets are not characterized by a low level of churning, but instead by a highly asymmetric distribution of churning rates between open-ended and fixed-term contracts. Similar results were found by Schivardi and Torrini (2008) and Hijzen, Mondauto and Scarpetta (2013) for Italy. The rapid adjustment of wages to the increase in employment protection is the price complement to the flows adjustment; higher churning, lower wages, confirming the high substitutability between the two contracts. Fixed-term workers lose in both dimensions.

The previous finding in Centeno and Novo (2012) that fixed-term workers bear most of the adjustment cost with higher churning rates is complemented with evidence that they also suffer the larger wage drops. This result is not a full evaluation of the costs and benefits of employment protection as we do not tackle the potential welfare benefits of more secure job positions. But we do know that this legal protection does come at a cost.

The evidence available for other countries is mixed. For the U.S., Autor et al. (2006) find no evidence of an impact on wages of wrongful-discharge laws, and Leonardi and Pica (2010) find a negative impact of an increase in severance payments, exploring a reform of the Italian Labor Code that extended severance payments to firms with fewer than 15 workers. However, these estimates apply to all workers equally, limiting their usefulness to understand what is the impact of employment protection in two-tier labor markets.

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Figures

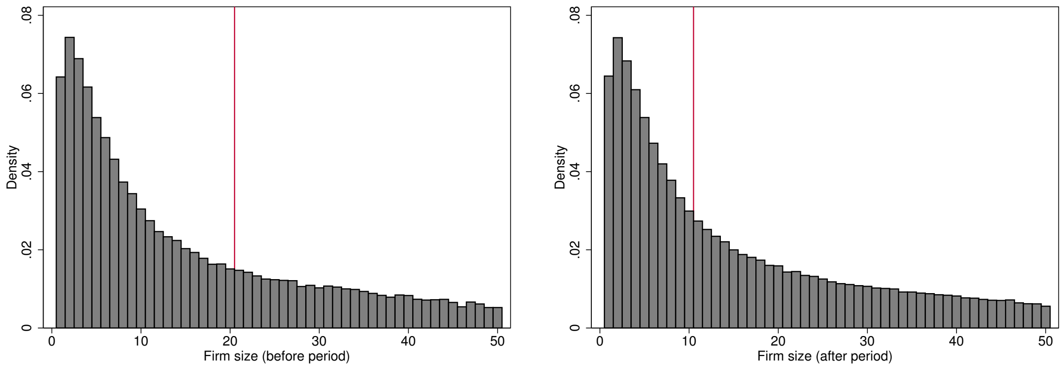

Figure 1: Histograms of firm size (workers per firm) in the before and after periods. Vertical lines indicate firm-size employment protection thresholds; to the right employment protection is stricter.

Tables

Table 1: Summary statistics: Match-level data, 2002-2008

Variable (match level)	Mean	Std. Deviation
Fixed-term contracts (in %)	25.8	43.8
Base wage	657.4	356.1
Hourly base wage	4.0	2.3
Total wage	807.8	442.2
Hourly total wage	4.8	2.8
Age (in years)	37.4	10.9
Educational level, percentage of workers with:		
4 or less years	27.9	44.9
4-6 years	23.9	42.6
7-9 years	20.7	40.5
10-12 years	17.9	38.3
College	9.6	29.5
Females (in %)	41.6	49.3
Immigrants (in %)	4.0	19.5
Minimum wage (in %)	8.3	27.5
Tenure (in months)	84.1	89.5
Firm size (average number of workers)	25.6	11.2
Foreign ownership (in %)	3.5	18.3
Number of matches	1 405 800	
Number of workers	1 302 865	
Number of firms	56 680	
Number of observations (matches × year)	3 581 305	
Open-ended contract	2 656 122	
Fixed-term contract	925 183	
Before		
Treatment	372 770	
Control	513 638	
After		
Treatment	1 128 155	
Control	1 566 742	

Notes: *Quadros de Pessoal*, match-level values 2002-2008. The "Before" period corresponds to 2002-2003 and the "After" period to 2004-2008. Each period, a treatment match is in a firm with 11 to 20 workers and a control match in a firm with 21 to 50 workers.

Table 2: Common trend estimation

	Base wage		Total wage	
	Monthly	Hourly	Monthly	Hourly
Treat × Time	0.028	-0.017	0.005	-0.030
	(0.277)	(0.528)	(0.892)	(0.458)
Treat	-0.186	-0.128	-0.185	-0.124
	(0.110)	(0.293)	(0.322)	(0.513)
Time	4.752	4.157	5.497	4.865
	(0.000)	(0.000)	(0.000)	(0.000)
No of observations		1766260		

Notes: Match (worker-firm) fixed effects estimates. Values in percentage points with p-values in parentheses. The estimation window corresponds to the "before" period, 2002 and 2003. Treatment units identify workers in firm with 11 to 20 workers and a control units workers in firm with 21 to 50 workers. The estimates are computed for all workers. See equation (4) for a list of control variables included in the regressions.

