Labor market liberalization and successful return to work for Italian injured workers.

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ABSTRACT

Policy reforms introduced in Italy after 1998 increased labor market flexibility but reduced job security. We investigate whether these reforms affected the successful return to work of injured workers by exploring a matched employer-employee data.

Multinomial logit estimates show that liberalization reforms decreased job security particularly for injured workers hired with temporary contracts. All injured workers lost protection if they earned low pre-injury wages. Women and immigrants were penalized more. Pre-injury individual characteristics become stronger predictors of long-term employment than firms’ characteristics. The more severe the injury, the higher the probability of being unemployed, regardless of the reforms.

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KEYWORDS
Occupational injuries; return to work; labor market; deregulation; multinomial logit; matched employer-employee data; Italy.
1. Introduction

In Italy the cost of occupational injuries in 2007 was estimated to be equal to 2.63% of the national GDP. One third was attributable to safety investments and actions initiated by firms to prevent injuries. The remaining 27 billion euros were attributable to medical or indemnity costs incurred by national agencies, to production and adjustment costs sustained by employers, and to productivity losses or legal expenses faced by workers and their families (INAIL, 2011).

All these expenses are known to be function of injury severity, and of the length of the time to recover and to return to work (RTW). Hence the extensive research to identify best practices that can speed the RTW process and the implementation of policies to facilitate it and enhance injured workers’ employment (Clayton et al. 2012; Barr et al. 2010).

However, the meaning of successful RTW may vary across stakeholders (Young et al., 2005b; Shaw et al., 2012). While for the insurance agency it may signify the end of disability payments, for employers it may indicate the time when productivity is fully restored. For workers it is likely to mean not only reentry to work but also ability to keep the job, the pre-injury wage, and to further advance in career (Young et al., 2005a). In this context, the seminal study by Butler et al. (1995) highlighted that the research focus on workers’ first RTW can produce a very misleading picture. It does not capture the real post-injury employment dynamics, as many injured workers fail to return to stable employment and instead drop off the labor force, or experience unemployment or new disability spells.

Given the emerged research focus on injuries’ longer term employment outcomes, it is surprising that the literature has neglected the role played by existing labor market regulations. A handful of studies has examined whether differences in national disabilities policies (benefit generosity and eligibility requirements) contributes to different degrees of sustainable RTW (Anema et al, 2009; Barr et al, 2010; Collie et al, 2016). However, to the best of our knowledge, to date no economic analysis has explored whether such RTW can also be affected by changes
in national labor market regulations such as the ones introduced in several European countries since the ‘90s to increase labor market flexibility. Our objective is to cast some light on such neglected topic. We exploit a new and large administrative database on work and injury histories in Italy to examine post first RTW success both in terms of individuals’ long-term employment probabilities and of their ability to maintain the pre-injury employment, contractual status and security. We compare RTW success before and after the labor market liberalization reforms introduced in 1998-2001. Such reforms aimed at reducing unemployment but decreased job security (Berton et al., 2012). We investigate whether these consequences were observable also among injured workers, and whether human capital and the degree of employment protection at the firm level affected a successful outcome. We explore whether effects were different for the traditionally most vulnerable groups of workers (women and immigrants). We research this in an institutional setting that differs greatly from the North American labor market that has been the object of most existing economic studies about injured workers’ RTW. In fact, in Italy injured workers are guaranteed a “de facto” full wage compensation while off work (Galizzi et al. 2016).

Our study is organized as follow: in section 2 we discuss the related literature, our research hypotheses, and their testable implications; in section 3 we describe the Italian relevant institutional setting and its evolution over time with specific attention to labor market deregulation. Section 4 addresses data, sample selection and methodological issues. Section 5 presents our estimated results, robustness checks and comparisons with not-injured workers. Finally, Section 6 includes a discussion of our results and our conclusions.

2. Related literature and theoretical background

Given the lack of knowledge about the relationship between injured workers’ RTW and different labor markets regulations, we build on two quite separate strands of economics
literature, i.e. the one that has analyzed the conditions favoring a successful RTW and the one that has focused on the effects of labor market deregulation.

The RTW literature has shown that what seems a relatively simple chain of events (some workers get injured on the job, take time off work to heal, and return to work) disguises several complexities. Workers, firms, insurance agencies, government agencies, they all may face aligning or conflicting incentives in facilitating this process (Boden and Galizzi, 2017). Workers’ demographics, firms’ attributes, injury characteristics and workers’ compensation rules about eligibility, generosity, and length of disability payment will also affect it (Anema et al., 2009; Barr et al., 2010; Collie et al., 2016). Regulations and norms about disabilities accommodation may play also a large role (Anema et al., 2009; Clayton et al., 2012; Gailey and Seabury, 2010). Hence, the rich literature that over the last thirty years has studied the different factors that may facilitate workers’ return to productive employment (Krause et al., 2001; Cullen et al., 2017). However, workers’ RTW may not represent the end of the chain of events ignited by the incident. A first RTW may be followed by additional spells of employment or changes in employers, or by new occupational injuries, or lead to labor force separation (Butler et al., 1995; Krause et al., 2001; Bültmann et al., 2007; Côté et al., 2008; Vogel et al., 2011; Berecki-Gisolf et al., 2012; Galizzi 2013; Biering et al., 2013; Young 2014). All these additional developments are then one more time affected by the severity and degree of full recovery from the injury (Côté et al., 2008); by workers’ pre injury characteristics (Galizzi, 2013); by firms’ ability to provide accommodation and potential retaliation against the injured worker (Strunin and Boden, 2004). Given the focus of our research, it is also important to note that these additional longer term employment outcomes can be affected by workers’ attachment to the job, earnings, job status (Awang et al., 2016; Galizzi et al., 2016; Seing et al., 2015), and by the overall cyclical economic conditions and tightness of the labor market (Institute for Work and Health, 2009). It is also the case that after a first RTW, a
worker’s rights and opportunities to remain employed and to have access to good jobs will depend on the more general labor laws and regulations that characterize each country, and, within each country, different sectors or firms. In the more general case of sickness absences, only very limited and mixed evidence exists about the relationship between degrees of employment protection and time off work (Frick and Malo, 2008). On the other hand, the literature on labor market deregulation highlights that, when reducing employment protection legislation (EPL), workers’ mobility increases while the effect on unemployment is ambiguous (as demonstrated in the seminal paper by Bertola, 1990). Giannelli et al. (2012) estimate that in Italy the duration of the first job spell of individuals entering the labor market decreased after the deregulation reforms (they study the period 1990-2000) and this effect was not counterbalanced by a higher probability of moving quickly to a new employer. Furthermore, they observe that “the share of workers with only one job spell within three years decreases, while the share of those with three or more spells increases”. The port of entry effect – where temporary jobs lead to permanent employment - continued after the reforms but became less noticeable, as less than half of workers could move to a permanent contract after a series of temporary spells (Berton et al., 2011).

