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IZA DP No. 8702

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Mário Centeno
Álvaro A. Novo

December 2014

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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Mário Centeno
Banco de Portugal,
ISEG, Universidade Técnica and IZA

Álvaro A. Novo
Banco de Portugal,
Universidade Lusófona and IZA

Discussion Paper No. 8702
December 2014

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 8702
December 2014

ABSTRACT

Paying for Others' Protection: Causal Evidence on Wages in a Two-Tier System*

In a segmented labor market, theory predicts that employment protection has an asymmetric impact on entry and incumbent wages. We explore a reform that increased the protection of open-ended contracts for a well-defined subset of firms, while leaving it unchanged for other firms. The causal evidence points to a reduction in wages for new open-ended and fixed-term contracts and no impact for more tenured workers. The reductions estimated for entrants oscillate between -0.9 and -0.5 p.p., covering a significant part of the expected increase in firing costs. Firms with larger shares of fixed-term contracts shifted the burden to these workers.

JEL Classification: J31, J32, J63

Keywords: wages, two-tier systems, quasi-experiment, employment protection

Corresponding author:

Álvaro A. Novo
Banco de Portugal
Economics Department
Av. Almirante Reis, 71-6
1150-012 Lisbon
Portugal
E-mail: anovo@bportugal.pt

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

1 Introduction

In the last decades, partial reforms of European labor markets increased the protection gap between open-ended and fixed-term contracts. Nowadays, due to this two-tier nature of labor markets, 1 out of 6 salaried workers in the Euro area has a temporary contract, but in Spain and Portugal the shares reach one-fourth. Because two-tier systems interfere with the incentives to investment in human capital and, ultimately, with economic growth, it is important to understand the consequences of such partial reforms. In particular, how does a two-tier reform affect wages? And with dual contracts, who pays for the extra protection? We answer these questions in the context of a two-tier reform of the Portuguese labor code that increased the protection of open-ended contracts. The resulting quasi-experiment shows that the additional protection is paid in the form of lower wages, but only among entry jobs.

In competitive markets, Lazear (1990) shows that the cost of protection will be passed on 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. For instance, Boeri (2010) predicts that increasing the protection of open-ended contracts will lead to a higher 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 their own protection. Concurrently, new open-ended or fixed-term contracts, with lower bargaining power, will pay a large share of the total cost of the protection of incumbents.

The growing importance of two-tier labor markets has motivated a resurgence of interest in the topic and Boeri (2010), Bentolila, Cahuc, Dolado and Le Barbanchon (2012), and Cahuc, Charlot and Malherbet (2012) extended the initial work of Abowd, Corbel and Kramarz (1999). However, establishing a causal relationship between employment protection and wages has been a challenge for the empirical literature. Two exceptions are Autor, Donohue III and Schwab (2006) and Leonardi and Pica (2013), but they do not provide direct evidence of the impact of two-tier labor markets. We overcome this by exploring a two-tier 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 (the control group, which we restrict to firms with 21 to 50 workers).

In this setting, we ask if wages of workers in treated firms reacted to the tighter employment protection. For that purpose, we use an administrative matched employee-employer dataset, *Quadros de Pessoal*, covering all private sector matches, for the 2002 to 2008 period. The difference-in-differences estimates indicate that wages in treated firms fell 0.3 percentage points (p.p.). This is true for base wages and total compensation, but also for the hourly counterparts. However, like suggested by the theory, the impact differs markedly between incumbents and new contracts, regardless of the type of contract. We obtain a fall in total wages of new contracts up to 0.9 p.p., while incumbents' are not impacted. Back-of-the-envelope computations suggest that these wage reductions cover at least half of the expected increase in firing costs.

As predicted by theory, increased employment protection results in a larger wage premium of incumbent open-ended contracts. In segmented labor markets, 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 and temporary jobs ends up paying for the extra protection. The empirical identification of a causal spillover on wages raises additional concerns on the optimality of a labor market split by contract type. Our results favor the adoption of non-discriminating contractual relationships, contributing to the current policy debate on the single contract in European countries.