Table 3: Difference-in-differences estimation

	Base wage		Total wage	
	Monthly	Hourly	Monthly	Hourly
All contracts	-0.289	-0.317	-0.272	-0.308
	(0.000)	(0.000)	(0.001)	(0.000)
		3581305		
Open-ended contracts	-0.227	-0.226	-0.213	-0.227
	(0.000)	(0.000)	(0.012)	(0.008)
		2656122		
Older (more than 36 months)	-0.094	-0.103	-0.061	-0.089
	(0.127)	(0.105)	(0.512)	(0.342)
		1990753		
Newer (up to 36 months)	-0.623	-0.707	-0.843	-0.885
	(0.006)	(0.003)	(0.023)	(0.018)
		665369		
Fixed-term contracts	-0.644	-0.735	-0.508	-0.537
	(0.001)	(0.000)	(0.106)	(0.088)
		925183		

Notes: Match (worker-firm) fixed effects estimates of the After $\times$ Treat coefficient; values in percentage points with p-values in parentheses adjusted for firm clustering. The "before" period corresponds to 2002 and 2003; the "after" period to 2004-2008. For each period, treatment units identify workers in firm with 11 to 20 workers and a control units workers in firm with 21 to 50 workers. The estimates are computed for four samples: for all workers; workers with open-ended contracts with tenure exceeding 36 months "Older open-ended contracts"; workers with open-ended contracts with tenure not exceeding 36 months "Newer open-ended contracts"; and workers with fixed-term contracts. Besides the treatment variables, the control variables included in the regressions are: (i) Dummy variable for minimum wage earners; (ii) Female indicator; (iii) Quadratic polynomial in (log) age; (iv) Quadratic polynomial in (log) tenure months; (v) Immigrant indicator; (vi) Educational attainment indicators: (a) 4-6 years, (b) 7-9 years, (c) 10-12 years, and (d) college degree. Workers with 4 or less years of schooling are the reference group; (vii) Foreign ownership majority indicator; (viii) Log firm size measured by the number of workers; (ix) Firm age dummies: 1,2,...,10, 11-15, 16-20 years, with the reference group, 21 or more years; (x) District indicators; and (xi) sector of activity (at 2-digits) indicators.

Table 4: Match, worker and firm heterogeneity

Total monthly wage	Age		Gender		Sector			Skill	
	< 35	≥ 35	Male	Female	Manuf	Constr	Services	White	Blue
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
Older open-ended contracts	0.095	-0.032	-0.272	0.157	-0.119	-0.711	-0.163	-0.498	0.116
	(0.598)	(0.780)	(0.035)	(0.228)	(0.400)	(0.035)	(0.236)	(0.035)	(0.260)
	675811	1314942	1149562	841191	738038	192252	1060463	423839	1566914
New open-ended contracts	-0.933	-0.653	-1.194	-0.310	-0.784	-2.149	-0.769	-0.660	-0.852
	(0.073)	(0.240)	(0.018)	(0.553)	(0.164)	(0.058)	(0.177)	(0.456)	(0.043)
	383644	281725	407159	258210	202480	99242	363647	126597	538772
Fixed-term contracts	-0.382	-0.296	-0.966	0.078	0.283	-1.581	-0.716	-0.528	-0.289
	(0.387)	(0.535)	(0.033)	(0.852)	(0.602)	(0.103)	(0.101)	(0.553)	(0.402)
	545352	379831	534680	390503	198343	117447	609393	143517	781666

Notes: Match (worker-firm) fixed effects estimates of the average treatment effect on the treated (After $\times$ Treat variable). Values in percentage points with p-values in parentheses adjusted for clustering. The "before" period corresponds to 2002 and 2003; the "after" period comprises 2004 to 2008. For each period, treatment units identify workers in firm with 11 to 20 workers and a control units workers in firm with 21 to 50 workers. See Table 3 for a list of control variables included in the regressions.

Table 5: Firm and worker robustness

Total monthly wage	Firms				Workers		Anticipation	Falsifcation
	Firm Fixed-E (A)	Status set before (B)	Always same status (C)	T: [13,17]; C: [26,50] (D)	Worker Fixed-E (E)	Same status (F)	Excludes 2003 (G)	pT: [21,30]; pC: [31,50] (H)
Older open-ended contracts	-0.482	0.093	-0.218	0.043	-0.008	-0.179	-0.061	0.062
	(0.006)	(0.336)	(0.049)	(0.718)	(0.929)	(0.103)	(0.512)	(0.590)
	1990753	1581376	1434125	1719743	1990753	1535549	1990753	1179543
New open-ended contracts	-0.745	-0.936	-0.620	-1.055	-0.748	-0.682	-0.843	-0.337
	(0.022)	(0.030)	(0.239)	(0.041)	(0.044)	(0.180)	(0.023)	(0.534)
	665369	414408	403874	554046	665369	500132	665369	343943
Fixed-term contracts	-0.763	-0.417	-0.546	-0.558	-0.554	-0.613	-0.508	0.111
	(0.015)	(0.209)	(0.187)	(0.168)	(0.074)	(0.136)	(0.106)	(0.782)
	925183	581116	559863	806001	925183	720490	925183	556894

Notes: Match (worker-firm) fixed effects estimates of the average treatment effect on the treated (After × Treat variable). Values in percentage points with p-values in parentheses and number of observations in square brackets. The "before" period corresponds to 2002 and 2003; the "after" period comprises 2004 to 2008. In panel (A), the treatment and control status are defined in the before period and kept the same each year throughout the after period regardless of the firm size. In panel (B), we consider only firms that never changed treatment status during the entire sampling period, i.e., it excludes movers by considering treatment firms that always had 11 to 20 workers and similarly control firms that always had 21 to 50 workers. In panel (C), we consider only workers that never changed treatment status during the entire sampling period. That is, treated workers are those that always worked for firms with 11 to 20 workers and similarly control workers are those that always worked for firms with 21 to 50 workers. In panel (D), firms that clustered around the size thresholds are eliminated from the sample. In particular, in the before period, firms with 18 to 25 workers are excluded and, in the after period, firms with 11 or 12 workers are also excluded; treatment status is defined each period. See Table 3 for a list of control variables included in the regressions.