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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 June 2012 that eroded employment protection to workers on permanent contracts, we use detailed administrative data to estimate how this reduction affected the cost of job loss. We employ a stacked-by-event research design, which compares workers moving into nonemployment before and after the reform. 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 more, among others, young workers and workers living in the South.

JEL-Codes: J630, J650, J300, J410, J680.

Keywords: layoffs, employment protection, dual labor markets, difference-in-differences, Italy.

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1 Introduction

It has long been argued that a reduction in employment protection is associated with greater job flows and lower job loss costs to workers (e.g., [Mortensen and Pissarides, 1999](#); [Cahuc and Zylberberg, 2004](#)). Recent empirical work has indeed backed up this argument, documenting that the cost of job termination is higher in countries with more stringent employment protection regulations ([Bertheau et al., 2023](#)). In this paper, we leverage detailed administrative data to examine the impact on the worker’s cost of job loss resulting from an Italian policy intervention, known as the Fornero reform. This reform eroded pre-existing high levels of employment protection by limiting the likelihood of workplace reinstatement after firing workers on open-ended (permanent) contracts and by cutting down the financial cost faced by employers following individual layoffs.

Italy offers an extremely useful benchmark for this type of evaluation. At the close of 2011, its economy teetered on the brink of default. International lending institutions urged the government to effect new policies, with the aim of tackling labor market duality, which had beset the country’s economic performance since the early 1990s (e.g., [Saint-Paul, 1997](#); [Boeri, 2011](#); [Boeri and Garibaldi, 2007, 2019](#)), and enhancing permanent employment prospects for “outsiders”, such as young workers. In June 2012, this led to the introduction of the Fornero reform, which implied a weakening of employment protection for employees in permanent jobs, who were typically considered labor market “insiders”, and it came as an unexpected addition to earlier reforms that negatively affected outsiders, especially temporary workers ([Daruich et al., 2023](#)).

In an environment with high levels of employment protection, the motivation of a job termination may be ambiguous and the distinction between individual layoffs and quits is likely to be tenuous. As shown by [Di Addario et al. \(2023\)](#), employers could use severance packages to incentivize workers to move into unemployment or accept external job offers they might otherwise decline (see also [Postel-Vinay and Turon, 2014](#)). In these circumstances, then, the actual composition of workers in the re-hiring market may not change around the reform, even though the reasons for job loss could differ. To account for this shift and for the impact of the reform across all segments of the labour market, we consider *all* workers in the re-hiring market, including those who terminate fixed-term contracts and those who quit to unemployment.

We use a variant of the [Sun and Abraham \(2021\)](#)’s estimator in a stacked-by-event design, comparing workers who moved into non-employment before and after the 2012 reform. This requires grouping workers into cohorts, with each cohort being defined by the month when a job ends. We focus on 12 untreated and 12 treated cohorts. Treated cohorts consist of individuals whose jobs ended between July 2012 (shortly after the reform introduction) and June 2013, while workers in untreated cohorts lost their jobs between July 2010 and June 2011. For each worker, irrespective of cohort, we have a balanced event study with a time window of 13 months before the event and an additional window of 33 months after, for a total of 47 months, inclusive of the month of the event (i.e., when the job terminates). The first window allows us to observe how treated and untreated cohorts performed in the year preceding job loss. The second time frame enables us to follow treated workers up to one year after the enactment of another reform, the 2015 Jobs Act, which reinforced many features of its 2012 predecessor and introduced new

incentives to recruit 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 untreated cohorts was affected by the Jobs Act. Moreover, in the initial 12 months post-displacement, we can precisely pinpoint the clean impact of the Fornero reform on treated cohorts. After this period, when untreated cohorts also gradually became exposed to the reform, our estimates capture the differential cumulative effect of immediate treatment compared to treatment at least 12 months post-displacement. Lastly, after 21 months post-termination, when the 2015 reform impacts only treated cohorts, we identify the cumulative impact of the two policy interventions on treated cohorts as opposed to just the effect of the 2012 reform on untreated cohorts.

We analyze three outcomes: earnings at re-hiring, the probability of re-employment, and the probability of being re-employed on an open-ended contract. They are all measured at the individual worker level and at monthly frequencies. Although the data do not report repeated information on wages, earnings on the month of re-hiring provide an important indicator of the conditions of the secondary labor market. Focusing on the two re-employment probability margins will permit us to understand if the reform tackled labor market duality successfully, leading to permanent job creation and benefiting traditional outsiders.

Identification hinges on two assumptions.¹ Since all individuals in the analysis switch by design from working to the non-employment focal event in the following month, our first hypothesis is the presence of parallel trends at baseline between treated and untreated cohorts. Because the timing of the reform is exogenous to individual workers, this condition holds as long the means of time-varying unobservables, which may contribute to assignment into cohorts, are unchanged over time conditional on worker fixed effects (Ghanem et al., 2023). We shall elaborate more on why this is likely to be the case in our setting and how this assumption is also related to differences in business cycle effects.² We will also provide additional evidence based on a variety of alternative approaches, including the synthetic difference-in-differences estimator introduced by Arkhangelsky et al. (2021), which allows for a direct adjustment of untreated counterfactuals on both individual and time effects in a stacked-by-event design like ours.

The second assumption is the absence of anticipation. If, anticipating the reform, individuals were to secure a new job in the months prior to job loss, this job would not identify the focal event and the worker would not be assigned to that cohort. The swift ratification of the Fornero reform (first discussed in Parliament in April 2012 and enacted two months later) gave workers little time for strategic behavior in the pre-displacement period, while firms had no incentive to anticipate dismissals before June 2012. There is also no evidence that the duration of temporary jobs changed between treated and untreated cohorts, with no new policy being introduced and employers continuing to set the length of fixed-term contracts following the same legal standards before and after the reform.

¹For a useful summary of the recent econometric literature on identification of difference-in-differences and related designs, see Roth et al. (2023).

²The intentional maximum three-year time gap between the two *most distant* cohorts and minimum 12-month gap between the two *closest* cohorts are set to ensure comparability, thereby minimizing issues related to differential business cycle effects and violations of parallel trends. We will return to this point in other parts of the paper.

We find that, twelve months after job loss, the Fornero reform led to a re-hiring wage penalty of 15% and a re-employment penalty of 7 percentage points. There is no evidence of a statistically significant impact on the probability of permanent employment. These effects operate through a lower employment protection for workers on open-ended contracts (insiders), and more specifically, a reduced likelihood of their workplace reinstatement after dismissal. Nearly three years post-job loss, penalties on earnings and employment probability were smaller but still statistically different from zero at 4% and 1 percentage point, respectively. While the exposure of untreated cohorts to the Fornero reform and the implementation of the 2015 Jobs Act may have contributed to narrowing the gaps, they did not eliminate them.

The lower likelihood of workplace reinstatement affected all workers, both laid-off and non-dismissed individuals. However, the reform yielded heterogeneous impacts which amplified existing inequalities in the labor market. Younger workers, industry stayers, and those employed in the South turned out to be among the main losers, facing large penalties across all outcomes. Interestingly, also full-timers experienced substantial penalties. If anything, this evidence suggests a “levelling down” effect for insiders towards outsiders.

To interpret the results, we use a stripped-down asymmetric information framework. The focus on informational frictions seems to be appropriate in this context for at least two reasons. First, information is crucial in the case of young workers, who have typically over-populated the re-hiring market in Italy with repeated short fixed-term jobs and high churning. Second, with widespread worker heterogeneity, information is essential to minimize skill mismatch. By reducing the likelihood of workplace reinstatement and simplifying the legal procedure following dismissals, a greater fraction of low productivity workers would be dismissed, rather than induced to quit. Under the Fornero reform, then, employers would increase their beliefs to come across low-ability workers rather than skilled ones in the secondary market. This would dampen firms’ expectations on average productivity, worsening workers’ re-hiring conditions.³

Our paper contributes to different strands of economics research. First, it adds to the literature that examines the impact of labor market regulations on the cost of job loss. To the best of our knowledge, only two other studies focus on this issue, but from different perspectives. One is the study by [Janssen \(2018\)](#), which investigates the relationship between the costs of job displacement and the decentralization of wage bargaining systems, a separate type of regulation from the one analyzed in this paper.⁴ The other study is by [Bertheau et al. \(2023\)](#), which uses a harmonized research design to compare the cost of job loss across seven European countries, including Italy. They document a great deal of cross-country heterogeneity in earnings loss following job displacement, with Italian workers facing some of the largest penalties, and argue

³Of course, other explanations are plausible. For example, a story based on foregone human capital accumulation could help us to understand earnings losses among displaced workers. Some of our results, however, are hard to reconcile with this interpretation. For instance, older workers and individuals who moved across industries (or occupations), who might have suffered a greater human capital depreciation, experienced lower penalties than their non-mover counterparts. In Sections 5 and 6, we shall return to these points in greater detail.

⁴Using Danish data on the manufacturing sector, [Janssen \(2018\)](#) shows that under a centralized wage bargaining system at the national level, displaced workers’ income losses are small, whereas under a decentralized firm-level wage bargaining system, the losses are larger, in part because displaced workers experience worse wage growth under the decentralized system.

that the most pronounced losses are observed in countries where the generosity of the welfare state tends to be the lowest, such as in Italy (e.g., [Boeri, 2011](#)). These results motivate our work, which is the first to focus explicitly on the effect of a reform that reduced employment protection rights on the cost of job loss to workers.

Second, a long-standing body of economic research has documented a strong link between job loss and substantial employment and earnings penalties for displaced 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., 2023](#)). Most of these recent studies identify penalties using mass layoffs or firm closures pointing to scarring effects of displacement ([Acabbi et al., 2024](#)). Our paper contributes to this strand of work by analyzing how the cost of displacement varies in response to a change in employment protection legislation and by considering the entire re-hiring market.

A third area of related research concentrates on reforms to employment protection legislation, which is too extensive to be reviewed here (see, among others, [Boeri, 2011](#)). Relevant to our work, however, are the results from the studies that argue that reducing employment protection, especially relaxing legal constraints on the use of fixed-term contracts, will impact labor market efficiency and lead to a substitution of workers on open-ended contracts with temporary job workers (e.g., [Bentolila and Dolado, 1994](#); [Cahuc and Postel-Vinay, 2002](#); [Blanchard and Landier, 2002](#); [Güell and Petrongolo, 2007](#); [Cahuc et al., 2016](#); [García Pérez et al., 2019](#); [Cahuc et al., 2023](#); [Créchet, 2024](#)). A fitting addition to this body of research is the recent study by [Saez et al. \(2023\)](#), which documents high job separation rates (involuntary to workers) resulting from the elimination of employment protection rights for older workers close to retirement in Sweden. Our analysis complements this line of work by investigating the consequences of reduced opportunities of workplace reinstatement on the costs of job loss for all workers below retirement age.

Finally, scores of studies focus on Italy. A benchmark for our analysis is the research on policy interventions that removed most of the employment protection for workers on temporary jobs, which finds that temporary contracts crowd out permanent jobs without employment gains (e.g., [Tealdi, 2019](#); [Hoffmann et al., 2022](#); [Daruich et al., 2023](#)). The same studies also document that younger workers are typically the main losers from this sort of reforms. Our paper examines a different reform, which weakened employment protection for workers on open-ended (and *not* fixed-term) contracts, and yet finds that young workers continue to get the short end of the stick. Other studies analyze the Fornero reform but, unlike our paper, focus on different outcomes, including the quality of job matches ([Berton et al., 2017](#)), firm-provided training ([Bratti et al., 2021](#)), and firm adjustments in skill demand ([Bottasso et al., 2023](#)).