2 The reform of employment protection

The Portuguese labor market is an extreme case of a two-tier system. This section describes 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

The Portuguese labor code offers two contracts concurrently: fixed-term and open-ended contracts. Because there are no major legal restrictions on temporary hiring, the labor code makes the two contracts legal (and economic) substitutes. The difference in severance payments for permanent and fixed-term contracts is 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 (Furtado Martins 2012). Altogether, the procedures extended the length of dismissal for cause processes, 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 awarded in court.

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 up to 20 workers from the legal procedures listed above. The reform of the labor code changed this threshold to 10 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 it remained the same for all other firms. Even though the costs of the procedural requirements are not explicitly defined in the legislation, firms may incorporate these expected costs and 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. 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 both cases, there are no noticeable discontinuities in the distribution of firms. Although it does not rule out strategic behavior by firms, it suggests that the policy does not influence the size distribution; firms adjust other dimensions, such as wages and labor force composition.

[FIGURE 1 (see page 21)]

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, this and other 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. Nonetheless, we will perform several robustness checks and test extensively the sensitivity of our results to the specific choice of the treatment and control groups.

3 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 will 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. The outsiders, with less bargaining power, stand to lose, in the form of lower wages.

4 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. 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 have 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 have they become 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 on this type of contract. 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 22)]

5 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

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

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 in quasi-experimental settings, the difference-in-differences setup 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. With movers, there may be also dependence across observations. Panel data models are a solution and, in particular, the fixed effects estimator addresses also the issue 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 also test the robustness of the results to the options of firm or 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 account for clustering.

6 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, 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.

6.1 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 that continues in the absence of the reform. Therefore, before presenting our causal estimates, 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. We reject the existence of a different growth path of log-wages across treatment and control firms, which is reassuring for our identification strategy. 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.[1]

[1]The common trend was also tested with a five-year window, 1999-2003, and the results convey the same message. The point estimates and $p-$values obtained were: 0.028 (0.277), -0.017 (0.528), 0.005 (0.892), and

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 would lead to the rejection of a common trend.

[TABLE 2 (see page 23)]

6.2 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. 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 p.p. – 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 24)]

Martins (2009) studies a similar reform that took place in Portugal in 1989. This reform exempted firms with less than 21 workers from the procedures listed in section 2. Using firm-level data, Martins (2009) finds that lower protection caused average wages to decrease, which is compatible with a fall in the bargaining power of workers. In our quasi-experiment, firms are able to pass on to wages the cost of the higher protection, outweighing the increase in the worker's bargaining power. In an argument that we will explore in the next section, the difference in the results may rest on the relative importance of fixed-term contracts. In our sample period, due to labor market segmentation, the marginal worker is on a fixed-term contract, whereas in the 80s and early 90s, the share of fixed-term contracts was very low. Therefore, with the marginal worker with lower bargaining power, the balance of power may have tipped off in favor of firms.

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. Leonardi and Pica (2013) find

-0.030 (0.458), respectively, for base, hourly base, total, and hourly total wages.

a negative impact of a reform of the Italian labor code that extended severance payments to firms with fewer than 15 workers. However, these studies apply to all workers equally, limiting their usefulness to understand the impact of employment protection in two-tier labor markets.

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 all workers, fixed-term contracts included (Lindbeck and Snower 2001, Boeri 2010).

