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Severance pay reforms, worker flows, and labor market duality

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Abstract
This paper studies how cuts to statutory severance pay affect worker flows, contractual allocation, firm-level hiring composition, and wages in the Portuguese labor market. The analysis exploits the 2011–2013 reforms, which reduced the transfer component of dismissal protection for both fixed-term and open-ended contracts while leaving most procedural safeguards intact. Using matched employer–employee data for 2010–2019, we combine sharp legal-timing contrasts around the November 2011 and October 2013 cutoffs with an exposure-based IV/control-function design that leverages heterogeneity in admission cohort, tenure, contract type, and grandfathered rights. The results show that lower severance pay does not uniformly improve reallocation. Around the November 2011 cutoff, separations shift away from exits from private-sector employment toward job-to-job moves, whereas the October 2013 cutoff does not reproduce this pattern, instead displaying weaker job-to-job responses and a more uneven reduction in exits. In the broader exposure estimates, lower severance entitlements are associated with higher turnover, but the composition of that turnover varies across worker groups and contract types, combining selective conversion from fixed-term to open-ended jobs with increased reliance on temporary contracts. Within continuing worker–firm matches, wage–severance associations are economically small, providing little evidence that lower severance rights were offset by higher wages. Overall, the reforms reshaped turnover and contract use without delivering uniform improvements in labor market reallocation, and temporary–permanent duality remains a central feature of the post-reform labor market.

Keywords: severance pay reforms; employment protection legislation; worker flows; wages; labor-market duality
JEL codes: J65; J63; J41; J23; J31

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1 Introduction

Employment protection legislation remains one of the most contested labor-market institutions in applied labor economics (Lazear, 1990; Blanchard and Landier, 2002; Boeri, 2011; OECD, 2020). A central question is whether reducing dismissal costs improves worker reallocation or instead reshapes adjustment in ways that leave labor-market segmentation largely intact. We study this question by focusing on statutory severance-pay entitlements, the transfer component of dismissal costs.[1] This margin is especially important in dual labor markets because firms can respond not only by changing separations and hiring, but also by shifting adjustment between fixed-term and open-ended contracts (Cahuc and Postel-Vinay, 2002; Boeri, 2011; Bentolila et al., 2020).[2]

Portugal offers a useful setting in which to study this issue. Before the reforms analyzed here, permanent contracts were heavily protected, while temporary contracts provided employers with a substantially more flexible adjustment margin, a structure long associated with labor-market duality in Southern Europe (Blanchard and Landier, 2002; Boeri, 2011; Bentolila et al., 2020). Between 2011 and 2013, a sequence of reforms reduced statutory severance-pay accrual for both contract types, changing the transfer component of dismissal protection while leaving procedural and legal-uncertainty elements largely intact.[3] This episode therefore provides a useful setting for studying how reductions in severance-pay schedules in a strongly dual labor market are associated with worker turnover, contractual upgrading from fixed-term to open-ended contracts, firm hiring composition, and wages.

This policy question was also explicit in the reform program itself. The 2011 Memorandum of Understanding on Specific Economic Policy Conditionality linked employment-protection reform, and severance-pay reform in particular, to reducing labor-market segmentation, fostering job creation, and easing worker transitions across firms and sectors (European Commission, 2011b). Our results therefore speak not only to the academic debate on employment protection, but also to a concrete policy logic advanced by national authorities and international institutions during the crisis-era reform program.

[1] Throughout, the paper sometimes refers to "severance-pay" and sometimes to "dismissal-cost." The empirical object is the statutory severance entitlement. The broader dismissal-cost language is used only as shorthand for the transfer component of dismissal protection, since the reforms mainly changed severance schedules while other components of dismissal protection moved much less.

[2] The terms "fixed-term" and "temporary" are used interchangeably, as are "open-ended" and "permanent".

[3] The severance-pay schedule was changed again more recently under Law 13/2023, which raised statutory severance accrual to 14 days of wages per year of service for permanent contracts and 24 days for temporary contracts.

The literature gives strong reasons to expect heterogeneous responses, but leaves a specific gap in dual labor markets. Existing work shows that employment-protection reforms can affect hiring, separations, and contract use, with effects depending on institutional design and on firms' ability to substitute across contractual margins (Kugler and Pica, 2008; Hijzen et al., 2017; Boeri and Garibaldi, 2019; Cahuc et al., 2022). Less is known about how a broad reduction in severance-pay schedules affecting both fixed-term and open-ended contracts is associated jointly with worker-flow composition, contractual upgrading, firm hiring composition, and wages. It is also uncommon for this literature to combine broader exposure-based estimates with threshold-local comparisons around sharp legal timing, especially in a crisis period in which macroeconomic shocks and cohort composition complicate causal interpretation.

The paper contributes to the literature in several ways. First, it examines a broad severance-pay reform affecting both temporary and permanent contracts, rather than a narrow easing of protection for one contract type alone. Second, it distinguishes job-to-job mobility from exit from private-sector employment, which matters directly in a recessionary dual labor market where separations need not imply efficient reallocation. Third, it links worker flows to within-firm contractual upgrading and to firm-level hiring and employment composition, allowing us to assess duality on both worker and firm margins. Fourth, it interprets wage effects through a worker-firm-match framework, asking whether the wage-severance-pay association survives within continuing worker-firm matches.

We study overall worker turnover, exit modes, within-firm conversion from fixed-term to open-ended contracts, firm hiring composition, and wages using matched employer-employee data over 2010–2019. The empirical design is layered. We begin with threshold-local comparisons around the November 2011 and October 2013 legal cutoffs within narrow hiring windows, which provide the cleanest legal-timing contrasts in a setting where broader cohort comparisons overlap with major macroeconomic turbulence. We then turn to broader instrumental variables (IV)/control-function estimates that use constructed severance-pay exposure to characterize the margins through which the reforms map into worker and firm outcomes.

The main conclusion is that lowering the transfer component of dismissal protection does not produce a clear improvement in labor-market reallocation. Instead, it shifts adjustment across contractual margins. The threshold-local evidence does not show a uniform increase in job-to-job mobility across the reform dates. In the broader mechanism estimates, lower severance-pay exposure is associated with higher turnover, but the composition of that turnover differs across

worker groups and contract types. The contract results point to persistent duality rather than a broad attenuation of segmentation: conversion improves only selectively, while firms also shift toward fixed-term hiring and higher fixed-term employment shares. By contrast, wage effects are economically small within continuing worker-firm matches.

These results suggest that lower severance pay is not well summarized by a simple flexibility narrative. Greater turnover is not equivalent to better reallocation if it does not translate into stronger job-to-job mobility or broader movement into stable employment. Likewise, improvement in one upgrading margin does not imply lower duality if firms continue to rely heavily on temporary contracts as their main adjustment technology. The Portuguese evidence therefore fits the view that partial labor-market reform can shift adjustment across contractual margins without eliminating the contractual structure through which firms buffer shocks.[4]

The remainder of the paper proceeds as follows. Section 2 reviews the related literature. Section 3 details the legal reforms and treatment incidence. Section 4 summarizes the theoretical channels. Section 5 describes the data and sample construction. Section 6 presents the modeling and identification strategy, and Section 7 provides descriptive evidence. Section 8 presents the main results. Section 9 concludes.

2 Literature review

This study relates to four connected strands of the literature: employment protection and worker flows, macro-institutional adjustment, labor-market duality, and wage incidence. The relevant literature is not simply a generic debate on whether employment protection is "good" or "bad." It is more specifically about which component of dismissal protection is being changed, on which contractual margin firms adjust, and whether greater flexibility improves worker reallocation or instead preserves a segmented equilibrium.

The theoretical starting point distinguishes dismissal protection that is mainly a transfer from the firm to the worker from protection that contains non-transfer components such as legal uncertainty, procedural constraints, and red-tape costs. Under fully flexible contracting, transfer components can be offset in wages, whereas non-transfer firing costs reduce both job creation and job destruction and therefore affect equilibrium worker flows and allocation dynamics (Lazear,

[4]This setting differs from the canonical partial reforms that liberalize temporary contracts while leaving permanent contracts unchanged. Portugal reduced statutory severance-pay schedules for both fixed-term and open-ended contracts. The reform was broad in coverage but partial in scope: it mainly changed the monetary transfer component of dismissal costs, while dismissal procedures and legal uncertainty continued to shape the effective cost of dismissals.

1990; Bentolila and Bertola, 1990; Hopenhayn and Rogerson, 1993; Mortensen and Pissarides, 1994; Pissarides, 2000; Blanchard and Landier, 2002; Ljungqvist, 2002; Garibaldi and Violante, 2005; Boeri et al., 2017).[5] This broader search-and-matching perspective matters directly here because the Portuguese reforms mainly changed severance-pay schedules, that is, the transfer component of dismissal costs, while the substantive welfare question depends on whether lower protection reallocates workers toward better matches or instead mainly generates additional instability.

The empirical worker-flow literature broadly confirms that dismissal-cost reforms matter, but it also shows that the response is highly sensitive to institutional design.[6] Evidence from the United Kingdom, Sweden, Germany, Italy, and France documents changes in firing hazards, hiring and separation rates, anticipation behavior near legal thresholds, and firm adjustment along both intensive and extensive margins (Marinescu, 2009; von Below and Thoursie, 2010; Bauer et al., 2007; Kugler and Pica, 2008; Cahuc et al., 2019). Related work on two-tier and threshold-based reforms shows that easing protection can raise hiring or turnover without necessarily generating a uniform improvement in employment performance, especially when firms can substitute across contract types or re-time separations (Boeri and Garibaldi, 2007, 2019; Hijzen et al., 2017; Daruich et al., 2023).

A useful benchmark is Italy's 2015 Jobs Act, studied for instance by Boeri and Garibaldi (2019) and Daruich et al. (2023). This reform reduced dismissal protection for newly signed open-ended contracts, largely replacing reinstatement with a more predictable severance-based remedy, in a dual labor market still characterized by extensive temporary work. Like the Portuguese episode, it combined crisis-era reform with a broad change in firing costs for new permanent hires. The Portuguese setting, however, extends the comparison beyond permanent hiring into exit-mode composition, conversion, and continued temporary reliance. Recent contributions further stress that behavioral responses depend on worker heterogeneity and on the margin through which lower protection operates (Bertoni et al., 2023; Englmaier et al., 2025).

For Portugal, heterogeneity in institutional effects is already reported in the existing evidence. Martins (2009) finds limited effects for a narrower procedural reform, while Centeno and Novo (2012) show that widening the protection gap between permanent and temporary contracts increased the share and excess turnover of fixed-term jobs. Taken together, these studies suggest

[5] Some reduction in hirings and separations may be desirable if it prevents excessive turnover. The relevant policy question is whether job protection pushes worker flows below an efficient level.

[6] For broader surveys of employment protection and labor mobility, see Kugler (2007), Boeri (2011), and OECD (2020).

that we should not expect all employment-protection reforms to move the same outcomes in the same direction. The object being reformed matters, and so does the contractual environment into which the reform is introduced.

Because the Portuguese severance reforms overlap the sovereign-debt adjustment period, our paper also relates to work emphasizing that labor institutions and macroeconomic shocks jointly shape unemployment, reallocation, and reform effects (Blanchard, 2007; Blanchard and Portugal, 2017). This perspective is reinforced by broader syntheses of European labor-market reform and dualism, which argue that the consequences of partial reform depend on the surrounding bargaining and contractual regime rather than on a single legal parameter in isolation (Boeri, 2011; Bentolila et al., 2020; OECD, 2020). Moreover, Fernandes and Ferreira (2017) show that the earlier financial crisis tightened financing constraints and affected firms' use of fixed-term contracts, while Ferreira and Saridakis (2017) document that the subsequent financial and sovereign-debt crises materially increased firm shutdown risk. Thus, the recessionary setting is not a background detail in the Portuguese case. It is one reason, for instance, why we interpret job-to-job mobility and exit from private-sector employment separately rather than treating all separations as equally informative.

The duality margin is especially important. A central lesson from the literature on fixed-term work is that temporary contracts can serve as screening devices, cyclical buffers, or churning technologies, with very different implications for conversion and mobility (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002; Booth et al., 2002; Cahuc et al., 2016; Faccini, 2014; Portugal and Varejão, 2022). In partial-reform settings, easing one side of the labor market while preserving contractual asymmetries can therefore fail to reduce segmentation, which is why we analyze conversion and contract composition jointly rather than in isolation (Boeri, 2011; Cahuc et al., 2022; Ichino et al., 2008).

Empirically, the literature does not support a single interpretation of temporary jobs: some studies describe them as stepping stones for selected groups or institutional contexts, whereas others emphasize repeated turnover, weak upgrading, and low-productivity uses of temporary labor (Cappellari et al., 2012; Filomena and Picchio, 2022; Di Porto and Tealdi, 2024; Fiaschi and Tealdi, 2024). This ambiguity is central for our study because conversion and composition need not move together.

The wage-incidence literature provides a third benchmark. The classic transfer-incidence logic predicts that mandated severance payments can be shifted into wages, but empirical re-

sponses are heterogeneous across worker groups, bargaining environments, and macroeconomic conditions (Kugler, 2005; Leonardi and Pica, 2013; Centeno and Novo, 2014; Cervini-Plá et al., 2014; Martins, 2021). In a coordinated bargaining environment such as Portugal's, those responses are particularly difficult to interpret because institutional wage floors, wage cushions, and firm sorting all shape the observed relationship between severance exposure and earnings (Card et al., 2016; Torres et al., 2018; Card and Cardoso, 2022; Addison et al., 2023).

Raw wage responses are difficult to interpret because employment-protection reforms can change who is employed, in which firms, and in which worker-firm matches. The natural reference point is therefore the worker-firm wage literature, which shows that observed wage gradients can mix within-match adjustment with worker sorting, firm wage premia, and match-specific heterogeneity (Abowd et al., 1999; Woodcock, 2015; Autor et al., 2006). We therefore estimate wage-severance associations within worker-firm matches, rather than reading baseline wage regressions as evidence that severance-pay costs are directly passed through to wages.

The methodological literature on staggered treatment timing also matters for positioning, even though the paper does not estimate a standard two-way-fixed-effects event-study model. Recent work has shown that binary staggered-adoption designs require particular care when treatment effects are heterogeneous across cohorts and over time (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). That concern is conceptually close to the one emphasized here, namely that broad cohort comparisons can be contaminated by changing composition and macroeconomic timing. The Portuguese design, however, differs from the usual absorbing-treatment setup because exposure is not a single binary switch: it depends jointly on admission date, tenure progression, contract type, and grandfathered rights. This is one reason why our empirical analysis relies on threshold-local comparisons and on explicitly constructed severance-pay exposure rather than on a conventional staggered DiD headline estimates.

