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Does Weaker Employment Protection Lower the Cost of Job Loss?*

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Abstract

Leveraging a major Italian reform enacted in July 2012 that eroded employment protection to workers on permanent contracts, we use detailed worker level administrative data to estimate how this reduction affected the cost of job loss. We employ a stacked event-study research design, comparing treated and untreated workers as they move from employment into nonemployment. Weakening employment protection led to additional penalties in terms of lower re-hiring earnings and lower re-employment probabilities. Heterogeneous effects of the reform deepened pre-existing divides, penalizing labor market outsiders, such as young workers, and curbing employment opportunities for individuals in poorer regions, such as the South.

JEL Classification: J63, J65, J30, J41, J68

Keywords: Layoffs, Employment protection, Dual labor markets, Difference-in-differences

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1 Introduction

It has long been argued that reducing employment protection increases job flows and lowers the cost of job loss faced by workers (e.g., Lazear, 1990; Bentolila and Dolado, 1994; Bertola and Ichino, 1995; Saint-Paul, 1996; Mortensen and Pissarides, 1999; Cahuc and Zylberberg, 2004; Kugler and Pica, 2008; Boeri, 2011). Recent empirical work supports this view, documenting that the cost of job termination is higher in countries with more stringent employment protection legislation (EPL) (Bertheau et al., 2023). The main contribution of this paper is to use rich administrative data to quantify the impact of a 2012 policy intervention on the cost of job loss in Italy. The intervention, known as the Fornero reform, substantially weakened employment protection for all workers on open-ended (permanent) contracts by reducing the likelihood of workplace reinstatement after dismissal and cutting firing costs for firms with more than 15 employees.

Italy offers a particularly informative setting for this type of evaluation. At the end of 2011, the country was close to default. Under pressure from international lenders, the government introduced new policies to address labor market duality that had beset the Italian economy since the early 1990s (e.g., Boeri and Garibaldi, 2007, 2019) and to improve permanent employment prospects for "outsiders", such as young workers. In June 2012, this led to the Fornero reform, an unexpected shift in direction that weakened protections for traditional "insiders" on permanent contracts, after earlier reforms had already eroded protections for outsiders, especially temporary workers (Daruich et al., 2023).

To assess the reform's impact on the entire re-hiring market, we consider *all* workers in the secondary labor market, including those whose fixed-term contracts have expired, those who quit to unemployment, and those employed in establishments with fewer than 15 employees.[1] This scope is new to the literature and matters for at least two reasons. First, it allows us to evaluate whether the cost of job loss differs by contract type — a key policy dimension, since one aim of the reform was precisely to reduce duality between permanent and non-permanent jobs. Second, it acknowledges that the re-hiring market pools workers from firms of all sizes and across different separation margins, ranging from expirations of fixed-term contracts and quits from smaller firms to individual and mass layoffs.

In environments with strong employment protection, such as pre-reform Italy, the motivation for job termination is often ambiguous and the boundary between individual layoffs and quits tends to blur. As shown by Di Addario et al. (2023), employers may use severance packages to induce workers to move into unemployment or accept outside job offers they would otherwise decline (see also Postel-Vinay and Turon, 2014). In such circumstances, the composition of workers in the re-hiring market need not change much around the reform, even if the recorded reasons for separation shift substantially. Once reinstatement becomes less likely, however, dismissals acquire different informational content, potentially reshaping both who enters nonemployment and how firms screen applicants.

To isolate the impact of the 2012 reform in this setting, we propose a stacked event-study

[1] Small firms are the backbone of the Italian economy. For instance, in 2012, micro-firms (companies with fewer than 10 workers) represented 95% of all enterprises and collectively employed about 50% of all employees (see <https://www.istat.it/comunicato-stampa/struttura-e-competitivita-delle-imprese-anno-2012/>).

design based on a variant of the interaction-weighted (IW) estimator of Sun and Abraham (2021). Our implementation departs from existing applications by targeting the cost of job loss across the entire re-hiring market rather than a single separation margin. Specifically, we split workers into treated and untreated groups, each consisting of cohorts defined by the month in which a job ends. Within each group, we estimate the dynamic cost of job loss using event-study specifications with a difference-in-differences interpretation that allows for heterogeneous treatment effects across cohorts, as in a standard IW approach. Our final estimator then differences the cohort-level IW coefficients between treated and untreated groups, yielding the reform-induced change in post-displacement earnings and employment trajectories across the entire secondary labor market.

We examine 12 untreated and 12 treated cohorts. Treated cohorts consist of individuals whose jobs ended in any month between July 2012 (when the reform came into force) and June 2013; untreated cohorts comprise workers who lost their jobs between July 2010 and June 2011.[2] For each worker, irrespective of cohort, we have a balanced event study with a 13-months pre-event window and a 33-month post-event window, for a total of 47 months inclusive of the event month. The event is the *first* job termination observed over this window, so cohort assignment is exogenous though not necessarily random. The pre-event window allows us to assess how treated and untreated cohorts performed in the year preceding job loss. The post-event window is long enough to follow treated workers up to one year after the enactment of the 2015 Jobs Act, which reinforced many features of the Fornero reform and introduced new incentives to hire on open-ended contracts.

Tracking the most recent untreated cohort for 33 months after job termination up to April 2014 means that no individual in the untreated group was exposed to the Jobs Act. In the initial 12 months post-displacement, we can therefore identify the impact of the Fornero reform on treated cohorts. After this period, when untreated cohorts gradually became exposed to the reform, our estimates capture the differential cumulative effect of immediate treatment compared to treatment at least 12 months after displacement. Beyond 21 months post-termination, when the 2015 reform affects only treated cohorts, we identify the cumulative impact of both policy interventions on treated cohorts relative to the effect of the 2012 reform alone on untreated cohorts.

We study three outcomes: earnings in the month of re-hiring, the probability of re-employment, and the probability of re-employment on an open-ended contract. They are all measured at the individual worker level at monthly frequencies. Although the data do not contain repeated information on wages, earnings at the point of re-hiring are an important indicator of the conditions in the secondary labor market. The two re-employment probability margins enable us to assess whether the reform fostered permanent job creation and reduced duality.

Identification hinges on two assumptions.[3] Because, by design, all individuals in the analysis switch from employment to the focal nonemployment event in a given month, our first assumption is that, absent the reform, treated and untreated cohorts would have exhibited parallel trends at baseline. Since the timing of the reform is exogenous to individual workers, this condition

[2]Comparing workers whose jobs ended in the same calendar months across years allows us to control for seasonality.

[3]For a useful summary of the recent econometric literature on identification in difference-in-differences and related designs, see Roth et al. (2023).

holds as long as the means of time-varying unobservables, which may affect cohort assignment, do not change systematically over time (Ghanem et al., 2026). Below, we will discuss why this is plausible in our setting and how it relates to differences in business cycle effects. A key feature of the design is that the cost of job loss is estimated separately within treated and untreated groups by comparing separated and not-yet-separated cohorts observed at the same calendar time. Time-varying aggregate conditions, including business cycle fluctuations, are thus differenced out by construction. In other words, identification relies not on a simple before-after comparison across regimes, but on within-calendar-time contrasts across cohorts facing the same aggregate environment.[4] We also report robustness checks using a range of alternative estimators, including the synthetic difference-in-differences approach proposed by Arkhangelsky et al. (2021), which accommodates nonrandom job termination and constructs counterfactual outcomes by reweighting both units and time periods in an event-study design like ours.

The second identifying assumption is the absence of anticipation. In our setting, anticipation would arise if workers, in response to the reform, secured new jobs in the months immediately before job loss, in which case the transition would no longer identify the focal event and the individual would not be assigned to the corresponding cohort. The swift ratification of the Fornero reform (first discussed in Parliament in April 2012 and enacted two months later) left workers with little scope for such strategic behavior, while firms had no incentive to advance dismissals before July 2012. Moreover, there is no evidence of changes in the duration of temporary contracts across treated and untreated cohorts: no new policies were introduced, and employers continued to set fixed-term lengths under the same legal standards throughout the sample period.

Our main findings challenge the conventional view. Twelve months after separation, the Fornero reform increased the cost of job loss: re-hiring earnings fell by 15% and re-employment probabilities fell by 7 percentage points. We find no statistically significant impact on the probability of obtaining a permanent job. Nearly three years after job loss, these penalties persist, though attenuated, at about 4% for earnings and 1 percentage point for employment. Exposure of untreated cohorts to the reform and the implementation of the 2015 Jobs Act may have narrowed these gaps, but did not eliminate them.

The lower likelihood of workplace reinstatement affected all workers, whether laid-off or not. However, the reform generated heterogeneous impacts that amplified pre-existing inequalities. Younger workers, industry stayers, and workers employed in the South faced particularly large penalties, as did full-timers. Rather than "levelling up" outsiders' opportunities, the reform appears to have "levelled down" insiders towards outsiders.

To interpret these results, we rely on a stripped-down asymmetric information framework. Unlike a standard search-and-matching model that characterizes the entire labor market (e.g., Mortensen and Pissarides, 1999), this allows us to focus explicitly on the secondary labor market where displaced workers search for re-employment. Emphasizing informational frictions is natural in our context for at least two reasons. First, information is crucial in the case of young workers,

[4]Sizeable business cycle differences existed around the reform, but our design does *not* rely on before-after comparisons. For example, the average baseline unemployment rate was 11.64% for treated cohorts and 8.27% for untreated cohorts; each is compared to its own pre-displacement counterpart, that is, 11.77% and 8.29%, respectively (see <https://fred.stlouisfed.org/series/LRHUTTTTITM156S>). The implied double difference is –0.11% (=–0.13%+0.02%), arguably a negligible differential in this context. More generally, our comparisons are made at common calendar dates so that cohorts face the same macroeconomic conditions.

who have historically over-populated the re-hiring market in Italy with repeated short fixed-term contracts and high churning. Second, in the presence of substantial worker heterogeneity, information is essential to limit skill mismatch. By reducing the scope for workplace reinstatement and simplifying the legal procedure following dismissals, the reform increased the share of low-productivity workers dismissed outright rather than induced to quit. As a result, employers updated their beliefs downward about the ability of workers they encountered in a segment of the secondary market. This shift in expected match quality lowered firms' expected productivity and, in turn, worsened workers' re-hiring prospects on average.[5]

Our paper contributes to several strands of economic research. First, we add to the literature that focuses on how labor market institutions shape the cost of job loss. To the best of our knowledge, only two existing studies address this issue directly, albeit from different perspectives. Janssen (2018) examines how displacement costs vary with the degree of wage-bargaining decentralization, a distinct institutional margin from the one we analyze.[6] Bertheau et al. (2023) use a harmonized design to compare displacement costs across seven European countries, including Italy. They document substantial cross-country heterogeneity in earnings losses, showing that the most pronounced penalties occur where the welfare state is least generous, as in Italy (e.g., Boeri, 2011). These findings motivate our analysis, which is the first to identify the effect on the cost of job loss of a reform that weakened employment protection rights.

Second, our paper speaks to the large literature on job displacement and worker outcomes. A long-standing body of research has documented sizable and persistent employment and earnings penalties following job loss, as well as substantial heterogeneity across workers (e.g., Jacobson et al., 1993; Stephens, 2002; Davis and von Wachter, 2011; Huttunen et al., 2018; Jung and Kuhn, 2019; Burdett et al., 2020; Lachowska et al., 2020; Schmieder et al., 2023; Braxton and Taska, 2023; Athey et al., 2026; Hanushek et al., 2025). Much of this recent work identifies penalties using mass layoffs or firm closures and emphasizes the presence of scarring effects (Acabbi et al., 2024). We contribute to this strand by showing how the cost of displacement responds to a change in EPL and by analyzing the entire re-hiring market rather than only workers affected by collective separation events.

Third, we relate to the extensive literature on EPL (see, among others, Boeri, 2011). Prior work shows that reducing job protection, especially by relaxing constraints on fixed-term contracts, affects labor market efficiency and often leads to a substitution from open-ended to temporary jobs (e.g., Bentolila and Dolado, 1994; Cahuc and Postel-Vinay, 2002; Blanchard and Landier, 2002; Garibaldi and Violante, 2005; Güell and Petrongolo, 2007; Cahuc et al., 2016; García Peréz et al., 2019; Cahuc et al., 2023; Créchet, 2024). Related studies analyze the effect of EPL on job creation and job destruction (see, for instance, Kugler and Pica, 2008). While these margins are connected to the cost of job loss, they operate at a different level, since not all

[5]Other explanations may be plausible. For example, foregone human capital accumulation could generate earnings losses after displacement. Some of our results, however, are hard to reconcile with this interpretation. Older workers and individuals switching industries or occupations, groups arguably more exposed to skill depreciation, experienced smaller penalties than younger workers or non-movers. Sections 5 and 6 discuss these patterns in more detail.