The rest of the paper proceeds as follows. In Section 2 we discuss the context within which the Fornero reform took place. The data and research design are presented in Sections 3 and 4, respectively. Our main results are reported in Section 5, while heterogeneity and channels are explored in Section 6. Section 7 concludes. A wealth of supplementary material is in the Online Appendix.

2 Institutional Background

At the outset of the 2008–2009 financial crisis, Italy had a particularly high employment protection of workers in permanent jobs and a strict regulation of temporary contracts to avoid overuse. As the international lending markets lacked confidence in the country’s ability to tackle the crisis, the Berlusconi government was replaced by a technocratic cabinet headed by Mario Monti in November 2011. Among a series of wide-ranging economic interventions, the new government passed the so called “Fornero” reform, named after the then labor secretary, which in June 2012 introduced a set of policies across several labor market dimensions, from apprenticeship to social insurance. To incentivize apprenticeships and encourage the diffusion of open-ended contracts, the reform raised the social security contributions that firms had to pay on temporary (fixed-term) contracts. One of the goals of these higher contributions was to use their proceeds to fund an ambitious unemployment insurance scheme, which would have also benefited workers with relatively short employment histories, e.g., young individuals.

Perhaps the most important of all changes was the overhaul of Article 18 of the *Statuto dei Lavoratori* (Workers’ Statute). Through this intervention, the lawmakers intended to curtail the pervasive duality in the labor market that penalized outsiders in general, and young workers in particular. Since 1970, in fact, the Statute protected workers on open-ended (permanent) jobs in the event of unlawful dismissal by any firm with more than 15 employees.⁵ The definition of unlawful dismissal was broad and included discriminatory and unjustified dismissals as well as dismissals due to economic reasons.⁶ The protection was substantial, as it implied a full compulsory reinstatement of unfairly dismissed workers, including payment of their forgone earnings and national insurance contributions. Although seldom enacted (with just about 3,000 cases brought to employment tribunals in a typical year before 2012), the possibility of reinstatement was a material deterrent to hiring on permanent contracts in and of itself. Workers may have opted not to be reinstated and, in this case, employers had to face high dismissal costs of up to 36 months of pay, even for short tenures.

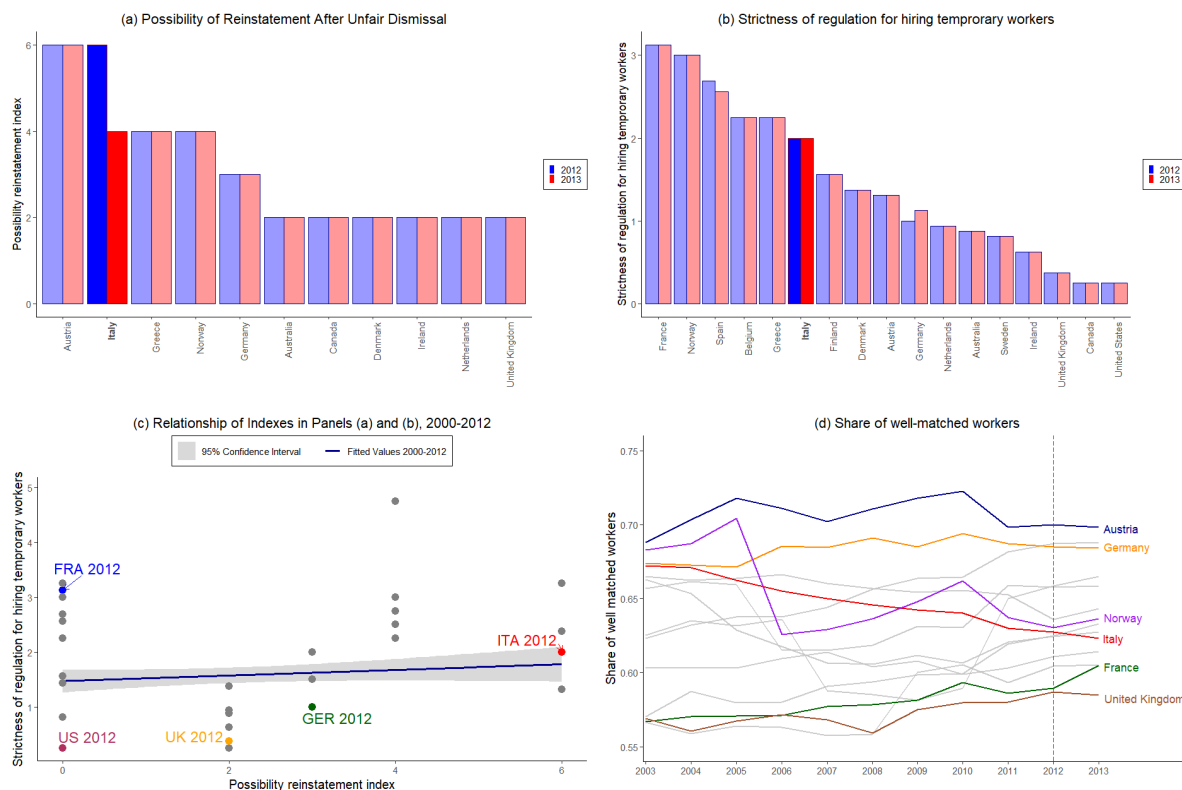
The Fornero reform drastically shifted power from the worker to the employer. First, it simplified the dismissal process from a legal perspective, with the introduction of an out-of-court conciliation service intended to facilitate the attainment of a settlement agreement among parties without the need for a resolution through an employment tribunal. Second, only if the conciliation service failed, a settlement would be determined by a judge. Most cases were encouraged to be resolved quickly, in an attempt to reduce uncertainty about the duration of the trial and curb legal expenses. Third, with reinstatement becoming very limited, workers lost the right to choose between monetary compensation or reinstatement in case of unfair dismissals. Fourth, severance payments in all other nondiscriminatory dismissals were substantially lowered to a maximum of

⁵There is evidence that, as a result of elevated firing costs, Italian firms with more than 15 employees heavily relied on different types of employment other than permanent jobs, including fixed-term contracts, short-time insurance schemes, delayed renewal of expired collective agreements, and “pirate” collective agreements, i.e., sector-level collective agreements signed by unidentified trade unions (e.g., [Lucifora and Vigani, 2021](#)).

⁶Firm downsizing and closure were accepted reasons for collective (not individual) dismissal. In such cases, however, firms had to give notice of their decisions and consult with trade unions. Procedural irregularities would have normally led to a settlement, which could have included workers’ reinstatement whenever possible.

24 monthly wages and they could have been as low as five months of pay. Although some local variation about the judicial interpretation of the law remained, the 2012 reform represented a considerable improvement from the employer’s viewpoint. It eliminated a sizeable portion of uncertainty about the resolution of dismissals of workers in permanent jobs, it expedited the layoff process significantly and made it cheaper.⁷

Figure 1: A cross-countries comparison of the OECD 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>).

To place the Italian institutional environment at the time of the reform in an international context, Figure 1 provides a visual representation of the employment protection conditions in 2012 and 2013 in Italy and a selection of other industrialized economies. In 2012 before the reform, Italy, along with Austria, offered permanent workers the highest possibility of reinstatement after

⁷By encouraging the use of apprenticeships, the reform also led to an increase in open-ended employment (e.g., Maida and Sonedda, 2024). Exploiting another aspect of the reform that introduced a cut in the employer-borne payroll tax for women effective from January 2013, Rubolino (2022) shows that this cut led to a growth in female employment without crowding out male employment. This specific dimension is not of interest in our analysis, although we shall consider potentially different responses by gender in Section 6.

an unfair dismissal (panel (a)). Following the Fornero reform in 2013, this index dropped to the same level as Greece and Norway’s (indicating a lower employment protection), but was still higher than in other countries, such as Germany and the United Kingdom.

Over the same two years, Italy did not experience any change in the rules on temporary workers’ hiring (panel (b)). These restrictions are typically considered necessary in order to reduce an excessive use of fixed-term work while incentivizing permanent employment (OECD, 2020). On this dimension, therefore, Italy remained similar to other European countries, such as Belgium and Finland, more deregulated than some, such as France and Norway, but less flexible than the Netherlands, the UK, Canada, and the US.

As for other similar interventions aimed at reducing labor market duality, the Fornero reform was introduced on the assumption of a strong positive correlation between the two indexes shown in panels (a) and (b). The rationale behind this correlation is simple: a stringent employment protection of permanent employment may reduce job flows of workers on open-ended contracts and promote a dual labor market with an excessive use of temporary jobs. If instead employers could easily (cheaply) dismiss permanent workers, then firms would not resort to using fixed-term contracts, and no strict rules on temporary workers’ hiring would be needed. 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 ($\text{corr}=0.051$, $\text{s.e.}=0.039$), a result that is robust to several other pre-reform time windows.

Despite weak grounds for the reform on this count, one could motivate the policy enactment on worker mismatch arguments. In other words, it is possible that the Italian labor market was characterized by a greater proportion of workers poorly matched to their jobs. Panel (d) of Figure 1, however, shows that Italy was broadly in line with other advanced economies (including the UK and France) on this dimension, both pre-reform and soon after its implementation, despite a longer run gradual deterioration. Should firms have reliable information on workers, poor quality matches could be curtailed to a large degree. In the absence of reliable information, however, firms may hire just on the basis of signals and beliefs on the average worker’s productivity. Reducing the possibility of job reinstatement, one of the policy tools that curb the power of employment protection legislation, might have affected firms’ beliefs about this average productivity. Moreover, as employers were allowed to dismiss workers more freely after the reform, employees had an incentive to be laid off rather than quit, as quitters were excluded from unemployment benefits. The combination of these two specific aspects of the new policy could have changed the recorded reason for a job termination, rather than the event itself, and possibly impacted firms’ expectations. Both features will be important for the interpretation of our empirical results.

Fornero reform notwithstanding, Figure 1 illustrates that the Italian labor market still had relatively strict regulations in place, especially on the possibility of reinstatement of dismissed permanent workers. With the adoption of the Jobs Act in March 2015, the government expanded the Fornero reform further in terms of the treatment of permanent workers following a dismissal. The new rules applied only to new open-ended contracts and “grandfathered” earlier rights on pre-existing jobs. This intervention circumscribed the possibility of reinstatement just to the case of

discriminatory dismissals, reducing judicial indeterminacy substantially. Monetary compensation became the default in the case of all economic unfair dismissals. Moreover, the Act allowed for employment protection to increase with tenure, with severance payment being flat at four monthly wages for the first two years of tenure and then rising with tenure up to 24 monthly wages for 12 or more years of tenure. The 2015 intervention also bolstered the out-of-court procedure already set in place by the Fornero reform, by providing a default. This consisted of an indemnity to be paid by the employer to the worker equivalent to two months of wages for a tenure of two years and then an additional month of pay per year of service, with a cap at 18 monthly wages after 18 years of tenure. Accepting this out-of-court settlement would have prevented further disputes between employers and workers and might have been in the best interest of both parties, as the sum of money paid was subject neither to social security contributions nor taxation.