Taken together, these strands of the literature suggest that the Portuguese episode we study in this paper is informative not because it delivers a single universal elasticity of employment protection, but because it helps clarify how a broad severance-pay reform in a dual labor market maps into worker-flow composition, contractual allocation, and wage incidence during a period of macroeconomic stress.

3 Institutional setting and exposure design

The reforms we study took place during Portugal's 2011 adjustment program under the European Commission, the European Central Bank, and the IMF (European Commission, 2011a). Labor-market reform was one component of that package, and this paper focuses on changes in statutory severance-pay accrual. Portugal formally requested international financial assistance on 7 April 2011, and the programme approved in May 2011 was implemented after a brief 2010 recovery had already given way to renewed recession and sovereign-debt adjustment (European Commission, 2011a; World Bank, 2026). Appendix Table A1 and Appendix Figure A1 summarize this macroeconomic context.

The timing matters for interpretation because adjacent hiring cohorts around the reform thresholds were exposed to a rapidly deteriorating environment of sovereign stress, fiscal adjustment, and weak credit conditions. It also conditions the placebo design: we use October pseudo cutoffs before the legal reforms, anchoring the balance exercise in the recession-era October 2008 cutoff and using October 2009 and October 2010 as additional outcome placebos rather than relying on a near-reform expansion date.[7]

Before the reforms, Portugal combined strict protection for open-ended contracts with a more flexible regime for temporary employment. Open-ended contracts entitled workers to 30 days of base wage and tenure-related regular pay per year of service, with a minimum entitlement of three months of pay. Temporary contracts generated severance through monthly accrual: three days of pay per month for contracts lasting up to six months and two days per month for longer contracts. The pre-reform regime did not impose the wage-base and total-liability caps introduced later.[8] This asymmetry reinforced a sizable protection gap between permanent and temporary jobs and gave firms a relatively flexible adjustment margin through fixed-term hiring, renewals, and non-renewals. Even after the reforms narrowed severance accrual differences across contract types, procedural dismissal costs and legal uncertainty remained more important for open-ended contracts, so the reforms should be interpreted as changes in the transfer component of dismissal protection rather than as a complete equalization of employment protection across contract types.

[7] October 2008 is the cleanest benchmark for the balance exercise because it lies in the immediate post-Global-Financial-Crisis contraction phase. The later October pseudo cutoffs are useful as additional outcome placebos because they move closer to the reform window while still preceding the legal changes, although they already overlap a different mix of sovereign-risk and adjustment-program dynamics.

[8] The relevant wage base is defined using current wage components, and no severance pay is due during the trial period, whose length depends on worker skill and contract type. This rule applied both before and after the reforms.

The key institutional point is that the reforms occurred in three waves and changed exposure differently by contract type, tenure, and admission date. They did not operate at a narrow firm-size threshold or on a single contractual margin. Instead, they changed statutory severance schedules for open-ended and temporary contracts in a way that generated overlapping regimes within the same calendar year and, in many cases, within the same firm.

The first change came into force on 1 November 2011 under Law 53/2011 and applied only to new contracts. For new open-ended contracts, severance accrual fell from 30 to 20 days per year of service, and the three-month minimum entitlement no longer applied to these new contracts. For fixed-term contracts, severance also moved toward a 20-days-per-year schedule, from the previous monthly accrual of two or three days depending on contract duration. The reform also introduced wage-base and total-liability caps. Economically, this first wave primarily affected workers newly entering jobs under the reformed schedule and therefore created the cleanest legal break between pre-reform and post-reform admission cohorts. But it did not occur in a stable macroeconomic window: it arrived after the April 2011 assistance request and during the first year of the adjustment programme, exactly the kind of crisis environment that affects firm survival, financing conditions, and contract use in their own right, as shown by Fernandes and Ferreira (2017) and Ferreira and Saridakis (2017).

The second change came into force on 1 November 2012 under Law 23/2012 and extended the 20-days-per-year schedule to existing contracts, while preserving rights already accrued under previous rules. Contracts signed before 1 November 2011 kept the old accrual schedule up to 31 October 2012, after which additional accrual followed the new schedule. For old-regime open-ended contracts, the three-month minimum entitlement remained protected under grandfathering. This generated persistent differences in effective severance exposure between pre- and post-reform hires observed in the same year. At the margin, two open-ended workers employed by the same firm and earning the same wage could face different statutory severance entitlements if one had been hired on 31 October 2011 and had accumulated protected rights under the old regime, while the other had been hired on 1 November 2011 and accrued severance only under the lower schedule. It is precisely this grandfathering structure that makes the Portuguese episode empirically useful: reform exposure varies not only by calendar year, but also by admission cohort and accumulated rights within the same firm and year.

The third change came into force on 1 October 2013 under Law 69/2013 and reduced accrual rates further, again preserving previously accrued rights. New open-ended contracts accrued 12

days per year of service. For new temporary contracts, the rules differed by contract type: fixed-term contracts accrued 18 days per year, while uncertain-term contracts accrued 18 days per year during the first three years and 12 days thereafter. Existing contracts followed transitional rules, with accrued rights protected up to 30 September 2013 and subsequent accrual depending on tenure and contract history. Taken together, these changes produced a large decline in statutory accrual for open-ended contracts and narrowed the temporary-permanent severance gap, though unevenly across tenure, contract type, and admission cohort. Appendix Figures A2 and A3 summarize the resulting statutory severance-pay schedules. As shown, two stylized facts stand out: the sizable reduction in severance pay for permanent contracts when comparing the pre- and post-reform schedules, and the reversal in relative severance entitlements, with permanent contracts carrying comparatively higher severance pay before the reforms but lower severance pay afterward.

In sum, this legal architecture creates multiple sources of severance-pay exposure. Appendix Figure A4 illustrates how the statutory schedules translate these legal changes into heterogeneous exposure by admission cohort, tenure, and contract type. Two workers in the same firm-year may face different effective exposure because they entered under different regimes and carry different accrued-rights histories. Likewise, two workers with the same admission year may face different schedules because they hold different contract types or cross key tenure thresholds at different dates. For this reason, treatment in the full reform setting cannot be summarized by one global post indicator; it is generated by admission cohort, tenure progression, contract type, and grandfathered rights. This is also why simple before-and-after comparisons are not particularly informative in this setting. By contrast, the threshold diagnostics use a local post-cutoff indicator only within narrow hiring windows around a single legal date, where it serves as a reduced-form benchmark.

Three institutional features support the identification strategy. First, timing was externally negotiated and enacted in law under the adjustment program rather than chosen by individual firms or workers. Second, grandfathering creates within-year exposure heterogeneity beyond simple before-after contrasts. Third, the reforms were broad rather than concentrated at a narrow eligibility cutoff. Still, because reform years overlap strong macroeconomic volatility, institutional timing is necessary but not sufficient: cohort-comparability diagnostics and threshold-local designs are required for interpretation. The institutional setting therefore motivates a design that treats legal timing as the starting point of identification, not its complete justification.

4 Theoretical framework

This section summarizes behavioral channels under lower severance-pay exposure. The goal is not a full structural model but a transparent set of comparative statics organizing empirical analysis across worker flows, contract conversion, and wages. The main point is that a fall in severance pay need not move all labor-market margins in the same direction. The same reform can simultaneously increase turnover, change the composition of exits, change firms' use of temporary contracts, and reshape wage outcomes through incidence, bargaining, and selection.

For separations, lower severance reduces expected termination costs and weakly raises firms' willingness to separate from low-value matches. This is the most direct employer-side channel and is consistent with the classic firing-cost and job-creation/job-destruction logic in Bentolila and Bertola (1990), Lazear (1990), Mortensen and Pissarides (1994), and Hopenhayn and Rogerson (1993). The worker side is less straightforward: lower continuation value and higher perceived risk can induce more active search, increasing either job-to-job mobility or exits from employment, in the spirit of the turnover and search frameworks of Jovanovic (1979) and Pissarides (2000).

The distinction between those two outcomes is central. A rise in job-to-job transitions is more easily reconciled with an efficiency or reallocation interpretation, whereas a rise in exits from employment is more consistent with weaker employment stability or with temporary jobs becoming more fragile. These channels need not move proportionally across groups. Incumbents may react more through reallocation, while newly hired workers may react more through exits. More generally, when the labor market is segmented, the same reduction in dismissal costs can produce different behavioral responses depending on whether workers are in protected permanent jobs or in more precarious temporary matches, a point emphasized by the two-tier labor-market literature surveyed in Cahuc and Postel-Vinay (2002), Blanchard and Landier (2002), and Boeri and Garibaldi (2007).

For conversion from fixed-term to open-ended contracts, predictions are ambiguous. Temporary contracts can serve at least three distinct functions. They can operate as a buffer stock for cyclical adjustment, in which case firms use them to absorb shocks and conversion should be limited. They can operate as a churning technology, thus temporary workers are repeatedly replaced rather than upgraded. Or they can operate as a screening device, whereby firms use temporary jobs to learn about match quality before offering a permanent contract. This dis-

tinction between dead-end, stepping-stone, and screening roles is central in Booth et al. (2002), Ichino et al. (2008), Faccini (2014), Filomena and Picchio (2022), and Portugal and Varejão (2022). If temporary contracts are primarily screening devices, lowering the relative cost of retaining workers on open-ended contracts can raise conversion probabilities. If temporary contracts are mainly used for buffering or churning, lower severance for permanent jobs need not increase within-firm conversion and may coincide with stronger temporary-contract reliance, as in models of dual labor markets and fixed-term restrictions such as Blanchard and Landier (2002) and Cahuc et al. (2022).

This ambiguity implies that conversion and composition must be interpreted jointly. Partial improvement in one transition margin does not necessarily imply lower duality if firms simultaneously increase temporary hiring or temporary employment shares. A reform can therefore generate more direct permanent hiring for some workers, or more within-firm upgrading for some surviving matches, while still leaving the aggregate allocation of employment tilted toward temporary contracts. That is why the firm-level hiring and composition margins are not ancillary in this paper; they are necessary to interpret the conversion results, exactly because partial reforms can reshape the margin of temporary-contract use without delivering broad-based reductions in duality (Boeri and Garibaldi, 2007, 2019; Cahuc et al., 2022).

For wages, at least three channels are relevant. First, transfer-incidence logic predicts offsetting wage adjustment: if severance pay is largely a priced component of compensation, lower future dismissal transfers may be associated with higher current pay, and vice versa, as in the compensating-differentials logic of Lazear (1990) and the empirical incidence literature summarized earlier. Second, insider bargaining can weaken with lower protection, reducing pay. This channel is especially plausible in institutional settings where bargaining power is linked to job security (Lindbeck and Snower, 1986, 2001). Third, observed wage variation can be compositional because reforms change which workers, firms, and matches are observed. This is why within-match wage estimates are central to interpretation. Without conditioning on worker-firm matches, it is difficult to distinguish genuine wage adjustment within continuing employment relationships from changes in who holds which jobs, a concern closely related to the worker-firm-match decomposition emphasized by Woodcock (2015).

Taken together, these channels imply that the empirical analysis should not be organized around a single summary effect of lower severance pay. The same reform may increase overall turnover while shifting its composition toward less desirable margins, may improve a narrow

upgrading channel without reducing duality more broadly, and may have little residual wage effect after conditioning on worker-firm matches. Table 2 organizes these predictions by outcome and channel, and serves as a mapping device between theory and empirical margins.

5 Data and sample construction

This study is based on Quadros de Pessoal (QP, Personnel Records), a longitudinal matched employer–employee dataset covering virtually the entire Portuguese private-sector workforce. Each October, private-sector firms with at least one wage earner are legally required to report standardized information on their workforce and organizational structure to the Ministry of Labor and Social Security, thereby ensuring broad coverage of firms, establishments, and employees. Civil servants, the self-employed, and domestic workers are not covered.

On the worker side, QP records information on age, gender, schooling completed, broad occupation, qualification level, contract type (fixed-term versus open-ended; part-time versus full-time), admission date, and tenure within the firm. It also provides a detailed decomposition of pay and hours, including base wage, overtime pay, regular and irregular remuneration components, and normal and overtime hours worked. On the firm side, QP reports employment headcount, year of foundation, sector of activity, location, legal form, ownership structure, sales volume, and the number and distribution of establishments. This combination of near-census coverage, longitudinal identifiers, and detailed worker and firm information makes QP well suited to analyzing several outcomes simultaneously, including worker mobility, contract margins, firm employment composition, and wages.

The empirical analysis covers the period from 2010-2019. This choice is deliberate. It spans the core phases most relevant for the Portuguese adjustment: the brief pre-program year 2010, with a slight economic recovery following the European Economic Recovery Plan (European Commission, 2008), the sovereign-debt crisis and adjustment years, and the subsequent recovery, while avoiding a much longer pre-crisis window that would make admission cohorts much less comparable from a business-cycle perspective. This is not meant to imply that 2010 and late 2011 share the same macroeconomic environment. They do not. Rather, the worker-flow analysis is organized around the years immediately surrounding the reform sequence, while acknowledging that workers are hired under sharply different aggregate conditions across recession, adjustment, and recovery. Because hiring cohorts in the worker-flow analysis span recession, adjustment,

and recovery, we do not assume broad-window cohort comparability. Appendix B therefore reports adjacent-threshold balance diagnostics and narrow-window estimates around the 2011 and 2013 legal cutoffs. The wage analysis instead uses 2002–2019 to support richer fixed-effect identification.

The worker-flow sample is organized into three groups: new labor-market entrants, newly hired workers with prior experience, and incumbent workers observed in a pre-existing worker-firm match at the start of the analysis period in 2010.[9] This grouping tracks legal incidence and behavioral heterogeneity. New entrants and new hires are exposed at spell start to post-reform conditions, while incumbents retain larger accrued rights under grandfathering clauses. In the estimation sample, new entrants and new hires are workers whose spell start falls between November 2009 and October 2015, while incumbents are workers already attached to the firm before November 2009.[10]

We apply the following sample restrictions. First, we keep firms with an average workforce of at least five workers. Second, we exclude workers whose monthly base wages were less than 80% of the legal minimum wage, corresponding to the lowest permissible wage for apprentices. Third, we exclude sectors with uneven coverage and collapse harmonized two-digit sector codes into six broad sectors: manufacturing, construction, commerce and hospitality, business services, social services, and other sectors, the latter comprising the remaining non-excluded two-digit sectors. Fourth, the analysis focuses on three main worker groups: new entrants, newly hired with some prior experience, and incumbent workers already attached to the firm. To mitigate possible 'false positives' regarding new entrants, we also define a threshold for their age at the job start spell of 34 years, hence matching the definition used by the Portuguese National Statistics Institute (INE) to represent young workers. In addition, we also top-code contractual duration at 540 months (45 years) for incumbent workers and convert the year intervals into 10 bins (1, 2, 3, 4-5, 6-8, 9-12, 13-18, 19-25, 26-35, and 36-45 years). Finally, missing values for any covariates entering our econometric analysis are dropped. Therefore, after applying all these filters, we end with an estimation sample encompassing each group of individuals in the following manner: new entrants, 1,171,784 observations of worker-year pairs corresponding to 446,295 individuals in 67,347 firms (6.86%); new hires with prior experience, 4,910,300 observations of worker-year

[9] New labor-market entrants are workers observed beginning their first employment spell, while newly hired workers already have prior observed labor-market experience before entering the focal firm during the analysis window.