[6]Using Danish manufacturing data, Janssen (2018) shows that income losses after displacement are smaller under centralized national wage bargaining than under decentralized firm-level bargaining, partly because post-displacement wage growth is weaker. Our data do not contain precise information on unionization, and therefore we cannot address this dimension of labor market duality.

separations translate into successful re-employment. Our contribution is to show that weakening employment protection does not necessarily ease re-hiring for dismissed workers and may instead increase the cost of job loss. This perspective distinguishes our analysis from Esposito (2022), who studies the effects of same 2012 reform on firms' job creation and job destruction decisions, and from Boeri and Garibaldi (2019), who examine the Jobs Act, again from the firm's side. A closely related recent contribution is Saez et al. (2023), who document high involuntary separations and wage reductions following the removal of employment protection for older Swedish workers near retirement. We complement this work by analyzing how reduced opportunities for workplace reinstatement shape the cost of job loss for the broader working-age population.

Finally, our paper contributes to the large empirical literature on the Italian labor market. Prior research on reforms that weakened protection for temporary workers finds that temporary contracts crowd out permanent jobs with little or no net employment gains (e.g., Tealdi, 2019; Hoffmann et al., 2022; Daruich et al., 2023), with younger workers bearing a disproportionate share of the adjustment costs.[7] We examine a different institutional margin — i.e., the erosion of protection for workers on open-ended (rather than fixed-term) contracts — yet once again find that young workers bear the heaviest burden. The Fornero reform has been studied elsewhere, but with a focus on other outcomes, including match quality (Berton et al., 2017), firm-provided training (Bratti et al., 2021), and firms' skill demand (Bottasso et al., 2025). As they analyze firms' responses, these papers exploit the 15-employee cut-off entailed by the reform. By contrast, we examine the cost of job loss from the worker's perspective across the entire secondary labor market, including transitions involving smaller firms. Sensitivity checks distinguishing workers from establishments above and below the 15-employee threshold confirm the baseline results and strengthen our economic interpretation while alleviating concerns about confounding business cycle effects.

The remainder of the paper proceeds as follows. Section 2 describes the context within which the Fornero reform took place. Sections 3 and 4 present the data and research design, respectively. Section 5 reports the main findings, Section 6 explores heterogeneity and channels, and Section 7 concludes. Additional material is provided in the Online Appendix.

2 Institutional Background

At the onset of the 2008–2009 financial crisis, Italy combined particularly high employment protection for workers on permanent contracts with strict regulation of temporary employment to limit its overuse. As international financial markets lost confidence in the country's ability to rein in the crisis, the Berlusconi government was replaced in November 2011 by a technocratic cabinet headed by Mario Monti. Among a series of wide-ranging economic interventions, the new government passed the so-called "Fornero" reform, named after the then Minister of Labor, which in June 2012 introduced changes across multiple labor market dimensions, from apprenticeship to social insurance. To promote open-ended contracts and incentivize apprenticeships, the reform raised the social security contributions paid by firms on temporary (fixed-term) contracts. Part

[7] In an earlier context where temporary work was less prevalent, a 1990 reform introducing unjust dismissal costs for firms with fewer than 15 employees reduced hiring wages, especially for workers with low bargaining power such as young workers (Leonardi and Pica, 2013).

of the additional proceeds was earmarked to finance a new unemployment insurance scheme, extending coverage to workers with short employment histories, e.g., young individuals.[8]

The most consequential component of the reform, however, was the overhaul of Article 18 of the *Statuto dei Lavoratori* (Workers' Statute). The stated objective was to curb the pervasive duality of the Italian labor market, which disproportionately penalized outsiders and young workers. Since 1970, the Statute protected workers on open-ended (permanent) contracts in firm with more than 15 employees against unlawful dismissal.[9] The legal notion of unlawful dismissal was broad, encompassing discriminatory and unjustified terminations as well as dismissals due to economic reasons.[10] Protection under Article 18 was substantial, as it implied a full compulsory reinstatement of unfairly dismissed workers, including payment of their forgone earnings and social security contributions. Although relatively few cases reached employment tribunals (around 3,000 per year prior to 2012), the possibility of reinstatement itself acted as a strong deterrent to permanent hiring. Workers could waive reinstatement, in which case employers faced high dismissal costs of up to 36 months of wages, even for short tenures.

The Fornero reform drastically shifted power from workers to firms. First, it simplified dismissal procedures by introducing an out-of-court conciliation service aimed at resolving disputes without litigation through an employment tribunal. Second, if conciliation failed, a settlement would be determined by a judge, in order to reduce uncertainty and legal costs. Third, reinstatement was severely restricted, and workers lost the right to choose between monetary compensation and reinstatement in unfair dismissal cases. Fourth, severance payments in nondiscriminatory dismissals were reduced to a maximum of 24 monthly wages and could be as low as five months of pay. Although some local variation in judicial interpretation remained, the reform represented a major improvement from the employer's perspective: it reduced uncertainty about the resolution of dismissals from permanent jobs, accelerated separations, and lowered firing costs.[11]

To place the Italian institutional environment in an international perspective, Figure 1 reports OECD indicators of employment protection in 2012 and 2013 for Italy and a set of advanced economies. Before the reform, Italy, together with Austria, featured the highest possibility of reinstatement after an unfair dismissal (panel (a)). Following the Fornero reform, this index fell sharply in 2013 to levels comparable to Greece and Norway, though still above those of Germany and the United Kingdom. For countries not shown in the panel, such as France and the United States, the index was zero in both years.

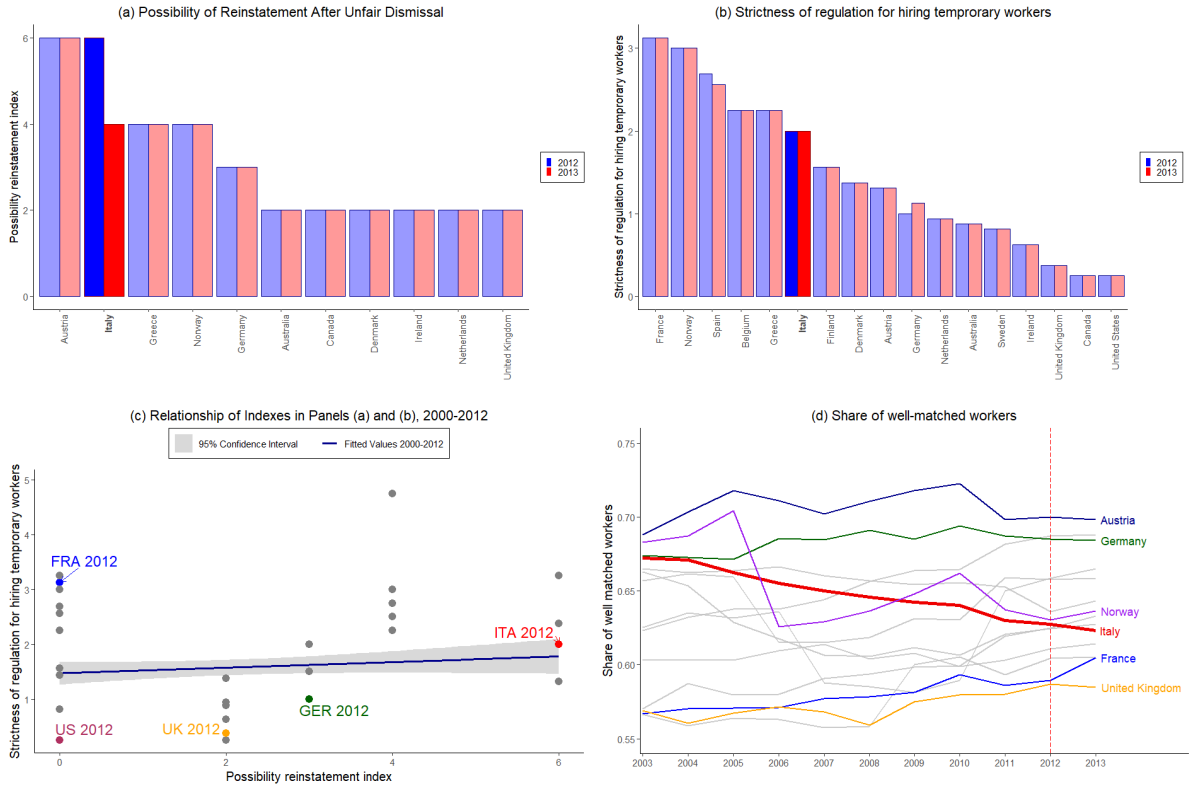
[8] It is worth noting that the unemployment benefits implied by the Fornero reform were *not* particularly generous, with a 75% earnings replacement capped at €1,180, followed by lower benefits for another six months. Temporary workers faced even less generous conditions.

[9] There is evidence that, as a result of elevated firing costs, firms above the 15-employee threshold relied heavily on nonstandard arrangements, including fixed-term contracts, short-time insurance schemes, delayed renewal of expired collective agreements, and so-called "pirate" collective agreements, i.e., sector-level collective agreements signed by unidentified trade unions (e.g., Lucifora and Vigani, 2021).

[10] Firm downsizing and closure were accepted grounds for collective (not individual) dismissal. In such cases, firms were required to give notice of their decisions and consult with trade unions. Procedural irregularities could trigger settlements that included workers' reinstatement whenever possible.

[11] By encouraging apprenticeships, the reform also increased open-ended employment (Maida and Sonedda, 2024). Our estimates, however, are unlikely to be driven by this channel for two reasons. First, apprenticeship eligibility in Italy is limited to individuals below age 30. Second, since apprenticeships are classified as open-ended contracts, this mechanism would predict positive effects for that contract type, which we do not find. Another dimension of the reform reduced employer payroll taxes for women from January 2013; while this may have increased female employment (see Rubolino, 2022), we find no evidence of relatively better post-displacement outcomes for women in our setting.

Figure 1: Cross-Country Comparisons of EPL Indexes



Source: OECD (2020) (OECD World Indicators of Skills for Employment) <https://www.oecd.org/employment/emp/oecdindicatorsofemploymentprotection.htm>

Notes: France, Sweden, Spain, Belgium, Finland and the United States are not included in panel (a) as their index is zero in both years. In panels (b) and (c), all countries are included, i.e., Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, Norway, Spain, Sweden, United Kingdom, and United States. The data used in panel (d) come from the OECD World Indicators of Skills for Employment (WISE). A good match is defined in terms of the adequacy of a worker's education and training skills in relation to the skills required in the job (see https://stats.oecd.org/Index.aspx?DataSetCode=WSDB).

Over the same two years, Italy did not experience any change in the regulation of temporary workers' hiring (panel (b)). Such restrictions are typically viewed as necessary to limit excessive reliance on fixed-term contracts while incentivizing permanent employment (OECD, 2020). Along this dimension, therefore, Italy remained similar to other European economies such as Belgium and Finland, more regulated than the Netherlands, the UK, Canada, and the United States, but more flexible than France, Spain, and Norway.

Like other interventions aimed at reducing labor market duality, the Fornero reform was implicitly premised on a strong positive correlation between the two policy indexes shown in panels (a) and (b). The rationale is simple: stringent protection of permanent employment may dampen job flows on open-ended contracts and induce firms to rely excessively on temporary employment, thereby reinforcing duality. If, instead, employers could dismiss permanent workers more easily and at lower cost, they would have less need to resort to fixed-term contracts, reducing the case for tight regulation of temporary hiring. The graph in panel (c) plots the two indexes over the pre-reform period from 2000 to 2012. The correlation is small and statistically indistinguishable from zero (corr=0.051, s.e.=0.039), a result that is robust across alternative pre-reform windows.

Despite these weak empirical grounds, the reform could be motivated by arguments related to worker-job mismatch. One possibility is that the Italian labor market was characterized by a relatively high share of workers poorly matched to their jobs in terms of qualifications and skills. Panel (d) of Figure 1, however, shows that Italy was broadly in line with other advanced economies (including the UK and France) both before and shortly after the reform, notwithstanding a gradual deterioration in later years. Should firms have reliable information on workers, poor quality matches could be curtailed to a large degree. In practice, however, employers often rely on signals and beliefs about workers' average productivity. Reducing the scope for job reinstatement — one of the policy tools that curbed the power of EPL — might have affected firms' beliefs about average productivity. Moreover, after the reform, employees had stronger incentives to be laid off rather than quit, because voluntary quitters were excluded from unemployment benefits. The combination of easier dismissals and benefit eligibility rules may thus have altered the recorded reason for job terminations, rather than the underlying separation event itself, with potential implications for firms' expectations and hiring strategies. Both features are important for interpreting our empirical results.

Fornero reform notwithstanding, Figure 1 illustrates that the Italian labor market continued to feature relatively strict regulations, particularly regarding reinstatement of dismissed permanent workers. With the adoption of the Jobs Act in March 2015, the government further expanded the Fornero reform in terms of the treatment of permanent workers following dismissals. The new regime applied only to newly signed open-ended contracts, while pre-existing jobs were "grandfathered" under the old system. The Jobs Act confined reinstatement almost exclusively to discriminatory dismissals, substantially reducing judicial discretion. Monetary compensation became the default remedy for economic unfair dismissals. Employment protection was also made tenure-dependent, with severance payments set at four monthly wages for the first two years of tenure and then rising with tenure, up to 24 monthly wages after 12 or more years. The 2015 intervention also bolstered the out-of-court procedure introduced by the Fornero reform by establishing a default settlement formula, i.e., two months of wages for two years of tenure, plus one additional month of pay per year of service, capped at 18 monthly wages after 18 years of tenure. Accepting this out-of-court settlement precluded further litigation and was attractive to both parties, as the payment was exempt from social security contributions and taxation.