Our study builds on such body of existing literature to test four different hypotheses among Italian injured workers. First, as described in detail in the section below, labor market reforms introduced in Italy in the late 1990s affected firms differently depending on specific firms’ attributes. Therefore,

**Hp1:** The probability of keeping the pre-injury job after a first RTW was reduced for workers who were employed in firms where EPL decreased due to labor law reforms.

Second, we know that a worker’ high wage is likely to indicate her level of human capital, as well as effort, dedication to the job, and overall value to the firm’s production process.
Therefore, higher wages might also imply a higher probability or staying in the same firm in the medium-long run after RTW, despite deregulation of the labor market that would allow the firm to dispose of the worker more easily. Therefore, we test the following hypothesis:

**Hp2:** Compared to low wage workers, high wage workers were more likely to secure their pre-injury employment relationship in the medium-long run even if EPL was reduced.

On the other hand, workers who had suffered more serious injuries may be less employable, e.g. because of functional limitations due to the accident. It is an open empirical question to assess whether deregulation of the labor marker changed their condition, for the better or for the worse. Hence we test:

**Hp3:** The reduction in EPL had a different effect on the long-term employment outcomes of employees who had suffered more serious injuries.

Finally, we know that women and immigrants are workers who compose the weaker segments of the labor market and are more often hired with temporary contracts (Venturini and Villosio, 2008; Olivetti and Petrongolo, 2008). Hence, we test a fourth hypothesis:

**Hp4:** The reduction in EPL had a larger effect on the long-term employment outcomes of more vulnerable employees such as injured women and immigrants

To test these hypotheses we study a variety of post first-RTW employment outcomes, including movements in and out of temporary/permanent contracts. We study a twelve years period (1994-2005) during which Italy introduced a set of labor market liberalization reforms that we illustrate below.

### 3. Institutional setting

Italian injured workers enjoy full job protection (if hired with a permanent contract) till the end of the healing period. However, after their RTW they are still at risk of layoffs if no viable accommodation is found, or may quit if they cannot cope with job demands. We aim at
understanding whether different rules about job security may affect their long run employment outcomes. In this paper, we focus on EPL reforms, defining a period “before” (1994-1997), a period “during” (1998-2001) and a period “after” (2002-2005) such reforms, in an institutional setting that was unchanged with respect to health insurance and welfare provisions. This makes Italy a very interesting case study because such potential confounding factors were limited and constant over time, so that it becomes neater to single out the effect of changing EPL.

**Employment protection legislation and liberalization reforms**

In the initial period that we study (1994-1997), the typical labor market contract for an Italian worker was a “permanent” one, i.e. a contract with no stated termination date and implying significant firing costs for the employer in case of not-consensual termination. Such firing costs included severance payments and compensation in case of unfair dismissal. They increased with firm size in correspondence of two legal thresholds, one at 15 and another one at 60 employees (Cavaletto and Pacelli, 2014). In addition, unionization typically increases with firm size, making layoffs more conflictual, lengthier and costlier for larger employers. Until the late 90s, the only departures from the typical permanent contracts were apprenticeships and on-the-job-training contracts for youth. Temporary contracts were very limited and subject to strict constraints: each firm faced limits in the number of temporary employees it could hire and was required to prove the temporary nature of the occupation.

Labor market liberalization reforms were concentrated between 1998 and 2001. Reforms started in 1998 (Law Decree No. 196/1997) and introduced and regulated new types of work (such as temporary agency work and quasi-dependent work\(^1\)). They continued in 2000 (Law Decree No. 61/2000) deregulating part-time contracts. Finally, in 2001 (Law Decree No. 368/2001) we saw the full liberalization of temporary contracts. Now they could last a few days

\(^1\) Formally “self-employment” this is a de facto subordinate employment relationship. It is similar to a free-lance job and might be called also contract-work.
or up to 36 months, became renewable and faced no restrictions on their use. Afterward, only law Decree No. 276/2003 modified the EPL setting but did not introduce substantial novelties. Further and deeper reforms took place after 2008 but they are excluded from our analysis since their effects were largely affected by the macroeconomic recession. We should mention that the public sector did not undergo most of the reforms we discussed here. Instead, it faced a prolonged hiring freeze. Because of this difference, we exclude public employees from the current study.

All these were reforms “at the margin”, decreasing EPL for new entrants on the labor market and for job movers, but not for incumbent workers who did not lose their open ended job. However, while small firms faced very low EPL even before the reforms were introduced, larger firms took advantage of them to decrease the average EPL of their workforce by expanding new hires with flexible contracts.

The effect of the reforms became rapidly visible, as temporary contract workers in the private sector increased from 600,000 in the first quarter of 1998 to about 1.2 million in the first quarter of 2006 (Eurostat data).

Workers’ Compensation

In Italy, a public insurance system provides medical and disability benefits to all employees, both permanent and temporary workers, to all self-employed manual workers and to a part of non-manual self-employed workers. Those excluded from the public system – mostly self-employed workers in the trade sector – have to resort to the private insurance sector (but they do not have the obligation to do so). The public system is managed by the National Workers’ Compensation Agency (INAIL), and is financed by firms through premia which are proportional to payroll, increase with jobs risk, and are adjusted through experience rating. Workers who get injured are entitled to a recovery period, the length of which is established

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2 All self-employed workers are excluded from our analysis.
by a doctor who is certified to work for INAIL. They receive paid medical care directly or indirectly provided by INAIL, and disability benefits ranging from 60% to 75% of their earnings. However, a top-up granted by employers according to collective agreements allows injured employees to earn a de-facto full wage replacement during their absence from work (Galizzi et al., 2016). The worker compensation system was the object of only minor adjustments in the last decades. The only relevant reform was delivered in 2000 (DLgs 38/2000). It introduced coverage also for incidents occurring on the way to or from work\(^3\), a compensation for potential biological damages, and new injury severity thresholds that entitle injured workers to permanent disability benefits.

Unemployment Benefits

Up to 2005 the system assisting workers who lost their job was highly segmented. Workers laid off by larger (above 15 employees) manufacturing firms through collective bargaining (involving 5 or more employees) could enjoy generous “mobility benefits”: compensation up to 75% of their wage for up to 4 years according to age and area of work. All other unemployed individuals were compensated by a system of unemployment benefits that was poorly endowed, provided only a 40% replacement rate and was subject to strict eligibility conditions on past employment\(^4\); overall, it did not reach the minimum standards set by the 1952 ILO Social Security Convention until 2008 (Leombruni et al., 2012).