[10] Note that the QP dataset reports information at year intervals, particularly in October of each year, implying that accessions between November 2009 and October 2010 are recorded in 2010, those between November 2010 and October 2011 in this latter calendar year, and so on.

pairs translating into 1,210,762 individuals in 107,389 firms (28.73%); and incumbent workers, 11,007,182 observations of worker-year pairs concerning 1,828,142 individuals in 94,196 firms (64.41%).

The main dependent variables are built at the spell-year level. The separation indicator equals one in the last observed year of a worker-firm spell when the match is no longer observed in year $t+1$. This separation outcome is then decomposed into two mutually exclusive groups: exit from private-sector employment, when the worker is not observed in another firm in $t+1$, and job-to-job transition, when the worker is observed with a different employer in $t+1$. We use "private-sector employment" because the data do not observe all non-employment and non-private-sector destinations.

The conversion outcome is defined more narrowly and captures within-firm transitions from fixed-term to open-ended contracts among temporary-job spells, rather than all movements into permanent employment across firms.

Wage outcomes are measured consistently with the wage components used to compute severance-pay entitlements.[11]

A final measurement point is important. Mobility outcomes are year-to-year snapshots. The design is highly informative about annual transition margins, but it does not observe all within-year intermediate states.

6 Modeling and identification strategy

The empirical strategy is organized in layers because treatment in this setting is not a single binary switch: effective severance-pay exposure depends jointly on admission cohort, tenure progression, contract type, and grandfathered accrued rights. We start with threshold-local parameterizations around the November 2011 and October 2013 legal cutoffs. These narrow-window models compare adjacent admission cohorts entering under different legal schedules and provide the main reduced-form benchmark, staying closest to the institutional timing of the reforms. Because the reform period coincides with recession, adjustment, and recovery, these local contrasts are more informative than broad cohort comparisons spanning the full hiring

[11] For consistency, monthly wages and severance pay are determined using the same wage components, namely the base wage and tenure-related regular pay. The QP data do not separately identify tenure-related increments from regular benefits. We therefore include regular benefits beyond the base wage when constructing the comparable wage measure, computed as [(bw+reg)/hnorm]*hmode, where bw is the monthly base wage, reg denotes regular benefits, hnorm is normal monthly hours worked, and hmode is the modal normal monthly hours worked by 4-digit occupation and year. See also Card et al. (2016) for a similar computation.

window. We then move to broader IV/control-function models for worker flows, mechanism models for contractual allocation at both the worker and firm levels, and finally worker-firm-match wage specifications that absorb persistent heterogeneity within employment relationships. These broader models trace how lower severance-pay exposure maps into turnover, contract conversion, firm hiring composition, and wages, while relying on stronger modeling assumptions than the threshold-local comparisons.

Layer 1: Threshold-local reduced-form models. Around each legal cutoff, we estimate narrow-window models of the form

$$Y_{ijc}^{m} = \alpha_j + \beta_c Post_{ijc} + \boldsymbol{X}_{ijc}'\delta_c + \rho_c r_{ijc} + \varepsilon_{ijc}, \tag{1}$$

where Y_{ijc}^{m} denotes outcome m for worker i hired by firm j around legal cutoff c. The outcomes are indicators for any job separation, exit from private-sector employment, and job-to-job transition, analyzed for new entrants and newly hired workers with prior experience. $Post_{ijc}$ equals one for spells starting at or after cutoff c. $\boldsymbol{X}_{ijc}$ is a vector of worker and firm controls, including age, gender, a college indicator, and log sales. The variable r_{ijc} is the month of hiring measured relative to cutoff c, included linearly to absorb smooth local time trends within the narrow window. Firm fixed effects are denoted by α_j.

The coefficient of interest is β_c. It measures the discrete difference in the outcome between post-cutoff and pre-cutoff hires within the local window, conditional on the controls, firm fixed effects, and the linear relative-month trend described above. Standard errors are clustered at the firm level.

The baseline threshold-local specifications reported in Table 4 are estimated in a ±6-month window around each cutoff and restrict the sample to firms observed hiring on both sides of the cutoff, so that pre- and post-cutoff hiring cohorts are as comparable as possible within a local window. These estimates ask whether outcomes move discretely at the legal cutoffs before the paper turns to broader mechanism models.

Layer 2: Competing-risks worker-flow model. This layer uses the broader severance-pay exposure design to study turnover composition in the full estimation samples. It first estimates overall separation responses (Table 5, columns 1 and 2) and then models worker-flow states jointly in a discrete-time competing-risks framework (Jenkins, 2005), distinguishing continuation with the same firm from two mutually exclusive separation margins: exit from private-sector

employment and job-to-job mobility (Table 5, columns 3 and 4). The same exercise is then repeated separately for the temporary and permanent employment relationship subsamples in Table 6. This decomposition matters because these outcomes have different implications for labor-market adjustment.

For the competing-risks component, let $D_{ijt} \in \{0, 1, 2\}$ denote the worker-flow state observed between years t and $t+1$, where $D_{ijt} = 0$ denotes continuation with the same firm, $D_{ijt} = 1$ exit from private-sector employment, and $D_{ijt} = 2$ job-to-job mobility. The transition probabilities are estimated using a multinomial logit model, with continuation as the base outcome:

$$\Pr(D_{ijt} = k \mid \boldsymbol{X}_{ijt}, SP_{ijt}, T_{ijt}) = \frac{\exp(\theta^k_{ijt})}{1 + \sum_{\ell=1}^{2} \exp(\theta^\ell_{ijt})}, \quad k = 1, 2, \tag{2}$$

where

$$\theta^k_{ijt} = \boldsymbol{X}'_{ijt}\beta_k + \gamma_k SP_{ijt} + \psi_k(T_{ijt}) + \tau_{kt}. \tag{3}$$

where $\boldsymbol{X}_{ijt}$ is the vector of observed characteristics of worker and firm (age, gender, college indicator, log sales, firm size dummies and sector dummies), SP_{ijt} denotes severance pay, $\psi_k(T_{ijt})$ captures the elapsed-tenure profile that defines the non-parametric baseline hazard rate, and τ_{kt} denotes year effects included to control for macroeconomic shocks. The disturbance term is not written separately in equation (3) because θ^k_{ijt} is the deterministic index of the multinomial logit model; the stochastic component is embedded in the logit errors that generate the transition probabilities in equation (2).

Layer 3: Competing-risks conversion model. This layer turns to contractual allocation at the worker level (Table 7). The analysis of conversions from fixed-term into open-ended contracts follows a similar approach as the one used in the second empirical layer (i.e., a competing-risks duration model with the same explanatory variables). Here, conversion models are estimated on temporary-contract subsamples and distinguish conversion, non-conversion continuation within the firm, and non-conversion and separation from the current firm (see, for instance, Portugal and Varejão (2022) for a similar empirical exercise). The first category contains more information to understand whether severance pay changes impacted conversions of employment contracts within the same employer, hence focusing on this type of transition when reporting the results. This margin is necessary for assessing whether lower dismissal costs are associated with broader progression into stable employment or instead with continued dependence on temporary contracts.

Layer 4: Firm-level hiring and contract-composition model. This layer examines contractual allocation at the firm level (Table 8). These firm-level models study hiring and workforce composition by contract type, including hiring shares, hiring ratios, hiring volumes, and employment shares. They complement the worker-level conversion results because partial reforms can improve selected upgrading margins while still leaving firms more reliant on temporary contracts overall.

Layer 5: Worker-firm-match wage specification. The wage analysis asks whether severance-pay variation predicts wages after conditioning on a worker-firm match, in the spirit of the worker-firm literature (Abowd et al., 1999; Woodcock, 2015). This addresses the concern that observed wage gradients may reflect sorting across workers, firms, and matches rather than wage adjustment within an ongoing employment relation.

We define the estimation sample as the largest connected set of the wage-stage worker-firm mobility network, following Abowd et al. (2002). All wage regressions use this common mobility-supported sample. We estimate the model for the broad sample and then separately for the three worker groups used throughout the paper: new entrants, newly hired workers with prior experience, and incumbents. The estimating equation is

$$\ln w_{ijt} = x'_{ijt}\chi + \zeta SP_{ijt} + \tau_t + \lambda_{ij} + \xi_{ijt}, \tag{4}$$

where $\ln w_{ijt}$ is the natural logarithm of the monthly wage of individual i working at firm j in year t; SP_{ijt} is severance pay; τ_t are year fixed effects; and λ_{ij} is a worker-firm-match fixed effect. The vector x'_{ijt} includes age squared, schooling, tenure squared, and log firm size. Linear age and tenure are absorbed by the worker-firm-match and year structure.

The specification absorbs worker-firm-match fixed effects, so the wage-severance coefficient is read from within-match changes in statutory severance exposure over time. This comparison nets out persistent match heterogeneity, common year shocks, and the included time-varying controls. Table 9 reports the resulting wage-severance associations.

Construction of severance-pay exposure. Monetary severance is built from statutory accrual rules by contract type, tenure, and admission cohort:

$$SP_{ijt} = \sum_{r \in R_{ijt}} \left(\frac{w_{ijt}}{30}\right) d_{r,ijt}.$$

For worker i employed at firm j in year t, SP_{ijt} is defined as the sum, across all applicable

legal-regime segments r, of the worker's daily wage base, $w_{ijt}/30$, multiplied by the number of severance days accrued in that segment, $d_{r,ijt}$. The set R_{ijt} collects the relevant regime segments for the worker-firm spell, allowing the same employment relationship to combine grandfathered rights accumulated under earlier rules and accruals generated under post-reform rules. Trial-period months are assigned zero accrued days. The length of the trial period depends on the worker's skill level and contract type:[12] top executives with open-ended contracts have an eight-month trial period; intermediate executives, supervisors and team leaders, and highly skilled professionals have a six-month period; and the remaining occupational groups have a three-month period. By contrast, fixed-term contracts have a 30-day trial period.

Endogenous severance-pay measure. Because monetary severance-pay exposure embeds the worker's wage base, it may be correlated with unobserved worker, firm, or match heterogeneity. The broader specifications therefore instrument actual severance-pay exposure with the reform-induced severance gap. For each worker-year, this gap, denoted $GapSP_{ijt}$, is defined as the difference between the severance entitlement that would have applied under the pre-reform legal schedule and the entitlement implied by the actual legal schedule (i.e., $GapSP_{ijt} = SP_{ijt}^{old} - SP_{ijt}^{actual}$). Both severance terms are computed using the same wage-tenure inputs. Hence, the identifying variation comes from how the law maps identical worker-firm histories into different statutory severance entitlements, rather than from independent variation in wages, tenure, or firm characteristics.

The instrument must satisfy relevance and exclusion. Relevance requires that the reform-induced gap be strongly correlated with actual severance-pay exposure, $\text{Cov}(SP_{ijt}, GapSP_{ijt}) \neq 0$, which is assessed in the first-stage estimates reported in the results. The exclusion restriction requires that, conditional on the included controls, fixed effects, contract type, tenure structure, and admission cohort, $GapSP_{ijt}$ affects worker and firm outcomes only through actual severance-pay exposure. This assumption is plausible because the gap is generated mechanically by national legal changes rather than by choices made by individual firms or workers.

This institutional setting supports the interpretation. The severance-pay reforms were negotiated and enacted during Portugal's adjustment programme with the European institutions and the IMF, and the legal changes applied through national statutory schedules. They did not operate through a narrow firm-size threshold or through a single contractual margin. The

[12] Workers are administratively assigned to one of eight skill-level categories based on their occupation and educational attainment: (i) top executives, (ii) intermediate executives, (iii) supervisors and team leaders, (iv) highly skilled professionals, (v) skilled professionals, (vi) semi-skilled professionals, (vii) non-skilled professionals, and (viii) apprentices, interns, and trainees. Trial-period rules follow Article 112, Law 7/2009.

remaining identifying variation therefore comes from the interaction between the common legal schedule and worker-firm histories that determine statutory severance entitlements.

This rationale also clarifies the wage-base concern. Because severance pay is built from current wage components, endogenous wages could contaminate naive severance-pay regressions. The broader worker-flow and firm specifications therefore include controls and fixed effects that absorb systematic wage-base differences associated with tenure, contract structure, worker composition, and firm characteristics. The wage block goes further by absorbing worker-firm-match fixed effects. Under this maintained interpretation, $GapSP_{ijt}$ shifts statutory severance-pay exposure through the legal schedule used to compute entitlements, rather than proxying for an omitted wage-setting or labor-demand shock. In the nonlinear worker-flow and conversion models, endogeneity is addressed using a control-function approach, which adapts the instrumental-variables logic to nonlinear outcome equations (Rivers and Vuong, 1988; Papke and Wooldridge, 2008; Petrin and Train, 2010). In linear specifications, the same identification logic corresponds to the usual IV interpretation; under standard linear-model conditions, the control-function and two-stage least squares (2SLS) estimators deliver numerically equivalent estimates for the endogenous regressor coefficient (Wooldridge, 2015). Appendix C provides the corresponding IV and control-function algebra.

7 Descriptive evidence and cohort comparability

Table 3 reports baseline descriptive statistics for the three worker groups, separating worker-level characteristics from job-level variables. On the worker side, new entrants are younger and have higher average years of schooling: across pre-reform and reform hiring cohorts, their mean age is about 23, average schooling rises slightly from 11.29 to 11.76 years, and the female share remains essentially unchanged at 0.49. Newly hired workers with prior experience are older, have lower average years of schooling, and have a lower female share: mean age is 34.94 before the reforms and 36.32 during the reform period, schooling is around 9.4–10.0 years, and the female share is 0.43–0.44. Incumbents are different again, with mean age close to 42, much longer employment duration, and a slightly higher female share. These worker-level contrasts matter because the three groups do not represent otherwise similar workers exposed to different severance rules; they also differ in career stage, accumulated experience, and attachment to firms.

The job-level variables highlight the segmentation margins most relevant for the empirical

design. New entrants and newly hired workers start more frequently in fixed-term contracts, with fixed-term shares rising from 0.76 to 0.83 among new entrants and from 0.70 to 0.75 among new hires with prior experience. During the reform period, both groups also display lower monthly wages and sharply lower severance-pay exposure, while their raw worker-flow profiles show lower exit rates from private-sector employment but higher job-to-job transition rates. Incumbents are very different: only 0.10 are observed on fixed-term contracts, employment duration is much longer, and severance-pay exposure is far larger, as expected under accumulated rights and grandfathering. Taken together, the worker- and job-level contrasts show that reform exposure is structurally and economically concentrated among early-spell workers, while incumbents remain shaped by longer firm attachment and grandfathered severance entitlements.