As the change in reinstatement rules alone was not deemed sufficient to stimulate permanent employment, the Jobs Act was preceded by a sizeable hiring subsidy introduced by the Budget Law in January 2015. The subsidy applied to new permanent hires of workers without a permanent job in the previous six months and not hired as apprentices. Employers were exempt from social security contributions on new open-ended contracts for three years, up to a cap around mean earnings (e.g., Sestito and Viviano, 2018; Boeri and Garibaldi, 2019). The subsidy was therefore especially generous for low-wage (possibly smaller) firms, but less so for high-wage firms. The Jobs Act maintained this provision.[12]

[12]Using a sample of firms with 10 to 20 employees between 2013 and 2016, Boeri and Garibaldi (2019) show a significant increase in open-ended hires and conversions from fixed-term to permanent contracts, with only modest effects on firings and mainly concentrated among large firms.

3 Data

3.1 Data Sources

Our data come from administrative records of the Italian Ministry of Labor and Social Policies. Since 2009, all public and private employers are legally required to submit a notification for every new hire, job separation, and internal job change. Each notification constitutes a record in the Comunicazioni Obbligatorie system. The Ministry releases a nationally representative randomized sample covering approximately 13% of all such flows. The resulting employer-employee matched dataset, with anonymized worker and firm identifiers, is known as *Campione Integrato delle Comunicazioni Obbligatorie* (or CICO; Integrated Sample of Mandatory Communications).

We use CICO data from January 2009 through June 2017. On the firm side, we observe location (region) and industry (five-digit classification). Firm size is not directly reported, as firms are not required to disclose it. Nevertheless, we can infer whether a company employs more than 15 workers using other legally mandated communications (see subsection 5.3). On the worker side, we observe gender, year and region of birth, region of work, and education. Each worker's career can be reconstructed at the monthly level in panel form from the first time the worker appears in the data. Workers hired after January 2009 can be followed throughout the period, while for workers whose job ends during the sample period we can recover the full job spell even if the job started before 2009.

For each job, CICO report start and end dates, contract type (e.g., open-ended, apprenticeship, fixed-term, and atypical), full-time or part-time status, occupation (four digits), and the collective bargaining agreement regulating the job.[13] The data also include detailed information on the reason for job termination, allowing us to identify layoffs accurately, and earnings at hiring from the Italian Social Security Institute (INPS), defined as total earnings in the first month on the job.

We focus on three outcomes, i.e., re-employment, re-employment in an open-ended contract, and earnings at hiring. Re-employment is measured by an indicator function that takes value one if the individual works at least 15 days a month independently of the type of labor contract. Re-employment in an open-ended contract equals one if an individual works at least 15 days a month in an open-ended (permanent) job. Finally, hiring earnings are defined by contractual earnings (net of overtime) in the first month of re-employment deflated to 2013 Euros and normalized by average earnings in the month prior to job loss. This normalization allows us to express the treatment effect in percent terms.[14] We hold earnings fixed at the level observed in the first month of a new job for all subsequent months of employment, and assign zero earnings to individuals who are not (yet) re-employed, following standard practice (e.g., Athey et al., 2026; Chen and Roth, 2024).

[13] Despite this information, union coverage and union membership cannot be determined precisely, so we do not consider such dimensions of labor market duality. For a recent study on the relationship between collective bargaining agreements and employment protection regulation in Italy, see Dustmann et al. (2025).

[14] Although Italy has no statutory minimum wage, collective bargaining agreements set sector- and occupation-specific earnings floors. We impute missing hiring earnings (which affect only 3% of the observations in the sample) using such floors by year, sector, and occupation. The results are unchanged if these observations are dropped. For the sake of space, these estimates are not shown but are available from the authors.

3.2 Sample Selection

To align the 2012 reform with our research design, our sample consists of employees who experienced a job termination in two 12-month windows, one starting in July 2010 and the other starting in July 2012. Since our interest is in the entire re-hiring market, we consider *all* types of separations, such as individual layoffs, mass layoffs, expirations of fixed-term contracts, and quits to unemployment. We include workers born in 1968 or later who were at least 15 years old in the year preceding displacement, and we exclude agency workers and employees in the home care sector. These restrictions limit the role of early-retirement programs and focus attention on individuals with strong labor market attachment.

Workers are grouped into cohorts defined by the month in which the job ends. Treatment is determined by exposure to the Fornero reform. Treated cohorts consist of workers whose job ended between July 2012 (when the reform was enacted) and June 2013, while untreated cohorts consist of workers who lost their job between July 2010 and June 2011. Using the same calendar months (albeit in different years) mitigates concerns about seasonality, which are particularly relevant for temporary employment, and helps preserve the credibility of parallel trends, by minimizing issues related to compositional changes. The final dataset contains 3,576,794 observations (1,324,037 treated and 2,252,757 untreated), comprising 76,102 workers and 142,909 firms. About 37% of both workers and firms belong to treated cohorts.[15]

For each worker in each cohort, we construct a balanced *event* study with a window of 13 months before job termination and 33 months after, for a total of 47 months including the event month (i.e., the month in which the job ends).[16] The pre-event window allows us to assess seasonality and common pre-trends, while the post-event window enables us to follow treated workers up to one year after the introduction of the 2015 Jobs Act. As a robustness check, we also estimate the model using a shorter post-event window of 21 months that excludes exposure to the Jobs Act (see Section A.2 in the Online Appendix). Although the shorter time window implies a slightly different sample selection — because more workers can be observed for 35 consecutive months rather than 47 — the resulting estimates are very similar to our main results.[17]

We require that each worker be observed for 13 consecutive months prior to job loss, without imposing continuous employment during that period. Left-censoring depends on how workers first appear in the data. If a worker first appears because of a separation, the spell can be used only if the underlying job lasted at least 13 months. If the job lasted less, the worker enters the sample only if a later (new) separation is observed with sufficient pre-event history, i.e., at least 13 consecutive months before the new separation. If workers first appear due to a contractual

[15] The number of firms exceeds the number of workers because individuals typically find re-employment in different firms after separation. The larger number of untreated observations reflects our cohort assignment rule: although untreated workers may later experience another separation during the treatment period, cohort assignment is by construction an absorbing state, so each worker can belong to only one cohort. Reassuringly, we have similar numbers of *overall* job separations in the two periods that define treatment status. Specifically, we observe 2,494,015 terminations between July 2010 and June 2011 (i.e., the period used to identify untreated cohorts), and 2,481,164 job exits between July 2012 and June 2013 (the period referring to treated cohorts).

[16] Online Appendix Figure A1 illustrates how each cohort contributes to the data over time and highlights the degree of time overlap between the untreated and treated cohorts.

[17] In this case, about 2% of workers observed for 35 consecutive months are not included in the baseline 47-month sample. This (arguably small) right-censoring arises from the balanced event-sample selection, as workers' employment histories are interrupted by job spells in private households or temporary agency work, which are not consistently recorded in the administrative data.

change or a new hire, they enter the sample only if they subsequently experience a separation within the relevant window. Although this selection rule is intended to be exogenous, we do not assume that it guarantees exogeneity a priori. Below, we provide balancing evidence showing that treated and untreated cohorts look very similar prior to displacement.

The timing of cohorts is chosen to maximize comparability. The first untreated cohort experienced job loss in July 2010, while the last treated cohort did so in June 2013. The three-year gap between the two *most distant* cohorts and the 12-month gap between the two *closest* cohorts are deliberate: they keep groups close in time while still allowing clean policy exposure. Although, as already mentioned, business cycle conditions differ somewhat between treated and untreated cohorts, business uncertainty was comparable over the same period (Bamieh et al., 2025). Moreover, our identification relies on double differencing, which absorbs common macroeconomic variation. We shall return to these issues in the next section and in subsection 5.3.

The first 12 months post-event offer a clean interpretation of the reform's impact. There are 13 months between the last untreated cohort (June 2011) and the first treated cohort (July 2012). Thus, 12 months after job loss, untreated workers remain unexposed to the reform, and differences between treated and untreated capture the effect of the policy. Beyond 12 months, untreated cohorts also become gradually exposed, and differences reflect the dynamic evolution of treatment effects rather than pure exposure. Furthermore, because untreated cohorts are followed at most through March 2014, none of them is affected by the 2015 Jobs Act. This allows us to use the last 12 post-event months for treated workers to assess whether the Jobs Act amplified or mitigated the effects of the Fornero reform.[18]

A key feature of our balanced event study is that cohort assignment is absorbing: each worker is assigned to a single cohort and never switches across cohorts. Workers are assigned to cohorts based on the *first* observed job separation for which the full 47-month window is available. For instance, if a worker loses a job in August 2012, is re-hired two months later, and separates again in December 2012, we assign the worker to the August 2012 cohort, provided that we observe the individual's labor market history from November 2011 to September 2015. The same logic applies to untreated cohorts. Workers may experience multiple job losses within the 47-month window. While cohort assignment is absorbing, job termination itself is not: once assigned to a cohort based on the first observable separation, workers may separate again. This feature allows us to study how outcomes in the re-hiring market evolve after displacement, rather than treating job loss as a one-time absorbing state. Although job loss may be endogenous, the cohort assignment rule is predetermined and applied symmetrically to treated and untreated cohorts. If a worker holds multiple fixed-term jobs in a given month, we retain the job that ends last; if one of the jobs is open-ended, we prioritize that job.[19]

[18] Two alternative selection criteria are arguably less satisfactory, and therefore not adopted. One is to backdate untreated cohorts well before July 2010–June 2011. Given that CICO records start only in January 2009, this leaves limited scope because of the 13-month pre-event window and raises comparability concerns across distant cohorts, threatening the parallel trends condition. Another alternative is to follow untreated cohorts only for 11 months after displacement, up to June 2012, just before the enactment of the Fornero reform. This would imply a very short post-separation horizon, providing little information on the dynamic effects of the policy.

[19] Multiple separations are well balanced between treated and untreated groups. Among untreated cohorts, 31.8% experience only the focal job loss, 67.8% experience at most two separations, while 88.6%, 97.3%, and 99.4% experience at most three, four, and five job terminations, respectively. The corresponding figures for treated cohorts are 31.7% (one), 71.3% (at most two), 90.1% (at most three), 97.6% (at most four), and 99.5% (at most

This sample design also limits concerns about anticipation. If workers anticipated displacement and secured a new job before the event, the data would record a hire rather than a separation, preventing the event from being classified as a focal job loss. Moreover, any pre-event termination must involve jobs shorter than 12 months, typically temporary contracts or probationary permanent jobs, whose structure did not change with the reform. Selecting a balanced event sample with 13 months before job loss therefore reduces the scope for treated cohorts to have pre-event behavior contaminated by the reform itself.

It is useful to compare our approach with alternative strategies used in the literature to address concerns about selective dismissals. Some studies focus on mass-layoffs (e.g., Bertheau et al., 2023) or firms' closures (e.g., Athey et al., 2026; Bardits et al., 2023) and rely on matching on rich pre-displacement characteristics, including earnings histories, to support parallel trends. These strategies are not well suited to our setting. First, our sample includes temporary workers, for whom long and stable employment histories are uncommon. Second, we observe earnings only at hiring, not full wage trajectories. Because our goal is to examine how the Fornero reform affects the entire re-hiring market rather than a narrowly selected group of displaced workers, we instead rely on cohort construction to achieve comparable composition between treated and untreated groups, after controlling for individual and time fixed effects and for cohort size within each group.[20]

Table 1 reports summary statistics for worker characteristics and outcomes by treatment status, both overall and prior to displacement. Although many differences are statistically significant because of the large sample size, they are economically negligible. Treated and untreated workers display very similar distribution in age, gender, education, geography, sector, monthly separation rates, and net job flows. Pre-termination work histories are also highly comparable, with only minor differences in running tenure and accumulated experience. Likewise, the share of workers employed in firms above and below the 15-employee threshold is well balanced across groups.[21]

The table also documents a change in the recorded reason for job termination following the reform: individual layoffs become more frequent and voluntary quits decline, while temporary contract expirations remain essentially stable. This pattern is consistent with the reform altering incentives over how separations are classified, without substantially changing the underlying incidence of job endings. Overall, observable characteristics and pre-separation trajectories are tightly balanced between treated and untreated cohorts, supporting the view that our estimates are unlikely to be driven by compositional shifts and instead reflect the impact of the reform. In Online Appendix A.3, we reproduce the same statistics separately for quitters, laid-off workers,

five). Pre-event separations are extremely rare: 99.68% and 99.72% of the untreated and treated observations, respectively, exhibit no job separation before the event. This balance supports the credibility of the parallel trends assumption. The two groups are balanced also along other observables. For example, 9.63% of treated cohorts and 9.60% of untreated cohorts are employed in the public sector. Because some workers experience nonemployment spells before holding the job lost at the focal event, we also examine balance in pre-event non-working months. The two distributions are very similar, with 91% of treated workers and 90% of untreated workers having fewer than four non-working months prior to the event.

[20] As a robustness check, we also implement propensity-score matched difference-in-differences which deliver results consistent with our baseline estimates. We return to this analysis in subsection 5.3.

[21] We have also examined balance across location variables (region of birth, region of residence, and region of work). Once again, differences between treated and untreated cohorts in both samples are economically very small. These statistics are not reported due to space constraints (given the inclusion of 20 dummy variables for each location), but are available upon request.