4. Study Population and Methods

Data

We use a database that combines individual employment histories from the Work Histories Italian Panel (WHIP) with injuries records from INAIL, the Italian National Workers’

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\(^3\) Such incidents *in itinere* are excluded from our analysis, as they were unobservable up to 2000.

\(^4\) Such conditions were: sector of activity (mostly manufacturing firms), at least two years of past employment, and at least one year of paid contributions over the last two years. As a result, the take up rate was very low.
Compensation Agency. The matched database is a 1:15 random sample of the population (about 1.5 million workers each year) covering the period 1994-2012, generating a unique source of information for the analysis of occupational injuries.

WHIP’s reference population includes all Italian workers and pensioners. It excludes only public sector employees hired with an open-ended contract and high skilled professions (e.g., lawyers) who are compensated with different insurance funds. The dependent employment section of WHIP is a matched employer-employee database that includes start and end dates of each employment spell, as well as worker characteristics (age, sex, place of birth), job characteristics (temporary vs. permanent contract, full-time vs. part-time, occupation, location), labor market outcomes (the number of days and weeks worked, earnings and social security payments) and firm characteristics (size, opening and closing date, sector, location, monthly new hires and separations, average wages).

INAIL data include a description of all injuries resulting both in permanent or temporary disabilities across the whole country and with time off work longer than three days. The data records a description of the injury event itself (when, how, where) and its consequences (nature of injury, part of body, length of temporary disability payment, and degree of permanent disability – if any).

The INAIL dataset and the WHIP dependent employment section have been matched and this is the dataset on which we base our analysis (see Bena et al., 2012, and Galizzi et al., 2015, for further details).

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5 Our study did not require ethics review and approval. In fact, the data we use has been listed under the Italian National Statistical Program that includes data-collection projects that comply with the national regulation on all issues regarding the use of personal data. The data has been positively evaluated by the Italian authority that is responsible to guarantee and protect privacy.

Please notice that in case of acceptance for publication, we will not be able to directly provide the dataset we used because the data owner is the Italian Ministry of Health, and we had access to it according to a strict confidentiality agreement. The data can be accessed by any researcher establishing a specific research agreement with the Italian Ministry of Health, however. We are obviously ready to provide information to guide other researchers through the procedures for accessing the data on injuries. Please also notice that all the results and descriptive statistics only related to the labour market - e.g. work contracts or wages - can be easily replicated with publicly accessible
Sample selection

For the purpose of this work, we select only employees who had a work incident between 1994 and 2005. As we observe outcomes up to three years after RTW this brings us at the beginning of the economic crisis in 2008. After that year, the Italian macroeconomic environment was deeply affected by the consequences of the financial crisis, and became less comparable to the previous decade.

We drop fatal events and injuries occurring on the way to or from work (as they were not compensated and therefore recorded before 2000), as well as those occurring in agriculture (where many are self-employed), and in education, health and personal services (which are mainly public sectors for which we do not have corresponding WHIP data).

As in most studies concerning injured workers, a serious concern is the issue of underreporting of injuries (Boden and Ozonoff, 2008), mainly from small establishments (Wuellner et al., 2016; Oleinick et al., 1995). Indeed, smaller firms have higher ability to under-report less severe injuries because governments’ health and safety controls are less frequently implemented among small firms. Furthermore, the additional insurance costs generated by an incident can be more significant for a small business and induce a larger incentive to underreport (evidence on Italy is provided in Galizzi et al., 2016). For our study, such concern is particularly relevant because it entails a potential bias in our estimations. As we described above, firm size modifies the degree of EPL, so that the estimated effect of the latter can be blurred by underreporting. Therefore, we exploit the information on the nature of incidents to study only injuries which usually require treatments at a hospital (fractures, anatomic losses and removals of an alien corpus). By implementing this restriction we limit the likelihood of an underreporting bias because hospitals are required to report injuries to INAIL.
Our final sample includes about 29,000 incidents for 27,442 workers. The sample composition does not change over the years we study with respect to the nature of injury (50% fractures, 8% anatomic losses and 42% removals of an alien corpus) and the part of body affected (37% upper extremities, 20% lower extremities, 33% head, 10% back or trunk) (see Appendix, table 1A). Therefore, we expect injury composition effects to play only a minor role in our results.

Measures of RTW outcomes

The outcome we analyze is the work status of the person \( n \) months after the first RTW. All permanent contract employees return to their previous job by law, while temporary contract workers can be unemployed upon return if their contract expired before the end of the healing period. We trace work status up to 36 months after RTW (we face no right censoring, as the database covers the period up to 2012). The outcomes we consider are: non-work, employed in the pre-injury firm, employed in a different firm with a permanent contract, employed in a different firm with a temporary contract\(^6\), on leave because of a new injury. Unconditional probabilities (Table 1) show that the most likely outcome was maintaining the pre-injury job. However, such probability was far below 100% across all three periods. Non-work was the second most likely outcome.

\(^6\) Fixed term, free-lance, training or apprentice contracts.
Table 1: Unconditional probabilities of post injury employment outcomes

<table>
<thead>
<tr>
<th></th>
<th>outcome after n months since RTW</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>3 months</td>
</tr>
<tr>
<td>1994-1997</td>
<td></td>
</tr>
<tr>
<td>non work</td>
<td>15.0</td>
</tr>
<tr>
<td>employed in the same firm</td>
<td>79.9</td>
</tr>
<tr>
<td>employed in a different firm - permanent contract</td>
<td>3.7</td>
</tr>
<tr>
<td>employed in a different firm - temporary contract</td>
<td>1.0</td>
</tr>
<tr>
<td>on leave due to a new_injury</td>
<td>0.4</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
</tr>
<tr>
<td>1998-2001</td>
<td></td>
</tr>
<tr>
<td>non work</td>
<td>14.9</td>
</tr>
<tr>
<td>employed in the same firm</td>
<td>78.1</td>
</tr>
<tr>
<td>employed in a different firm - permanent contract</td>
<td>4.3</td>
</tr>
<tr>
<td>employed in a different firm - temporary contract</td>
<td>2.2</td>
</tr>
<tr>
<td>on leave due to a new_injury</td>
<td>0.5</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
</tr>
<tr>
<td>2002-2005</td>
<td></td>
</tr>
<tr>
<td>non work</td>
<td>15.4</td>
</tr>
<tr>
<td>employed in the same firm</td>
<td>77.9</td>
</tr>
<tr>
<td>employed in a different firm - permanent contract</td>
<td>3.6</td>
</tr>
<tr>
<td>employed in a different firm - temporary contract</td>
<td>2.8</td>
</tr>
<tr>
<td>on leave due to a new_injury</td>
<td>0.3</td>
</tr>
<tr>
<td>Total</td>
<td>100.0</td>
</tr>
</tbody>
</table>

Note: Among those who were hired pre-injury with a permanent contract, only 1.7%, 1.4% and 0.8% (in the three periods respectively) became temporary contract workers in the same firm – out of all possible outcomes. Among those who were hired pre-injury with a temporary contract, 18.2%, 15.2% and 12.6% became permanent contract workers in the same firm – out of all possible outcomes.