The 2010–2015 hiring window spans recession, adjustment, and recovery. Appendix Figure A5 shows how reform-induced severance-pay exposure diverges over time, while Appendix Figures A6 and A7 document changes in contract mix, sector allocation, firm-size composition, and raw outcome profiles across cohorts. These patterns are informative in their own right: the reform period is also a period in which firms change whom they hire, under which contracts, and in which parts of the economy. Many profiles evolve smoothly rather than displaying sharp breaks at reform dates, suggesting that raw differences across cohorts reflect both policy exposure and changing cohort composition. This motivates the threshold-local design used later, where comparisons are restricted to hires close to the legal cutoffs. It also cautions against interpreting unconditional changes in turnover or conversion rates as reform effects, since temporary hiring prevalence, service-sector incidence, and small-firm hiring can themselves shape these outcomes.

Appendix Figure A8 adds a duration perspective to these descriptives. Survival rates fall sharply during the first years of a spell, especially for new entrants and newly hired workers with prior experience, while incumbents display much flatter survival profiles. This pattern reinforces that the main margins of adjustment are concentrated early in employment relationships, precisely where fixed-term contracts and lower severance-pay exposure are most prevalent. At the same time, these Kaplan–Meier estimates remain unconditional: they describe raw tenure profiles but do not separately identify the roles of reform exposure, cohort composition, and macroeconomic conditions.

Appendix B examines whether adjacent hiring cohorts are comparable around the 2011 and 2013 legal cutoffs. Tables B1 and B2 show mixed but broadly similar balance evidence for

the covariates used in the threshold-local specifications. Standardized differences are generally modest. In the regression-based balance checks, however, age differs at the 2011 cutoff, while college incidence and log sales differ at the 2013 cutoff. These results support local comparisons as useful diagnostics, while also making clear that adjacent cohorts are not perfectly balanced.

Table B3 reports placebo diagnostics around October pseudo cutoffs. Panel A uses October 2008, the recession-era false threshold, to assess local balance. Scalar covariates are broadly balanced in standardized-difference terms, although the local coefficient diagnostics point to some remaining imbalance in age and log sales. Panel B applies the Table 4 outcome specification to October 2008, October 2009, and October 2010 pseudo cutoffs. The placebo coefficients for any separation, exit from private-sector employment, and job-to-job mobility are small and statistically insignificant across these pseudo cutoffs. The conversion placebo is statistically different from zero at October 2008, but not at the October 2009 or October 2010 placebo cutoffs. We therefore treat the placebo evidence as reassuring for the main worker-flow margins, while retaining caution for the conversion margin in the most crisis-adjacent placebo.

8 Main findings

This section presents the empirical results for worker flows, contract upgrading, firm hiring composition, and wages across the three groups defined above: new labor-market entrants, newly hired workers with prior experience, and incumbent workers already attached to the firm. We start with threshold-local estimates because those comparisons stay closest to the legal timing of the reforms. We then turn to the broader IV/control-function specifications to characterize mechanisms by worker group and contract type. Finally, we examine wages as an additional mechanism, asking how much of the wage-severance-pay association remains once persistent worker-firm-match heterogeneity is taken into account.

Three results organize the section. Result 1 is that turnover responses are reform- and group-specific rather than uniformly reallocative. Result 2 is that contractual upgrading is selective, while aggregate reliance on temporary contracts persists. Result 3 is that wages provide little independent support for a transfer-offset interpretation once persistent worker-firm-match heterogeneity is absorbed. This structure turns a heterogeneous set of estimates into a smaller number of economically interpretable patterns.

Result 1: Turnover composition is reform- and group-specific. The worker-flow results

should be read in the order of Tables 4–6. The central question is not simply whether separations rise on average, but whether lower severance-pay exposure changes the composition of mobility across exit modes and worker groups. Table 4 provides the most local evidence by reporting threshold-local reduced-form estimates around two legal dates: November 2011, when a lower severance-pay schedule first applied to newly signed contracts, and October 2013, when the statutory severance-pay schedule was further reduced. The coefficients compare post-cutoff and pre-cutoff hiring cohorts within a narrow ±6-month window around each date, conditional on the controls, firm fixed effects, and a linear control for month of hiring relative to the cutoff. Because the specifications are estimated as linear probability models, the reported coefficients can be read as percentage-point changes in the probability of each separation outcome.

Around the November 2011 threshold (columns 1 and 2), post-cutoff cohorts are 3.25 and 5.11 percentage points less likely to separate for new entrants and newly hired workers with prior experience, respectively. The decline is concentrated in exits from private-sector employment, which fall by 12.51 and 11.17 percentage points. At the same time, job-to-job transitions rise by 9.26 and 6.06 percentage points. Taken together, the local comparison suggests that the first reform is associated with lower overall separation risk but a marked recomposition of mobility away from exit and toward direct employer-to-employer moves.

Around the October 2013 threshold (columns 3 and 4), post-cutoff cohorts are also less likely to separate, with any-separation coefficients of -1.78 percentage points for new entrants and -1.92 percentage points for newly hired workers with prior experience. The exit margin is less uniform: exits from private-sector employment are not statistically different from zero for new entrants and fall by 2.26 percentage points for newly hired workers with prior experience. In contrast to 2011, job-to-job mobility does not rise; the coefficient is negative for new entrants and close to zero for newly hired workers with prior experience. Thus, the 2013 threshold does not reproduce the 2011 pattern of stronger job-to-job mobility; its clearest local effect is lower separation, with reduced exit concentrated among hires with prior experience.[13]

The localized evidence therefore does not support a uniform reallocation story in which lower severance pay consistently increases job-to-job mobility. The 2011 threshold points to a shift away from exit from private-sector employment and toward direct employer-to-employer moves. The 2013 threshold, however, does not reproduce that pattern: its clearest local effect is

[13] Appendix Table B4 indicates that the localized effects vary with the outcome horizon. For the November 2011 reform, the shift away from exit and toward job-to-job mobility remains visible over 12-, 24-, and 36-month horizons. For the October 2013 reform, the cumulative evidence is more mixed, with lower separation and exit concentrated among hires with prior experience and no comparable increase in job-to-job mobility.

lower overall separation, with reduced exit concentrated among newly hired workers with prior experience and no comparable increase in job-to-job transitions. More generally, the composition of worker flows differs across the two cutoffs rather than pointing to a single mobility margin that responds uniformly to lower severance pay. This heterogeneity links the Portuguese episode to evidence that worker-flow responses to dismissal-cost changes are context- and margin-specific (Marinescu, 2009; Kugler and Pica, 2008; Bertoni et al., 2023; Englmaier et al., 2025). It is also consistent with the macro-institutional view that reform effects depend on the surrounding environment rather than following a mechanical response to lower dismissal costs (Blanchard, 2007; Blanchard and Portugal, 2017). In a recession-and-adjustment setting, lower severance pay need not translate primarily into stronger job-to-job reallocation; it may instead reshape the composition of separations, with changes in exit from private-sector employment and direct employer-to-employer moves differing across reform episodes and worker groups.

Tables 5 and 6 move beyond localized evidence to broader mechanism estimates. Table 5 reports worker-flow responses, distinguishing all separations from the two competing separation margins: exit from private-sector employment and job-to-job transition. Table 6 applies the same logic after splitting workers by contract type, showing whether these flow responses differ between fixed-term and open-ended contracts. These estimates are not substitutes for the localized evidence; they show how severance-pay variation maps into different flow margins under the broader IV/control-function design. Coefficients are reported as severance-pay elasticities, so a negative (positive) estimate implies that lower severance-pay exposure is associated with a higher (lower) incidence of the corresponding outcome.[14] The emphasis is therefore on response intensity rather than simple before-and-after contrasts.

Table 5 shows a common directional pattern across worker groups, though the first-stage evidence is stronger for new hires with prior experience and incumbents than for new entrants. In the full all-separation specification, the severance-pay elasticities are negative across all worker groups, indicating that lower severance pay is associated with more turnover overall.

The competing-risks columns 3 and 4 show that this aggregate response masks substantial heterogeneity across exit modes. For new entrants (Panel A), the strongest full-specification response is concentrated in job-to-job transitions, with a large negative elasticity, while the elasticity for exit from private-sector employment becomes much smaller once the full controls

[14] Whenever coefficients are discussed as elasticities in the mechanism tables, they are evaluated at sample means unless otherwise stated. First-stage strength is reported explicitly because weak-instrument concerns are part of the interpretation, with first-stage F-statistics above 10 used only as a rule-of-thumb diagnostic rather than as a substitute for judgment (Stock et al., 2002).

are added. For newly hired workers with prior experience (Panel B), both margins remain responsive in the full specification, with a slightly larger job-to-job elasticity. For incumbents (Panel C), the response is also present on both margins, but it is much more concentrated in job-to-job transitions; the elasticity for exit from private-sector employment is smaller and only weakly significant in the full specification. Taken together, these estimates support the baseline prediction that lower severance pay is associated with higher turnover, but they qualify the mechanism: the adjustment does not map one-for-one into the same type of mobility for all worker groups.

Table 6 sharpens this point by splitting the same worker-flow outcomes by contract type. The clearest message is not that one contract type always displays larger responses, but that the open-ended-contract margin delivers the more consistent pattern across worker groups. For newly hired workers with prior experience (Panel B), both fixed-term and open-ended coefficients are negative and statistically precise across all three outcomes once the full set of controls is added, with larger full-specification magnitudes among fixed-term contracts. For incumbents (Panel C), the contrast is sharper: fixed-term estimates are close to zero and statistically insignificant, whereas open-ended coefficients remain clearly negative, especially for job-to-job transitions. For new entrants (Panel A), the pattern is more mixed. Both contract types show stronger job-to-job transitions, and all-separation coefficients are negative, but the margin for exit from private-sector employment moves differently across contracts: fixed-term coefficients are positive, while the open-ended estimate is negative in the baseline split but no longer statistically precise once full controls are added.

The contract-type split should therefore be interpreted more cautiously than the pooled worker-group results in Table 5. First-stage diagnostics are uneven in Table 6: all new-entrant and new-hire columns remain below the Stock-Yogo 10% threshold, although the open-ended new-hire columns are above the simpler F-statistic rule of thumb of 10. For incumbents, the open-ended columns are comfortably above the Stock-Yogo threshold, while the fixed-term columns remain just below it. We therefore read Table 6 as evidence that contract margins respond differently, while treating the broader worker-flow response as clearest in the pooled estimates and, within split samples, more consistently visible on the open-ended-contract margin.

The main substantive point is not that mobility rises in a generic sense, but that lower severance pay changes the composition of turnover differently across worker groups, exit modes, and contract types. This finding speaks directly to the employment-protection literature. Rather

than asking only whether lower firing costs increase labor-market dynamism, our results suggest that the relevant question concerns the composition of that dynamism. In this sense, the paper complements studies that find strong flow effects while qualifying their interpretation: higher turnover does not necessarily imply better reallocation if it is not accompanied by stronger job-to-job mobility or clearer upgrading into more stable employment.

Result 2: Selective conversion, persistent duality. Together with the worker-flow estimates, the contract results are central to the mechanism analysis because they show whether lower severance pay altered not only turnover margins, but also the contractual margins through which firms and workers adjust. Table 7 studies within-firm conversion from fixed-term to open-ended contracts,[15] while Table 8 examines hiring and employment composition by contract type. Jointly, these tables ask whether lower dismissal costs were associated with stronger progression into stable employment or instead with continued reliance on temporary work. This is the key duality question in the paper, and it is also where the Portuguese context speaks most directly to the broader European literature on partial labor-market reform.

The conversion evidence is selective rather than generalized. In the full specification of Table 7, the conversion elasticity is essentially zero for new entrants (Panel A), strongly negative for newly hired workers with prior experience (Panel B), and modestly negative for incumbents (Panel C), implying that lower severance-pay exposure is associated with higher conversion rates for workers with prior experience and incumbents. The economically meaningful response is therefore concentrated most clearly among workers hired with prior experience, with a smaller incumbent response and no clear effect for new entrants.

These conversion estimates should nevertheless be read cautiously. The Kleibergen-Paap diagnostics remain below the conventional Stock-Yogo threshold in all three panels of Table 7, including the strongly significant estimate for workers hired with prior experience. The conversion results therefore help characterize the direction of within-match upgrading, but they should not be interpreted as stand-alone evidence that the reforms broadly reduced labor-market duality.

Table 8 is crucial because it shifts the interpretation from individual upgrading to aggregate contract use. At the firm level, lower severance-pay exposure is associated with more fixed-term hiring and a larger fixed-term employment share. The estimates are precise and economically

[15]The conversion outcome is intentionally narrow: it captures within-firm contractual upgrading rather than all possible transitions into permanent employment across firms. This matters because repeated movement across firms through temporary contracts can still signal persistent segmentation even when within-firm conversion rises for some surviving matches.

meaningful on this point: the severance-pay elasticity is −0.1857 for fixed-term hirings as a share of total hirings, −0.4504 for the fixed-term–open-ended hiring ratio, −0.6272 for the number of fixed-term hirings, and −0.3539 for the fixed-term employment share. The first-stage diagnostics are also comfortably above the conventional Stock-Yogo benchmark.

The combined message from Tables 7 and 8 is therefore not contradictory. The reforms are associated with selective upgrading for some fixed-term workers, especially those hired with prior experience, but lower severance-pay exposure is also associated with greater reliance on temporary contracts at the firm level. Lower dismissal costs may therefore improve selected within-match upgrading margins while leaving the broader labor market more dependent on temporary contracts as an adjustment technology. For this reason, the duality conclusion is anchored primarily in the firm-level composition response, with the conversion elasticities providing suggestive evidence on which workers are more likely to experience within-firm upgrading.

This mixed pattern fits naturally with the duality literature. It is consistent with frameworks in which temporary contracts perform both screening and buffering roles, so that some surviving fixed-term matches are upgraded even while firms continue to use temporary contracts intensively as an adjustment device (Blanchard and Landier, 2002; Booth et al., 2002; Centeno and Novo, 2012; Cahuc et al., 2022; Portugal and Varejão, 2022; Ichino et al., 2008). The results therefore do not support a strong version of the claim that lowering dismissal costs automatically compresses the gap between temporary and permanent work. Instead, they fit better with the view that partial reform can leave the structure of duality largely intact, even when selected upgrading margins improve.