As the change in the possibility of reinstatement rules was not deemed sufficient to increase permanent employment, the Jobs Act was preceded by a sizeable hiring subsidy for any new job opened on a permanent basis by the Budget Law in January 2015. This benefited workers without a permanent job in the six months before the incentivized hire and those not hired as apprentices. Employers were exempted from paying social security contributions on all new open-ended hires for three years up to a cap located around mean earnings (e.g., Boeri and Garibaldi, 2019). Thus, the subsidy was particularly generous for low-wage (possibly smaller) firms, but less so for high-wage firms. The Jobs Act upheld this provision.⁸

3 Data

3.1 Data Sources

Our data come from the Italian Ministry of Labor and Social Policies administrative records. Since 2009, each firm (whether public or private) fills out a form for every new hire, every job termination and every internal job change. Each form is a building block of the data known as *Comunicazioni Obbligatorie*. The Ministry provides a nationally representative randomized sample that covers about 13% of such flows. This is an employer-employee dataset with anonymized identifiers for both and is known as *Campione Integrato delle Comunicazioni Obbligatorie* (or CICO; Integrated Sample of Mandatory Communications).⁹

We use the version of the CICO data that spans from January 2009 to June 2017. We know the employer’s location (region) and industrial sector (five digits); but no other firm information is available, including firm size. On the worker’s side, we have information on gender, year and region of birth, region of work, and education. Each worker’s career can be constructed at the monthly level in a panel format, from the first time the worker joins the CICO files. If workers enter the data as new hires, we can track them from January 2009 onwards. If they terminate

⁸Using a sample of firms with 10 to 20 employees between 2013 and 2016, Boeri and Garibaldi (2019) find evidence of a significant increase in open-ended hires and the transformation of fixed-term jobs into permanent contracts, as a result of the 2015 Jobs Act. They find only modest effects on firings and mainly concentrated among large firms. See also Sestito and Viviano (2018).

⁹Access to the CICO data is publicly available for research purposes by completing a form downloadable at <https://dati.lavoro.gov.it/microdati-la-ricerca>.

their job over the period, we can recover their full job spell even if the job started before 2009.

For each job in the sample, we have information on start date, end date, type of contract (e.g., open-ended, apprenticeship, fixed-term, and atypical), whether it is full- or part-time, occupation (four digits), and the specific type of collective bargaining agreement that regulates the job.¹⁰ CICO contains detailed information on the reason why the job ended. This allows us to identify layoffs precisely. It also contains accurate information from the Italian Social Security Institute (INPS) on hiring earnings, i.e., 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 hiring earnings. Re-employment is measured by an indicator function that takes value one if the individual works at least 15 days a month. Re-employment in an open-ended contract is measured by a dummy variable that is equal to one if an employee works at least 15 days a month in an open-ended (permanent) job. Finally, hiring earnings are defined by earnings in the first month of re-employment deflated to 2013 Euros and normalized by the average earnings observed one month before job loss. This normalization allows us to express the treatment effect in percent terms.¹¹ We include individuals who are not (yet) re-employed by assigning a zero value to their earnings (e.g., [Athey et al., 2023](#); [Chen and Roth, 2024](#)).

3.2 Sample Selection

To fit the 2012 reform into the research design described in the next section, our sample consists of employees who terminated their jobs over two 12-month periods, one starting in July 2010 and the other starting in July 2012. Since we focus on the entire re-hiring market, we consider *all* types of job loss, including individual layoffs, mass layoffs, expirations of fixed-term contracts, and quits to unemployment. We include workers who were born from 1968 onwards and were at least 15 years old in the year preceding job displacement and exclude both agency workers and those employed in the home care sector. With these selections, we limit the potential influence of early-retirement programs and focus on individuals who have a strong labor market attachment.

Workers are grouped into cohorts, with a cohort being defined by the month in which a job ends. Treatment is defined by the 2012 reform. We focus on 12 untreated and 12 treated cohorts. Treated cohorts consist of individuals who ended their job between July 2012 (when the Fornero reform was enacted) and June 2013, while untreated workers lost their job between July 2010 and June 2011. Defining the two windows over the same months (albeit in different years) is meant to limit the concern of different seasonality patterns before and after the reform. As seasonality is likely to be prominent in the case of temporary employment, this design minimizes compositional changes that may undermine the validity of the parallel trends assumption. We end up with a sample of 3,576,794 observations (1,324,037 treated and 2,252,757 untreated, respectively),

¹⁰Despite this last piece of information, we can determine neither union coverage nor union membership precisely, and thus cannot analyze this dimension of labor market duality. For a recent study on the complex relationship between collective bargaining agreements and employment protection regulation in Italy, see [Dustmann et al. \(2023\)](#).

¹¹Although there is no minimum wage in Italy, collective bargaining agreements set an earnings floor by industrial sector and occupation. We impute missing values (which affect only 3% of the observations in the sample) using such floors by year, sector, and occupation. The results do not change if such cases are dropped from estimation. For the sake of space, these results are not shown but are available from the authors.

comprising 76,102 workers (37% of whom are treated) and 142,909 firms (37% referring to workers in treated cohorts).¹²

For each worker in each cohort, we construct a balanced *event* study, with a time window of 13 months before the event and another time window of 33 months after, for a total of 47 months, including the month of the event, i.e., when the job ends.¹³ The first window gives us sufficient room to check for seasonality effects and common trends, even if we just invoke parallel trends at baseline. The second time window allows us to follow treated workers up to one year after the introduction of the 2015 Jobs Act.¹⁴

In this setup, the last treated cohort was exposed to the Fornero reform 12 months following its introduction (i.e., in June 2013), whereas the first untreated cohort experienced a job termination 24 months before the reform, in June 2010. The three-year gap between the two *most distant* cohorts and the 12-month gap between the two *closest* cohorts are arguably short, an explicit selection strategy to enhance comparability between the two groups, which broadly shared the same economic downturn affecting the Italian economy in those years, minimizing problems of differential business cycle effects and of possible violations of the parallel trends assumption. We shall come back to these issues in Section 5.

When comparing treated to untreated cohorts, the first 12 months post-event offer a clean evaluation of the impact of the Fornero reform. This is because there are 13 months between the last untreated cohort (defined by job loss in June 2011) and the first treated cohort (defined by job loss in July 2012). Thus, 12 months after job loss, the former is still untreated, and the interpretation of the difference in the estimates is straightforward, i.e., it captures the effect of the reform. Thirteen months after job loss, instead, the last untreated cohort also becomes exposed to the Fornero reform and our interpretation must change, as the difference in estimates between treated and untreated cohorts captures the immediate impact of the reform as opposed to its impact 13 months after displacement. Furthermore, following the most recent untreated cohort for 33 months after job termination in June 2011 (up to March 2014) means that no individual in untreated cohorts was affected by the 2015 Jobs Act. We can therefore cleanly evaluate whether the Jobs Act attenuated or magnified the changes set in place by the Fornero reform using the last 12 periods for the treated group.¹⁵

An important feature of our sample selection is the construction of the balanced event study,

¹²Individuals who terminate a job would typically find a new job in a different firm. This explains why we have more firms than workers.

¹³Figure A1 in Online Appendix A illustrates how each cohort contributes to the data over time.

¹⁴We have conducted robustness checks in which we reduce the post-job-loss window to 21 months so that the 2015 reform is not accounted for (see Online Appendix A). Although the shorter time window imposes a different sample selection because more workers can be observed for 35 consecutive months rather than 47, the estimates from this exercise do not differ from our main results. We take this evidence as an indication that our balanced event sample is unlikely to suffer from the potential impact of compositional issues.

¹⁵Two alternative selection criteria are arguably less satisfactory, and we thus do not adopt them. One is to backdate the time window for untreated cohorts well before July 2010–June 2011, keeping in mind that the CICO records are available only from January 2009 and therefore give us only limited room to pursue this selection. This is in part due to the 13-month window pre-event, but also because the farther away in the past one goes, the more issues of comparability across cohorts we have to be willing to face, with greater threats to the parallel trends condition. Another alternative is to follow untreated cohorts only for 11 months after displacement, up until June 2012, just before the enactment of the Fornero policy. This, however, implies that the post-separation period would be very short, giving us limited opportunities to learn about the dynamic impact of the reform.

where each worker in treated and untreated groups belongs only to one cohort. This is equivalent to assuming that the assignment to a cohort is an absorbing state. In doing so, we rule out that workers switch from one cohort to another if they experience multiple job separations over the observation period. Workers are assigned to cohorts according to the first observed job separation for which we have information over the relevant 47 months of coverage. For instance, an individual could lose her job in August 2012, find a new job two months later, and lose the new job again in December 2012. We assign this worker to the August 2012 cohort (and we must observe their working history from November 2011 to September 2015). The same criterion based on the first observed job loss over the CICO data applies to workers in untreated cohorts.¹⁶ They are assigned to this group if the first observed job termination is within the untreated window with the full coverage of the 47 months around it. If a worker holds multiple fixed-term jobs in a given month, we use the job that ends later. If a worker with multiple jobs in the same month has one that is open-ended, then we retain this job.

With this sample design, we limit the extent of anticipating job loss for both treated and untreated groups. If workers could anticipate the termination of their jobs and were able to find a new one immediately before the event, this job termination cannot be a focal event because, in the relevant month, we are recording the start of the new job rather than the end of the previous one. Moreover, pre-event terminations can only be to jobs that lasted less than 12 months, otherwise the recorded termination would be the focal event itself. These episodes, therefore, must come from either a temporary job or a permanent job in the probation period, which was equal to six months. The length of a temporary job is set ex-ante by employers and it is binding. As these features did not change with the reform, selecting a balanced event sample with 13 months before job loss limits the concern that the parallel trends assumption be violated by treated cohorts having pre-event periods *after* the passage of the reform.

It may be useful to place our sample selection strategy in the context of what other recent papers in the literature have done to address concerns of selective dismissals. Some use mass-layoffs (e.g., [Bertheau et al., 2023](#)), others firms' closures (e.g., [Athey et al., 2023](#); [Bardits et al., 2023](#)). They use matching techniques in pre-dismissal years to obtain appropriate counterfactuals and impose restrictions on workers' employment histories to ensure the parallel trends assumption holds. While these strategies work well in their specific contexts, in ours they do not seem to be appropriate. First, as our sample includes workers on temporary jobs, we cannot restrict the attention to individuals with long employment histories. Second, we do not have information on

¹⁶Multiple terminations are balanced between treated and untreated groups. For untreated cohorts, 31.8% have just one job loss (focal event), 67.8% have at most two episodes, while 88.6%, 97.3%, and 99.4% of the distribution have at most three, four, and five job terminations. The corresponding figures for treated cohorts are 31.7% (one focal event), 71.3% (two episodes), 90.1% (three), 97.6% (four), and 99.5% (five), respectively. Focusing on the pre-event periods, which may matter most for the credibility of the parallel trends assumption, 99.68% and 99.72% of the untreated and treated observations, respectively, have no job separations, suggesting a strong balance and indicating that pre-event terminations are negligible. The two groups are balanced also along other observables. For example, in terms of public-private sector composition, 9.63% and 9.60% of treated and untreated cohorts are employed in the public sector. As some workers experience periods of non-employment before securing the job that they will eventually lose in the focal event, we have also examined the balance of non-working months between treated and untreated cohorts in the pre-event period. The two distributions are quite similar, with 91% of treated workers and 90% of untreated workers having less than four non-working months.

the entire earnings history but only on earnings at hiring. Because we focus on the impact of the Fornero reform rather than on the cost of job loss, we are interested in the entire re-hiring market, and not just a segment of it. Without a large set of observable characteristics to match treated employees using long work trajectories, we then select our sample to have a similar composition between treated and untreated groups once we control for individual fixed effects, time fixed effects, and the size of each cohort within each group.¹⁷

In line with the [Sun and Abraham \(2021\)](#)’s approach, we do not condition on covariates.¹⁸ We ought to stress that, with treated and untreated cohorts exposed to nearly identical distributions of monthly job separations and net job flows, our results are unlikely to be driven by business cycle effects or excess labor supply in the post-reform re-hiring market.¹⁹ Additional descriptive statistics document a change in the reason of job termination after the reform. As expected, the fraction of individually laid-off workers increased. With a smaller share of quitters, firms may have adapted their behavior to a context where it is easier to settle disputes with permanent workers in case of a dismissal.²⁰

4 Research Design

The estimation strategy consists of four steps. In the first three steps, we modify the interaction-weighted (IW) estimator proposed by [Sun and Abraham \(2021\)](#) to estimate the impact of job loss for treated and untreated cohorts separately. In the last step, we calculate the difference between the two groups in the interaction-weighted (D-IW) estimator, which gives us an assessment of the impact of the Fornero reform.