Table 1: Summary Statistics for Treated and Untreated Cohorts

	Full Sample			Pre-dismissal Sample		
	Untreated (S.D.)	Treated (S.D.)	Diff (S.E.)	Untreated (S.D.)	Treated (S.D.)	Diff (S.E.)
Age (years)	31.451	31.659	-0.208^{***}	30.032	30.239	-0.207^{***}
	(4.787)	(3.635)	(0.005)	(4.662)	(3.468)	(0.009)
Male (=1 if yes)	0.575	0.564	0.011^{***}	0.575	0.564	0.011^{***}
	(0.494)	(0.496)	(0.001)	(0.494)	(0.496)	(0.001)
Education:						
Below high school	0.506	0.488	0.018^{***}	0.506	0.488	0.018^{***}
	(0.500)	(0.500)	(0.001)	(0.500)	(0.500)	(0.001)
High school	0.337	0.347	-0.010^{***}	0.337	0.347	-0.010^{***}
	(0.473)	(0.476)	(0.001)	(0.473)	(0.476)	(0.001)
Post-secondary	0.157	0.165	-0.008^{***}	0.157	0.165	-0.008^{***}
	(0.364)	(0.371)	(0.000)	(0.364)	(0.371)	(0.001)
Work experience (top quartile)	0.496	0.508	-0.012^{***}	0.060	0.105	-0.046^{***}
	(0.500)	(0.500)	(0.001)	(0.237)	(0.307)	(0.001)
Separations (top three quartiles)	0.045	0.043	0.002^{***}	0.000	0.000	0.000^{***}
	(0.207)	(0.202)	(0.000)	(0.000)	(0.000)	(0.000)
Net Job Flows (top three quartiles)	0.046	0.042	0.004^{***}	0.024	0.020	0.004^{***}
	(0.209)	(0.201)	(0.000)	(0.154)	(0.141)	(0.000)
Costs (top three quartiles)	0.022	0.026	-0.004^{***}	0.042	0.062	-0.020^{***}
	(0.148)	(0.159)	(0.000)	(0.200)	(0.241)	(0.001)
Job Tenure (days)	569.990	751.643	-181.653^{***}	910.443	1221.782	-311.339^{***}
	(742.430)	(932.567)	(0.896)	(1035.965)	(1171.504)	(2.266)
Lagged Experience (days)	879.010	1001.490	-122.480^{***}	76.879	150.084	-73.205^{***}
	(1095.664)	(1272.907)	(1.275)	(347.097)	(529.124)	(0.882)
Firm size above 15 (=1 if yes)	0.093	0.095	-0.002^{***}	0.082	0.083	-0.001^{***}
	(0.291)	(0.293)	(0.000)	(0.275)	(0.276)	(0.001)
Quit (=1 if yes)	0.300	0.262	0.038^{***}	0.412	0.353	0.059^{***}
	(0.458)	(0.440)	(0.001)	(0.492)	(0.478)	(0.001)
Temporary Job Ended (=1 if yes)	0.182	0.179	0.003^{***}	0.130	0.125	0.005^{***}
	(0.386)	(0.384)	(0.000)	(0.336)	(0.331)	(0.001)
Laid off (=1 if yes)	0.148	0.156	-0.008^{***}	0.188	0.241	-0.054^{***}
	(0.355)	(0.363)	(0.000)	(0.390)	(0.428)	(0.001)
Employed (=1 if yes)	0.703	0.665	0.038^{***}	0.931	0.939	-0.008^{***}
	(0.457)	(0.472)	(0.001)	(0.254)	(0.239)	(0.001)
Employed in open-ended contract (=1 if yes)	0.180	0.205	-0.026^{***}	0.089	0.107	-0.018^{***}
	(0.384)	(0.404)	(0.000)	(0.285)	(0.309)	(0.001)
Self-employed (=1 if yes)	0.006	0.008	-0.002^{***}	0.006	0.008	-0.002^{***}
	(0.077)	(0.088)	(0.000)	(0.080)	(0.090)	(0.000)
Hiring earnings (in 2013 Euros)	778.703	736.324	42.379^{***}	831.853	881.934	-50.081^{***}
	(824.238)	(789.843)	(0.889)	(863.953)	(843.381)	(1.783)

Notes: The table reports mean (and standard deviation) of variables for treated and untreated cohorts, both over the whole period and before dismissal. "Work experience (top quartile)" is an indicator variable equal to 1 if the worker's past-experience is in the top quartile of the distribution, conditional on worker's age. "Separations (top three quartiles)" is an indicator variable equal to 1 if the job is linked to a number of job separations in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Net flows (top three quartiles)" is an indicator variable equal to 1 if the job is associated with a number of net flows (hirings minus separations) in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Costs (top three quartiles)" is an indicator variable equal to 1 if the job benefited from tax rebates above the bottom quartile of the distribution, conditional on age.

and those whose temporary contracts expired. In the empirical analysis and in line with the Sun and Abraham (2021)'s approach, we do not condition on covariates.[22]

4 Research Design

The estimation strategy proceeds in four steps. In the first three, we adapt the interaction-weighted (IW) estimator proposed by Sun and Abraham (2021) to compute the dynamic effect of job loss separately for treated and untreated cohorts. In the final step, we calculate the difference between the two groups to obtain a difference-in-interaction-weighted (D-IW) estimator, which identifies the impact of the Fornero reform on the cost of job loss.

A key departure from the standard Sun-Abraham setup is that we use a balanced event panel

[22] Although adding covariates can complicate the interpretation of the estimates as cohort-specific average treatment effects on the treated, we repeated the baseline analysis including time-varying controls and found results similar to those discussed in Section 5. Including covariates when using the Callaway and Sant'Anna (2021)'s estimator — where covariate inclusion is natural — also leave our main estimates unchanged. These results are available upon request.

rather than a balanced panel approach. This choice ensures that cohort composition does not vary across relative periods and that cohort weights are time-invariant. We exploit the staggered timing of job separations (e.g., July 2010, August 2010, and so on) and compare each cohort that separates in a given month to all the cohorts that have not yet separated at the same calendar time. Under the assumption of parallel trends in untreated potential outcomes across cohorts at the same relative time (Sun and Abraham, 2021), calendar-time shocks are differenced out within each treatment group. For example, consider the untreated cohorts (from July 2010 to June 2011): when the earliest cohort moves into nonemployment (July 2010), their change in outcomes is compared to the change in outcomes for the not-yet-separated cohorts (August 2010–June 2011) at that given calendar time (i.e., July 2010).[23]

Step 1 — We estimate cohort-specific average treatment effect on the treated (CATT) for job loss using a linear two-way fixed effects model with cohort-by-relative time interactions. Equation (1) is estimated separately for treated and untreated cohorts:

$$y_{ijt} = \alpha_i + \lambda_t + \sum_m \sum_{\ell \neq -1} \delta_m^\ell (\mathbf{1}\{M_{ij} = m\} D_{ijt}^\ell) + \epsilon_{ijt}, \tag{1}$$

where i indexes workers and j treatment group (with $j = \mathcal{U}$ referring to untreated cohorts, and $j = \mathcal{T}$ to treated cohorts) at time (month) t; $\mathbf{1}\{z\}$ is an indicator function that the event z is realized; M_{ij} is the month of job termination that assigns worker i to cohort m in group j; and D_{ijt}^ℓ indicates being ℓ months away from job loss at calendar time t. The terms α_i and λ_t are worker and time fixed effects, respectively; y denotes our labor market outcomes; and ϵ_{ijt} is the error term. Each δ_m^ℓ is the CATT coefficient which has a DiD interpretation relative to the month before termination ($\ell = -1$ being normalized to 0), exploiting its staggered time structure (Goodman-Bacon, 2021).

Step 2 — We calculate sample weights given by the sample analogue of $Pr\{M_{ij} = m \mid M_{ij} \in \{\mathcal{T}, \mathcal{U}\}\}$, i.e., the share of each cohort within the treated or untreated group. Weights are non-negative, constant across event times, and sum up to one within each group $\mathcal{T}$ or $\mathcal{U}$.

Step 3 — We construct the interaction-weighted (IW) estimator for each group j as a weighted average of the CATT estimates from regression (1), using the cohort weights computed in the second step:

$$\widehat{\nu}_{\ell,j} = \sum_m \widehat{\delta_m^\ell} \widehat{Pr}\{M_{ij} = m \mid M_{ij} \in \{\mathcal{T}, \mathcal{U}\}\}, \tag{2}$$

Following Sun and Abraham (2021), we estimate pointwise confidence intervals for the IW estimates, and do not rely on bootstrapping at this stage.[24]

[23] Online Appendix B.1 formally illustrates the comparisons underlying identification when the last untreated (or treated) cohort moves into nonemployment and no not-yet-separated cohorts remain. For the first and the last untreated and treated cohorts, the Appendix also explains the comparisons used to estimate the effects one month, 12 months, and 24 months after separation. Online Appendix Table B.1 generalises these examples to all twelve cohorts and the three post-separation horizons.

[24] In the empirical analysis, for each group j, we show 11 pre-event estimates $\widehat{\nu}_{-12}, ..., \widehat{\nu}_{-2}$, normalizing $\nu_{-1} = 0$, and 34 estimates from the month of separation to the end of the observation window, i.e., $\widehat{\nu}_0, \widehat{\nu}_1, ..., \widehat{\nu}_{33}$. Normal-

Step 4 — To identify the impact of the Fornero reform, in each period ℓ, we compute the difference between the two IW coefficients:

$$\Delta\widehat{\nu}_\ell = \widehat{\nu_{\ell,\mathcal{T}}} - \widehat{\nu_{\ell,\mathcal{U}}} \tag{3}$$

We refer to expression (3) as the difference-in-interaction-weighted (D-IW) estimator. It measures how the cost of job loss for workers displaced after the reform differs from that faced by workers before the reform, and can be interpreted as a stacked DiD estimator that is robust to heterogeneous treatment effects. Inference on $\Delta\widehat{\nu}_\ell$ is based on clustered bootstrapped standard errors.

Some clarifications are in order. Given our cohort design, the D-IW estimator has a clean causal interpretation over the first 12 months after job loss: untreated cohorts are not yet exposed to the Fornero reform, so $\Delta\widehat{\nu}_\ell$ captures the effect of the policy. Beyond 12 months, untreated cohorts begin to be exposed in a staggered way (from $\ell = 13$ to $\ell = 24$ depending on cohort), and $\Delta\widehat{\nu}_\ell$ then reflects the evolution of treatment effects rather than pure exposure. After 21 months, when the 2015 Jobs Act was enacted, the D-IW estimator captures the combined effect of both reforms for treated cohorts relative to exposure to the 2012 reform alone for untreated cohorts.[25]

Identification does not impose parallel trends between treated and untreated cohorts in all pre-event periods, but only that cohorts are comparable at the time of displacement (i.e., at baseline). This requires that the composition of the re-hiring market at the time of separation evolves smoothly across cohorts, or equivalently that the mean of time-varying unobservables is stable over time (Ghanem et al., 2026). In our setting, this condition is plausible for four reasons. First, we include all types of job termination, thereby capturing the entire re-hiring market rather than a single margin (such as temporary contract expirations or mass layoffs). Second, the balanced event study design ensures that each cohort in $\mathcal{T}$ and $\mathcal{U}$ contributes to every relative period, preventing changes in composition across event times; indeed, cohort weights are time-invariant. Third, all workers are employed in the month prior to separation, which provides a common baseline from which post-displacement dynamics are measured. Fourth, because identification relies on within-period DiD comparisons, the D-IW estimator differences out aggregate, common time-varying macroeconomic conditions.