Statistical analysis

We estimate a standard multinomial logit model of the probability of different employment outcomes at different intervals (12 and 36 months) after the first RTW, where $y_j$ takes values $j=0$ for non-work, $j=1$ if employed in the pre-injury firm, $j=2$ if employed in a different firm with a permanent contract, $j=3$ if employed in a different firm with a temporary contract, $j=4$ if on leave because of a new incident (hence $K=5$): .
Eq. (1): \[ \text{Prob}(y_i = j) = \frac{e^{b_j X_i}}{1 + \sum_{k=1}^{K-1} e^{b_k X_i}} \]

We then examine how probabilities changed during and after the labor market reforms. Hence, to test whether changes in the institutional framework influenced the probability of each outcome we interact the covariates of interest with the three reform periods. We define a set of dummies \( D = \{ D_1, D_2, D_3 \} \) signaling respectively periods “before” (1994-1997), “during” (1998-2001) and “after” (2002-2005) market liberalization reforms. Not all covariates \( X \) are actually interacted with \( D \) (although most of them are, as detailed in the empirical section); hence the estimated regressors in eq. (1) can be further decomposed as \( b_j X_i = g_j Z_i * D + h_j W_i \).

Given our research hypotheses the covariates of main interest are the following.

(i) A measure of EPL (log of the number of employees in the firm) as firm size is highly related to firing costs and union protection to test \( H_{p1} \).

(ii) A measure of human capital (log of one-year-lagged real daily wage) capturing individual productivity, status and labor market value to test \( H_{p2} \).

(iii) A measure of injury severity (log of days off work) to investigate whether more seriously injured employees were affected differently by the reforms to test \( H_{p3} \).

We add several controls to capture pre-injury individual health (workers’ past illness rate and age), propensity to change jobs (past intermittent employment spells\(^7\), and total work experience\(^8\)), as well as firm’s propensity to establish long-term employment relationships (firm’s age, experienced growth or decline in employment, and excess turnover\(^9\)). We also include some measure of the relative importance of the individual workers to the firm: whether

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\(^7\) We count one change for workers who went from employment to unemployment, two changes if they went from employment to unemployment to employment again and so on. We then divide the total number of changes by the total number of pre-injury quarters.

\(^8\) Past experience plus firm tenure, in days - at the day of the accident

\(^9\) Gross worker turnover minus (absolute value of) job creation at the firm level. This measures a firm’s propensity to keep or rotate its workforce.
their pre-injury wages were above the mean wage by occupation in the firm; whether they were employed part time, and performing manual tasks. We include information about one outcome that clearly affects employment after RTW: whether the injury had resulted in a permanent disability. Finally, we account for the business cycle: geographical areas and regional unemployment rate by gender.

Summary statistics of all our covariates, calculated by time periods D, are presented in Table 2. Notice that males and manual workers are the vast majority of our sample, as we are focusing on individuals employed in occupations highly exposed to the risk of incidents. The most notable changes across the time periods is the increase in the number of immigrants (an increase of 16 percent point over 10 years) and the decline in unemployment rate, as the business cycle slowly improves from the recession of early ‘90s to the recovery or early 2000. Also, return to work time lengthens; because of this reason, we control for this time dimension in our models.

Table 2: Summary statistics

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<tr>
<th></th>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>lagged real daily wage</td>
<td>mean</td>
<td>62.89</td>
<td>62.30</td>
<td>61.55</td>
</tr>
<tr>
<td>wage above the mean wage by occupation in the firm</td>
<td>share</td>
<td>0.27</td>
<td>0.25</td>
<td>0.25</td>
</tr>
<tr>
<td>past intermittent spells</td>
<td>mean</td>
<td>0.42</td>
<td>0.51</td>
<td>0.50</td>
</tr>
<tr>
<td>number of employees in the firm</td>
<td>median</td>
<td>25.08</td>
<td>23.33</td>
<td>22.33</td>
</tr>
<tr>
<td>growing firms</td>
<td>share</td>
<td>0.39</td>
<td>0.44</td>
<td>0.43</td>
</tr>
<tr>
<td>shrinking firms</td>
<td>share</td>
<td>0.25</td>
<td>0.24</td>
<td>0.23</td>
</tr>
<tr>
<td>firm age</td>
<td>mean</td>
<td>15.86</td>
<td>16.18</td>
<td>16.58</td>
</tr>
<tr>
<td>excess turnover</td>
<td>mean</td>
<td>0.31</td>
<td>0.33</td>
<td>0.32</td>
</tr>
<tr>
<td>days of absence before RTW</td>
<td>median</td>
<td>34</td>
<td>38</td>
<td>41</td>
</tr>
<tr>
<td>past illness rate</td>
<td>mean</td>
<td>0.18</td>
<td>0.18</td>
<td>0.19</td>
</tr>
<tr>
<td>years of past experience plus firm tenure</td>
<td>mean</td>
<td>7.17</td>
<td>8.19</td>
<td>9.10</td>
</tr>
<tr>
<td>female</td>
<td>share</td>
<td>0.08</td>
<td>0.09</td>
<td>0.09</td>
</tr>
<tr>
<td>part time</td>
<td>share</td>
<td>0.02</td>
<td>0.03</td>
<td>0.05</td>
</tr>
<tr>
<td>migrants</td>
<td>share</td>
<td>0.10</td>
<td>0.17</td>
<td>0.26</td>
</tr>
<tr>
<td>manual occupation</td>
<td>share</td>
<td>0.94</td>
<td>0.93</td>
<td>0.93</td>
</tr>
<tr>
<td>worker’s age</td>
<td>mean</td>
<td>36.70</td>
<td>36.93</td>
<td>37.96</td>
</tr>
<tr>
<td>unemployment rate</td>
<td>mean</td>
<td>7.65</td>
<td>6.50</td>
<td>5.44</td>
</tr>
<tr>
<td>north-west</td>
<td>share</td>
<td>0.31</td>
<td>0.33</td>
<td>0.33</td>
</tr>
<tr>
<td>north east</td>
<td>share</td>
<td>0.28</td>
<td>0.29</td>
<td>0.28</td>
</tr>
<tr>
<td>center</td>
<td>share</td>
<td>0.17</td>
<td>0.18</td>
<td>0.17</td>
</tr>
<tr>
<td>south</td>
<td>share</td>
<td>0.24</td>
<td>0.21</td>
<td>0.22</td>
</tr>
</tbody>
</table>
Finally, we stratify our analysis by the nature of employment contract, i.e. we estimate our models including and excluding temporary contract workers\(^{10}\), to assess whether the liberalization reforms affected them differently. For the same reason we also stratify by gender and by nationality, to cast more light on the outcomes of most vulnerable workers (Hp. 4)\(^{11}\).