One point of departure from the Sun-Abraham’s setup is worth re-emphasizing. To recover the cost of job loss, we do not use the last cohort to be treated as a control group. This is because we do not have a balanced panel setting where the yet-to-be-treated cohort is observed in the pre-event period only. We instead use an alternative counterfactual, i.e., being treated at a different time, through the comparison *before* and *after* the enactment of the Fornero reform in a balanced event sample.

We do not model job termination as an absorbing state. In fact, over the sample period, workers may lose their jobs more than once. Since our main aim is to focus on treatment effect dynamics and identify how the re-hiring market has changed after the 2012 reform, the inclusion

¹⁷For robustness, nevertheless, we will also use propensity score matching to pair treated workers to a sub-sample of untreated workers. We shall return to this analysis in subsection 5.3, but we can anticipate that its results are by and large in line with our main findings.

¹⁸Although their inclusion may complicate the interpretation of our 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 — an appropriate exercise in that framework — also does not change our main estimates. All these results are not reported for space concerns and are available upon request.

¹⁹Specifically, the difference between treated and untreated groups in the share of being in the top quartile of monthly job separation and that in net job flows are 0.0001 and 0.004, respectively (see Table A1 in Online Appendix A).

²⁰The proportion of observations linked to a quit for treated cohorts is about four percentage points lower than among untreated cohorts (25% and 29%, respectively). The fraction of observations linked to individual layoffs instead goes up by about five percentage points (24% versus 19%), while the share associated with a temporary job termination is roughly constant at approximately 18% for both groups.

of all observed job losses in the 47-month window (before and after the major event which defines a worker’s treatment status) is key. To avoid confusion, the assignment to a cohort (i.e., when job loss occurs) is absorbing, but — once the major event is determined — workers can experience multiple job separations.

Step 1 — In the first step, we estimate the cohort-specific average treatment effect on the treated (CATT) relative to having terminated the job in a given pre- and post-reform period. Specifically, we estimate a linear two-way fixed effects model that interacts relative period indicators with cohort indicators for untreated and treated workers separately:

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

where i indicates 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 when worker i experiences job termination which assigns the worker to group $j \in \{\mathcal{T}, \mathcal{U}\}$, while D_{ijt}^ℓ is an indicator for worker i in group j being ℓ periods away from job loss at calendar time t . The terms α_i and λ_t are worker and time fixed effects, respectively, and ϵ_{ijt} is the error term. Each coefficient δ_m^ℓ corresponds to the cohort-specific average treatment effect on the treated that has a difference-in-differences (DiD) interpretation relative to one month before job termination ($\ell = -1$ being normalized to 0) due to its staggered structure (Goodman-Bacon, 2021). Finally, y denotes our labor market outcomes, that is, hiring earnings, the probability of being in a job, and the probability of being in a job with an open-ended contract.

Step 2 — Here we calculate sample weights given by $Pr\{M_{ij} = m \mid M_{ij} \in \{\mathcal{T}, \mathcal{U}\}\}$, i.e., the sample analogue of the share of each cohort in the relevant periods defined by set \mathcal{T} or set \mathcal{U} . In our analysis, all cohorts are characterized by 47 ℓ periods (months), $\{-13, \dots, -1, 0, 1, \dots, 33\}$, and a given cohort receives the same weight in each of such periods. Weights are non-negative and sum up to 1 in either set \mathcal{T} or set \mathcal{U} .

Step 3 — To obtain the interaction-weighted (IW) estimator, we take a weighted average of the CATT estimates for each group j in regression (1) with the weights computed in the second step. The IW estimator is then given by

$$\hat{\nu}_j = \sum_\ell \sum_m \widehat{\delta}_m^\ell \widehat{Pr}\{M_{ij} = m \mid M_{ij} \in \{\mathcal{T}, \mathcal{U}\}\}, \quad (2)$$

where each $\widehat{\delta}_m^\ell$ comes from equation (1) and $\widehat{Pr}\{E_{ij} = e \mid E_{ij} \in \{\mathcal{T}, \mathcal{U}\}\}$ from the second step.²¹ As in Sun and Abraham (2021), we construct pointwise confidence intervals which are valid

²¹In the empirical analysis reported in the next section, for each group j , we will show 11 estimates pre-reform $\widehat{\nu}_{-12}, \dots, \widehat{\nu}_{-2}$, normalizing $\nu_{-1} = 0$ and accounting for the intercept term, and 34 estimates from the reform month to the end of the observation period for each cohort, i.e., $\widehat{\nu}_0, \widehat{\nu}_1, \dots, \widehat{\nu}_{33}$. Normalizing relative to the period before the event is common practice, whereas excluding the earliest period before the event avoids potential issues of multicollinearity while allowing for a constant term in regression equation (1).

for each IW estimator $\hat{\nu}_j$, without relying on bootstrapping at this stage as, for instance, in [de Chaisemartin and D’Haultfoeuille \(2020\)](#).

Step 4 — To obtain an assessment of the impact of the Fornero reform on the cost of job loss, we compute the difference between the IW estimates for the treated cohorts, $\hat{\nu}_{\mathcal{T}}$, and the IW estimates for the untreated cohorts, $\hat{\nu}_{\mathcal{U}}$. That is,

$$\Delta\hat{\nu} = \hat{\nu}_{\mathcal{T}} - \hat{\nu}_{\mathcal{U}}. \quad (3)$$

We refer to expression (3) as the difference-in-interaction-weighted (D-IW) estimator. This measures the average treatment effect for workers who experienced a job loss just after the introduction of the 2012 reform compared to that of workers who faced a job termination before the same reform. Our D-IW estimator, therefore, has the same interpretation as a stacked DiD and is robust to heterogeneous treatment effects. For statistical inference on $\Delta\hat{\nu}$, we use clustered bootstrapped standard errors.

A few clarifications are in order. Given our sample selection, we recover the clean impact of the Fornero reform on treated cohorts over the first 12 months post job loss. As the IW estimator measures the cumulative effect of job loss up to a given period, after 12 months the $\Delta\hat{\nu}$ measures the difference between being fully impacted by the reform (treated cohorts) with the impact of being exposed to it at least after 12 months from job termination (untreated cohorts). Notice that untreated workers may not necessarily be exposed to the 2012 policy for two reasons. First, they will be impacted by the reform only if they are still in the re-hiring market when the policy kicks in. Second, for workers in \mathcal{U} , each CATT estimate, $\widehat{\delta}_m^\ell$, depends on the potential exposure to the reform in a staggered manner, with the oldest cohort (m =July 2010) being exposed after 24 months (ℓ =24) and the youngest (m =June 2011) after 13 months (ℓ =13) of its implementation. The cost of job loss faced by individuals in \mathcal{U} and \mathcal{T} could then become more similar 12 months after displacement than before. Finally, after 21 months post-separation when the 2015 Jobs Act was enacted, the D-IW approach pins down the combined impact of the two policy interventions for individuals in \mathcal{T} as opposed to the effect of only the 2012 reform for individuals in \mathcal{U} .²²

Our D-IW estimator invokes parallel trends between treated and untreated cohorts at baseline and *not* in all the pre-event periods. Although the assignment to job termination cohorts may not be random, the parallel trends assumption holds as long as the composition of the re-hiring market at the time of the event is similar before and after the reform. This requires the mean of time-varying unobservables conditional on worker fixed effects to be unchanged over time ([Ghanem et al., 2023](#)). In our application, this condition is plausible on four grounds. First, we examine all reasons for job termination and not just layoffs. This is likely to give us a more complete picture of the re-hiring market, which is expected to vary smoothly over the sample period, rather than one of its segments, which can vary sharply over time. Second, we have a balanced event study such that each cohort in the \mathcal{T} and \mathcal{U} groups contributes to all event

²²For a discussion of the interpretation of estimates when there are multiple treatments, see [de Chaisemartin and D’Haultfoeuille \(2023a,b\)](#).

periods. As a result, differences in cohort composition cannot matter more in some periods than others. Third, because sample shares are fixed for each cohort in the balanced event study, our weights are time-invariant. Finally, by design, all workers must have a job in the month before losing it. These features make the parallel trends assumption at baseline reasonable and allow for a meaningful comparison of the after-job-loss periods to a period where all individuals were working.

5 Main Results

5.1 Baseline Estimates

Figure 2 displays our baseline results. The top three panels show the interaction-weighted estimates of job loss for treated and untreated cohorts separately, $\hat{\nu}_{\mathcal{T}}$ and $\hat{\nu}_{\mathcal{U}}$, for our three outcomes, i.e., monthly hiring earnings (panel (a)), the probability of being in a job (panel (b)), and the probability of being in a job with an open-ended contract (panel (c)), along with the 95% confidence bands. For the same three outcomes, the bottom three panels (d)–(f) report the D-IW estimates, $\Delta\hat{\nu}$, with their corresponding 95% confidence intervals. The three vertical lines in each plot highlight the event timeline and relevant institutional changes. In particular, the first (dashed) line on the left indicates the month before job termination. The second (light dashed) line is at 12 months from the major event. From the following month, the most recent untreated cohort is exposed to the Fornero reform for the first time. The third (dashed) line on the right is drawn at 21 months from job loss, when the most recent treated cohort is exposed to the 2015 Jobs Act.