5 Main Results

5.1 Baseline Estimates

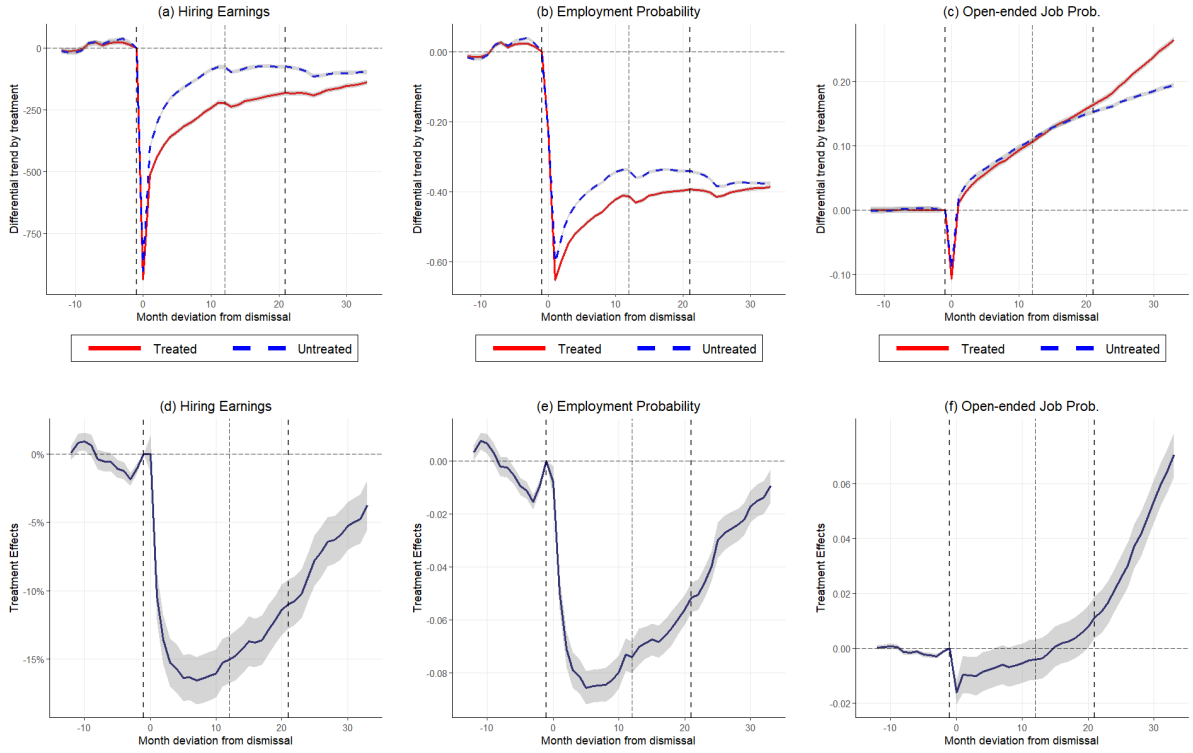
Figure 2 displays our baseline results using the interaction-weighted (IW) and difference-in-interaction-weighted (D-IW) estimators introduced in Section 4. The top three panels show the IW event study profiles separately for treated and untreated cohorts, $\widehat{\nu}_{\ell,\mathcal{T}}$ and $\widehat{\nu}_{\ell,\mathcal{U}}$, for our three outcomes, i.e., hiring earnings (panel (a)), the probability of re-employment (panel (b)), and the probability of re-employment on an open-ended contract (panel (c)), along with the 95%

ization relative to the period before the event is standard practice, whereas excluding the earliest pre-event period helps avoid multicollinearity while retaining a constant term in regression equation (1).

[25] For a discussion of the interpretation of estimates with multiple treatments, see de Chaisemartin and D'Haultfœuille (2023, 2026).

confidence bands. The bottom three panels (d)–(f) report the corresponding D-IW estimates, $\Delta\widehat{\nu}_\ell$, which isolate the causal impact of the Fornero reform by differencing treated and untreated cohorts at each event time, with their corresponding 95% confidence intervals. The three vertical lines in each plot summarize the event institutional timeline. The first (dashed) line on the left marks the month before job loss. The second (lightly dashed) line is 12 months post displacement, after which untreated cohorts start to be exposed to the reform in a staggered fashion. The third (dashed) line on the right is 21 months after job loss, when treated cohorts are additionally gradually affected by the 2015 Jobs Act.

Figure 2: Benchmark Effects on Labor Market Outcomes



Notes: The gray area around the point estimates refers to the 95% confidence interval obtained from clustered bootstrapped standard errors (100 replications) in panels (d)–(f). For monthly hiring earnings in panel (a), the impacts are expressed in real Euros (base=2013), while in panel (d) they are expressed in percent terms. In the other four panels, the estimates are expressed in percentage point terms. In panels (a)–(c), the IW estimates for treated cohorts are in red, while those for untreated cohorts are in blue.

Pre-trends — Across outcomes, the D-IW estimates are close to zero in the months preceding job loss, to the left of the first dashed line. Small deviations appear for hiring earnings and employment (see panels (d) and (e)), driven almost entirely by differences in very short-term contract durations among untreated workers. When we restrict the sample to individuals with positive earnings, these discrepancies disappear (see Online Appendix C.1).

We do not interpret this pattern mechanically as evidence of parallel trends at baseline, but rather as consistent with the identification strategy discussed in the previous section. First, by construction, all treated and untreated workers are employed at baseline, $\ell = -1$.[26] As a

[26] We cannot impose this condition further back in time because the estimating sample comprises all workers in the re-hiring market, and not only those with stable labor market attachment.

consequence, periods *prior* to $\ell = -1$ do not contribute to the identification of counterfactual trends (see Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Roth et al., 2023). Second, the main source of the small pre-event deviations from zero in Figure 2, namely differences in temporary contract length, is determined by firms rather than workers. This limits the scope for workers' anticipatory behavior around the reform. Taken together, these features support the plausibility of baseline comparability between treated and untreated cohorts, which is the key assumption underlying our D-IW estimator.[27]

Earnings effects — Panel (a) of Figure 2 plots the earnings cost of job loss in levels. To gauge magnitudes and facilitate comparison with other studies, we normalize post-separation earnings relative to average earnings in $\ell = -1$, approximately €900 for untreated workers. For untreated cohorts, hiring earnings dropped sharply by about 45% in the month after job loss. Earnings recovered quickly but never returned to pre-dismissal levels. From the twelfth month onward, untreated workers faced a persistent earnings loss of about €80 per month, roughly 9% of baseline earnings.[28]

The key policy effect is shown in panel (d). One month after job loss, treated workers earned about 8% less than otherwise comparable untreated workers. The gap widened over time, peaking at roughly 17% eight months after displacement. Relative to the 9% loss faced by untreated workers, the weakening of employment protection under the Fornero reform increased the hiring-earnings penalty by approximately one-quarter. After month 9, the gap gradually narrowed but never closed over the 33-month horizon: by the end of the sample period, treated workers still earned 3% less. Part of this partial convergence reflects the staggered exposure of *untreated* cohorts to the Fornero reform after 12 months and, later, the introduction of the 2015 Jobs Act for treated cohorts. Importantly, when conditioning on re-employment, the earnings penalty remained remarkably stable, with treated workers earning again 3% less than their untreated counterparts. This shows that the reform worsened re-hiring conditions not only by lowering employment probabilities, but also by reducing the quality of jobs obtained by those who returned to work. These results are reported in Online Appendix C.1.

Employment effects — Panels (b) and (e) trace re-employment dynamics. Immediately after job loss, re-employment probabilities collapsed for both groups, but treated cohorts fared systematically worse. One month after displacement, treated workers were five percentage points less likely to be employed than untreated workers. The gap widened over time, reaching approximately eight points between five and ten months after separation (panel (e)). After one year, $\mathcal{T}$-workers had about a 40% reduction in employment relative to baseline, compared with just

[27] Workers may still have private information about impending separations and adjust behavior in advance. As noted by Callaway and Sant'Anna (2021), however, violations of no-anticipation are likely to be limited when the reform path is not known ex ante or when workers do not choose the timing of separation. Moreover, workers who anticipate job loss and immediately find another job before the event are excluded from the cohort by design. Even if some anticipatory behavior remains, as long as it induces similar bias before and after the 2012 reform, this bias is eliminated by differencing the estimated coefficients.

[28] As a benchmarking exercise, one year after termination, $\widehat{\nu}_{\mathcal{U}}$ is one-fifth the size of that reported by Bertheau et al. (2023) for Italy (which is estimated to be around 40%), and just over one-third of that found by Athey et al. (2026) for Sweden (24%). Our results, however, cannot be directly compared with such studies as these focus on only one specific reason for job loss (i.e., mass layoffs in the former and firm closures in the latter). For instance, we find a lower impact than in Bertheau et al. (2023), in part because those who terminated a fixed-term contract had already lower earnings on average than those who were mass laid off from a permanent job. Our estimates are instead comparable to those reported in Krolikowski (2018) and Schmieder et al. (2023).

over 32% for $\mathcal{U}$-workers (panel (b), in line with Bertheau et al., 2023).

As untreated cohorts became exposed to the Fornero reform, the gap narrowed somewhat, but $\mathcal{T}$-workers continued to face a sizable disadvantage: even after 21 months, they remained about five percentage points less likely to be employed. The introduction of the 2015 Jobs Act further compressed the penalty, which fell to one percentage point in the final month of the sample, even though it remained statistically significant. Throughout, the underlying IW profiles were large and stable at about 40%, indicating persistent scarring from job loss for both treated and untreated workers.

Permanent employment — A central goal of the Fornero reform was to promote open-ended contracts in the re-hiring market. Panels (c) and (f) of Figure 2 provide little support for this aim. During the first eight to nine months after job loss, treated workers were approximately one percentage point less likely to return to work on a permanent contract than untreated workers.[29] Over the subsequent year, when $\mathcal{U}$-cohorts were also exposed to the reform, treated workers did not perform better; instead, the two groups simply converged to similar probabilities.

Only around 20 months following separation, shortly after the introduction of the hiring incentives and just before the implementation of the 2015 Jobs Act, did permanent re-employment become significantly more likely for treated cohorts. The effect increased by about one percentage point around the 2015 reform to approximately seven points by the end of the observation window. This pattern suggests that the later hiring subsidies and possibly other institutional changes, rather than the Fornero reform itself, led to the eventual increase in permanent re-employment, consistent with existing evidence (Sestito and Viviano, 2018; Boeri and Garibaldi, 2019).

Discussion — We now provide a simple economic interpretation of our findings. The central result is that reducing reinstatement rights under the Fornero reform lowered both re-hiring earnings and re-employment probabilities for treated cohorts in the post-displacement labor market. These penalties add to an already substantial pre-reform cost of job loss, when open-ended workers enjoyed stronger protections. Weakening protection, therefore, neither mitigated post-displacement earnings losses nor generated greater permanent job creation in the re-hiring market. In a standard search-and-matching framework, relaxing employment protection is predicted to stimulate job creation and raise hiring probabilities (e.g., Cahuc et al., 2016; Bottasso et al., 2025). This prediction, however, typically applies to hiring from nonemployment at the time of the reform. Our setting is different. We focus on workers who have already separated and are competing for jobs after displacement. For this group, informational frictions, rather than vacancy creation, are likely to be the key mechanism driving the observed penalties.

To rationalize our results, we draw on Gibbons (1992)'s hybrid equilibrium model, which emphasizes employer learning under imperfect information (see also Kahn and Lange, 2014). The core idea is that the reform did not alter the overall composition of workers in the re-hiring market, but instead affected how low-skill workers sort across equilibria. Intuitively, by changing the informational content of separations, the reform diluted the signal conveyed by effort, lowering firms' beliefs about applicant quality and compressing equilibrium wages in the

[29] Even before the 2012 reform, a non-trivial share of workers in the re-hiring market moved from fixed-term to permanent contracts, reflecting the use of fixed-term employment as a screening device for open-ended positions. An analysis of the mechanisms behind these transitions is interesting but outside the scope of this study.

pooling segment. By changing the type of job termination, the reform increased the share of low-skill workers who mimic high-effort behavior and enter the pooling equilibrium rather than the low (separating) equilibrium. This weakened the informational content of effort within the pooling segment, reduced firms' beliefs about worker quality, and lowered equilibrium wages even in the absence of large aggregate compositional changes. Additional details are available in Online Appendix C.2; here we outline the main mechanisms.

Consider the hiring environment before the Fornero reform. A firm chooses a wage-effort package $\{w, e\}$. Effort e is costly for workers, and productivity y increases in effort. Workers are heterogeneous in ability: type H (high ability or high skill) and type L (low ability), where L-types have steeper indifference curves, reflecting a higher marginal disutility of effort. Let ϕ denote the share of H-types in the post-displacement labor market and $(1-\phi)$ the share of L-types. The firm does not directly observe ability. Instead, it infers worker type from imperfect signals, such as past employment histories and reasons for separation.

When a worker is inferred to be low ability, the firm offers a contract, $\{w^L, e^L\}$, with low effort and low pay. Let ψ be the fraction of the *remaining* L-types who mimic high-ability behavior by supplying a higher effort level $e_p^b > e^L$, which is the same effort chosen by all H-types. These workers enter a pooling segment of the market in which firms cannot distinguish true H-types from mimicking L-types. Unable to separate the two groups, the firm offers them the following pooling hybrid equilibrium wage:

$$w_p^b = \pi\big(H|\{w_p^b, e_p^b\}\big)y(H, e_p^b) + \Big[1 - \pi\big(H|\{w_p^b, e_p^b\}\big)\Big]y(L, e_p^b), \tag{4}$$

where $y(\cdot, e)$ is the pre-reform productivity profile that increases with effort e, and π is the firm's belief to recruit a high-ability worker in the pooling equilibrium. Using Bayes's rule, one can show that

$$\pi\big(H|\{w_p^b, e_p^b\}\big) = \frac{\phi}{\phi + (1-\phi)\psi}, \tag{5}$$

that is, the firm's beliefs about meeting a high-ability worker in the pooling equilibrium depend on the population share ϕ and on the mass of mimickers ψ. In equilibrium, a larger ψ lowers firms' posterior beliefs about applicant quality and therefore reduces the pooling wage.