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. Then, the former are divided into the flow of new jobs on open-ended contracts (tenured up to 36 months, which is also the maximum length of fixed-term contracts before 2004) and the incumbents (with more than 36 months of tenure). For each group of workers, we estimate equation (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 impact on the wages of workers on open-ended contracts, -0.20 p.p., (second panel of Table 3). However, the impact differs substantially with tenure. For the incumbents on open-ended contracts (third panel), there is a small impact on base wages, 0.1 p.p., 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 than in the counterfactual. 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 know that firms adjusted downwards wages to cover for higher expected firing costs. But how do we interpret the magnitude of the wage reductions? We put it in perspective with the additional firing costs. Table 4 presents back-of-the-envelope computations. The estimated impacts on total wages reported in column (3) of Table 3 are used to compute the wage loss for an average worker in a treatment firm in 2008. This is done separately for the three groups of workers. The annual reduction in wages ranges from 8 euros for more tenured open-ended contracts to 95 euros for new open-ended contracts. With the composition of employment in 2008, the average reduction in the wage bill of a worker is 37 euros.

[TABLE 4 (see page 25)]

We can compare this figure with the expected firing cost. The increase in firing costs equals the legal expenses associated with the additional uncertainty and longer procedural costs in dismissal cases. We know that the separation rate in treated firms is 27%, but there is no information on the share of these workers that litigate. The incidence of litigation is usually low, even more in smaller firms, and restricted to dismissals for cause arising from disciplinary or economic reasons. Thus, we consider three different scenarios for the probability of litigation: low, 5% of all separations involve a formal process; medium, 7.5%; and high, 10%. We assume a cost of 2,500 euros per process, half the value estimated for Italy by Leonardi and Pica (2013). This seems reasonable given the simpler Portuguese procedure and the income differences of the two countries. With these assumptions, the expected increase in firing costs ranges from 34 to 68 euros. This implies that the wage reduction covers between half and all of the expected increase in firing costs.

Overall, these results confirm two-tier model predictions. Mandated benefits result in lower wages, but incumbents do not fully pay for the extra protection received. 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. Our results are also compatible with the largest business cycle wage elasticity of entry jobs (Martins et al. 2012). Although we do not address the potential welfare benefits of more secure employment, we do know that this legal protection does come at a cost.

7 The two-tier intensity

In the previous section, we concluded that there is a sharing rule of the costs of the extra protection, according to which only the flow of new contracts have their wages reduced. If that is the case, the intensity of the two-tier (share of fixed-term contracts) within each firm will result in different abilities to adjust to the legislation. For instance, consider two firms with 16 workers; firm A has 14 open-ended contracts, while firm B has 8 such contracts. Firm B has a higher two-tier intensity than A. Do they react differently?

In order to test this, we split the sample in two groups: below and above the median share of fixed-term contracts, 18%, in our sample. For the sake of brevity and given the similarity of the impacts estimated for the different wage measures, we concentrate on the total monthly wage. The results are presented in Table 5.

[TABLE 5 (see page 25)]

First note that both for low and high two-tier intensity firms, the overall impact, -0.25 p.p., is quite similar to the one reported in column (3) of Table 3. This is an important result as it shows that the average reduction in wages does not depend on the two-tier intensity; firms always incorporate the additional firing costs, the question is who pays? There is a sharp contrast in the spillover to fixed-term contracts. Firms with high two-tier intensity use exclusively fixed-term contracts to finance the extra costs, -0.69 p.p.; firms that rely on the permanent tier use only new open-ended contracts to cover for the costs, -0.83 p.p.. In both cases, because the adjustments are restricted to specific groups, the coefficients are larger than in our baseline regression.

It is important to note that the heterogeneity along the intensity of the two-tier regime is fully consistent with Boeri (2010) – some workers pay for the others' protection. The two-tier nature of the labor market imposes on fixed-term contracts a share of the cost of the policy, without any gain in terms of protection given the low conversion rate. The estimated impact captures a net effect on wages for new temporary contracts because Centeno and Novo (2012*a*) show that this reform caused an increase in the demand for these contracts. However, as Centeno and Novo (2012*b*) and Reis (2013) put forward, the marginal worker in Portugal is on a fixed-term contract and wage adjustments operate to a large extent through this type of jobs.