Result 3: Within-match wage effects are economically small. Table 9 asks whether severance-pay exposure has a meaningful wage effect after conditioning on worker-firm matches. It does not: the estimates are statistically precise, but economically very small. In the broad connected worker-firm sample, the estimated marginal effect is 0.0000125. The corresponding estimates are 0.0000568 for new entrants, 0.0000432 for newly hired workers with prior experience, and 0.0000205 for incumbents. These magnitudes provide little evidence that lower severance-pay exposure generated sizable wage adjustment within continuing worker-firm matches.

This finding sharpens the contrast with the previous flow and contract blocks. The reform's strongest associations are with worker turnover and contract allocation, not with within-match pay setting. Because the reforms reduced severance-pay levels, a simple transfer-offset mechanism would predict higher current wages for workers whose future severance rights fell. The

positive coefficients in Table 9 point in the opposite direction: if anything, lower severance-pay exposure is associated with slightly lower within-match wages. Given the small magnitudes, however, the main implication is the absence of a sizable transfer-offset response. Consistent with the worker-firm-match literature (Abowd et al., 1999; Autor et al., 2006; Woodcock, 2015), the wage evidence is therefore better read as providing little support for an interpretation in which lower severance pay was mainly offset through wage adjustment within ongoing worker-firm matches.

Overall, the empirical analysis supports a narrower, more nuanced interpretation of the reform than a single headline turnover elasticity. Relative to the testable predictions in Table 2, the broad IV evidence supports the baseline signs for higher separations, higher job-to-job mobility, and higher exit from private-sector employment, but the threshold-local evidence rejects any simple claim that these margins move uniformly across reform episodes. The conversion evidence supports the "ambiguous" prediction rather than a monotone upgrading story, while the wage results show that any wage response within continuing worker-firm matches is limited. The strongest evidence comes from threshold-local worker-flow shifts and from the firm-level contract-composition results. Taken together, these findings suggest that lower severance pay reshaped turnover and contractual allocation, but did not deliver a clear reduction in labor-market duality. More broadly, the results provide episode-specific evidence from a dual labor market operating under crisis-era macroeconomic volatility. They are informative for other partial EPL reforms, while also suggesting that the effects of similar reforms may differ in calmer macroeconomic settings or in institutional environments with weaker temporary-permanent segmentation.

9 Conclusion

This paper studies how successive severance-pay reforms implemented between 2011 and 2013 are associated with worker turnover, contract allocation, firm-level hiring composition, and wages in Portugal. The reforms primarily reduced statutory severance-pay schedules, the transfer component of dismissal costs linked to tenure, contract type, admission date, and grandfathered rights, while leaving other dimensions of employment protection largely unchanged.

Using matched employer–employee data for 2010–2019, the results show that these changes reshaped worker flows and contract use without delivering a clear improvement in labor-market

reallocation or a uniform reduction in segmentation. Rather than targeting a single elasticity of employment protection, the paper shows how the same reform package generates heterogeneous effects across worker flows, upgrading margins, firm hiring behavior, and wages in a dual labor market where temporary and permanent contracts remain distinct adjustment channels.

The empirical design is central to this interpretation. Threshold-local comparisons around the 2011 and 2013 cutoffs do not support a simple narrative in which lower severance pay uniformly increases job-to-job mobility. Around the November 2011 cutoff, post-reform cohorts exhibit lower exits from private-sector employment alongside higher job-to-job transitions. By contrast, around the October 2013 cutoff, the job-to-job response weakens and turns negative for new entrants, while reductions in exits from private-sector employment are concentrated among hires with prior experience. Complementary IV/control-function estimates suggest higher overall turnover under lower severance-pay levels, but the composition of that turnover varies across worker groups, exit modes, and contract types. These findings caution against equating greater labor-market fluidity with uniformly improved mobility or matching.

The contract allocation results reinforce this interpretation. Conversion from fixed-term to open-ended contracts is associated with clearer upgrading for new hires with prior experience, but only a modest response among incumbents. At the firm level, the evidence points to increased reliance on fixed-term hiring and higher fixed-term employment shares. Taken together, these patterns indicate a narrow upgrading margin for selected matches rather than a broad reduction in firms' use of temporary contracts as an adjustment margin. The wage analysis provides little support for a wage-offset channel: within continuing worker–firm matches, the association between severance-pay exposure and wages is economically small. This cautions against interpreting raw wage–severance gradients as evidence of direct pass-through of dismissal costs to wages.

Taken together, the findings show that lowering the transfer component of dismissal costs in a dual labor market does not, by itself, improve reallocation or reduce segmentation. While the reform is associated with changes in turnover, conversion, and hiring composition, temporary contracts remain a central adjustment margin. The policy implication is therefore more limited than a general case for increased labor-market flexibility. Severance-pay cuts can reshape the volume and composition of worker flows, but they are unlikely to weaken temporary–permanent segmentation as long as the incentives sustaining the use of temporary contracts remain in place. In dual labor markets more broadly, reductions in severance-pay schedules should be viewed as a

partial instrument rather than a stand-alone strategy. If the objective is to reduce segmentation, such reforms may need to be complemented by policies that limit excessive reliance on temporary contracts and strengthen pathways into permanent employment.

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Main Tables

Table 1: Portuguese labor-market context, 2006–2019.

Year	GDP	Unemp.	FTC	Conversions	Hirings	Separations	Exit priv. emp.	Job-to-job	Wage	GVA/worker
2006	1.63	7.6	20.63	-	-	17.09	9.49	7.60	-	-
2007	2.51	8.0	22.83	2.34	20.12	18.16	10.13	8.03	-0.54	1.94
2008	0.32	7.6	23.86	2.89	20.52	18.59	11.67	6.92	3.54	-0.03
2009	-3.12	9.4	23.65	2.90	17.33	20.11	13.60	6.52	3.56	-0.89
2010	1.74	10.8	21.36	6.56	20.51	19.13	12.60	6.53	2.18	1.22
Adjustment program period (2011–2014)										
2011	-1.70	13.4	22.98	2.39	18.88	20.21	14.62	5.60	-0.70	-4.14
2012	-4.06	16.5	22.97	2.09	16.17	17.46	11.57	5.89	-1.76	-2.40
2013	-0.92	17.1	24.95	2.24	18.19	17.02	10.15	6.87	-0.89	1.43
2014	0.79	14.5	27.16	2.22	19.98	18.36	10.50	7.86	1.08	3.01
2015	1.79	12.9	28.48	2.37	21.23	18.73	10.13	8.60	-0.94	0.33
2016	2.02	11.5	29.85	2.73	22.06	19.52	9.97	9.56	0.89	0.98
2017	3.51	9.2	31.64	2.70	23.37	21.15	10.48	10.67	2.01	3.32
2018	2.85	7.2	32.93	2.90	24.35	22.57	11.97	10.59	2.87	1.34
2019	2.68	6.6	32.63	3.55	24.81	21.17	12.70	8.48	3.55	2.11

Notes: All indicators in percentage. Unemp. denotes the unemployment rate, FTC the share of fixed-term contracts, and Exit priv. emp. the share of separations exiting private-sector employment. Wages are computed as $(bw + reg)/\text{hnorm} * \text{hmode}$, where bw is the monthly base wage, reg regular benefits, $hnorm$ normal monthly hours, and $hmode$ the modal value of normal monthly hours by four-digit occupation and year; when multiple modes exist, the minimum is used. The construction follows Card et al. (2016). In the *Quadros de Pessoal* data, the table keeps all firm types, excludes workers paid less than 80% of the statutory minimum wage for apprentices, restricts ages to 16–64, and drops observations with missing values in the reported variables. The resulting sample contains 4,549,642 workers employed in 410,154 firms before collapsing to the year level. *Sources:* National Statistics Institute (INE) for real GDP growth and unemployment, *Quadros de Pessoal* for contract, hiring, separation, and wage measures, and SCIE for real gross value added per worker (winsorized at the 1st and 99th percentiles).

Table 2: Testable predictions.

Outcome	Lower severance pay: baseline sign	Main mechanism	Why the sign may vary
	(1)	(2)	(3)
Job separations	Higher	Lower dismissal costs make low-value matches easier to terminate and can also reduce the value of staying in the current match.	Incumbents may respond more through job-to-job mobility, while recent hires may respond more through exit from private-sector employment.
Job-to-job mobility	Higher	Weaker attachment to the current match can increase outside search and reallocation toward better matches.	If portability of accumulated rights matters strongly, workers may prefer to stay rather than move.
Exit from private-sector employment	Higher	Firms may separate more easily from lower-productivity or more fragile matches when dismissal costs fall.	The margin depends on whether workers move quickly into another job rather than disappear from the next October snapshot.
Temporary-to-permanent conversion	Ambiguous	A smaller protection gap can make permanent retention less costly once a good match is identified.	If firms use temporary contracts mainly as a buffer stock or as part of churn, lower dismissal costs need not increase within-firm upgrading.
Wages	Ambiguous	The reform can affect wages through transfer incidence, bargaining power, and worker-firm sorting.	Reduced-form wage changes need not reflect only within-match bargaining because reforms can change which workers, firms, and matches are observed.

Table 3: Descriptive statistics by worker group.

	Panel A: Hiring cohorts															
	New entrants								New hires							
	Hired before reforms (Nov. 2009-Oct. 2011)				Hired during reforms (Nov. 2011-Oct. 2015)				Hired before reforms (Nov. 2009-Oct. 2011)				Hired during reforms (Nov. 2011-Oct. 2015)			
Variable	Mean	P10	P50	Max	Mean	P10	P50	Max	Mean	P10	P50	Max	Mean	P10	P50	Max
Worker-level																
Female	0.49	0.00	0.00	1.00	0.49	0.00	0.00	1.00	0.43	0.00	0.00	1.00	0.44	0.00	0.00	1.00
Schooling	11.29	6.00	12.00	16.00	11.76	9.00	12.00	16.00	9.42	4.00	9.00	16.00	9.95	4.00	9.00	16.00
	[3.36]				[3.10]				[3.64]				[3.62]			
Age	23.32	18.00	23.00	34.00	23.23	19.00	23.00	34.00	34.94	23.00	33.00	68.00	36.32	24.00	35.00	68.00
	[4.25]				[3.87]				[10.03]				[10.20]			
Emp. duration	5.26	1.00	5.00	12.00	4.98	1.00	4.00	12.00	5.72	1.00	5.00	12.00	5.56	1.00	5.00	12.00
	[3.42]				[3.35]				[3.39]				[3.38]			
Job-level																
Fixed-term	0.76	0.00	1.00	1.00	0.83	0.00	1.00	1.00	0.70	0.00	1.00	1.00	0.75	0.00	1.00	1.00
Conv. rate	0.15	0.00	0.00	1.00	0.15	0.00	0.00	1.00	0.13	0.00	0.00	1.00	0.14	0.00	0.00	1.00
	[0.35]				[0.36]				[0.34]				[0.35]			
Exit private-sector emp.	0.42	0.00	0.00	1.00	0.36	0.00	0.00	1.00	0.37	0.00	0.00	1.00	0.29	0.00	0.00	1.00
Job-to-job	0.20	0.00	0.00	1.00	0.26	0.00	0.00	1.00	0.22	0.00	0.00	1.00	0.27	0.00	0.00	1.00
Monthly wage (/100)	7.71	4.76	6.18	2419.69	7.17	4.74	6.00	2848.26	8.12	4.99	6.48	1452.48	7.82	4.82	6.20	4430.32
	[17.12]				[14.74]				[7.70]				[8.43]			
Severance pay (/100)	6.00	0.00	2.88	1246.61	1.83	0.00	1.39	69.21	7.76	0.00	3.35	1526.64	2.17	0.00	1.69	68.01
	[12.57]				[2.09]				[14.59]				[2.53]			

	Panel B: Incumbents ($\leq$ Oct. 2009)			
Variable	Mean	P10	P50	Max
Worker-level				
Female	0.51	0.00	1.00	1.00
Schooling	10.16	4.00	9.00	16.00
	[4.19]			
Age	41.99	29.00	41.00	68.00
	[10.49]			
Emp. duration	146.85	37.00	125.00	540.00
	[101.08]			
Job-level				
Fixed-term	0.10	0.00	0.00	1.00
Conv. rate	0.03	0.00	0.00	1.00
	[0.17]			
Exit private-sector emp.	0.23	0.00	0.00	1.00
Job-to-job	0.11	0.00	0.00	1.00
Monthly wage (/100)	12.42	5.36	8.41	1519.16
	[13.21]			
Severance pay (/100)	160.10	23.52	86.17	12561.56
	[237.04]			

Notes: The table displays descriptive statistics for the three worker groups used in the empirical analysis. For new entrants and newly hired workers with prior experience, variables are measured at job start, that is, within the first 12 months of the spell. For incumbent workers already employed at the firm, variables are measured at their first observation in the analysis period. Standard deviations for continuous variables are reported in square brackets. 'P10' denotes the 10th percentile and 'P50' the 50th percentile. Female and fixed-term are indicator variables; schooling and age are measured in years, employment duration in months, and exit private-sector emp. denotes exit from private-sector employment. Source: Own calculations.

Table 4: Threshold-local worker-flow estimates around the 2011 and 2013 reform cutoffs.

	November 2011 cutoff		October 2013 cutoff	
Outcome	New entrants	New hires	New entrants	New hires
Any separation	-0.0325***	-0.0511***	-0.0178***	-0.0193***
	(0.0081)	(0.0052)	(0.0055)	(0.0041)
Exit from private-sector employment	-0.1251***	-0.1117***	0.0048	-0.0226***
	(0.0181)	(0.0104)	(0.0111)	(0.0057)
Job-to-job transition	0.0926***	0.0606***	-0.0227**	0.0033
	(0.0176)	(0.0105)	(0.0099)	(0.0058)
Observations	55,024	212,583	61,879	238,998

Notes: The table reports the post-cutoff coefficient estimated in a ±6 month window around the November 2011 and October 2013 reform cutoffs, restricting the sample to firms that hire on both sides of each cutoff. All specifications include age, gender, the college indicator, a linear control for month of hiring relative to the cutoff, log sales and firm fixed effects. Standard errors clustered at the firm level are reported in parentheses. Columns distinguish new entrants from newly hired workers with prior experience. Source: Own calculations.

Table 5: IV mechanism evidence on overall worker flows by worker group.