To be an equilibrium wage, the right-hand side of equation (4) must equal $qy(H, e_p^b) + (1-q)y(L, e_p^b)$, where q (or alternatively, $1-q$) is the true probability of observing a high- (low)-skilled worker in the pooling equilibrium. Since $\pi = q$ in equilibrium, rearranging expression (5) yields

$$\psi = \frac{\phi(1-q)}{(1-\phi)q}. \tag{6}$$

Now, suppose the Fornero reform, R, is introduced. The reform could affect both ϕ (through a compositional change in H- and L-type workers in the overall pool of displaced workers) and q (through a change in the actual share of high-skill workers in the pooling equilibrium). The

impact of R on the mass of mimicking low-ability workers, ψ, is ambiguous, as shown by

$$\frac{\partial \psi}{\partial R} = \underset{(+)}{\frac{\partial \psi}{\partial \phi}} \underset{(-)}{\frac{d\phi}{dR}} + \underset{(-)}{\frac{\partial \psi}{\partial q}} \underset{(-)}{\frac{dq}{dR}}. \tag{7}$$

An increase in ψ can arise either because the overall share of high-ability workers ϕ falls, or because firms revise downward the probability $\pi = q$ of meeting a high-ability worker in the pooling equilibrium. From the comparative statics (6), a decline in q increases ψ, expanding the mass of low-ability workers who pool at high effort rather than accept the low separating contract. As the pooling segment becomes more diluted, firms reduce the pooling wage w_p^b.

This informational friction mechanism requires neither an expansion of the re-hiring market nor large compositional shifts in observables (i.e., a small $d\phi/dR$ in expression (7)). It operates entirely through belief updating. This is exactly what the data show: a reclassification of the reasons for separation, with quits declining and individual layoffs increasing, but essentially no change in observable worker characteristics. Hence, the Fornero reform mainly affects the pooling segment of the market, where most displaced workers search. In the separating segment, where workers are clearly identified as low ability, the contract (w^L, e^L) is largely unaffected. The wage penalty therefore arises in the pooling equilibrium, generating a downward shift in post-displacement earnings and employment probabilities, consistent with the sign and persistence of the D-IW treatment effects.[30]

Finally, consider an alternative mechanism based on relative labor costs. If the reform mainly raised the cost of temporary relative to permanent jobs, we would expect an immediate increase in permanent re-employment probabilities and stronger negative effects concentrated among non-laid-off workers, especially those exiting fixed-term contracts. Under our informational friction interpretation, by contrast, the penalty applies broadly to all workers in the pooling equilibrium, regardless of separation type. The next subsection evaluates such contrasting predictions.

5.2 Distinguishing Laid-Off from Other Workers

We now repeat the analysis distinguishing between jobs that ended with an individual dismissal and jobs that did not. Figure 3 displays the D-IW estimates, $\Delta\widehat{\nu}_\ell$, for hiring earnings, re-employment, and permanent re-employment by layoff status.[31] The underlying IW profiles are reported in Online Appendix C.3. As in the baseline analysis, the small pre-displacement deviations from zero are driven by short-term contracts among untreated cohorts and are absent when focusing on workers dismissed from permanent jobs.

Turning to post-separation outcomes, between three and 12 months after job loss, $\mathcal{T}$-workers who were laid off faced an earnings penalty of 13–17% relative to untreated laid-off workers (see

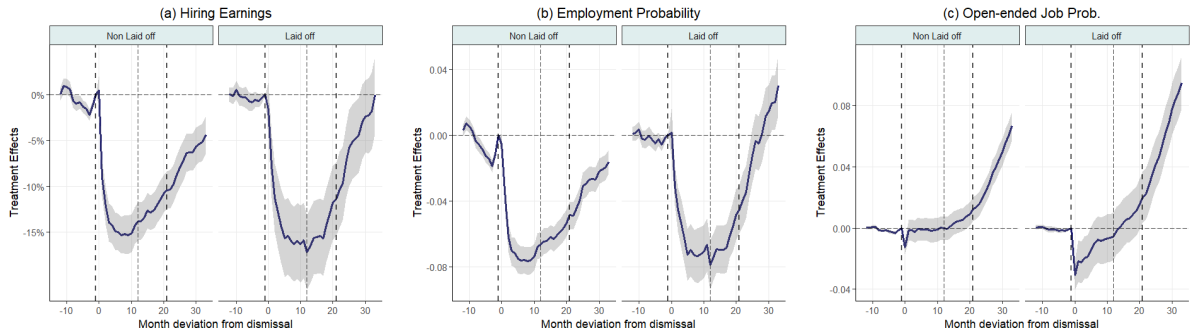
[30] This interpretation is consistent with the evidence in Bottasso et al. (2025). They show that the Fornero reform did not affect hiring or separation rates for H-type workers in firms with more than 15 employees relative to smaller firms. They also find that the reform did not induce firms above the 15-employee threshold to increase the recruitment of L-type workers on open-ended contracts, while separations among these workers increased, predominantly through layoffs rather than voluntary quits. This pattern supports the view that, within the pooling equilibrium, firms anticipated a larger pool of low-quality applicants following the reform and adjusted wages accordingly. Further supporting evidence by firm size is provided in Subsection 5.3.

[31] To avoid contamination between groups, each effect is estimated separately by separation type (de Chaisemartin and D'Haultfœuille, 2023).

the graph on the right-hand side of panel (a)). On the left-hand side, treated workers who were not dismissed — those who quit, completed a fixed-term contract, or experienced a mass layoff — incurred a similar earnings penalty of approximately 14% over the same period. The difference between the two groups, on the order of 3% or less, is generally not statistically distinguishable from zero. Thus, the Fornero reform generated a broad earnings penalty across the entire re-hiring market, and not just one concentrated on a specific segment.[32]

Only after the 2015 Jobs Act did laid-off treated workers begin to recover. By the end of the sample period, their hiring earnings converged back to those of their pre-2012 counterparts. In contrast, the earnings of non-laid-off workers never recovered and remained 4% significantly lower after 33 months. A natural interpretation is that individually laid-off $\mathcal{T}$-workers disproportionately came from permanent, higher-quality matches prior to separation, whereas many non-dismissed workers exited temporary jobs or voluntarily quit.[33] The open-ended nature of the pre-separation contract may therefore have served as a more favorable signal in the secondary market.

Figure 3: Treatment Effects on Labor Market Outcomes, by Type of Job Separation



Notes: Each panel reports D-IW estimates. The gray area around the point estimates refers to the 95% confidence interval obtained from clustered bootstrapped standard errors (100 replications). For other details, see the notes to Figure 2.

Panel (b) shows a similar pattern for employment probabilities. For the first two years after job termination, differences between dismissed and non-dismissed workers were small and statistically insignificant. However, once treated cohorts became exposed to the 2015 Jobs Act, laid-off workers began to outperform others. In the last three months of the observation window, they enjoyed a re-employment premium of about two to three percentage points, while non-dismissed workers faced a penalty of roughly two points. The resulting gap of three to five percentage points is statistically different from zero at conventional levels over the final year of the sample.

Finally, panel (c) of Figure 3 documents that the Fornero reform did not improve immediate access to permanent contracts for laid-off workers. During the first six months after separation, treated dismissed workers were two to three percentage points less likely to obtain an open-ended job than untreated dismissed workers (see the graph to the right of the panel). From month 6

[32] Conditioning on re-employment confirms this interpretation (see panel (d) in Appendix Figure C1). Among employed workers, laid-off individuals still suffered a persistent earnings loss of about 5%, implying that most of the aggregate earnings effect operates through the re-employment margin.

[33] Only 2% of terminations, recorded as collective dismissals, were due to mass layoffs for *both* treated and untreated cohorts.

to month 19, their probability of permanent re-employment is statistically indistinguishable from that of their untreated dismissed counterparts and from that of other (non-dismissed) workers, shown in the graph to the left. But from month 20 onward, permanent re-employment probabilities rose sharply for both groups of treated workers, reaching increases of up to nine percentage points for laid-off workers and almost seven points for non-dismissed workers by the end of the sample. These late gains are consistent with the introduction of hiring incentives and the Jobs Act rather than with the Fornero reform itself.

To summarise, laid-off and non-laid-off workers exhibit remarkably similar trajectories for almost two years after job loss. Differences emerge only later, when additional institutional changes favored permanent hiring. At that point, dismissed workers — who were more likely to come from permanent and possibly higher-quality jobs — benefitted disproportionately. This suggests that the informational content of dismissals changed after the 2012 reform: separation from an open-ended job becomes a relatively stronger signal of worker quality in the secondary market, potentially widening the "insider–outsider" gap (e.g., Bentolila and Dolado, 1994; Bertola and Ichino, 1995; Daruich et al., 2023).

This evidence also speaks to the alternative cost-based mechanism mentioned at the end of subsection 5.1. If the reform mainly increased the cost of temporary employment, we would expect larger immediate penalties among non-dismissed workers and faster gains in permanent hiring. Instead, Figure 3 shows broadly similar short-run penalties across separation types. Consistent with this, Figure C17 in the Online Appendix shows that excluding workers from temporary jobs yields very similar estimates. The results are therefore hard to reconcile with a pure relative-cost channel and instead support our informational-frictions interpretation.

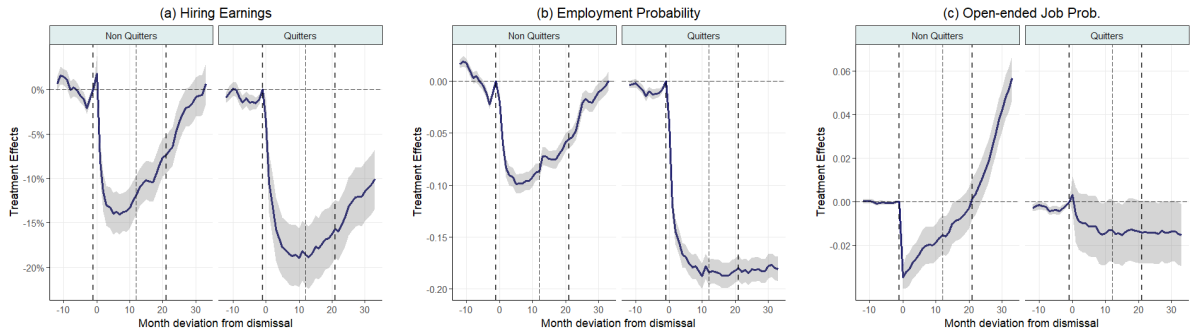
So far, we have examined all job terminations, including quits to unemployment, which provides a comprehensive picture of the re-hiring market. Under strict employment protection, firms can "bribe" workers to quit in order to avoid high firing costs, making quits observationally close to layoffs (on this issue, see also Postel-Vinay and Turon, 2014; Di Addario et al., 2023). However, after the reform, firing became less onerous, and even this type of quits may contain a genuine voluntary (worker initiated) component. We thus perform additional analyses distinguishing quitters from all the other workers.

Figure 4 reports the corresponding D-IW estimates. Restricting attention to non-quitters delivers results very similar to those reported in Figure 2.[34] More importantly, quitters experience systematically worse outcomes after separation: larger earnings losses, lower re-employment probabilities, and — after approximately 21 months — smaller chances of obtaining an open-ended contract. If the reform increased adverse selection in the secondary market, the resulting deterioration in re-hiring conditions affected everyone, but it was particularly binding for workers whose reservation wages were high, such as voluntary quitters. Their slower reintegration is therefore consistent with worsened market conditions rather than with a mechanical shift in contract costs.[35]

[34] Burdett et al. (2020) report comparable robustness when restricting attention to mass layoffs. See their footnote 18 (p. 1778) and their Appendix B.3. A similar finding is also reported by Jarosch (2023). These results provide useful benchmarking for our estimates.

[35] See also the additional estimates in Online Appendix C.5.

Figure 4: Treatment Effects on Labor Market Outcomes, Distinguishing Quitters from Non-Quitters



Notes: Each panel reports D-IW estimates. The gray area around each point estimate refers to the 95% confidence interval obtained from clustered bootstrapped standard errors (100 replications). For other details, see the notes to Figure 2.

Summary — Job loss is associated with long-lasting economic costs, in terms of lower re-employment probabilities, reduced access to open-ended contract, and sizable hiring-earnings penalties in the secondary market. The erosion of employment protection implied by the Fornero reform amplified such costs. Importantly, the penalties were not concentrated on dismissed workers alone. Non-dismissed workers (mainly quitters to nonemployment) suffered particularly large and persistent losses. Relative to them, laid-off treated workers experienced smaller earnings penalties and a more modest re-employment disadvantage.

Guided by the hybrid equilibrium model sketched above, we interpret these patterns as evidence that the reform changed the informational content of dismissals. Firms revised downward their beliefs about the average quality of workers in the pooling equilibrium, compressing wage offers and reducing job finding prospects. Because many laid-off treated workers held permanent jobs before being dismissed, this feature might have served as a relatively favorable signal in the post-reform re-hiring market. Although foregone human capital accumulation remains, in general, a key determinant of earnings losses after displacement (e.g., Topel, 1990; Burdett et al., 2020), our results highlight the additional role that informational frictions played in this case. After the reform, worsened inference about worker quality reduced wages and employment probabilities, with especially severe consequences for workers whose separations did not convey strong positive signals to employers.

5.3 Robustness

It is important to corroborate our main estimates with a set of sensitivity checks. We perform five complementary exercises. First, we assess the credibility of the parallel trends assumption using the synthetic difference-in-differences (SDiD) estimator of Arkhangelsky et al. (2021) in a stacked design. Second, we re-estimate the effects in a balanced panel framework that is robust to treatment effect heterogeneity. Third, we construct cohort-level estimates derived from the structure of the D-IW design to graphically assess whether job loss costs primarily reflect the reform rather than business cycle conditions. Fourth, we bound the role of time (business cycle)

effects using a conservative robust-DiD approach. Fifth, we examine heterogeneity by firm size, distinguishing workers in establishments with fewer than 15 employees from those in larger firms.