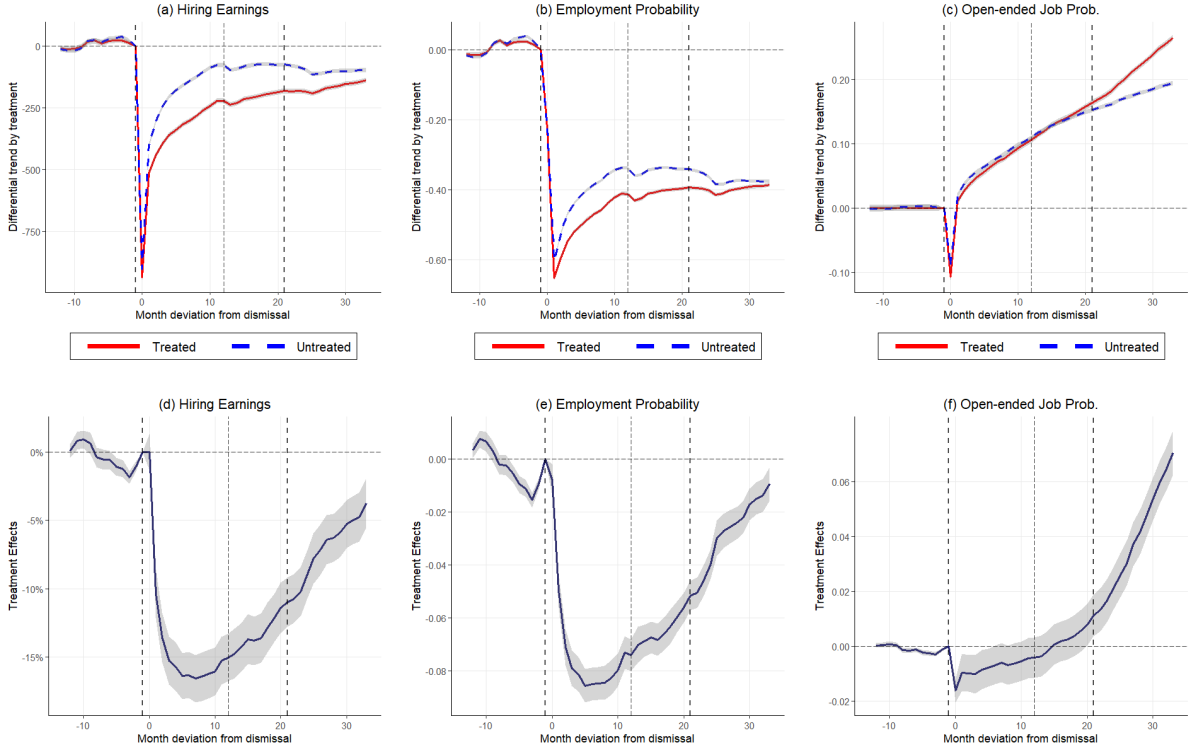
For each outcome, the D-IW estimates are close to zero in the months before job loss to the left of the first dashed line, except for a few instances in the case of hiring earnings and the probability of working (see panels (d) and (e), respectively). Such deviations from zero are almost entirely due to the fall in employment probability, which is in turn driven by differing lengths of short-term contracts held by workers in untreated cohorts. When we consider the same outcomes for the subsample of individuals with positive earnings, all discrepancies disappear (see Online Appendix B).

The other pre-job-loss estimates are indistinguishable from zero. Although we do not interpret this as unreserved evidence of parallel trends at baseline, we argue that the violation of this condition is weak for two reasons, which we already discussed in the previous section but are worth repeating. First, by construction, both treated and untreated groups are employed at baseline, $\ell = -1$.²³ In line with Callaway and Sant’Anna (2021) and Sun and Abraham (2021), this means that all periods *prior* to $t-1$ do not contribute to the identification of the counterfactual trends (see also Roth et al., 2023). Second, fixed-term job duration, which contribute to the pre-event deviations from zero in the D-IW estimates of Figure 2, is set by firms, not by workers. This explains the inclusion of temporary workers in our sample, whose turnover behavior could

²³We cannot impose this condition for more months before job loss because the estimating sample comprises all workers in the re-hiring market, and not just those with more stable labor market attachment.

not be driven by anticipation of the 2012 reform.²⁴

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 (with 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.

In panel (a) of Figure 2, we plot the earnings cost of job termination in levels, so that we can identify the magnitude of the impact. To facilitate comparison with other studies, we normalise earnings in each month after job loss relative to average earnings in $\ell = -1$, approximately €900 for untreated workers. For individuals in untreated cohorts, the earnings cost is estimated to be 45% of baseline earnings in the month after job termination. Although hiring earnings quickly bounced back and recovered much of the lost ground after 12 months, they never reached pre-dismissal levels again. From the twelfth month following job termination and over the next 21 months until the end of the analysis period, the earnings drop for untreated workers remained stable at about €80, or 9% of baseline earnings.²⁵

²⁴Of course, it is always possible that workers have private information about job terminations and adjust their behavior before the actual event. As noted by Callaway and Sant’Anna (2021), however, the violation of no anticipatory behavior is likely to be limited when the reform path is not known a priori or when workers do not choose when to terminate the job. As mentioned already, if workers anticipate the possibility of a job loss and find another job immediately before the event, they cannot be part of the cohort by design. Moreover, even if this anticipatory behavior exists but it is similar before and after the 2012 reform, it does not pose a threat to the parallel trend assumption at baseline across treatment groups, which is what matters in our analysis.

²⁵As a benchmarking exercise, one year after termination, $\hat{\nu}_U$ 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. (2023) for Sweden (24%). Our results, however, cannot be directly compared with such studies, as these focus on

More relevant to our research questions, Figure 2 provides clear evidence of new conditions in the re-hiring market, with treated workers’ earnings being more negatively affected as a result of the Fornero reform. A month after job loss, treated workers’ earnings were about 8% significantly lower than those of untreated workers (panel (d)). Their relative hiring earnings continued falling until the eighth month after dismissal when their wage penalty was around 17%. Weakening employment protection to permanent workers thus led to a substantial deterioration of normalized hiring earnings by an additional 26% (i.e., 17 extra points to be added to the 9% loss faced by untreated individuals). From the ninth month after job loss onward, treated workers’ earnings started to improve but never caught up with the earnings of untreated workers over the 33-month horizon under analysis. At that point, the penalty was 3%. A possible explanation for this partial recovery is due to the impact of the Fornero reform on *untreated* cohorts starting 12 months after job loss. The 2015 reform, whose effects emerged from 21 months onward, could have contributed further to reducing the gap.²⁶

A similar picture emerges in the case of re-employment, shown in panels (b) and (e). The negative re-employment gap faced by treated cohorts was substantial soon after job loss, going from five percentage points the month after termination (when the IW estimates reveal a 60% and 65% lower probability of being in a job for untreated and treated workers, respectively) to about eight percentage points five to ten months following job loss (panel (e)). After one year, \mathcal{T} -workers had about a 40% lower probability of being employed, while \mathcal{U} -workers just over 32%. This latter estimate echoes the results shown in Bertheau et al. (2023). As the 2012 reform started to affect untreated cohorts, \mathcal{T} -workers faced marginally better chances of re-employment, although they still suffered a considerable five percentage point penalty after 21 months (with $\hat{\nu}_{\mathcal{T}} = -0.39$ and $\hat{\nu}_{\mathcal{U}} = -0.34$ in panel (b)). The 2015 reform had an additional positive impact on the re-employment probability of treated cohorts. The gap narrowed to one percentage point in the last month under analysis, even though it remained statistically significant, while the underlying IW estimates continue to be large and stable for both groups, at about 40%.

Although the Fornero reform intended to encourage permanent contracts as the main pathway into the re-hiring market, the results in panel (f) of Figure 2 do not show support in favor of this aim. Treated workers had approximately one percentage point *lower* probability to re-enter the labor market with an open-ended contract than their untreated counterparts for about 8 to 9 months after job loss (with the IW estimates in panel (c) going from two to eight percentage points, and the corresponding estimates for untreated cohorts being 1 percentage point above).²⁷ For an additional year after that point, when untreated cohorts were affected by the Fornero

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 lower earnings on average than those who were mass laid off from a permanent job. Our estimates are instead comparable to those reported in Krolkowski (2018) and Schmieder et al. (2023).

²⁶This pattern is largely driven by the effects on the re-employment probability, which we discuss next. In Online Appendix B, we replicate the analysis on the sub-sample of individuals who are employed. In that case, the wage penalty for treated cohorts is nearly constant over time.

²⁷Even before the 2012 reform, a non-negligible fraction of workers in the re-hiring market were able to switch from fixed-term to permanent contracts. This was the case, for example, when firms used fixed-term jobs as a screening device to hire workers on a permanent basis. Exploring the reasons behind this switch is interesting but goes beyond the scope of our work.

reform, \mathcal{T} -workers faced equal (but not better) chances. It is only 20 months following job loss, just one month after the introduction of the hiring incentives and one month before the implementation of the 2015 Jobs Act affecting only treated cohorts, that we find a significantly positive treatment effect on the likelihood of gaining a permanent job. This increased from about one percentage point around the 2015 reform to approximately seven percentage points at the end of the observation period.

Discussion — We now provide a simple economic interpretation of these results. We find that limiting the possibility of workplace reinstatement lowered re-hiring earnings and dampened re-employment chances for treated cohorts. These penalties ensued from an already significant cost of job loss pre-reform, when employment protection was higher for workers in open-ended contracts. Weakening employment protection, therefore, did not deliver an abatement in labor market frictions with greater permanent job creation and lower earnings losses after displacement, as one would expect from standard theory.

Gibbons (1992)’s hybrid equilibrium model offers useful insights into how firms’ re-hiring practices might have changed in response to the 2012 reform. In a nutshell, the reform led employers to believe that, besides the usual fraction of low-ability workers whom they kept offering low wages, they faced a larger share of low-skill workers who could not be separated out from other (high-ability) workers in the secondary market. This meant that all individuals in this latter group received lower wage offers than pre-reform. More details on the model are available in Online Appendix B. Here, we focus on its main implications.

Assume the following hiring strategy be in place before the Fornero reform. A firm chooses a wage-effort package, $\{w, e\}$. Effort e is costly to workers, who are either of type H or of type L , where H and L refer respectively to high and low ability (or skill), and low-ability workers are assumed to have steeper indifference curves. Let ϕ (and $1-\phi$) indicate the share of H - (and L)-type workers in the whole re-hiring market. The firm has imperfect information about workers’ skills. If it infers that the worker is an L -type, it offers a low equilibrium wage-effort package, $\{w^L, e^L\}$. Suppose an unknown share ψ of the *remaining* L -type workers choose effort level $e_p^b (> e^L)$, which is also the effort level chosen by all H -type workers. Unable to separate the two groups, the firm offers them the following pooling hybrid equilibrium wage:

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

where $y(\cdot, e)$ is the pre-reform productivity profile which 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

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

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 π must equal q in equilibrium, rearranging expression (5) yields

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

Now, suppose the Fornero reform, labelled R , is introduced. This could affect both ϕ (through a compositional change in H - and L -type workers in the entire re-hiring market) and q (through a change in the actual share of high-skill workers in the pooling equilibrium). The impact of R on the probability that a low-skill worker chooses an effort level consistent with the pooling hybrid wage is then ambiguous, as shown in the following expression:

$$\frac{\partial \psi}{\partial R} = \underbrace{\frac{\partial \psi}{\partial \phi} \frac{d\phi}{dR}}_{(+)(-)} + \underbrace{\frac{\partial \psi}{\partial q} \frac{dq}{dR}}_{(-)(-)} \quad (6)$$

The estimates we found in the empirical analysis above imply a positive sign of (6). If ψ increases (because, for example, after the reform more workers were made redundant rather than induced to quit), the employer’s belief to meet a high-ability worker in the pooling hybrid equilibrium declines, and thus companies offer a lower post-reform wage. In the other (low) equilibrium, the wage offer remains unaffected. This informational friction story depends only on the fraction ψ of low-skill workers in the pooling equilibrium and does not require us to observe more individuals in the re-hiring market after the reform. To be consistent with our findings, it also implies that compositional changes (i.e., the first term in expression (6)) be either zero or small. If this is the case, the parallel trend assumption is also unlikely to be violated. As previously mentioned, our data indicate a shift in the recorded reasons behind separations, with a decrease in the proportion of quits and a nearly equivalent increase in the share of individual layoffs following the reform.