We present our main results from the multinomial logit model plotting the conditional probabilities of the different outcomes in different periods:

Eq. (2): 
\[
Prob(y = j|D = D_n, Z_m = a, X_-)
\]

Where \( n \) indicates one of the three periods, \( X_- \) indicates the set of all \( X \) excluding \( Z_m \), and \( Z_m \) can refer to individual wage, firm size or length of absence due to the incident, and \( a \) indicates a specific value (usually a quartile) of \( Z_m \).

Then, Hp1 can be written as

\[
Prob(y = 1|D = D_1, \text{firmsize} = \text{large}, X_-) > Prob(y = 1|D = D_3, \text{firmsize} = \text{large}, X_-)
\]

i.e. in a firm facing EPL reduction due to the reform (large firm), the probability of keeping the pre-injury job (outcome 1) in the pre-reform period (period 1) is larger than in the post-reform period (period 3).

Hp2 can be written as

\[
Prob(y = 1|D = D_1, \text{wage} = \text{low}, X_-) > Prob(y = 1|D = D_3, \text{wage} = \text{low}, X_-)
\]

i.e. for a worker earning a low wage, the probability of keeping the pre-injury job (outcome 1) in the pre-reform period (period 1) is larger than in the post-reform period (period 3).

Analog inequalities can be written for the other hypotheses.

---

\(^{10}\) In our sample, the share of temporary contract workers increased from 8% in the first period to 14% in the second and 16% in the third one.

\(^{11}\) When not stratified, we add to the model controls for gender and nationality as well.
5. Results

Estimated conditional probabilities of each outcome at 1 and 3 years after RTW (Table 2A in Appendix) confirm the picture previously suggested by the unconditional probabilities (Table 1). The reforms did not affect the average probability of remaining with the pre-injury firm and this remained by far the most likely RTW outcome. At the same time, the probability of non-work decreased at the start of the reform (after period 1), but new employment became less secure. In fact, the probability of getting a new job with a permanent contract decreased after the completion of the reform (after period 2), and that of getting a temporary contract in another firm increased significantly in every period and roughly doubled after 36 months. Being off work because of a new work related injury remained an extremely unlikely event that was not influenced by the reforms. Therefore, we will not further discuss the results about this very last outcome.

We are interested in testing whether the above discussed trends are influenced by firm (i.e. employment protection rules/firm size, as for Hp1) and individual (i.e. human capital/wage, as for Hp2) characteristics, severity of the injury (as for Hp3), gender, and nationality (as for Hp4). We want to check also whether results vary by pre-injury contract type. We present our main estimation results from the multinomial logit model by plotting the conditional probabilities of the different employment outcomes in different periods and for different values of the covariates of interest\(^{12}\), as specified in Eq (2). In the following figures we look at the slopes of the marginal effects of covariates over time, as they indicate the conditional probability of outcome j at values \(a=\{a_1, a_2, a_3\}\) of the covariate of interest Z over time, i.e. as the reforms develop.

\(^{12}\) Full estimation results are available upon request (see Table 3A in the Appendix for an example)
Figure 1 plots how the conditional probabilities of our four main RTW outcomes changed over the three reforms periods and varied in correspondence of the size of the firm that was employing the worker when the incident occurred. We show only results for outcomes one year after the first RTW but the ones for three years after are substantially equivalent (see Figure 1A in Appendix). Each line refers to a given firm size (10, 100 and 1000 employees) representative of small, medium and large Italian firms. We find that pre-injury employment at a larger firm did no longer secure a job in the same firm after the reforms started: the conditional probability of keeping the pre-injury job was higher in large firms before the reforms (period 1) but the “size protection” effect disappeared after the reforms (period 3). Consistently the conditional probability of non-work was higher in smaller firms, but becomes not significantly different across firm sizes after the reforms, while the likelihood of finding a new job with only a temporary contract increases across all firms sizes.

13 Average firm size is very small in Italy, as about one third of private sector workers are employed in firms with less than 15 employees. A firm with 10 employees is below the EPL thresholds (at 15 and 60 employees), while firms with 100 employees are above them. Firms with 1000 employees are large for Italian standards, and usually unionized.
It is possible that the results concerning the probability of remaining in the same firm hide differences in RTW success by pre-injury workers’ contractual status. It is plausible that injured workers may benefit of more firm’s good will and accommodations if originally hired with a permanent and not temporary contract. In fact, if we remove workers who had been working with a temporary contract at the time of the injury, we find that the conditional probability of remaining in the same firm for permanent contract workers only (about 90% of all workers in our sample) remains higher in correspondence of larger firm size, and is not affected by the reforms. Hence, related to Hp1, we can conclude that being in a large firm does no longer guarantee job protection after the reforms only to those injured workers who were hired with a temporary contract at the time of the injury, despite that fact that the possibility of post injury accommodation increases with size (Figure 2). Results still hold after three years (Figure 2A in Appendix).
We focus now on wages as a measure of human capital accumulation that could protect workers, as for Hp2.

**Figure 3** is analogue to the previous ones and shows conditional probabilities in correspondence of the first, second and third quartile of the real wage distribution (50, 60 and 70 euro per day). It shows that, as the reforms were introduced, workers with lower pre-injury wages became clearly less protected against labor market liberalization: they faced decreasing probability of remaining in their pre-injury firm, end up with relative higher probability of no-work, and a higher likelihood of moving to a new firm under an only temporary contract. Results after three years are unchanged, although the effect of the reforms on the probability of no-work becomes milder (Figure 3A in Appendix).
Figure 3: Conditional probability of outcomes after one year since RTW, by (real daily) wage quartiles (50, 60, 70 euro)

Note: Wage 50 euro: ____ Wage 60 euro: ----- Wage 70 euro: ……; and 90% confidence intervals.