SDiD benchmark — The first check evaluates whether non-random job termination across workers or periods could violate the parallel trends condition underlying our D-IW approach. We use SDiD in a stacked design. This estimator combines features of synthetic control and difference-in-differences by reweighting both units (workers) and time periods (events), so that treated and untreated cohorts have comparable pre-treatment trends in expectation (see Figure 1 in Arkhangelsky et al. (2021)). Unlike traditional synthetic control methods, SDiD does not attempt to perfectly fit pre-treatment outcomes, which limits the risk of overfitting and preserves sampling variability. The resulting weights balance pre-event averages and trends and ensure that post-event comparisons are made across similar economic conditions.

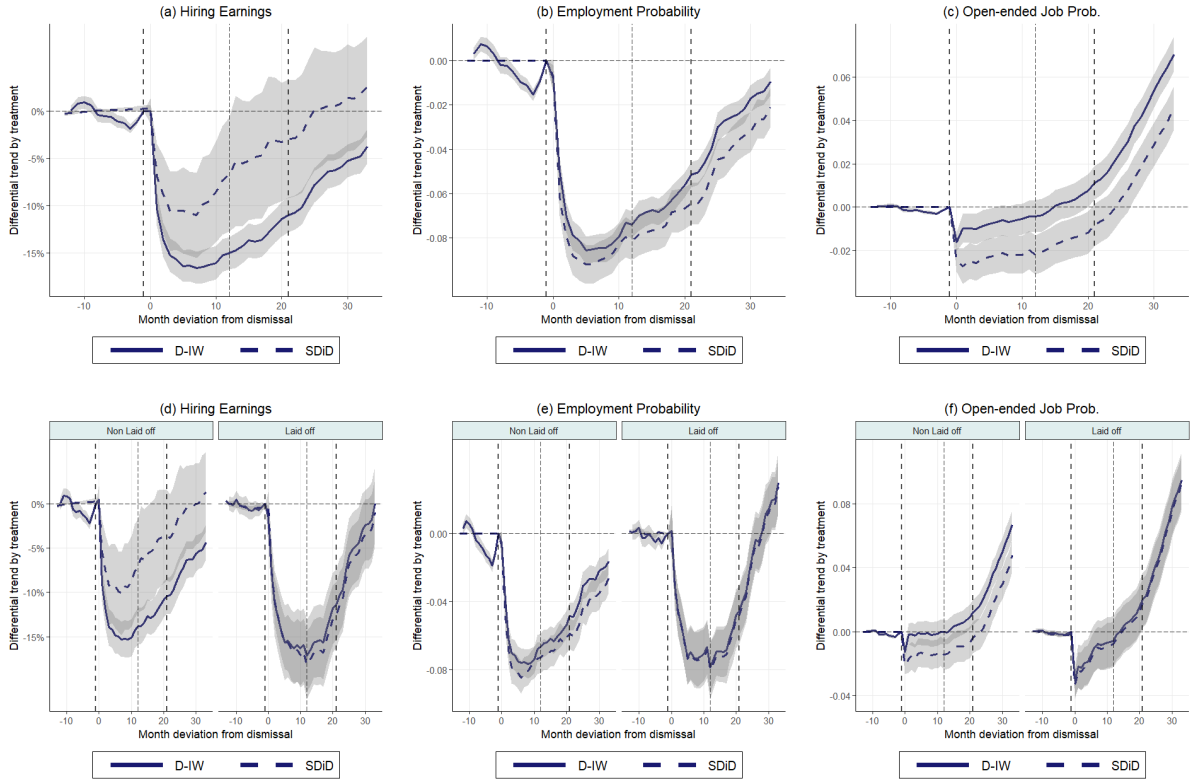
This procedure also mitigates concerns that our estimates might reflect differential exposure to aggregate conditions rather than the reform itself, since time periods are reweighted to be comparable across groups. One limitation relative to our D-IW estimator is that SDiD does not exploit the staggered timing of month-of-job-loss within cohorts. Moreover, because SDiD relies on weights, the comparison is 'local': some untreated workers receive large weight while others contribute little or nothing. In contrast, our D-IW framework uses time-invariant cohort weights and includes the full sample, stabilizing composition across event time.[36]

Figure 5 presents the SDiD results together with the corresponding D-IW profiles from panels (d)–(f) of Figure 2 and those from Figure 3. For hiring earnings (panel (a) of Figure 5), the SDiD effects are slightly smaller in absolute value but almost always statistically indistinguishable from the D-IW estimates. For re-employment probabilities (panel (b)), SDiD effects are somewhat larger (again in absolute value), while for permanent employment (panel (c)) they are more negative (and less positive). These differences are driven mainly by non-laid-off workers, who exhibit shorter pre-separation tenure (see the plots on the left in each panel (d)–(f) of Figure 5). Even in such cases, however, the overall picture under SDiD closely mirrors our benchmark D-IW results. This check therefore does not reveal a systematic bias arising from violations of the parallel trends assumption or differential business cycle exposure.

Balanced panel estimators — The second exercise evaluates whether our findings depend on the balanced event structure of the D-IW estimator. In subsection C.7.2 of the Online Appendix, we report the estimates found with two estimators robust to heterogeneous treatment effects in a balanced *panel* framework, following Callaway and Sant'Anna (2021) and Sun and Abraham (2021). We construct a balanced panel with untreated cohorts spannig July 2010–June 2011 and treated cohorts spanning July 2012–June 2013. Because job terminations are staggered, not all cohorts are observed in every calendar period, so cohort composition varies over time, potentially creating more scope for violations of parallel trends than in our baseline balanced-event design. Despite this, the estimates in Appendix Figures C23 and C24 closely match our benchmark results. Appendix Figure C25 further shows that our conclusions remain unchanged when adding a rich set of covariates, such as age, gender, indicators of regional mobility and prior

[36] We also implement a matched difference-in-differences approach using propensity scores. The results are consistent and reported in Online Appendix Figure C21.

Figure 5: Comparing SDiD to D-IW Estimates



Notes: Panels (a)–(c) refer back to the D-IW estimates in Figure 2, and panels (d)–(f) to the D-IW estimates in Figure 3.

work experience, job flow indicators, tax rebate exposure, and education quartiles.

For completeness, in Online Appendix Figure C22, we also report stacked DiD estimates (e.g., Cengiz et al., 2019).[37] These estimates are slightly smaller but remain statistically and economically consistent with the D-IW findings. We nonetheless argue that the balanced event framework is preferable in our context, because it allows for longer post-event horizons and eliminates changes in cohort composition through time-invariant weighting, thereby limiting changes in the sample structure that are inherent to the design.

Cohort-specific estimates — To provide a graphical diagnostic of whether job loss costs primarily reflect the reform rather than business cycle conditions, we construct cohort-level estimates derived from the structure of our D-IW design.[38] Recall that our main D-IW estimates are produced for each event period. However, alternative aggregations are possible (see, among others, Roth et al., 2023). Here, we build a cohort-level object that averages cohort-specific treatment effects (CATTs) over the first 12 months following job termination, separately for treated and untreated cohorts. Intuitively, for each cohort m in group $j \in \{\mathcal{T},\mathcal{U}\}$, we compute a single estimate of the

[37] Results from a stacked event study that includes the 12-month gap between treated and untreated cohorts (i.e., from July 2011 and June 2012) and those based on de Chaisemartin and D'Haultfoeuille (2020) are similar and available upon request.

[38] We thank an anonymous referee for suggesting this analysis.

cost of job loss over the first year after displacement,

$$\widehat{\nu}_{12,m,j} = \sum_{\ell=0}^{12} \widehat{\delta}^{\ell}_{m,j}.$$

These cohort-specific estimates summarize average outcomes for workers separating in the same calendar month and allow us to assess whether job loss costs vary across cohorts within each treatment group, and whether systematic differences emerge between treated and untreated cohorts. Appendix Figure C26 plots the $\widehat{\nu}_{12,m,j}$ estimates, where the horizontal axis corresponds to the calendar month of job separation.[39]

Within each group, the estimates are relatively flat, indicating limited sensitivity to business cycle conditions: job loss costs do not vary substantially across cohorts exposed to the same institutional regime. By contrast, the estimates differ markedly across groups, being systematically and significantly more negative for treated cohorts, especially for re-employment probabilities and earnings. Some seasonality is visible in August and December, but it is common to both groups and therefore unlikely to account for the estimated differential patterns. Overall, this exercise shows that the reform-induced penalties are unlikely driven by time-varying macroeconomic conditions, reinforcing the causal interpretation of our D-IW results.

Bounding time effects — The fourth check addresses the potential threat induced by time effects. The parallel trends assumption requires that the mean of time-varying unobservables be stable across cohorts. If differential business cycle effects are present, this condition may fail. Our design already mitigates business-cycle confounding by construction, and treated and untreated cohorts exhibit very similar pre-event dynamics. Still, we bound the role of time effects under a conservative hypothesis, which is in line with the Rambachan and Roth (2023)'s "credible" DiD approach.

Because all workers are employed at $\ell = -1$ and lose their jobs between $\ell = -1$ and $\ell = 0$, any systematic differential time effect should already emerge within this pre-event window. In Figure 2, the largest pre-event deviation is -0.015 (s.e.=0.003) at $\ell = -3$ for the probability of re-employment. Suppose conservatively that this deviation is entirely driven by time effects and subtract 0.015 from all post-event coefficients. Even under this extreme bound, the reform-induced re-employment penalty remains economically large (between six and eight percentage points) and statistically significant over the first 12 months after job loss, when the policy impact is cleanest. This shows that implausibly large and persistent time shocks would be required to overturn our core results. The results from this exercise are reported in Online Appendix C.9.[40]

[39] Each panel of the figure in the Online Appendix also displays the treatment-group-specific average effect obtained by averaging cohort-level estimates within each group, i.e.:

$$\widehat{\nu}_{12,j} = \sum_{m} \widehat{\nu}_{12,m,j} \Pr(M_{ij} = m \mid M_{ij} \in \{\mathcal{T},\mathcal{U}\}),$$

shown as a horizontal line. At the bottom of each panel, we report the difference between these averages,

$$\Delta\widehat{\nu}_{12} = \widehat{\nu}_{12,\mathcal{T}} - \widehat{\nu}_{12,\mathcal{U}},$$

together with its bootstrapped standard error.

[40] Related evidence by industry and firm location (see Online Appendix Figure C28) shows substantial heterogeneity across industries in the same geographic area, which is difficult to reconcile with aggregate time shocks that would affect local sectors more uniformly.

Firm size heterogeneity — The final exercise splits the sample by firm size, distinguishing workers in establishments with fewer than 15 employees from those in larger firms. Because CICO does not directly report firm size, we identify establishments above the 15-employee threshold using mandatory reporting rules, such as those in Law 68/1999 (see Sestito and Viviano, 2018). Finer thresholds cannot be measured reliably (Boeri and Garibaldi, 2019).

The results are shown in Online Appendix Figure C29. Across all outcomes, negative effects are stronger for workers previously employed in organizations with more than 15 employees, which after the Fornero reform could more easily resort to dismissals. Such differences persist even after the introduction of permanent-contract hiring incentives under the Jobs Act. This pattern is consistent with our information-based interpretation: post-reform, employers expect dismissed permanent workers, especially from larger firms, to cluster in the pooling equilibrium, strengthening the productivity signal associated with their separation.

Although the estimates for workers in large firms are noisier because of smaller sample size, the negative effects are consistently larger than for workers in small firms. This pattern is difficult to reconcile with a pure business-cycle explanation, as aggregate shocks should affect workers across firms of different size more uniformly. By contrast, it supports the view that the reform had changed separation and screening behavior precisely where dismissal practices were altered. While the policy affected the entire re-hiring market, it was particularly detrimental for workers coming from larger firms, for whom the informational content of dismissals was more salient.

Taken together, all five robustness exercises support our baseline D-IW estimates. SDiD reweighting yields similar dynamic effects; balanced panel estimators replicate the magnitude and persistence of the post-displacement penalties; the cohort-level diagnostic shows that the cost of job loss is stable across cohorts within institutional regimes but sharply different between treated and untreated groups, indicating that the effects are not driven by business-cycle variation; conservative bounds on time effects leave the main conclusions intact; and the firm-size split identifies the strongest responses precisely along the institutional margin most affected by the reform. Italy experienced weak labor market conditions throughout the period for both treated and untreated cohorts, but the consistency of the estimates across designs, aggregations, and subsamples makes it unlikely that business cycle confounding alone could drive our findings. Instead, the evidence points to a structural effect of the Fornero reform on the re-hiring market, rather than an artifact of sample selection, timing, or aggregate shocks.