5.2 Distinguishing Laid-Off from Other Workers

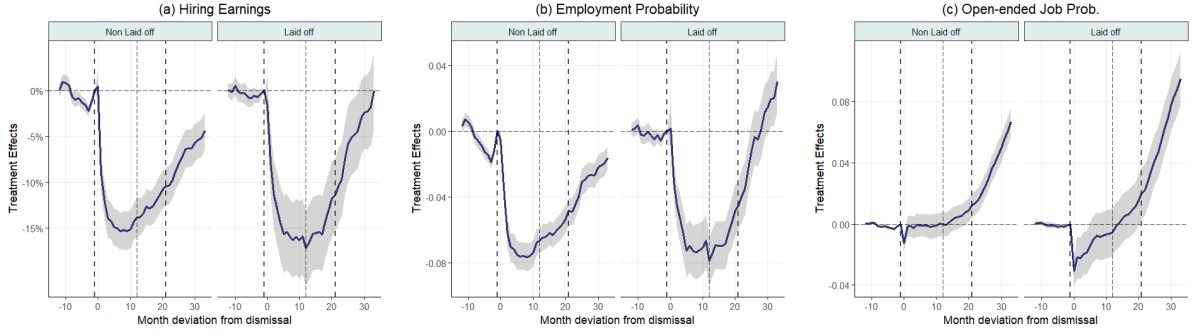
We have argued that the conditions in the re-hiring market worsened after the Fornero reform. We now provide further evidence for this claim by repeating our previous analysis while distinguishing between jobs that ended with a dismissal and jobs that did not.

Figure 3 displays the D-IW estimates, $\Delta \hat{\nu}$, for the three outcomes of interest by layoff status.²⁸ The underlying IW estimates are reported in Online Appendix B. We reiterate that the small deviations from zero in the pre-job-loss periods are likely due to different lengths of short-term jobs for untreated cohorts, as these deviations do not emerge for those who were individually laid off from permanent jobs. Between three and 12 months after job loss, \mathcal{T} -workers who were laid off faced a 13–17% earnings penalty compared to \mathcal{U} -workers (see the graph on the right in panel (a)). The other treated workers, who either quit, terminated a fixed-term job, or were mass laid off, incurred an earnings penalty of approximately 14% over the same period (as reported on the

²⁸To avoid the contamination problems discussed by [de Chaisemartin and D’Haultfoeuille \(2023b\)](#), each effect is estimated separately for each type of job separation.

left). The difference of at most 3% between the two groups is in general not statistically different from zero. The 2012 intervention thus had a spillover effect across the whole re-hiring market, making it difficult for *all* workers, and not just those directly targeted, to regain their earning power after dismissal.²⁹

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 (with 100 replications). For other details, see the notes to Figure 2.

It is only after the 2015 Jobs Act that laid-off treated workers' earnings regained ground, and by the end of the sample period, they fully caught up with their pre-2012 counterparts. The earnings of workers who were not laid off, instead, never recovered over the 33-month horizon and were still 4% significantly lower at the end of the period under analysis. One possible explanation for the steep pay recovery of individually laid-off \mathcal{T} -workers is that, prior to termination, they were in permanent jobs, whereas the large majority of the other workers might have completed fixed-term contracts or voluntarily left their jobs.³⁰ The open-ended nature of the previous contract could have provided a more favorable signal to employers recruiting in the secondary market.

In the case of the likelihood of being in a job (panel (b)), the differences between dismissed workers and other workers were always small and statistically indistinguishable from zero up to two years after job termination, when treated cohorts started to get exposed to the 2015 Jobs Act. From that point onwards, laid-off workers experienced a *greater* probability of re-employment, enjoying a premium of about 2–3 percentage points in the last three months of the observation period, 31 to 33 months after dismissal. Non-dismissed workers, instead, faced lower chances of employment post-termination throughout the period with a penalty at the end of the time window of 2 percentage points. The differential between those two groups of approximately 3–5 percentage points is statistically different from zero at conventional levels over the last 12 months of the sample.

²⁹If we condition on individuals who were employed (see panel (d) in Appendix Figure B1), we find that laid-off workers suffered a statistically different from zero earnings penalty of about 5% upon job loss and throughout the sample period. Combining this result with the evidence in Figure 3 suggests that the impact on earnings is largely driven by the re-employment margin.

³⁰About 2% of terminations, recorded as collective dismissals, were due to mass layoffs for *both* treated and untreated cohorts.

Finally, panel (c) of Figure 3 documents that the Fornero reform did not help laid-off individuals find an open-ended contract immediately after dismissal. Up to six months after job loss, these workers were about 2–3 percentage points less likely to find a job with an open-ended contract than their untreated counterparts (see the plot on the right of the panel). From the sixth to the nineteenth month after the job termination event (which corresponds to the month when the hiring incentives were introduced and two months before the Jobs Act affected \mathcal{T} -workers), the laid-off workers’ likelihood of getting a permanent job was indistinguishable from that of their untreated dismissed counterparts and similar to that of other (non-dismissed) workers, shown in the graph on the left of panel (c). From the twentieth to the thirty-third month, both groups of treated workers saw their chances of getting a permanent job rising quite substantially, from 1 to 9 percentage points for laid-off workers and from 1 to almost 7 percentage points for non-dismissed workers. Both effects are likely to be driven by the 2015 hiring incentives.

Laid-off workers’ outcomes, therefore, were comparable to those of other workers up to two years after job loss. From that point onwards, their labor market position improved compared to their non-dismissed counterparts. They were more likely to be employed and to have a job with an open-ended contract. Since layoffs were concentrated among individuals with permanent (possibly higher-quality) jobs, after the 2012 reform the informational content of dismissals has arguably changed in favor of dismissed workers, potentially exacerbating the “insider-outsider” gap in the secondary market (e.g., [Bentolila and Dolado, 1994](#); [Daruich et al., 2023](#)). This, however, is apparent only two years after the introduction of the Fornero reform, when our treatment cohorts became exposed to generous hiring subsidies for jobs opened on a permanent basis and rising severance payments with tenure.

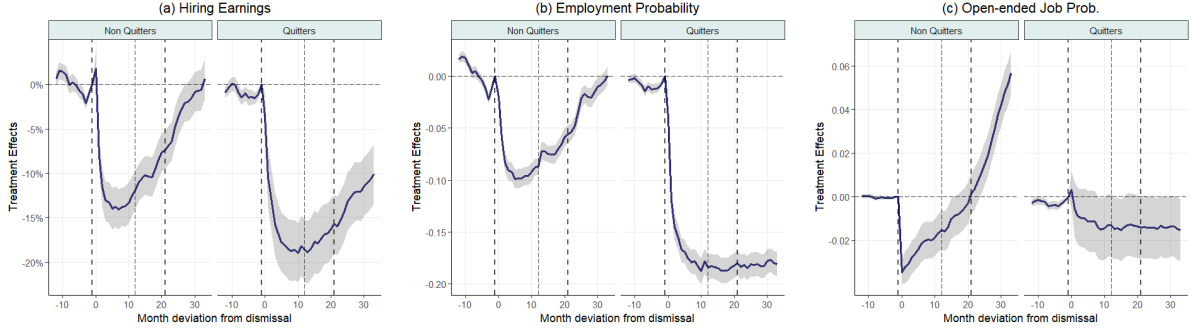
So far we have examined all job terminations, including quits to unemployment. As underlined in earlier sections, this provides us with a comprehensive picture of the re-hiring market. In the presence of strong employment protection legislation, as emphasized by [Di Addario et al. \(2023\)](#) for the case of Italy, firms can “bribe” their employees to quit to unemployment to avoid high firing costs (on this issue, see also [Postel-Vinay and Turon, 2014](#)). This form of job loss is arguably close to a conventional layoff and makes the distinction between quits and layoffs rather opaque. Nevertheless, we cannot rule out that even this type of quits may have a voluntary (worker initiated) component. We thus perform additional analyses distinguishing workers who quit from all the other workers.

The estimates in Figure 4 document that, when we focus only on non-quitters, we find similar results to those reported in Figure 2.³¹ To the extent that the 2012 intervention made employers believe in heightened adverse selection in the secondary market, the conditions for all workers in that market deteriorated, not just for those who were displaced. In fact, following job separation, quitters suffered significantly greater earnings penalties, lower re-employment probabilities, and, 21 months after separation, smaller chances of being on open-ended contracts compared to laid-off

³¹Interestingly, [Burdett et al. \(2020\)](#) show that the estimates found on all separations are similar to those obtained when they restrict their focus only on mass layoffs. See their footnote 18 (p. 1778) and their Appendix B.3. A similar finding is also reported by [Jarosch \(2023\)](#). These results give a strong benchmarking and further credibility to our estimates.

individuals and employees who ended a fixed-term contract. Workers voluntarily quitting their jobs, therefore, seemed to have found it difficult to integrate back into the active workforce.³²

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 (with 100 replications). For other details, see the notes to Figure 2.

Summary — Job loss is invariably associated with long-lasting individual economic costs, in terms of lower probabilities of finding a new job, being re-employed on an open-ended contract, and sustaining substantially lower earnings at hiring in the secondary market.

The erosion of employment protection implied by the Fornero reform elevated workers' labor market costs further. But it was non-dismissed workers (mainly quitters to non-employment) who suffered the most from job loss. Compared to them, in fact, laid-off treated workers experienced a lower earnings penalty and a more modest re-employment disadvantage. Following the insights from the hybrid equilibrium model sketched above, we interpret this result as evidence that the reform changed the informational content of dismissals, with firms believing that the average value of workers' skills in the pooling equilibrium worsened after the reform. As most of the laid-off treated workers were in permanent jobs before being dismissed, employers might have perceived this as a desirable signal of the workers' match value. Although foregone human capital accumulation is likely to remain a central factor determining the size of earnings losses among displaced workers (e.g., [Topel, 1990](#); [Burdett et al., 2020](#)), our results point to the key role played by informational frictions, with quits and terminations of temporary jobs that were not necessarily followed by better-paid employment.

5.3 Robustness

It is important to corroborate our main estimates with a series of sensitivity checks. We perform three exercises. In the first, we evaluate the credibility of the parallel trends assumption using the synthetic difference-in-differences (SDiD) estimator proposed by [Arkhangelsky et al. \(2021\)](#) in a stacked design. In the second test, we verify whether our results depend on the balanced

³²On this issue, see also the additional estimates reported in Online Appendix B.

event structure and re-fit the model adapting the D-IW estimator to a balanced panel setup. The third exercise assesses the extent to which business cycle effects influence our baseline results.