	All separations		Competing risks by exit mode	
	Baseline (1)	Full controls (2)	Baseline (3)	Full controls (4)
Panel A: New entrants				
Observations: 1,171,784				
Any separation	-0.2341***	-0.1900***	—	—
	(0.010)	(0.011)		
Exit from private-sector employment	—	—	-0.1501***	-0.0767***
			(0.019)	(0.020)
Job-to-job transition	—	—	-0.8483***	-0.9255***
			(0.074)	(0.082)
KP rk Wald F-stat Test	13.806	13.586	—	—
Stock-Yogo 10% threshold	16.38	16.38	—	—
Mean exit from private-sector employment (%)	—	—	18.89	18.89
Mean job-to-job transition (%)	—	—	13.34	13.34
Panel B: New hires with prior experience				
Observations: 4,910,300				
Any separation	-0.1641***	-0.1217***	—	—
	(0.005)	(0.007)		
Exit from private-sector employment	—	—	-0.1774***	-0.1267***
			(0.009)	(0.009)
Job-to-job transition	—	—	-0.2016***	-0.1427***
			(0.027)	(0.022)
KP rk Wald F-stat Test	25.074	24.086	—	—
Stock-Yogo 10% threshold	16.38	16.38	—	—
Mean exit from private-sector employment (%)	—	—	15.31	15.31
Mean job-to-job transition (%)	—	—	12.86	12.86
Panel C: Incumbents				
Observations: 11,007,182				
Any separation	-0.1368***	-0.1253***	—	—
	(0.019)	(0.020)		
Exit from private-sector employment	—	—	-0.0644***	-0.0322*
			(0.020)	(0.019)
Job-to-job transition	—	—	-0.3065***	-0.3901***
			(0.033)	(0.040)
KP rk Wald F-stat Test	130.969	118.011	—	—
Stock-Yogo 10% threshold	16.38	16.38	—	—
Mean exit from private-sector employment (%)	—	—	7.74	7.74
Mean job-to-job transition (%)	—	—	3.71	3.71
Year dummies	✓	✓	✓	✓
Sector dummies	✓	✓	✓	✓
Firm characteristics		✓		✓
Demographic characteristics		✓		✓

Notes: Columns (1) and (2) report IV logit estimates for all separations, while columns (3) and (4) report IV competing-risks estimates for exits from private-sector employment and job-to-job transitions. Severance pay is instrumented with the gap between counterfactual severance pay absent the reforms and actual severance pay under the reforms. Coefficients are severance-pay elasticities. Specifications (1) and (3) include tenure controls, broad-sector dummies, and year dummies. Specifications (2) and (4) additionally include age, gender, the college indicator, firm-size dummies, and log sales. Kleibergen-Paap first-stage statistics and the Stock-Yogo 10% critical value are reported below the estimates. Source: Own calculations.

Table 6: IV mechanism evidence by contract type and exit mode.

	Baseline specification		Full controls	
	Fixed-Term (1)	Open-Ended (2)	Fixed-Term (3)	Open-Ended (4)
Panel A: New entrants				
Observations: 1,171,784				
Any separation	-0.1681***	-0.2905***	-0.1426***	-0.2135***
	(0.020)	(0.012)	(0.020)	(0.015)
Exit from private-sector employment	0.1354*	-0.2950***	0.1943***	-0.1369
	(0.073)	(0.020)	(0.075)	(0.170)
Job-to-job transition	-1.5127***	-0.4475***	-1.5974***	-0.5397***
	(0.338)	(0.049)	(0.352)	(0.061)
KP rk Wald F-stat Test	8.440	6.176	8.394	5.682
Stock-Yogo 10% threshold	16.38	16.38	16.38	16.38
Mean exit from private-sector employment (%)	26.65	9.32	26.65	9.32
Mean job-to-job transition (%)	18.07	7.52	18.07	7.52
Panel B: New hires with prior experience				
Observations: 4,910,300				
Any separation	-0.1881***	-0.1532***	-0.1763***	-0.0868***
	(0.022)	(0.006)	(0.022)	(0.015)
Exit from private-sector employment	-0.1881***	-0.1583***	-0.1554***	-0.0656***
	(0.029)	(0.008)	(0.028)	(0.018)
Job-to-job transition	-0.2030***	-0.1593***	-0.2200***	-0.1120***
	(0.035)	(0.014)	(0.047)	(0.014)
KP rk Wald F-stat Test	7.602	12.400	7.432	11.083
Stock-Yogo 10% threshold	16.38	16.38	16.38	16.38
Mean exit from private-sector employment (%)	21.62	9.13	21.62	9.13
Mean job-to-job transition (%)	17.57	8.26	17.57	8.26
Panel C: Incumbents				
Observations: 11,007,182				
Any separation	-0.0013	-0.1341***	0.0027	-0.1349***
	(0.004)	(0.020)	(0.003)	(0.021)
Exit from private-sector employment	0.0064	-0.0606***	0.0103	-0.0385*
	(0.006)	(0.021)	(0.006)	(0.020)
Job-to-job transition	-0.0461	-0.3105***	-0.0540	-0.4078***
	(0.083)	(0.035)	(0.086)	(0.043)
KP rk Wald F-stat Test	15.056	121.596	14.526	109.140
Stock-Yogo 10% threshold	16.38	16.38	16.38	16.38
Mean exit from private-sector employment (%)	15.69	7.12	15.69	7.12
Mean job-to-job transition (%)	8.19	3.36	8.19	3.36
Year dummies	✓	✓	✓	✓
Sector dummies	✓	✓	✓	✓
Firm characteristics			✓	✓
Demographic characteristics			✓	✓

Notes: Columns (1) and (2) report IV logit estimates for all separations by contract type. Columns (3) and (4) report IV competing-risks estimates for exits from private-sector employment and job-to-job transitions by contract type. Severance pay is instrumented with the gap between counterfactual severance pay absent the reforms and actual severance pay under the reforms. Coefficients are severance-pay elasticities. Specifications (1) and (2) include tenure controls, broad-sector dummies, and year dummies. Specifications (3) and (4) additionally include age, gender, the college indicator, firm-size dummies, and log sales. Kleibergen-Paap first-stage statistics and the Stock-Yogo 10% critical value are reported below the estimates. Source: Own calculations.

Table 7: Conversions from fixed-term to open-ended contracts.

	Baseline controls (1)	Full controls (2)
Panel A: New entrants		
Observations: 626,557		
Conversions from Fixed-Term to Open-Ended Contracts	-0.0562	-0.0023
	(0.072)	(0.044)
KP rk Wald F-stat Test	8.399	8.355
Stock-Yogo 10% threshold	16.38	16.38
Mean Conversion Rate (%)	10.17	10.17
Panel B: New hires		
Observations: 2,354,283		
Conversions from Fixed-Term to Open-Ended Contracts	-0.8435***	-0.6668***
	(0.112)	(0.101)
KP rk Wald F-stat Test	7.443	7.284
Stock-Yogo 10% threshold	16.38	16.38
Mean Conversion Rate (%)	9.53	9.53
Panel C: Incumbents		
Observations: 697,140		
Conversions from Fixed-Term to Open-Ended Contracts	-0.0193***	-0.0411***
	(0.006)	(0.013)
KP rk Wald F-stat Test	13.135	12.765
Stock-Yogo 10% threshold	16.38	16.38
Mean Conversion Rate (%)	13.30	13.30
Year dummies	✓	✓
Sector dummies	✓	✓
Firm characteristics		✓
Demographic characteristics		✓

Notes: Columns (1) and (2) report IV estimates for within-firm conversion from fixed-term to open-ended contracts.Severance pay is instrumented with the gap between counterfactual severance pay absent the reforms and actual severance pay under the reforms. Coefficients are severance-pay elasticities. The baseline specification includes tenure controls, broad-sector dummies, and year dummies; the full specification additionally includes age, gender, the college indicator, seniority, firm-size dummies, and log sales. Kleibergen-Paap first-stage statistics and the Stock-Yogo 10% critical value are reported below the estimates. Source: Own calculations.

Table 8: Hiring and contract composition.

Panel A: Hiring composition		
	Fixed-term hirings / total hirings	Fixed-term hirings / open-ended hirings
	(1)	(2)
Severance pay	-0.1857***	-0.4504***
	(0.021)	(0.119)
KP rk Wald F-stat Test	90.325	53.811
Stock-Yogo 10% threshold	16.38	16.38
Observations	271,994	133,025
Year dummies	✓	✓
Sector dummies	✓	✓
Firm characteristics	✓	✓
Demographic characteristics	✓	✓
Panel B: Hiring volumes		
	Number of fixed-term hirings	Number of open-ended hirings
	(3)	(4)
Severance pay	-0.6272***	0.0661***
	(0.115)	(0.059)
KP rk Wald F-stat Test	76.606	76.606
Stock-Yogo 10% threshold	16.38	16.38
Observations	526,884	526,884
Year dummies	✓	✓
Sector dummies	✓	✓
Firm characteristics	✓	✓
Demographic characteristics	✓	✓
Panel C: Employment stocks		
	Share of fixed-term employment	Share of open-ended employment
	(5)	(6)
Severance pay	-0.3539***	0.1287***
	(0.049)	(0.021)
KP rk Wald F-stat Test	76.606	76.606
Stock-Yogo 10% threshold	16.38	16.38
Observations	526,884	526,884
Year dummies	✓	✓
Sector dummies	✓	✓
Firm characteristics	✓	✓
Demographic characteristics	✓	✓

Notes: IV estimates for firm-level hiring and employment composition outcomes by contract type. Severance pay is instrumented with the reform-induced gap between counterfactual severance pay absent the reforms and actual severance pay under the reforms. Coefficients are severance-pay elasticities. All specifications include year and broad-sector dummies, firm characteristics, and demographic controls. Source: Own calculations.

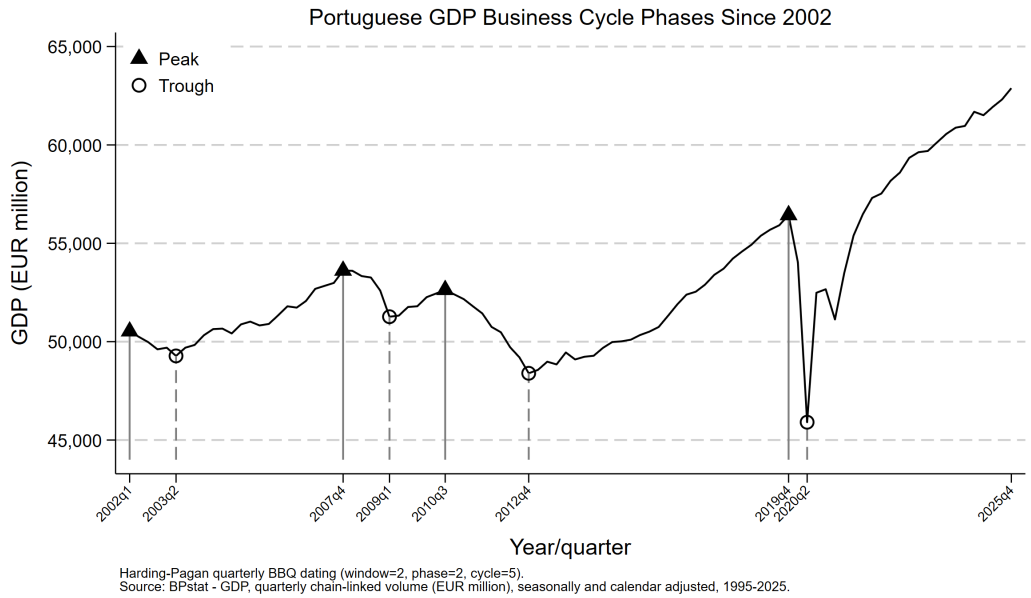
Table 9: Within-match wage estimates.

	(1) Broad sample	(2) New entrants	(3) New hires with prior experience	(4) Incumbents
Severance pay	0.0000125***	0.0000568***	0.0000432***	0.0000205***
	(0.0000004)	(0.0000083)	(0.0000029)	(0.0000010)
Observations	42,310,827	1,171,144	4,909,269	10,992,877

Notes: The table reports within-match wage estimates in the largest connected set of the worker-firm mobility network. Coefficients are severance-pay marginal effects. The dependent variable is the log of monthly wage. All specifications control for age squared, schooling, tenure squared, log firm size, and year fixed effects, and absorb worker-firm-match fixed effects. Columns (2)–(4) estimate the same specification separately for the three main worker groups used throughout the analysis. Standard errors clustered at the worker level are reported in parentheses. Source: Own calculations.

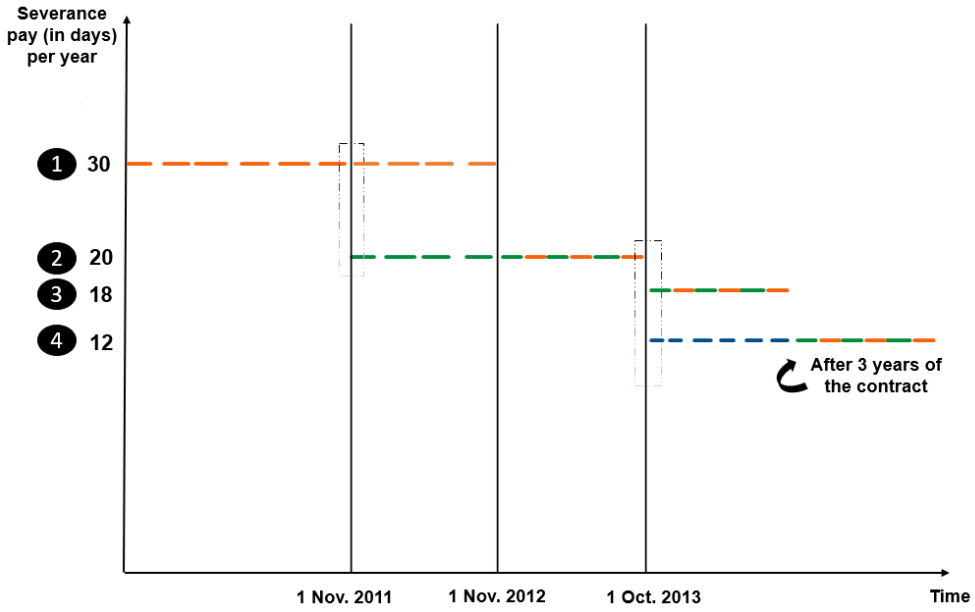
Appendix A: Additional Tables and Figures

Appendix Figure A1: Portuguese business-cycle chronology and GDP dynamics since 2002.