6 Heterogeneity and Channels

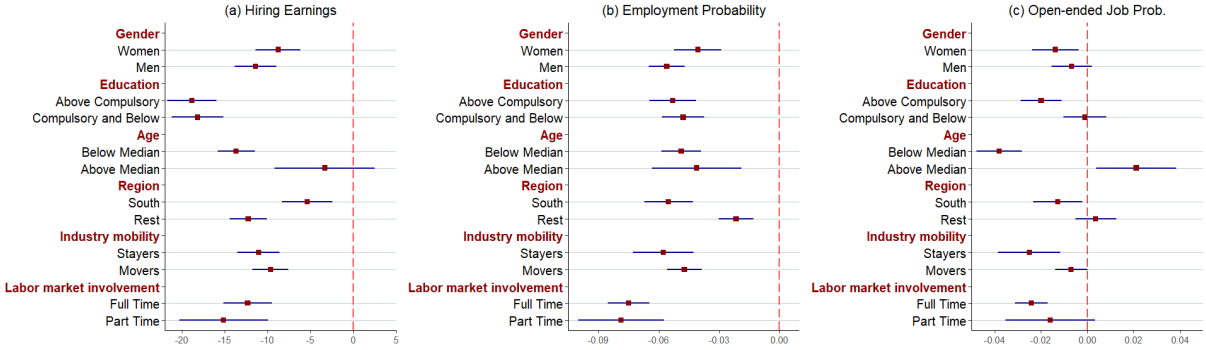
Having established the average effects of the Fornero reform on earnings and employment in the re-hiring market, this section examines how these impacts vary across workers and jobs. We study heterogeneity by gender, education, and age, as well as by workplace location, industry mobility, and part-time versus full-time status. These dimensions are informative also for assessing whether the reform operates mainly through human capital losses (e.g., reduced transferability of skills) or through changes in firms' re-hiring strategies and beliefs about worker productivity, as emphasized in our model.

Workers are grouped according to their pre-separation characteristics, i.e., the attributes of

the job from which they separated, except for part-time versus full-time status and job location, which are defined based on the current job. To summarize the effects concisely, we report outcomes at three time horizons relative to job loss: one month, 12 months, and 33 months. The first two capture the impact of the Fornero reform, while the last reflects the cumulative effects of the 2012 reform and the subsequent 2015 Jobs Act. The underlying D-IW estimates are in Online Appendix D.1.

One month from job loss — Figure 6 reports short-run heterogeneity. We find no meaningful gender differences, with both men and women facing similar penalties across all three outcomes. We proxy skills using educational qualifications, distinguishing workers with at most lower secondary schooling from those with upper secondary or higher education. Both groups experienced declines in earnings and re-employment, but high-education workers faced a significantly larger drop in the probability of obtaining an open-ended contract (panel (c)). This pattern is hard to reconcile with a simple human capital story, which would predict smaller losses for more educated workers. Instead, it points to a change in firms' screening behavior. Conditional on separation, being fired becomes a stronger negative signal for highly educated workers, when firms expect a larger stock of low-productivity applicants in the post-reform pooling equilibrium.

Figure 6: Heterogeneity One Month from Job Loss



Notes: For each attribute, the figure shows D-IW estimates and the corresponding 95% confidence intervals obtained from clustered bootstrapped standard errors (100 replications). The vertical dashed red line indicates zero effect. Skills are proxied by education. The low-education group includes individuals with at most lower secondary schooling; the high-education group includes individuals with upper secondary or higher qualifications, including college and university degrees. Southern regions are Sicily, Calabria, Apulia, Basilicata, Campania, Molise, and Sardinia. Industry stayers and movers are defined using a 12-industry classification based on five-digit industry codes.

Age differences are sharper. We split workers at the median age in the full sample (i.e., 34 years). Younger workers suffer nearly a 10% larger earnings penalty at hiring and a six percentage point larger decline in the probability of permanent re-employment than older workers. Rather than mitigating labor market duality, the erosion of employment protection worsened the outcomes of younger workers. In the framework of our model, this is consistent with firms having noisier priors about younger workers' productivity and, therefore, relying more heavily on the signal embedded in dismissals once firing costs fall.

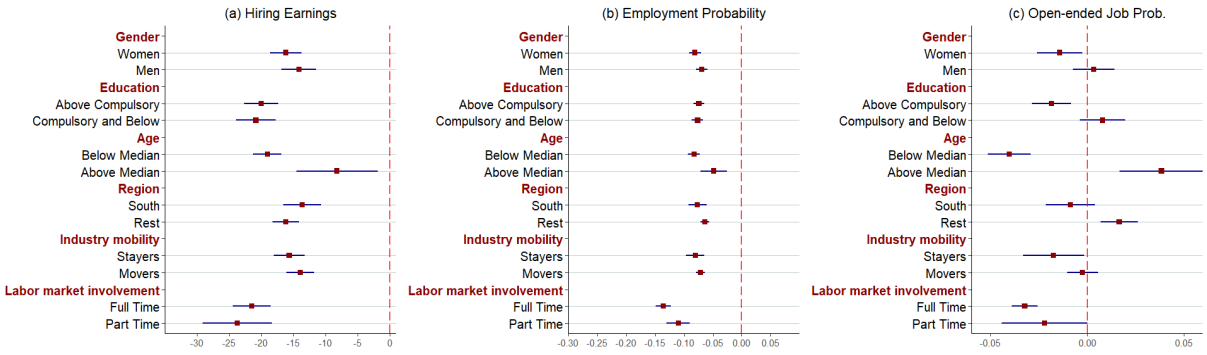
Workplace location also matters. Workers in the South faced a three percentage point larger decline in re-employment probabilities than those in the Center–North (panel (b)), but a smaller earnings penalty at hiring of about 7%. This asymmetry is consistent with binding collective agreements on wage floors or more compressed wage structures in the South, which limit downward wage adjustment and shift the response toward quantities rather than prices.

Industry mobility provides a clean way to contrast human capital and information channels. Workers who remained in the same industry retained more sector-specific skills, but their separation may convey more information to potential employers familiar with the relevant task structure and technology needed to perform the job in that industry. Movers, by contrast, lose industry-specific human capital but may face weaker (less negative) signaling effects. The evidence in Figure 6 favors the information channel. Compared to movers, stayers experience a statistically significant two percentage point lower probability of obtaining a permanent contract. Differences in earnings and overall re-employment point in the same direction, although they are estimated less precisely. Finally, splitting by part-time versus full-time status at separation yields no statistically significant differences in the first month. Early penalties are therefore not driven by hours arrangements per se.

Overall, short-run heterogeneity is difficult to explain through skill depreciation alone and is instead consistent with a reform-induced change in the way firms interpret job loss as a productivity signal.

One year from job loss — The estimates at the twelfth-month mark are in Figure 7. Gender gaps remained negligible. Education differences persisted for permanent re-employment, with highly educated workers facing a three percentage point larger penalty (panel (c)), while differences in earnings and overall re-employment remained small. Age effect differentials, by contrast, widened across all outcomes: younger workers experienced larger and more persistent losses in both hiring earnings and employment probabilities. These patterns reinforce the interpretation that the reform primarily affected beliefs and screening, rather than slowing human capital accumulation. If the dominant channel were skill erosion, then education should mitigate losses over time; instead, the disadvantage of young and highly educated workers concentrates precisely on permanent matches, where firms are most sensitive to perceived productivity.

Figure 7: Heterogeneity at 12 Months from Job Loss

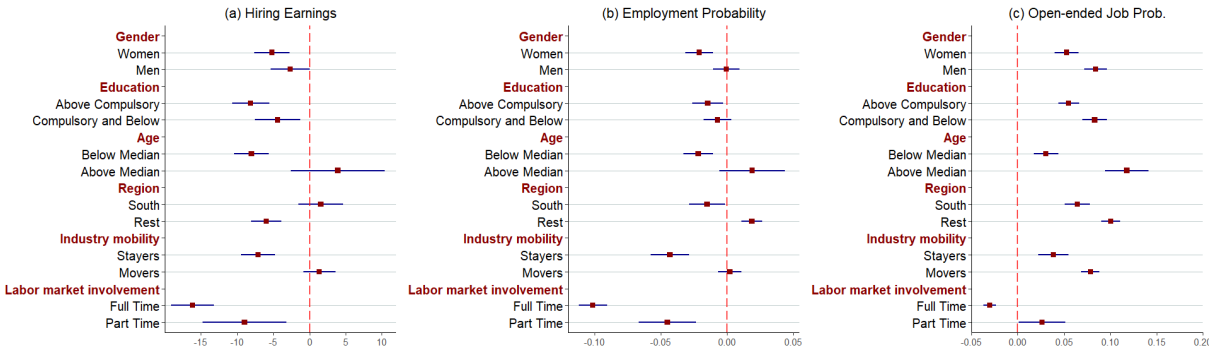


Notes: For details, see the notes to Figure 6.

At the one-year horizon, earnings and employment deteriorated across all regions. Workers in the South, however, faced an additional penalty of up to a three percentage points in permanent re-employment (panel (c)). Differences by industry mobility and hours arrangement remained economically large but statistically similar across groups, suggesting that the reform broadly worsened re-hiring conditions rather than reallocating losses across these margins.

After 33 months from job loss — Figure 8 shows the estimates at the end of the observation window. At this horizon, the reform intensified most pre-existing divides. Penalties grew along gender, age, workplace location, and sectoral mobility, with the notable exception of earnings in the South. A new pattern emerges along the full-time/part-time margin: full-time workers now experienced *larger* earnings and re-employment penalties than part-time workers. This shift might be due to the hiring incentives introduced by the Jobs Act. Rather than indicating improvement for part-timers, the pattern reflects a general deterioration of conditions in the re-hiring market, with part-time matches absorbing the adjustment when firms remain reluctant to commit to full-time contracts. After almost three years, the reform did not converge groups to a common outcome but instead magnified segmentation across observable characteristics.

Figure 8: Heterogeneity at 33 Months from Job Loss



Notes: For details, see the notes to Figure 6.

Summary and Discussion — Three main conclusions emerge from Figures 6–8. First, the Fornero reform did not generate a "leveling-up" effect. Instead, it deepened several pre-existing divides: younger workers, highly educated workers, workers in the South, industry stayers, and — over a longer horizon — full-timers faced systematically larger penalties in permanent re-employment opportunities and earnings at hiring. Second, the structure of these effects is inconsistent with a simple human capital mechanism. If losses were driven mainly by skill depreciation, then movers, low-education workers, and older workers would have been worse off. We observe the opposite along several margins, particularly for open-ended contracts.

Third, the evidence aligns closely with the implications of our information-based equilibrium model. By lowering firing costs, the reform changed both the informational content of separations and firms' perceived composition of job seekers in the secondary market. Employers updated their beliefs about average productivity and adjusted wages and hiring thresholds accordingly. Because permanent jobs generally involve higher commitment, firms responded most strongly along that margin. Groups for which employers have noisier priors (such as younger workers) or for whom

dismissal conveys sharper information (such as industry stayers and highly educated workers) were disproportionately negatively affected.

7 Conclusion

This paper studies the relationship between employment protection and workers' cost of job loss in Italy, using the 2012 Fornero reform, which substantially reduced the scope for reinstatement of workers on open-ended contracts following dismissal. The policy objective was to stimulate hiring in permanent jobs and reduce the persistent dualism that has characterized the Italian labor market since the early 1990s. Adapting the approach proposed by Sun and Abraham (2021) to a stacked event-study design, we estimate how the reform changed workers' post-displacement earnings and employment trajectories.

We find that the reform generated sizable penalties in the re-hiring market. Twelve months after job loss, treated workers experienced a re-hiring earnings loss of about 15% and a decline in re-employment probabilities of 7 percentage points, while the impact on permanent re-employment was statistically indistinguishable from zero. These effects apply to individually laid-off workers and to other workers (including quitters and those affected by mass layoffs and expiration of fixed-term contracts), indicating that the reform did not mitigate labor market duality. If anything, it intensified it: traditionally vulnerable groups (such as younger workers) as well as industry stayers turned out to bear the largest costs of the reform.

We interpret these results through a hybrid equilibrium framework in which employers update beliefs about worker productivity in response to changes in employment protection legislation. By lowering the likelihood of reinstatement, the Fornero reform altered the informational content of job separation and employers' perception of the composition of applicants in the re-hiring market. Firms rationally revised downward the probability of encountering a high-productivity worker in the pooling segment of the market, and this compressed wage offers and raised hiring thresholds. In this environment, post-reform matches are more likely to involve lower expected productivity, providing a natural explanation for the persistent earnings and employment penalties we identify. Although the generous hiring subsidies for new open-ended jobs introduced by the 2015 Budget Law and upheld by the Jobs Act eventually turned permanent re-employment penalties into advantages for insiders (especially men, older workers, residents of the Center–North, and cross-industry movers), treated cohorts continued to exhibit lower re-hiring earnings and re-employment probabilities relative to untreated workers almost three years after displacement.

Taken together, our findings show that relaxing employment protection generates substantial heterogeneity in post-separation outcomes across workers and jobs. Such heterogeneity is likely to be relevant in many economies, particularly those characterized by labor market dualism. By reshaping firms' beliefs, screening practices, and match outcomes in the secondary labor market, employment protection reforms can contribute to labor market polarization, disproportionately penalizing outsiders. Evidence from Sweden (Athey et al., 2026) and Germany (Hanushek et al., 2025) suggests these effects are not unique to Italy. Our results indicate that policies lowering dismissal protection should be complemented with targeted support for displaced workers to prevent the costs of job loss from falling unevenly across the labor force.

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