The first check allows us to deal with cases for which job termination may not be random either across individuals or periods, and where unconfoundedness could be violated. We use this approach to probe the parallel trend condition. The SDiD estimator assigns greater weight to: (i) \mathcal{U} -workers who are, on average, more similar to the targeted \mathcal{T} -workers in terms of observed characteristics; and (ii) periods that are, on average, more similar to the targeted treated periods. This setting speaks also to our third exercise, because concerns that the results might be driven by time effects rather than the reform itself are limited by design here, with periods being made as comparable as possible across treated and untreated groups. Since treatment is defined by the Fornero reform, the SDiD method does not account for staggered entry into month-of-job-loss within each group, whereas our D-IW estimator does. Furthermore, the use of weights makes this alternative approach ‘local’, as the estimates do not reflect before-and-after differences in outcomes for all treated and untreated workers. Some untreated workers, in fact, contribute more if they are assigned higher weights, while others may not contribute at all. In our method, instead, although we do not use all pre-event observations for the parallel trends assumption — only those at baseline — we include all treated and untreated workers with weights that are time-invariant and equal to the cohort share in the relevant treatment group.³³

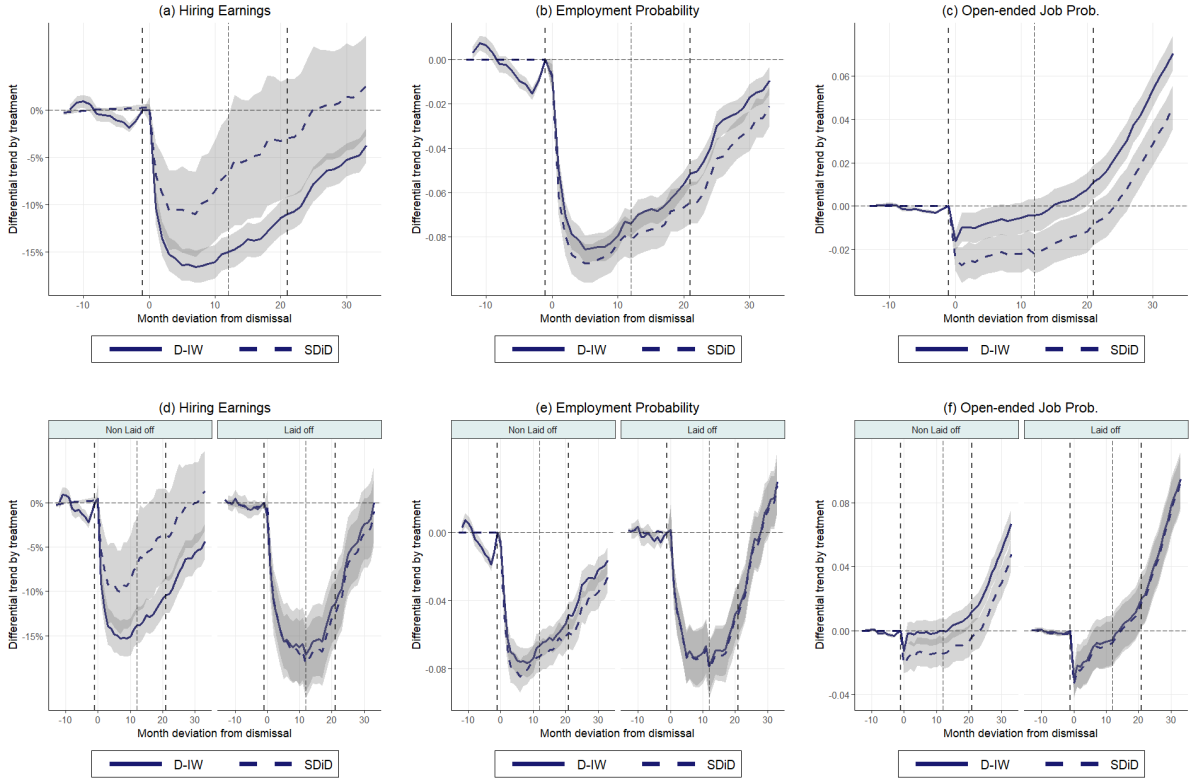
Figure 5 presents the SDiD results. To ease their appraisal, we also report our D-IW estimates from panels (d)–(f) of Figure 2 and those from Figure 3. In panel (a) of Figure 5, the SDiD impacts on earnings are smaller (in absolute value) than, but almost always statistically indistinguishable from, their D-IW counterparts. They are instead slightly larger (again in absolute value) for re-employment probabilities in panel (b), and more negative (and less positive) for permanent re-employment probabilities in panel (c). The differences in results between the two sets of estimators seem to be driven mainly by workers who were not laid off, who possibly had shorter job tenure (see the plots on the left in each panel (d)–(f) of Figure 5). Even in such cases, however, the picture that emerges from the SDiD results is very similar to that shown by our D-IW benchmark.

The findings from the second sensitivity check are in Online Appendix B, where we report the results found with two estimators robust to heterogeneous treatment effects in a balanced *panel* setting (rather than a balanced event setting like ours), following the approaches suggested by Callaway and Sant’Anna (2021) and Sun and Abraham (2021) more closely. We also report the estimates obtained from stacked DiD regressions (e.g., Cengiz et al., 2019).³⁴ Albeit slightly (and often insignificantly) smaller, all these alternative estimates strongly uphold our benchmark D-IW results. Despite these similarities, we nonetheless argue that the balanced event framework is preferable in our context, because (i) it allows for a longer post-event analysis; and (ii) differences in cohort composition cannot play a role with time-invariant weights for each cohort.

³³An additional exercise involves the use of a matched difference-in-differences approach (using propensity scores). The findings from this exercise are consistent with our results and are reported in Online Appendix B.

³⁴Results from a de Chaisemartin and D’Haultfoeuille (2020)-type estimator are not shown for the sake of brevity, but confirm the other estimates. They are available from the authors.

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.

The third check is motivated by the potential threat induced by time (business cycle) effects. The validity of the parallel trends assumption hinges on the condition that the mean of time-varying unobservables, conditional on worker fixed effects, be constant over time. In the presence of differing business cycle impacts, this condition may not hold true. To understand this issue better, it is useful to dig deeper into the economic situation in Italy at the time of the Fornero reform (see also the discussion in Section 2). Italy experienced a double-dip recession, with the second downturn precipitated by a debt crisis. In the spring of 2011, concerns about contagion from the Greek bailout negotiations and lacklustre stress tests by the European Banking Authority heightened anxieties about spillovers to other nations. These were compounded by the May 2011 S&P’s announcement of a negative outlook on Italy’s credit rating. To avert a full-blown debt crisis, confidence on the incumbent Berlusconi government was withdrawn in November 2011, when the Monti cabinet was appointed.

In our analysis, \mathcal{U} -cohorts lost their jobs under the Berlusconi government amid mounting economic uncertainty and pressure on firms. In contrast, \mathcal{T} -cohorts experienced a job loss under the Monti government, during which there was still a growing debt but lower uncertainty regarding the official commitment to implementing the necessary reforms to secure financial assistance from international institutions. It may be hard, therefore, to determine whether differences in re-hiring probabilities between treated and untreated cohorts were affected differently by different fluctuations in the business cycle.

As discussed above, suggestive evidence indicates that, in terms of overall monthly hires, separations, and net job flows (calculated as hires minus separations), the two treatment groups show no statistically significant differences (see Table A1 in the Online Appendix A). Moreover, our methodology mitigates business cycle effects by construction, because each period’s estimate is a weighted moving average of the coefficients estimated over a 12-month window, with the weights given by the cohort share in each treatment group.

We can nonetheless bound the role played by time effects under a conservative hypothesis. Unlike other papers in the literature, in our setup, workers lose their jobs between $\ell = -1$ and $\ell = 0$, but they can be in any labor market state before the main event. If there were differential business cycle effects, we expect them to show up at any time, including the pre-event window. Figure 2 reveals small deviations from zero over that window, with the largest (negative) discrepancy of 0.015 (s.e.=0.003) at $\ell = -3$ in the case of the re-employment probability. We have argued how these deviations may arise from different lengths of temporary jobs, which are likely to be decided by employers rather than workers. Leaving these considerations aside, let us assume that our estimates are entirely driven by time effects, and subtract 0.015 uniformly from all post-job-loss coefficients. Although the re-employment penalty induced by the Fornero reform is lower by definition, it is still economically substantial (between 6 and 8 percentage points) and statistically significant in the first 12 months after job loss, when we can recover a clean impact of the policy intervention.³⁵ Interestingly, the spirit of this exercise is also in keeping with the [Rambachan and Roth \(2023\)](#)’s “honest” DiD approach, the results of which are in Online Appendix B.

6 Heterogeneity and Channels

Having analyzed the main effects of the introduction of the Fornero reform on earnings and employment in the re-hiring market, this section quantifies whether the impacts differ by worker’s gender, education, and age and by workplace geographic location, industry mobility, and part-time versus full-time employment. Each of these characteristics is important not only to uncover salient dimensions of heterogeneity but also to assess whether the additional penalties (or premia) could be driven by a loss in human capital accumulation or a change in firms’ re-hiring strategies.

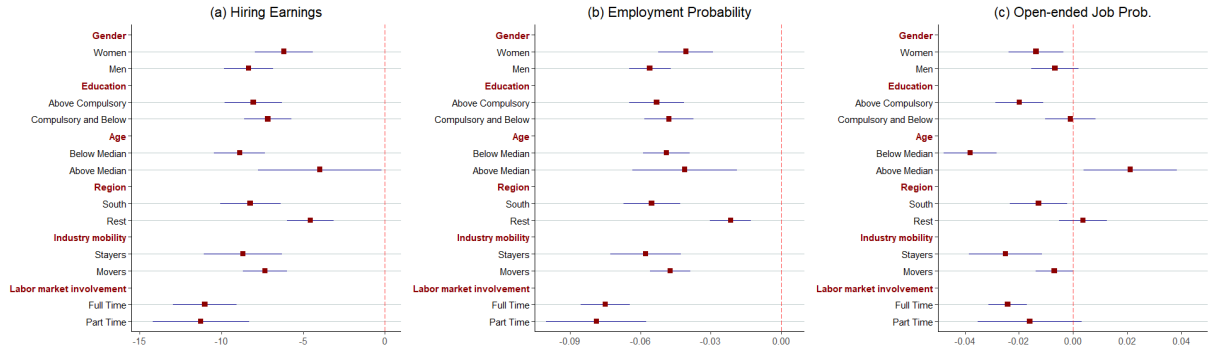
To provide a comprehensive (and yet succinct) picture of the dynamic impact of the reform, we present results at three different time points from the job loss event, i.e., after one month, after 12 months, and after 33 months. The first two periods allow us to see the impact of the 2012 reform, while the latter gives evidence of the cumulative effects of both the Fornero reform and the Jobs Act.³⁶

³⁵Similar considerations apply to the analysis by industry and firm location (see Online Appendix B). This additional evidence is of relevance for at least two reasons. First, it shows different effects across industries in the *same* geographic location, a result that is unlikely to be driven by an aggregate time shock which is expected to affect different industries fairly uniformly. Second, it indicates that, in sectors where workers are highly skilled — such as the professional activities sector — the impact of the Fornero reform on earnings is *positive* after 12 months. This result is consistent with the notion that, in this specific case, the sign of equation (6) is determined by a larger share of low-skill workers who get separated from their high-skill counterparts in the post-reform low equilibrium.

³⁶The underlying D-IW estimates are in Online Appendix C. The IW estimates instead are not reported for space concerns but are available from the authors.

One month from job loss — Figure 6 summarizes the results. We cannot detect gender differentials, with both men and women facing similar penalties across all three outcomes. Proxying skills with educational qualifications, we stratify workers into low- and high-education groups. The former comprises individuals with at most lower secondary school qualifications; the latter is defined by individuals with upper secondary or higher qualifications, including college and university degrees. Although earnings and re-employment chances worsened for both skill groups, high-education individuals faced a significantly lower probability of being re-employed in a job with an open-ended contract (panel (c)). Since it is hard to reconcile this result with a simple human capital story, our interpretation is that employers changed their re-hiring practices, as they might have expected to face a larger share of low-skill workers in the pooling equilibrium after the Fornero reform. Being fired may be a worse signal for highly educated workers.