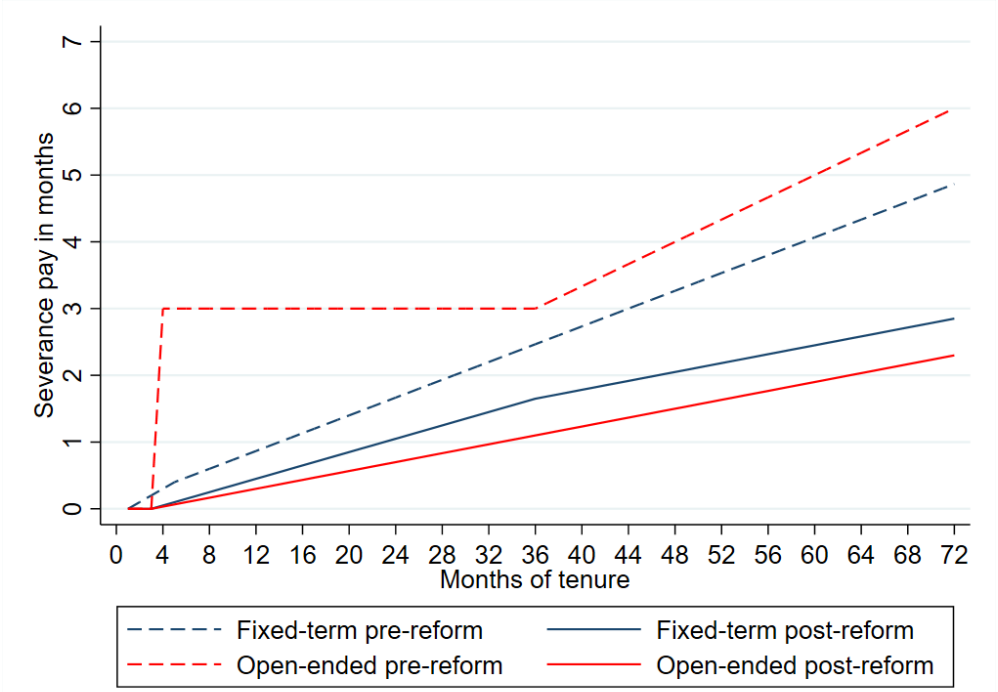
Notes: The figure reports Portuguese GDP dynamics and business-cycle dating from 2002 onward. It complements Table 1 by showing that October 2008 falls in the contraction phase that followed the Global Financial Crisis, before the later sovereign-debt and adjustment-program phase became the dominant macroeconomic environment. This timing motivates the use of October 2008 as the baseline placebo benchmark in Appendix B. Source: Own calculations.

Appendix Figure A2: Statutory severance-pay schedules.



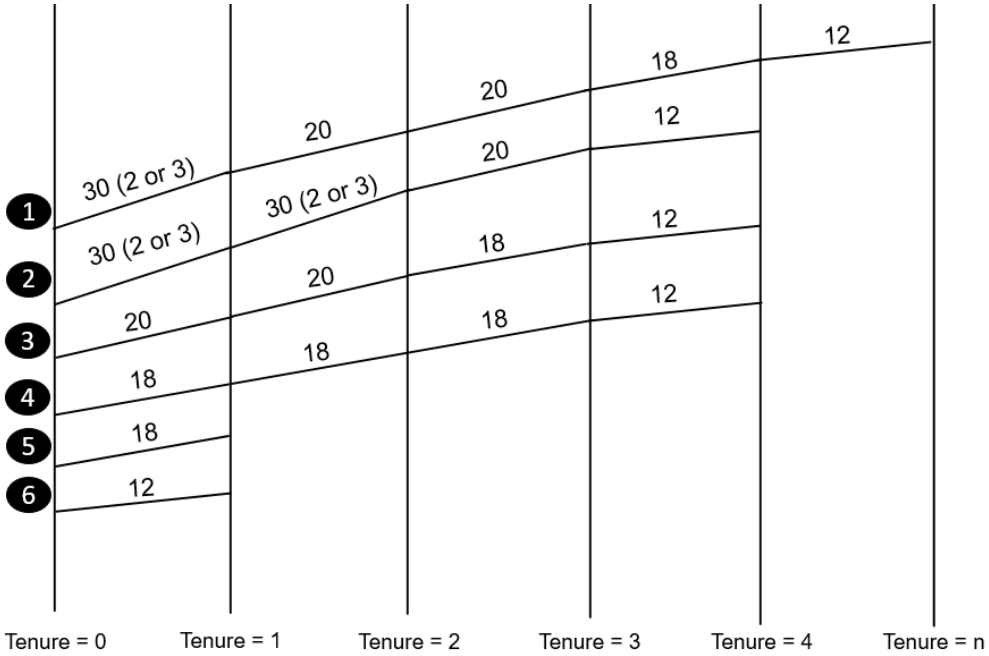
Notes: The vertical axis represents severance pay in days of base wage and tenure-related regular pay for each full year of work, while the horizontal axis shows the corresponding period. For simplicity, the minimum of three months' severance pay for permanent contracts signed before 1 November 2011 is not depicted, nor are the caps that could be paid to workers. The different paths represent the discontinuities imposed by the severance-pay reforms, conditional on the admission date with the firm. Lines with different colors distinguish workers hired under different severance-pay regimes followed over time. 1: workers hired before 1 November 2011 kept 30 days of base wage and tenure-related regular pay until 31 October 2012. 2: workers hired between 1 November 2011 and 30 September 2013 were entitled to 20 days for each full year, and those hired under the old severance-pay regime were also entitled to 20 days for the same tenure level after the transition. 3: workers hired under the old regime and those hired between 1 November 2011 and 30 September 2013 received 18 days for each full year if contract duration was less than three years on 1 October 2013, maintaining this amount until completing three years of the contract. 4: new contracts signed from 1 October 2013 onward were entitled to 12 days for each full year; workers hired under previous regimes were also entitled to the same amount after three years of contract duration.

Appendix Figure A3: Severance-pay schedules by contract type before and after the reforms.



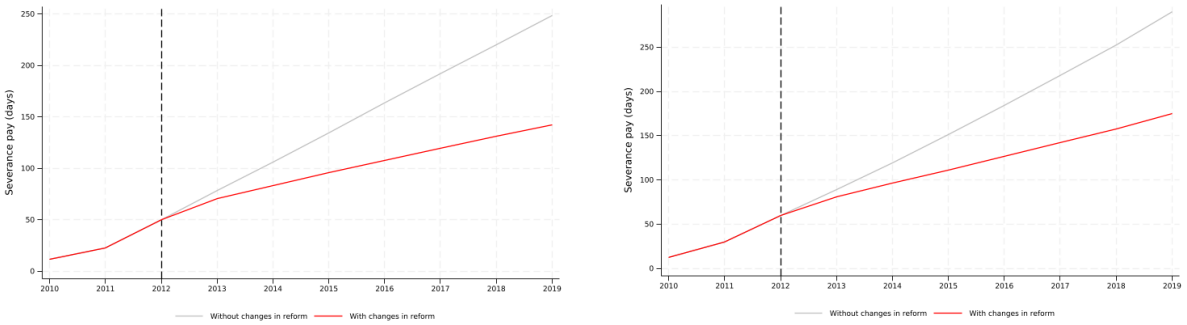
Notes: The figure shows how severance pay differs before and after the reforms for temporary and permanent contracts, according to tenure. Over the reform period, severance pay on permanent contracts was reduced from 30 days of base wage and tenure-related regular pay for each year of work to 12 days, while severance pay on temporary contracts was reduced from 2–3 days for each working month to 18 days for each year when contract duration was up to three years and 12 days thereafter. The standard duration of fixed-term contracts in Portugal is up to three years, while uncertain-term contracts may last up to six years, which explains the six-year horizon displayed on the x-axis. The trial period is also considered, during which no severance pay is due. This period is assumed to be 90 days for permanent positions and 30 days for temporary jobs. The caps on severance pay induced by the reforms are not accounted for.

Appendix Figure A4: Severance-pay rules across contract paths.



Notes: The different paths, characterized by different slopes, represent differences in severance-pay rules conditional on the starting date with the firm. 1: hired before 1 November 2011 but did not complete three years by 1 October 2013, for example workers hired in October 2011. 2: hired before 1 November 2011 and completed three years by 1 October 2013, for example workers hired in October 2010. 3: hired between 1 November 2011 and 30 September 2013, for example workers hired in November 2011. 4: hired on 1 October 2013 with an uncertain-term contract. 5: hired on 1 October 2013 with a fixed-term contract. 6: hired on 1 October 2013 with an open-ended contract. Values in parentheses represent severance pay in days for each year of seniority for fixed-term contracts, corresponding initially to three days of base wage and tenure-related regular pay for each month of work if contract duration was less than six months and two days otherwise.

Appendix Figure A5: Average severance-pay exposure with and without reforms by year.

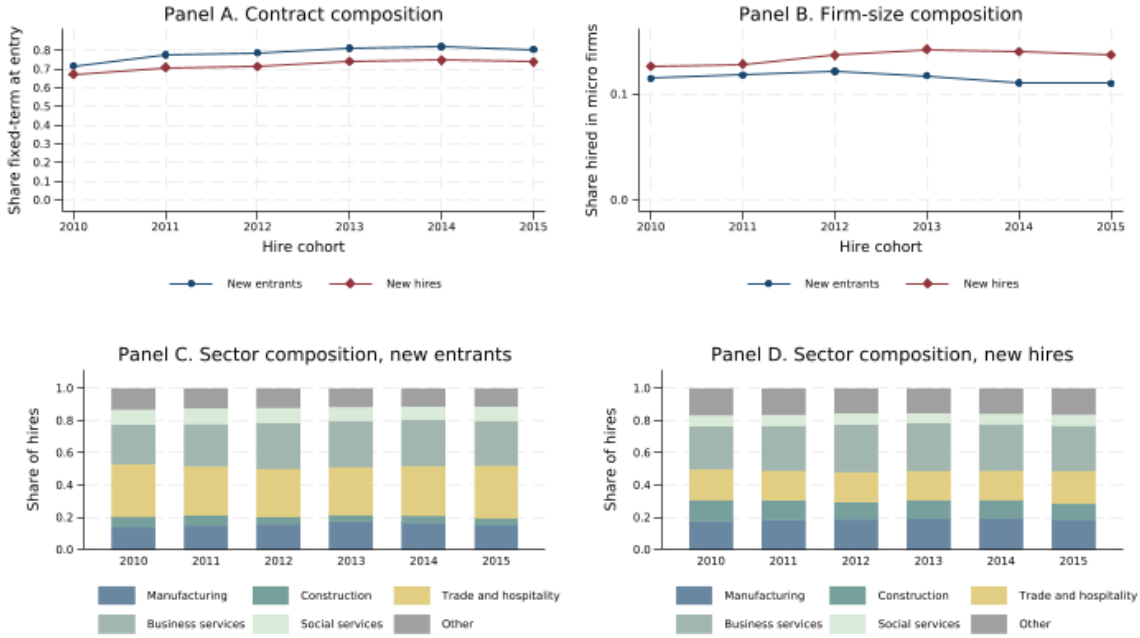


Panel A: New entrants

Panel B: New hires with prior experience

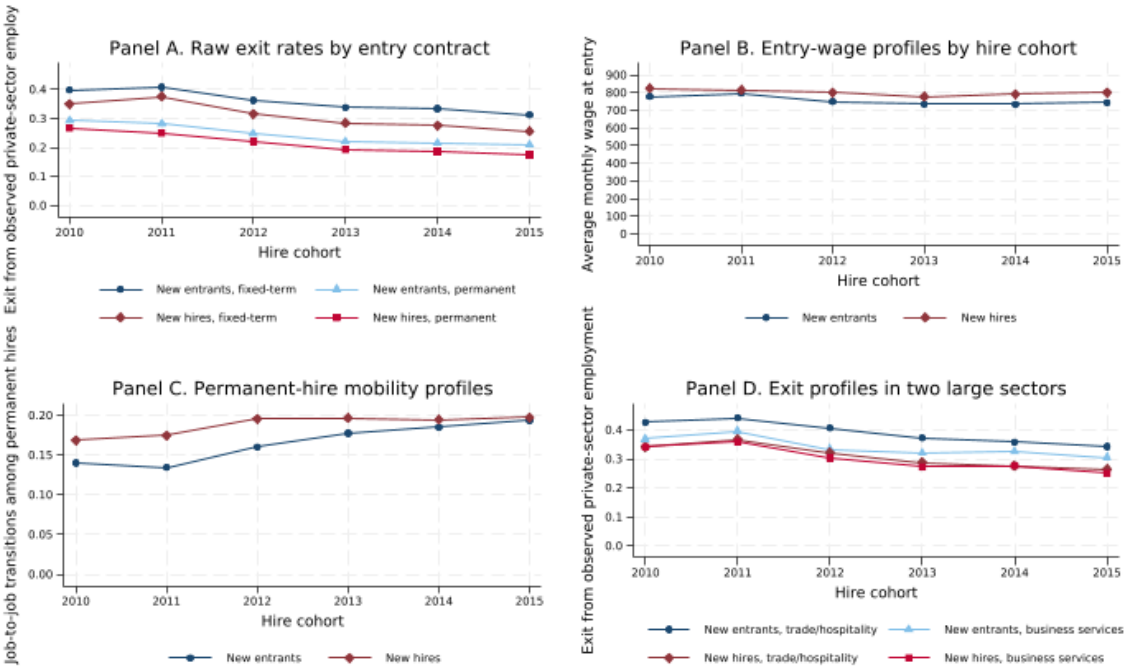
Notes: The figure contrasts severance-pay exposure under a no-reform counterfactual with exposure under the actual reform scenario for hiring cohorts admitted before the reforms, that is, workers whose admission dates fall between November 2009 and October 2011 and who are observed over time within a given firm. Panel A refers to new entrants and Panel B to newly hired workers with prior experience. Source: Own calculations.

Appendix Figure A6: Hire-cohort composition.



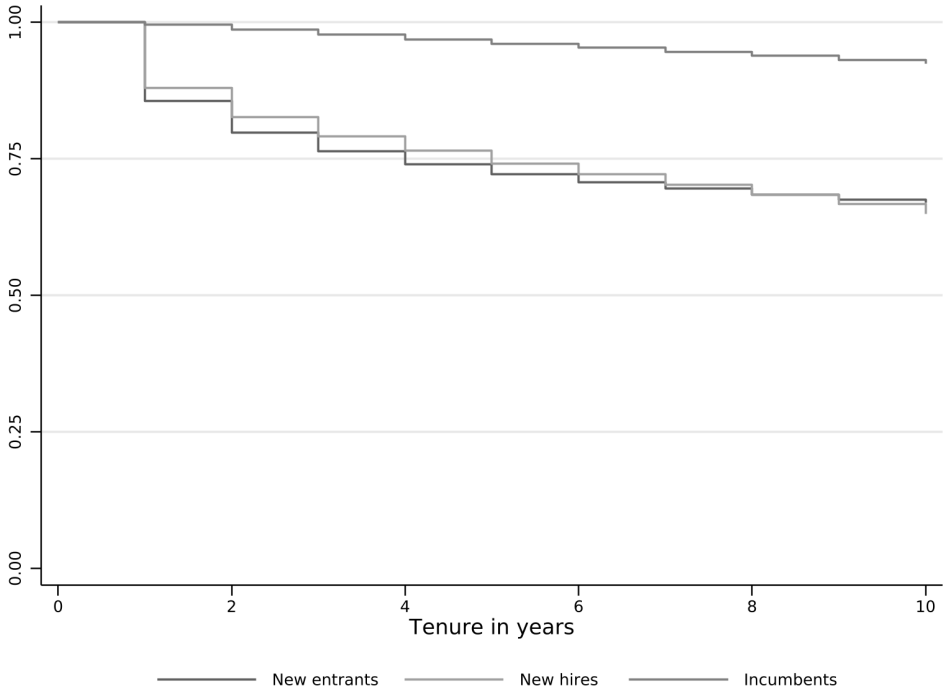
Notes: The figure reports hire-cohort composition for new entrants and newly hired workers with prior experience. Panel A shows the share of hires entering on fixed-term contracts, and Panel B shows the share of hires entering micro firms. Panels C and D decompose hires by broad sector for new entrants and newly hired workers with prior experience, respectively. Shares are computed by hire cohort. Source: Own calculations.