8 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 generates different policy impacts. 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 also higher incentives to litigate. In this section, we explore these sources of heterogeneity.

Age

The results by age show that young workers (less than 35 years) pay a higher price than older workers for the additional employment protection (columns (A)-(B) of Table 6). Concentrating on the estimates for the younger, the impact is larger for new open-ended contracts, a wage loss of 0.93 p.p., which compares with a wage loss of 0.38 p.p. 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 shields older worker from the cost of the new protection.

[TABLE 6 (see page 26)]

Gender

The results by gender present a sharp contrast, Table 6, 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, for whom the losses are close or exceed one percentage point. The wage reduction for tenured males on open-ended contracts is much smaller, slightly above 0.25 p.p.. These results are fully consistent with a lower labor supply elasticity for male workers (Blundell and MaCurdy 1999). Bertola, Blau and Kahn (2002) show also that employment protection has the smallest effect with an elastic labor supply.

Manufacturing, construction, and services

In columns (E)-(G) of Table 6, 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; the wage losses due to the more stringent employment protection are larger than 1.5 p.p. for new contracts and 0.7 p.p. for older open-ended contracts. In the services sector, the impacts estimated are still large, but they do not exceed -0.7 p.p. for new contracts. In the manufacturing sector, the point estimates are not statistically significant, suggesting that this sector did not adjust.

These results seem to reflect the varying degrees of flexibility in production technologies across sectors. Centeno and Novo (2012*a*) 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, therefore, may 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 from these workers into close substitutes workers, but with lower 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 (column (H) of Table 6).

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.

9 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. As pointed out by Lee (2005), the consideration of fixed effects may go some way into solving this endogeneity. Nonetheless, it is informative to redefine the sample under analysis to assess the sensitivity of our results to potential sources of bias.

This section looks at the endogeneity of treatment responses coming separately for firms and workers. 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.

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, we tackle this issue with four different exercises, which we compare with the baseline estimation (reported again in column (A) of Table 7).

[TABLE 7 (see page 27)]

The first robustness considers the baseline sample, but estimated with firm fixed effects rather than match fixed effects. The impacts reported in column (B) are more negative than the ones obtained in the baseline estimation. Firm fixed effects alone only make our results stronger.

In column (C), the treatment status is set in the before period and kept 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 on existing open-ended contracts.

However reassuring the results hitherto, the major concern in our identification is still the behavior of firms close to the size thresholds. Firms may strategically choose a smaller size to avoid the 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 (D)). In particular, in the before period, with a 20-worker threshold, firms with 18-25 workers are excluded and, in the after period, with a 10-worker threshold, the exclusion covers firms with 11 or 12 workers. Again, all point estimates are larger than in our baseline exercise. This suggests that slashing their labor force is not a technologically valid or efficient option and that instead firms adjust wages.

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 (E) are in the range discussed for match fixed effects, although with a smaller impact for new open-ended contracts.

Anticipation effect

One common feature to this type of reforms is the fact that they are discussed before implementation. To avoid the new extra costs of the policy, firms may start adjusting their workforce prior to the enactment of the law. In particular, firms may anticipate to 2003 separations and hires. Column (F) presents the results obtained with a sample in which all new hires and separations occurred in 2003 are dropped (in the latter case, the corresponding 2002 employment matches). There is no sign of anticipation as the results do not change when compared with our baseline estimates.

Falsification test

In the final column of Table 7, 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. The results are reassuring as all coefficients of interest are not statistically significant.

All alternative sample definitions 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.

10 Conclusions

Wages adjust downwards to 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 contribution of our paper to the literature on labor market segmentation is that wage losses accrue to new contracts, while incumbent workers do not lose. There is a large negative spillover effect to the wages of workers on fixed-term contracts, whose protection remained the same. Whenever the two-tier intensity is important within firms, fixed-term workers pay for others' protection. This result highlights the strong segmentation of the Portuguese labor market and the channels of wage flexibility introduced by new contracts (Martins et al. 2012). This generates a wage premium for permanent jobs and is consistent with the reduced role of wages as an incentive device for fixed-term workers, whose probability of entering a long-term relationship with the firm is quite low (only around 15% of these contracts are converted into a permanent one, Centeno and Novo 2012*a*).