This time the results do not change when we limit our analysis to employees who were under permanent contracts when injured (Figure 4): conditional probabilities of keeping the pre-injury job were not different by wage quartile before the reforms, but afterward they are different (although differences are not strongly significant). This holds after three years as well (Figure 4A in Appendix) and is consistent with our Hp2.
Therefore, our first conclusion is that reforms made successful RTW less dependent on national employment protection rules and potential union protection and firm’s accommodation (linked to firm size) and more a function of individual’s human capital (as measured by wage). Such results confirm our first two hypotheses. Notice that our estimates (Table 3A in Appendix) control for (1) the individual propensity to move (number of pre-injury employment spells, interacted with periods), (2) total individual labor market experience (pre-injury days of employment, interacted with periods), (3) firm propensity not to invest in long-term relationships with the workforce (excess firm turnover interacted with periods). All these regressors have the expected impacts (1 and 3 decrease the probability to stay, 2 increases it), but have no significant dynamics linked to the reforms.
Severity of the Injury

When we focus on the role played by the severity of injury (captured by the length of the spell off work), we find that more severe injuries were associated with lower conditional probability of keeping the pre-injury job and higher conditional probability of non-work after one year (Figure 5). Surprisingly, none of the patterns we study are influenced by the reform as lines remain pretty much parallel. The same outcome arises after three years (not reported). It is important to recall that our analysis controls also for other measures of general health (age, the annual frequency of pre-injury sick leaves, and permanent disability caused by the injury). All these regressors were also interacted with periods and we did not find significant interactions.

Hence, our findings indicate that reforms did not alter the likelihood of different long-term employment outcomes by workers’ injury severity, i.e. more vulnerable workers because of their worst injury experience were neither advantaged nor penalized by the reforms. This contradicts our Hp. 3. However, it is important to notice that one more time, we observe a striking increase in the overall likelihood of landing on a new temporary job over the reforms period.
Figure 5: Conditional probability of outcomes after one year since RTW, by quartiles of number of days of off after injury (9, 40, 80 days)

Note: Days off work 9: ____ Days off work 40: ----- Days off work 80: ......; and 90% confidence intervals.

Women and migrants

To test our fourth hypothesis, we analyze our main results separately in the case of women and immigrants only. It is important to recall that the share of immigrants in our sample increased from 10% to 17% and to 26% over the three periods while the share of women remained stable around 9%. Among women, temporary contracts increased more than in the whole sample, from 8%, to 19%, to 20% in the three periods considered; the same holds for immigrants, among which they increased from 8% to 16% to 19%. Notice however that the smaller sample size decreases the precision of the estimates with respect to the whole sample.

We focus on the conditional probability of keeping the pre-injury job after one year, for the sake of clarity and brevity. In Figure 6 we see that for both women and immigrants (regardless of contract type) the probability of remaining in the same firm flips after the reforms and

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14 As already mentioned, in the whole sample the share of temporary contracts increased from 9% to 14% to 16%.
becomes negatively related to firm size, showing a more penalizing effect of the reforms for them with respect to the whole population of injured workers (compare with Figure 1). For immigrants, this process started as soon as the reforms rolled in (period 2); for women, as the reforms were completed (period 3). Both women and immigrants show the same pattern of the full sample with respect to the protective effect of high wages; however, for women the protection appears later, only in the period after the reform.

*Figure 6: Conditional probability of staying in the same firm at one year, by firm size (10, 100, 1000 employees) and by (real daily) wage quartiles (50, 60, 70 euro)*

If, again, we exclude those working with a temporary contract at the time of the injury, we see (Figure 7) that - as in the whole sample - the effect related to wages is unchanged. However, the protective effect of firm size for permanent contract workers tends to fade away, during the reforms for migrants and after the reforms for women. This suggests that larger firms took advantage of the liberalization reforms to reduce the share of injured women and migrants on
their payroll with a temporary contract and also of those they had previously hired with a permanent contract. This supports our Hp. 4, i.e. a more penalizing effect of the reforms for injured women and immigrants\(^{15}\).

**Figure 7:** Conditional probability of staying in the same firm at one year, by firm size (10, 100, 1000 employees) and by (real daily) wage quartiles (50, 60, 70 euro). Permanent contract workers only

Note: firm size 10 employees: _____ firm size 100 employees: ----- firm size 1000 employees: ……..; and 90% confidence intervals.
Note: Wage 50 euro: ____ Wage 60 euro: ----- Wage 70 euro: ……..; and 90% confidence intervals.

Finally, with respect to the severity of the incident and the consequent length of the absence, results related to women and immigrants are unchanged with respect to the whole sample (not reported): one more time reforms were found to be ineffective on changing the subsequent probability of non-work for injuries of different severities.

**Comparison of injured vs. non injured workers.**

To better assess the implications of our findings, we should compare the observed employment outcomes for injured workers with the ones of Italian workers who did not experience an

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\(^{15}\) Results on the probability of no-work mirror those discussed here but are imprecisely estimated (Figures 5A and 6A in Appendix).
occupational incident. However, such direct comparison is not straightforward because we are focusing on a very specific subset of the population of workers (i.e. those more exposed to the risk of a work incident) and we study their working career after a quite relevant health shock (i.e. an injury that was likely to require immediate care). No comparable situation can be found in the general working population. Furthermore, the general working population (in non-agricultural private firms) is composed by more women, fewer migrants, fewer manual workers, and higher wage earners with respect to our sample of workers employed in more risky occupations (and described in Table 2). The general working population is also exposed to the risk of a work incident for a shorter time: in fact, they have higher rates of part-time and less average work experience. All this would imply a quite different career path for our injured workers compared to non-injured employees regardless of the occurrence of a shock like a work incident.

Hence, following Fadlon and Nielsen (2017), we look for a more appropriate comparison. Our comparison group is drawn from the same sample of injured workers used in the main analysis, but is selected only among those who will face a work accident later in time. Here we are assuming that they all come from the same population of workers at risk, and that the time of the accident is random. In Fadlon and Nielsen’s words (page 10) we “[…] compare households [workers in our case] with the same expectations over the distribution of future paths, but with different realizations, to isolate the unanticipated component of the shock […] exploiting the potential randomness of the timing of a severe […] health shock within a short period of time.”

Those authors proceed to estimate a proper impact evaluation applying a difference in differences strategy. However, the aim of our research is different, because we do not focus on the outcomes per se as they did, but on whether the consequences of the actual occurrence of an incident are influenced by the institutional setting of the labor market. We are interested on how the probability of the different outcomes is affected by covariates like firm size or
individual wages. Therefore, we replicate the analysis as in the previous figures (e.g. **Figure 1**) and we compare the displayed patterns over the different reform periods. Finally, we use the estimated conditional probabilities to mimic a difference in differences comparison.