Appendix Figure A7: Raw hire-cohort outcomes.



Notes: The figure reports raw hire-cohort outcome profiles for new entrants and newly hired workers with prior experience. Panel A shows separations, Panel B shows wages, Panel C shows job-to-job transitions, and Panel D shows the broad-sector composition of hires. Profiles are computed by hire cohort. Source: Own calculations.

Appendix Figure A8: Kaplan-Meier survival estimates.



Notes: The figure reports Kaplan-Meier survival estimates for worker-firm spells. The horizontal axis measures tenure in years, constructed by converting contract duration from months to years and rounding to the nearest year. Survival profiles are shown separately for the worker groups used in the analysis and are right-censored at 10 years. Source: Own calculations.

Appendix B: Cohort Comparability and Narrow-Window Diagnostics

Table B1: Balance of observables around the 2011 threshold.

Variable	Mean, pre-cutoff (1)	Mean, post-cutoff (2)	Standardized difference (3)	Cutoff coefficient (4)	p-value (5)
Age	32.748	33.029	0.028	0.316	0.033
Female	0.461	0.460	-0.002	0.004	0.546
College	0.189	0.195	0.015	-0.003	0.610
Log sales	16.218	16.192	-0.011	0.004	0.307

Notes: The table presents balance diagnostics for workers hired in a ±6 month window around the November 2011 reform cutoff. Columns (1) and (2) show pre- and post-cutoff means, and column (3) reports the standardized difference, computed as the post-cutoff mean minus the pre-cutoff mean divided by the standard deviation within the local window. Columns (4) and (5) report the cutoff coefficient and its p-value from local balance regressions with firm fixed effects and a linear control for month of hiring relative to the cutoff. Economically, the cutoff coefficient asks whether workers hired just after the legal threshold differ from those hired just before it, after netting out persistent firm differences and smooth local hiring-month trends. Source: Own calculations.

Table B2: Balance of observables around the 2013 threshold.

Variable	Mean, pre-cutoff (1)	Mean, post-cutoff (2)	Standardized difference (3)	Cutoff coefficient (4)	p-value (5)
Age	34.000	33.578	-0.040	-0.391	0.195
Female	0.462	0.451	-0.022	-0.010	0.334
College	0.186	0.206	0.049	0.011	0.015
Log sales	15.983	16.156	0.075	-0.017	0.000

Notes: The table presents the same balance diagnostics as Table B1, but for workers hired in a ±6 month window around the October 2013 reform cutoff. Columns (1) and (2) show pre- and post-cutoff means, and column (3) reports the standardized difference, computed as the post-cutoff mean minus the pre-cutoff mean divided by the standard deviation within the local window. Columns (4) and (5) report the cutoff coefficient and its p-value from local balance regressions with firm fixed effects and a linear control for month of hiring relative to the cutoff. Source: Own calculations.

Table B3: October placebo-cutoff diagnostics.

Panel A: Balance around the October 2008 placebo cutoff

Variable	Mean, pre-cutoff (1)	Mean, post-cutoff (2)	Standardized difference (3)	Cutoff coefficient (4)	p-value (5)
Age	31.808	32.140	0.033	-0.401	0.003
Female	0.438	0.430	-0.016	-0.003	0.472
College	0.163	0.162	-0.001	-0.006	0.145
Log sales	15.869	15.757	-0.047	-0.046	0.000

Panel B: Outcome discontinuities across October pseudo-cutoffs

Outcome	Oct. 2008 New entrants	Oct. 2008 New hires	Oct. 2009 New entrants	Oct. 2009 New hires	Oct. 2010 New entrants	Oct. 2010 New hires
			p-values in parentheses			
Any separation	0.009	0.002	0.003	-0.004	0.016	0.005
	(0.254)	(0.630)	(0.761)	(0.445)	(0.142)	(0.296)
Exit from private-sector employment	0.015	-0.000	-0.009	-0.007	0.002	0.004
	(0.167)	(0.991)	(0.519)	(0.305)	(0.887)	(0.684)
Job-to-job transition	-0.007	0.002	0.012	0.003	0.014	0.002
	(0.562)	(0.777)	(0.301)	(0.618)	(0.203)	(0.875)
Conversion from fixed-term to open-ended contract	-0.034	-0.023	-0.003	0.006	-0.023	-0.011
	(0.012)	(0.036)	(0.829)	(0.325)	(0.248)	(0.111)

Notes: The table reports placebo threshold tests around October pseudo cutoffs before the severance-pay reforms. Panel A reports local balance diagnostics around the October 2008 pseudo cutoff. Columns (1) and (2) show pre- and post-cutoff means, and column (3) reports the standardized difference, computed as the post-cutoff mean minus the pre-cutoff mean divided by the standard deviation within the local window. Columns (4) and (5) report the coefficient and p-value on the placebo post-cutoff indicator from local balance regressions with firm fixed effects and a linear control for month of hiring relative to the pseudo cutoff. Panel B reports placebo discontinuities in outcomes around October 2008, October 2009, and October 2010 pseudo cutoffs; p-values are shown in parentheses below coefficients. Panel B uses the same entry controls as Table 4: age, gender, the college indicator, and log sales, plus firm fixed effects and a linear control for month of hiring relative to the pseudo cutoff. Conversion rows are estimated on fixed-term local hires only. Each pseudo-cutoff sample contains hires in a ±6 month window and keeps only firms hiring on both sides of the corresponding date. Source: Own calculations.

Table B4: Cumulative exit outcomes in the threshold-local design.

	November 2011 cutoff		October 2013 cutoff	
Horizon outcome	New entrants	New hires	New entrants	New hires
Panel A. Any separation within horizon				
Within 12 months	-0.0571*	-0.0890***	-0.0025	-0.0166*
	(0.0307)	(0.0122)	(0.0096)	(0.0089)
Within 24 months	-0.0382***	-0.0792***	-0.0053	-0.0178***
	(0.0146)	(0.0086)	(0.0087)	(0.0061)
Within 36 months	-0.0318***	-0.0568***	-0.0108	-0.0148***
	(0.0112)	(0.0080)	(0.0068)	(0.0051)
Panel B. Exit from private-sector employment within horizon				
Within 12 months	-0.0971***	-0.0961***	0.0071	-0.0157***
	(0.0276)	(0.0110)	(0.0100)	(0.0053)
Within 24 months	-0.1144***	-0.1162***	0.0070	-0.0203***
	(0.0203)	(0.0107)	(0.0107)	(0.0053)
Within 36 months	-0.1187***	-0.1114***	0.0061	-0.0195***
	(0.0183)	(0.0111)	(0.0107)	(0.0056)
Panel C. Job-to-job transition within horizon				
Within 12 months	0.0400***	0.0071	-0.0097	-0.0009
	(0.0105)	(0.0084)	(0.0073)	(0.0068)
Within 24 months	0.0762***	0.0370***	-0.0123	0.0025
	(0.0157)	(0.0108)	(0.0092)	(0.0060)
Within 36 months	0.0868***	0.0546***	-0.0169*	0.0047
	(0.0167)	(0.0108)	(0.0097)	(0.0054)
Observations	55,024	212,583	61,879	238,998

Notes: The table reports cumulative 12-, 24-, and 36-month outcomes in the threshold-local design. Entries are coefficients from firm fixed-effects linear probability models estimated in a ±6 month window around the November 2011 and October 2013 reform cutoffs, restricting the sample to firms that hire on both sides of each cutoff. All specifications include a linear control for month of hiring relative to the cutoff and entry controls for age, gender, the college indicator, and log sales. Standard errors clustered at the firm level are reported in parentheses. Source: Own calculations.

Appendix C: Estimation Algebra and Identification Details

This appendix formalizes the instrumental-variables and control-function logic used in the broader mechanism specifications. The purpose is not to introduce a separate source of identifying variation, but to make explicit how the reform-induced severance-pay gap enters the first stage and how the resulting residual is used in nonlinear worker-flow and conversion models, including how inference deals with the generated residual.

C.1 Endogenous severance-pay exposure and first stage. Let SP_{ijt} denote the actual monetary severance-pay exposure of worker i employed at firm j in year t. This variable is potentially endogenous because it embeds the worker's wage base and accumulated job history, both of which may be correlated with unobserved worker, firm, or match components. The instrument is the reform-induced gap between the counterfactual entitlement that would have applied under the pre-reform schedule and the entitlement implied by the actual legal schedule:

$$GapSP_{ijt} = SP_{ijt}^{old} - SP_{ijt}^{actual}.$$

Both terms are computed using the same observed wage-tenure inputs. The variation used for identification therefore comes from the way national legal schedules map otherwise similar worker-firm histories into different statutory entitlements. Let Z_{ijt} collect the exogenous controls and fixed effects included in the relevant specification. The linear first stage is

$$SP_{ijt} = Z_{ijt}'\pi + \phi GapSP_{ijt} + v_{ijt}. \tag{C.1}$$

Instrument relevance requires $\phi \neq 0$, or equivalently $Cov(SP_{ijt}, GapSP_{ijt} \mid Z_{ijt}) \neq 0$, and is assessed in the first-stage diagnostics reported with the mechanism tables.

C.2 Linear IV and control-function equivalence. Consider the structural equation

$$Y_{ijt} = Z_{ijt}'\beta + \delta SP_{ijt} + \varepsilon_{ijt}. \tag{C.2}$$

If SP_{ijt} is correlated with ε_{ijt}, ordinary regression does not recover δ. The control-function representation assumes that the endogenous part of the structural error is proportional to the first-stage innovation,

$$\varepsilon_{ijt} = \rho v_{ijt} + \eta_{ijt}, \qquad E[\eta_{ijt} \mid Z_{ijt}, GapSP_{ijt}] = 0. \tag{C.3}$$

Substituting (C.3) into (C.2) gives

$$Y_{ijt} = Z'_{ijt}\beta + \delta SP_{ijt} + \rho v_{ijt} + \eta_{ijt}. \tag{C.4}$$

Since v_{ijt} is not observed, the feasible equation replaces it with the first-stage residual $\widehat{v}_{ijt}$ from (C.1):

$$Y_{ijt} = Z'_{ijt}\beta + \delta SP_{ijt} + \rho \widehat{v}_{ijt} + e_{ijt}. \tag{C.5}$$

In a linear second-stage model, estimating (C.5) delivers the same point estimate for δ as the usual 2SLS estimator, under the standard linear-model conditions described by Wooldridge (2015). The difference is only operational: 2SLS replaces the endogenous regressor with its first-stage fitted component, whereas the control-function formulation keeps SP_{ijt} in the equation and adds the first-stage residual. This is the logic used for the linear firm-composition and wage specifications, where the empirical implementation estimates the IV model directly by 2SLS.

C.3 Nonlinear residual inclusion. The worker-flow and conversion specifications are nonlinear because the outcomes are modeled as discrete states. In those cases, the paper uses the corresponding first-stage residual as a control function, following the residual-inclusion logic of Rivers and Vuong (1988), Papke and Wooldridge (2008), and Petrin and Train (2010). For a generic transition state k relative to a normalized base state, the multinomial-logit index is

$$\Theta^k_{ijt} = Z'_{ijt}\beta_k + \gamma_k SP_{ijt} + \rho_k \widehat{v}_{ijt}. \tag{C.6}$$

The corresponding transition probability is

$$Pr(S_{ijt} = k \mid Z_{ijt}, SP_{ijt}, \widehat{v}_{ijt}) = \frac{\exp(\Theta^k_{ijt})}{1 + \sum_{h \in \mathcal{K}} \exp(\Theta^h_{ijt})}. \tag{C.7}$$

For the worker-flow models, the states distinguish continuation with the same employer from exit from private-sector employment and job-to-job mobility. The all-separation models are the binary analogue of the same residual-inclusion approach. For the conversion models, the state space is restricted to temporary-contract spells and distinguishes within-firm conversion into an open-ended contract from non-conversion continuation and non-conversion separation from the firm. The estimated coefficients are transformed into the severance-pay elasticities reported in the tables.

C.4 Generated residuals and bootstrap standard errors. Because $\widehat{v}_{ijt}$ is estimated in

the first stage, the second-stage residual-inclusion model contains a generated regressor. In the nonlinear control-function specifications, inference therefore uses worker-level cluster bootstrap standard errors for the reported marginal effects and elasticities, rather than treating the first-stage residual as fixed.[16] This is particularly relevant for the worker-flow and conversion models, where the object reported in the tables is a transformed effect from a multinomial-logit residual-inclusion model. For the linear firm-composition and wage specifications, the paper instead estimates the IV model directly by 2SLS, with standard errors clustered at the firm level in the firm-composition models and at the worker level in the wage models.

C.5 Interpretation of the residual term. The coefficient ρ_k captures the remaining association between the unobserved determinants of severance-pay exposure and the unobserved determinants of transition state k, after conditioning on controls and fixed effects. If $\rho_k = 0$, the residual inclusion is unnecessary for that margin. If $\rho_k \neq 0$, the residual absorbs the component of SP_{ijt} that is correlated with the structural error, allowing γ_k to be interpreted as the severance-pay response under the maintained control-function assumptions. The sign of ρ_k is therefore informative about the direction of endogeneity, in the same spirit as a Hausman-type comparison (Hausman, 1978), although the substantive object of interest remains the severance-pay coefficient.

C.6 Identifying assumptions. The approach relies on three conditions. First, the gap must be relevant for actual severance-pay exposure, which is evaluated using the reported first-stage diagnostics. Second, conditional on the included controls, fixed effects, contract type, tenure structure, and admission cohort, the gap must affect worker and firm outcomes only through actual statutory severance-pay exposure. Third, the nonlinear residual-inclusion models require the control-function restriction that the part of the structural error correlated with SP_{ijt} is captured by the first-stage innovation. These assumptions are supported, but not mechanically guaranteed, by the institutional design of the reforms: the severance-pay gap is generated by national legal schedules and grandfathering rules rather than by choices of individual firms or workers. This is why the paper reads the broader IV/control-function estimates together with the threshold-local evidence, first-stage diagnostics, and placebo-cutoff checks.

[16] Using the more conventional worker-level clustered standard errors yields the same results.