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 recent 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, failing to match the evidence because it ignores the nature of flows in two-tier labor markets. As the results in Centeno and Novo (2012*a*) 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. The rapid adjustment of wages to the increase in employment protection is the price complement to the flows adjustment: higher churning matched with lower wages.

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Figures

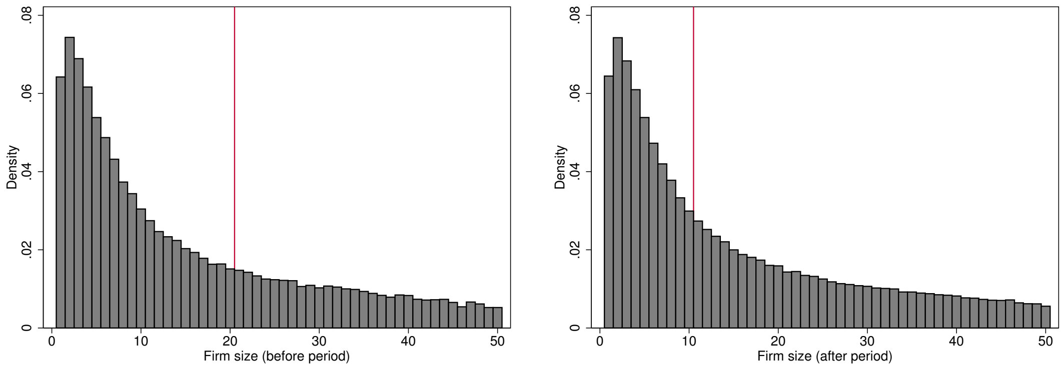

Figure 1: Histograms of firm size (workers per firm) in the before and after periods. Vertical lines indicate firm-size employment protection thresholds; to the right employment protection is stricter.

Tables

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

Variable	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 (2002–2003)		
Treatment	372 770	
Control	513 638	
After (2004–2008)		
Treatment	1 128 155	
Control	1 566 742	

Notes: 2002-2008 data from *Quadros de Pessoal*. The "before" period corresponds to 2002 and 2003; the "after" period to 2004-2008. For each period, treatment units identify workers in firms with 11 to 20 workers and control units workers in firms with 21 to 50 workers.

Table 2: Common trend estimation

	Base wage		Total wage	
	Monthly	Hourly	Monthly	Hourly
Treat × Time	0.065	-0.009	0.024	-0.050
	(0.452)	(0.920)	(0.874)	(0.743)
Treat	0.029	0.107	0.373	0.484
	(0.888)	(0.608)	(0.311)	(0.190)
Time	3.456	3.422	4.019	3.929
	(0.000)	(0.000)	(0.000)	(0.000)
No of observations	945779			

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 firms with 11 to 20 workers and control units workers in firms 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 × Treat coefficient; values in percentage points with p-values in parentheses adjusted for clustering. The "before" period corresponds to 2002 and 2003; the "after" period to 2004-2008. For each period, treatment units identify workers in firms with 11 to 20 workers and a control units workers in firms with 21 to 50 workers. The estimates are computed for four samples: 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: Wage losses and procedural costs: Back-of-the-envelope computations

	Wage losses		
	Old open-ended contracts	New open-ended contracts	Fixed-term contracts
Percentage wage loss[(1)]	-0.061%	-0.843%	-0.508%
Average total wage in 2008 (in euros)	928	809	782
Annual expected wage loss (in euros)[(2)]	7.93	95.45	55.62
Share of contracts in 2008	0.53	0.17	0.30
Average wage loss (in euros)[(3)]		37.11	
	Litigation and expected costs		
	Low	Medium	High
Probability of litigation[(4)]	5%	7.5%	10%
Separation rate of open-ended contracts		27%	
Direct cost of litigation (in euros)		2,500	
Expected increase in procedural costs (in euros)	33.75	50.63	67.50
	Coverage ratio		
Coverage ratio of procedural costs by wage loss	110%	73%	55%