To be more specific, we proceed as follows. First, we attribute to not-yet-injured workers a random date of “pretend injury” and a random duration of “pretend leave”, so that the distributions in the true and pretend injured samples are as similar as possible both in terms of time off work and tenure at the time of the incident (appendix). We impose that the “pretend injury” and “pretend RTW” happen more than one year before the calendar day of the actual injury, so that we always measure their outcomes before the actual injury occurs. Unfortunately, in this case the sample size for period 3 (after the reforms) becomes quite small, because of the right censoring of work accident data at December 2005; estimates are less reliable for that period, hence we must focus mainly on periods before and during the reforms.

In terms of unconditional probabilities we calculate that our “not yet/pretend injured workers” face a much lower probability of no-work with respect to actual injured ones, and a higher probability of keeping their pre-injury job across the first two periods. This seems to suggest a clear vulnerability of injured workers when the incident actually occurs (Table 4A in Appendix). Such general finding is confirmed when we replicate our multinomial logit analysis for the “not yet/pretend” injured group and plot again the conditional probabilities of the different outcomes in different periods (**Figure 8** and Figures 9-12 in the Appendix). We notice that over the reforms period (period 2) the “not yet/pretend” injured workers employed in small firms increase their probability to stay in the same firm one year after the imputed incident\(^\text{16}\). This was not the case for the actual injured workers (back in Figure 1). In addition the estimated probabilities of not working one year after the imputed incident day were much lower for

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\(^\text{16}\) Results on only permanent contract workers disappear by firm size and are unchanged by wage, as in the sample of actual accidents (appendix).
workers in each firms size compared, again, to what was estimated for the real injured ones (Figure 1). “Not yet/pretend” injured workers who earned low, median and high wages also increased their probability to stay in the same firm and decreased their probability of no-work during the reforms. Instead, the probability to stay in the same firm was decreasing among actually injured low wage workers, and it stayed constant for median and high wage ones; among actually injured workers the probability of no-work decreased, but to a lesser extent among low wage workers (see Figure 3).

Figure 8: “Not yet injured/pretend” injured workers - Conditional probabilities of staying in the same firm or not working one year after “pretend” RTW, by firm size (10, 100, 1000 employees) - first row - and by (real daily) wage quartiles (50, 60, 70 euro) – second row

![Figure 8](image)

Note: firm size 10 employees: _____ firm size 100 employees: ---- firm size 1000 employees: ……; and 90% confidence intervals.

Note: Wage 50 euro: ____ Wage 60 euro: ----- Wage 70 euro: ……; and 90% confidence intervals.

Finally, we use the point estimates and confidence intervals of the conditional probabilities of staying in the same firm, as in Figure 8, Figure 1 and Figure 3, and mimic a difference in differences comparison, without any possibility to verify the common trend assumption. However, we believe such assumption should hold because before period 1 both groups are “not yet injured”. In Table 3 we note that actually injured workers suffer larger penalties in terms of the likelihood of remaining in the same firm one year after a first RTW, especially if
they were employed in small firms and earned lower pre injury wages. This suggests that reforms penalized more the actually injured workers compared to the not injured ones because their longer term employment prospects became more uncertain.

Table 3: Difference in differences measure of conditional probabilities of outcomes (remaining with same employer) after one year

<table>
<thead>
<tr>
<th>size</th>
<th>period</th>
<th>T/C</th>
<th>Margin</th>
<th>[90% Conf Interval]</th>
<th>wage</th>
<th>period</th>
<th>T/C</th>
<th>Margin</th>
<th>[90% Conf Interval]</th>
</tr>
</thead>
<tbody>
<tr>
<td>SMALL</td>
<td>2</td>
<td>T</td>
<td>0.689</td>
<td>0.680 - 0.698</td>
<td>LOW</td>
<td>2</td>
<td>T</td>
<td>0.690</td>
<td>0.682 - 0.699</td>
</tr>
<tr>
<td>SMALL</td>
<td>1</td>
<td>T</td>
<td>0.687</td>
<td>0.677 - 0.696</td>
<td>LOW</td>
<td>1</td>
<td>T</td>
<td>0.712</td>
<td>0.702 - 0.721</td>
</tr>
<tr>
<td>SMALL</td>
<td>2</td>
<td>C</td>
<td>0.756</td>
<td>0.749 - 0.763</td>
<td>LOW</td>
<td>2</td>
<td>C</td>
<td>0.762</td>
<td>0.755 - 0.768</td>
</tr>
<tr>
<td>SMALL</td>
<td>1</td>
<td>C</td>
<td>0.731</td>
<td>0.726 - 0.737</td>
<td>LOW</td>
<td>1</td>
<td>C</td>
<td>0.759</td>
<td>0.755 - 0.764</td>
</tr>
<tr>
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<td></td>
<td>-0.022</td>
<td>-0.020 - -0.024</td>
<td>DID</td>
<td>-0.024</td>
<td>-0.021 - -0.026</td>
<td></td>
<td></td>
</tr>
<tr>
<td>MEDIUM</td>
<td>2</td>
<td>T</td>
<td>0.710</td>
<td>0.702 - 0.719</td>
<td>MEDIAN</td>
<td>2</td>
<td>T</td>
<td>0.702</td>
<td>0.694 - 0.709</td>
</tr>
<tr>
<td>MEDIUM</td>
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<td>T</td>
<td>0.740</td>
<td>0.730 - 0.749</td>
<td>MEDIAN</td>
<td>1</td>
<td>T</td>
<td>0.712</td>
<td>0.704 - 0.720</td>
</tr>
<tr>
<td>MEDIUM</td>
<td>2</td>
<td>C</td>
<td>0.769</td>
<td>0.762 - 0.776</td>
<td>MEDIAN</td>
<td>2</td>
<td>C</td>
<td>0.768</td>
<td>0.761 - 0.774</td>
</tr>
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<td>0.789 - 0.800</td>
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<td>1</td>
<td>C</td>
<td>0.760</td>
<td>0.755 - 0.765</td>
</tr>
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<td></td>
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<td>-0.002 - -0.006</td>
<td>DID</td>
<td>-0.018</td>
<td>-0.016 - -0.020</td>
<td></td>
<td></td>
</tr>
<tr>
<td>LARGE</td>
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<td>T</td>
<td>0.727</td>
<td>0.712 - 0.742</td>
<td>HIGH</td>
<td>2</td>
<td>T</td>
<td>0.714</td>
<td>0.705 - 0.724</td>
</tr>
<tr>
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<td>T</td>
<td>0.785</td>
<td>0.770 - 0.799</td>
<td>HIGH</td>
<td>1</td>
<td>T</td>
<td>0.712</td>
<td>0.703 - 0.722</td>
</tr>
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<td>C</td>
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<td>2</td>
<td>C</td>
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<td>0.767 - 0.782</td>
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<td>C</td>
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<td>0.837 - 0.854</td>
<td>HIGH</td>
<td>1</td>
<td>C</td>
<td>0.761</td>
<td>0.754 - 0.767</td>
</tr>
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<td>DID</td>
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<td></td>
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<td>0.009 - 0.004</td>
<td>DID</td>
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<td>-0.010 - -0.013</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