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 (with 100 replications). The vertical dashed red line is drawn at zero, indicating the Fornero reform has no effect. Skills are proxied with educational qualifications. The low-education category comprises individuals with at most lower secondary school qualifications; the high-education group is defined by individuals with upper secondary or higher qualifications, including college and university degrees. The regions in the South are Sicily, Calabria, Apulia, Basilicata, Campania, Molise, and Sardinia. Industry stayers and movers are defined on a 12-industry categorization of five-digit industry information.

One of the aims of the reform was to reduce labor market duality, a persistent feature of the Italian labor market characterized by a pronounced generational divide whereby young workers have been usually seen as outsiders (e.g., [Daruich et al., 2023](#); [Bianchi and Paradisi, 2022](#)). Albeit better educated on average, they may lack the work experience needed in the re-hiring market, in particular in permanent jobs. To ascertain the role played by age, we divide the sample into two groups, workers below and workers above the median age, or younger and older workers, respectively.³⁷ Younger workers faced a 5% percent larger pay penalty at hiring than their older counterparts and a 6 percentage point penalty in the probability of being re-employed in an open-ended contract. The erosion of employment protection, therefore, did not result in better outcomes for younger workers and, in fact, it might have aggravated the duality along the age dimension. These estimates confirm the importance of a change in the informational content of

³⁷The median age in the full sample is 34 years, while the average age (in years) of the individuals in the younger group is 29.9 and of those in the older is 37.

dismissals induced by the reform, as firms were likely to have less information on younger workers.

Profound regional disparities are another hallmark of the Italian labor market. [Boeri et al. \(2021\)](#) document large productivity gaps, with firm value-added being at least 50% higher in the North than in the South. Workplace location, therefore, may be part of the channels which magnified or attenuated the impacts of the erosion of employment protection induced by the Fornero reform. We find clear evidence of magnification. On average, in fact, workers located in the South suffered a 3% greater hiring earnings penalty and a 3 percentage point larger employment disadvantage than their counterparts in the Centre-North as a result of job loss after the policy enactment (panels (a) and (b), respectively).

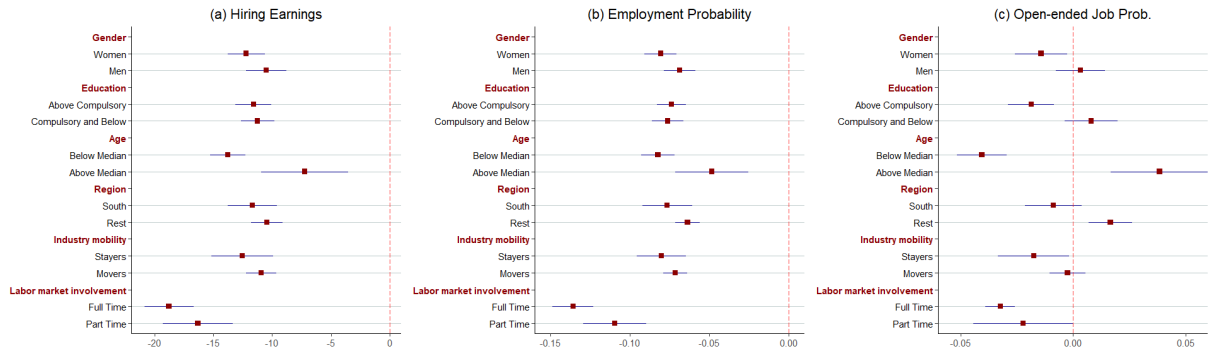
Some displaced workers in the re-hiring market may decide to search in the same industry in which they were employed before losing their jobs. While they are likely to have greater industry-specific human capital, their job termination might signal undesirable features to other employers in the same industry, where recruiters could have a better appreciation of the skills needed for the job. Other workers, instead, may decide to move to different industries, where their job loss could carry lower informational content than that attached to stayers. If movers experience worse labor market outcomes after a job loss, then we expect the reduction in their human capital to be a plausible channel. Conversely, if stayers face worse outcomes, this could reflect an altered informational content of their job termination. The evidence in [Figure 6](#) lends more support to this latter mechanism. Compared to movers, stayers faced 2 percentage point significantly lower chances of re-employment in a job with an open-ended contract (panel (c)). They also bore greater pay penalties upon re-employment and a lower re-employment probability, although such differences are not statistically significant at conventional levels.

Finally, even though the data do not allow us to identify the role played by hours worked, we can decompose the cost of job loss into the components due to part-time and full-time employment status at the time of separation. Across all three outcomes, the estimates in [Figure 6](#) show no statistically significant difference between part-timers and full-timers.

One year from job loss — The estimates for this exercise are in [Figure 7](#). Twelve months after job loss, both male and female workers continued to experience similar penalties along all outcomes, with no statistically significant differentials. As before, we find a negative educational gradient for the probability of re-employment on an open-ended contract (panel (c)), whereby high-education workers faced a statistically significant 3 percentage point larger penalty than their low-education counterparts. As before, there are no educational differences for the other two outcomes. Age effect differentials, instead, deepened considerably along all outcomes. The latter two sets of results (on education and age) re-emphasize our interpretation based on a change in firms' re-hiring strategies led by revised firms' beliefs about the average worker's productivity.

One year after job loss, the reform deteriorated earnings and employment conditions of all workers in the country, regardless of their workplace location. Workers in the South, however, faced a statistically significant 3 percentage point penalty of being in a job with an open-ended contract (panel (c)). Finally, effect differences between industry movers and stayers and between part-timers and full-timers are never statistically significant, although all four groups of workers

Figure 7: Heterogeneity at 12 Months from Job Loss

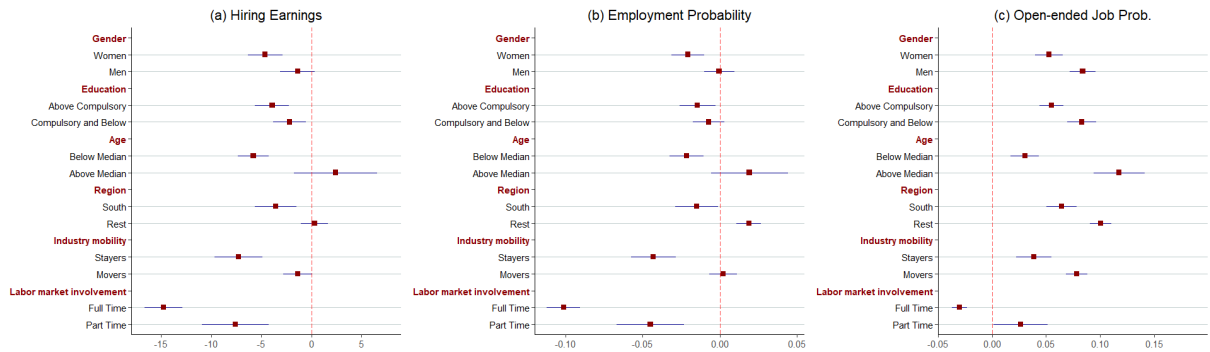


Notes: For details, see the notes to Figure 6.

suffered substantial penalties after the reform, with the only exception of movers' open-ended job probabilities (panel (c)).

After 33 months from job loss — Figure 8 shows the estimates found after 33 months from job loss, at the end of the observation period. At that point in time, across the three outcomes, the reform intensified all divides along all observable characteristics under analysis, with the exception of education. Interestingly, and for the first time, full-timers received lower earnings at hiring and faced greater re-employment penalties than their part-time equivalents. These effects might have been driven by the hiring incentives provided by the 2015 Jobs Act, but the greater role played by part-time arrangements are an indication of the worsened conditions of the re-hiring market.

Figure 8: Heterogeneity at 33 Months from Job Loss



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

Summary and Discussion — The general picture that emerges from Figures 6–8 is that the erosion of employment protection rights induced by the introduction of the Fornero reform deepened many of the pre-existing divides in the Italian labor market, emphasizing the lack of a levelling-up effect of the policy. Almost three years after job loss, female workers, younger workers, those employed in the South, those who remained in the same industry pre-termination, and full-timers faced larger penalties than their respective counterparts.

Although a number of stories could rationalize these results, we find the explanation based on the simple hybrid equilibrium model sketched in subsection 5.1 to be persuasive. In a labor market with elevated churning of jobs and highly heterogeneous workers (whether new hires or those on the secondary market), information on workers’ quality is crucial to firms. Even though the Fornero reform abated some of the firing costs faced by businesses, closing a job position or opening a new vacancy is always costly to companies, with costs likely to increase with workers’ heterogeneity. Without reliable information on workers’ quality, the wage offered by a firm must equate the employer’s expectation of the average productivity in the market. If employees are believed not to meet this average productivity requirement — a belief which a company could update using stereotyping and something that may stem from a lower probability of workplace reinstatement post-reform — they will not receive a job offer, especially if this is for an open-ended position.

Employers might have held less information on younger employees (who could be disproportionately better educated). Belief updates through stereotyping could have led to the penalties found for women, employees whose workplace was located in the South, and workers who stayed in the same industry post-displacement (this last result being harder to rationalize with explanations based on search frictions or human capital depreciation). Finally, greater reliance on part-time employment could just underline deteriorated conditions for all workers in the re-hiring market.

7 Conclusion

This paper analyzes the relationship between employment protection and workers’ cost of job loss, leveraging a 2012 Italian reform, known as the Fornero reform, which substantially reduced the possibility of reinstatement in the workplace for employees on open-ended contracts. The policymaker’s objective was to boost hiring in permanent jobs and, thus, to reduce the stubborn duality problem that has characterized the Italian labor market since the early 1990s at least. Fitting the estimator proposed by [Sun and Abraham \(2021\)](#) to a stacked-by-event design, we compare workers who lost their job before and after the reform.

Defying conventional wisdom, the reform led to a re-hiring pay penalty of 15% and a re-employment penalty of 7 percentage points twelve months post-job loss, when the impact on permanent re-employment chances was statistically indistinguishable from zero. With the estimated penalties affecting both individually laid-off and other workers equally (the latter group comprising mass laid-off individuals, quitters, and temporary workers ending fixed-term contracts), the reform did not seem to alleviate labor market duality. In fact, if anything, it deepened it, so that traditionally vulnerable workers (such as women, young workers, and those employed in the South) as well as other workers (such as those who stayed in the same industry pre-termination and full-timers) turned out to be the main losers.

We interpret these results within a basic hybrid equilibrium framework, in which employers update their beliefs using both the fact that workers were less likely to be reinstated as a result of

the Fornero reform and their own stereotypes about employees. In this environment, companies expect to be more likely to meet (and offer a job to) a low-skilled worker rather than a high-skilled one post-reform, and this could explain the penalties identified in the analysis. Although the generous hiring subsidies for new open-ended jobs introduced by the 2015 Budget Law and upheld by the Jobs Act flipped permanent employment probability penalties into advantages for labor market insiders (especially for men, older workers, those living in the Centre-North of the country and cross-industry movers), treated cohorts continued to exhibit lower re-hiring earnings and re-employment probabilities compared to their untreated counterparts almost three years after job loss.

Taken together, our findings underline a pervasive heterogeneity among workers, a feature that is likely to be relevant to many economies, and particularly those characterized by some form of labor market dualism. Such heterogeneity could be an important source of labor market polarization, which may disproportionately (and negatively) affect vulnerable workers and may warrant targeted policy interventions to support outsiders, irrespective of the strictness of employment protection regulations.

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