Notes: (1) Coefficients from column (3) in Table 3. (2) Portuguese workers receive 14 base wages. (3) Average wage loss given the share of contracts. (4) Probability of litigation of workers dismissed for a cause such as disciplinary or economic.

Table 5: Difference-in-differences estimation by two-tier intensity

	Share of fixed-term contracts	
Total monthly wage	Low	High
All contracts	-0.255	-0.235
	(0.009)	(0.086)
	1790942	1790363
Open-ended contracts	-0.244	-0.052
	(0.013)	(0.748)
	1680160	975962
Older (more than 36 months)	-0.086	0.065
	(0.424)	(0.721)
	1265885	724868
Newer (up to 36 months)	-0.826	-0.754
	(0.052)	(0.300)
	414275	251094
Fixed-term contracts	1.276	-0.694
	(0.195)	(0.036)
	110782	814401

Notes: Match (worker-firm) fixed effects estimates of the After $\times$ Treat coefficient; values in percentage points with $p-$values in parentheses adjusted for clustering and below the number of observations. The estimation samples are splitted according to the two-tier intensity, low and high, respectively firms with a share of fixed-term contracts below and above the mediam. See notes to Table 3 for other estimation details.

Table 6: 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 and below the number of observations. The "before" period corresponds to 2002 and 2003; the "after" period comprises 2004 to 2008. For each period, treatment units identify workers in firms with 11 to 20 workers and control units workers in firms with 21 to 50 workers. See Table 3 for a list of control variables included in the regressions.

Table 7: Firm and worker robustness

Total monthly wage	Baseline (A)	Firm Fixed-E (B)	Firms Status set before (C)	T: [13,17]; C: [26,50] (D)	Workers Worker Fixed-E (E)	Anticipation Excludes 2003 flows (F)	Falsifcation pT: [21,30]; pC: [31,50] (G)
Older open-ended contracts	-0.094	-0.482	0.093	0.043	-0.008	-0.061	0.062
	(0.127)	(0.006)	(0.336)	(0.718)	(0.929)	(0.512)	(0.590)
	1990753	1990753	1581376	1719743	1990753	1990753	1179543
New open-ended contracts	-0.623	-0.745	-0.936	-1.055	-0.748	-0.843	-0.337
	(0.006)	(0.022)	(0.030)	(0.041)	(0.044)	(0.023)	(0.534)
	665369	665369	414408	554046	665369	665369	343943
Fixed-term contracts	-0.644	-0.763	-0.417	-0.558	-0.554	-0.508	0.111
	(0.001)	(0.015)	(0.209)	(0.168)	(0.074)	(0.106)	(0.782)
	925183	925183	581116	806001	925183	925183	556894

Notes: Fixed effects estimates of the average treatment effect on the treated (After × Treat variable); values in percentage points with p-values in parentheses adjusted for clustering and below the number of observations. The "before" period corresponds to 2002 and 2003; the "after" period comprises 2004 to 2008. Panel (A) replicates the baseline estimates. Panel (B) considers firm fixed-effects. In panel (C), 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 (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. Panel (E) replicates the baseline procedure but it considers worker fixed-effects. Panel (F) considers the possibility of anticipation effects by eliminating from the sample observations that generated worker flows in 2003; in particular, it excludes from the sample all workers hired in 2003 and from 2002 all workers that are accounted as separations in 2003. In panel (G), the falsification exercise considers as a control group firms with 31 to 50 workers and as a pseudo-treatment group firms with 21 to 30 workers. See Table 3 for a list of control variables included in the regressions.