6. Discussion and Conclusions

The Italian labor market underwent a sequence of dramatic reforms between 1998 and 2001. Their goal was to increase labor market flexibility and, as a consequence, produce greater employment. Our study assesses how such reforms changed patterns of sustainable RTW among those workers who suffered an occupational injury. Our estimation of a multinomial logit model that accounts for individual, firm and injury characteristics indicates that continuous work with the pre-injury employer remained the most common employment outcome for injured workers after a first RTW. However, this was true for only around 70% (after one year) and 50% (after three years) of injured workers and such percentages remained overall stable before, during, and after reform periods. The second most common outcome, non-work, became a bit less frequent. Before the reform 25% of injured workers were no longer
employed three years after their first RTW. After the reform this percentage decreased to 23%. More workers kept working but with a different employer and a temporary contract (from 6% before the reform to 11% after the reforms). This suggests that labor market liberalization reforms made slightly easier for injured workers to remain employed. However, this happened at the expense of future job security: injured workers found more easily jobs with new employers, but fewer jobs with permanent contracts.

Reforms also weakened the employment protection that Italian workers had historically enjoyed when hired by larger firms, who face higher firing costs, stronger union representation, and likely offer more accommodations to the injured. After the reforms, workers who were injured in larger establishments did not enjoy any longer a higher probability of long-term successful RTW (conditional on wages and the other controls), both in terms of their ability to keep their job with the pre-injury firms and in terms of losing employment (no work). Such loss of overall employment protection associated with firms’ size was driven by the most vulnerable workers: the ones who at the time of the injury were already hired with temporary contracts (males or females), or the ones who were immigrants, even if hired with a permanent contract. This suggests that the traditionally more vulnerable workers were also the ones for whom a post-injury successful RTW was more heavily compromised by the introduction of the reforms. Liberalization gave employers more leeway in terms of decreasing any long-term commitment toward these employees.

At the same time, our results confirm the importance of higher pre-injury wages (conditional on firm size and the other controls), in securing successful long-term employment. Previous research had already shown that higher wage workers are the more likely to return to work sooner after an incident (Galizzi et al., 2016). Given the same labor market experience, higher wage may proxy more successful investment in human capital and, therefore, a higher productivity that employers do not want to lose. Or it may signal higher worker’s status within
the company, another characteristic that may lead to higher likelihood of receiving accommodation by employers. The findings of our current study suggest that such factors are determinant also of successful RTW over the long run despite the potential higher flexibility introduced by reforms. Our estimates show that high wage workers kept being more likely to maintain their job with the pre-injury employer, or to avoid non work or a temporary contract if they changed employer one or three years after their first RTW, even if they are women or immigrants.

Finally, we do not find any clear effect of the reforms once we estimate predicted employment outcomes by differences in injury severity. Clearly, the worst the injury, the bigger the challenges across all the employment outcomes we studied. However, liberalization of the labor market neither worsened nor improved employment outcomes and trends one/three years after RTW across the different levels of injury severity.

We acknowledge that our study has limitations. For example, our data is quite rich in terms of recorded workers’, firms’, and injuries’ characteristics, but it does not permit us to compare the experience of injured workers with the ones of similar workers employed, for example, in the same occupation and in the same firms. This limits our ability to establish clearly whether the reforms affected differently injured and non-injured employees. However, the comparison exercise we conducted using “not yet injured workers” as a comparison group suggests that the reforms may have indeed increased employment uncertainty more for injured workers than for the non-injured ones. This is clearly an area that should be further tested with different data sets.

Our study represents also only a first step in our effort to understand the full effect of labor market reforms on injured workers’ well-being. We assess our outcomes mainly in terms of job/employment security, but other dimensions of job quality such as wages, job title, and full vs. part time status are also very important dimensions of employment quality, and they deserve
further analysis. Although our measure of successful RTW enriches the ones used in previous studies by considering also the degree of job security attached to new employment contracts, we are aware that it remains an imperfect measure of potential success. Individuals differ in their needs, desires and aspirations. Therefore, a RTW on a job that is less secure but pays better or promises immediate or future promotions may be a preferable outcome to certain injured workers (Young, 2014). Similarly, to certain individuals a temporary job that comes with more interesting duties, or with flexible time, could be more desirable than a more structured, permanent but less flexible job (Krause et al., 2001; Berecki-Gisolf et al., 2012; Young et al., 2005 b; Shaw et al., 2012). Finally, a RTW to a secure job may still be not successful if the worker keeps experiencing physical or mental health limitations (Bültmann et al., 2007). These are job dimensions and workers’ preferences that cannot be captured by our administrative data but would require survey or qualitative data. Despite such limitations, our study provides new and original evidence that the outcomes of occupational injuries do not happen in a vacuum, and are not only affected by workers’ compensation, disability, or human resource policies. They are shaped also by the more general rules that affect each country labor market. This implies that policies regulating the experience of injured workers need to be aware that their final effect is likely to depend on a much wider set of national or sectorial labor market regulations. At the same time, general labor market policies reforms cannot forget how they will end up affecting the experiences of some of the less visible, but more vulnerable workers, such as the ones who carried already the burden of an occupational injury.

Our finding that market liberalization policies make potentially easier to find new - but less secure - employment for injured workers, has very important implications. There is evidence that more insecure jobs are associated with higher frequency and severity of injuries (Fabiano et al., 2008) and, more generally, higher risk of poor health, of future sickness absences, and
of larger use of health services (Laszlo *et al.*, 2010). All these negative consequences are likely to be amplified for a person who already suffered an injury and ends up being reemployed in a more insecure job. Therefore, workers’ compensation policies and labor market liberalization policies need to be highly coordinated to avoid severe unintended consequences as higher costs caused by new lost production and medical expenses.
7. References


