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Labour market institutions and long term adjustments to health shocks: Evidence from Italian administrative records

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ABSTRACT

We investigate how labour market institutions shape the labour response to acute cardiovascular shocks in a highly regulated labour market over the long run. Using Italian administrative data that covers the history of employment, social security and hospitalization of workers, we exploit several identification strategies to remove bias from observable and unobservable confounders. Significant reductions in employment and labour income emerge in the short run and persist over time, with no evidence of re-entry in the longer run. No adjustment is detected in working hours or wages for those who maintain employment. While the Disability Insurance (DI) system contributes to insure workers against the consequences of health shocks, we offer new evidence on the value of employment protective institutions in strengthening unhealthy workers' labour inclusion, in particular for blue-collar workers.

1. Introduction

Fostering older and unhealthy workers' inclusion in the labour market is a daunting task that requires complex choices, trading-off the provision of incentives to remain active and the protection that motivates social insurance. Knowledge of how labour market institutions shape behavioural responses to health deteriorations over time is central to shaping the policy agenda wisely. Indeed, empirical evidence has so far contributed to a relative consensus on the detrimental effect of health shocks on labour outcomes in terms of employment, income from labour and even wealth because of increased health expenditure (Dobkin et al., 2018).[1] However, much less is known about the role of institutional factors in mediating these effects.

To our knowledge, only two works exist (García-Gómez, 2011; Trevisan and Zantomio, 2016) that discuss the topic in the light of cross-country institutional variation.[2] Both document differences in employment effect magnitude across European countries and somehow place Northern countries (i.e. Denmark, Netherlands) and Southern (i.e. Italy) at opposite sides of the effect magnitude spectrum: highest in Northern, negligible in Southern countries. Furthermore, both relate – although at a rather descriptive level – effect magnitude to dimensions such as higher hiring rates, generosity of disability compensation, provision for disability integration policies, and (lack of) mandatory quotas for disabled workers.

Still, many questions remain unanswered. For example, to what extent do institutional dimensions such as employment protection, working hours' flexibility and compensation structure rigidities activate different margins of adjustment over time? Are existing social insurance provisions covering against the financial consequences of health deteri-

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[1] On labour outcomes effects of health shocks in specific national contexts, see e.g. Bradley et al. 2013, García-Gómez et al. 2010, Moran et al. 2011, Cai et al. 2014, Halla and Zweimüller 2013, Flores et al. 2020, Wu 2003.

[2] García-Gómez (2011) studies the effects of subjectively perceived health deterioration in specific European countries; Trevisan and Zantomio (2016) study older workers' response to major health conditions in subgroups of European countries.

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oration? Does employment protection legislation (EPL) actually play a role in fostering employment for workers seeking to maintain employment after a health shock, or do firms find *de-facto* routes to dismiss them? What is the interplay between EPL and income protection granted through publicly funded DI programmes?

The present work aims at contributing toward answering similar questions, offering evidence on employees' long-run adjustments to health deteriorations in Italy, a country featuring a highly regulated labour market with limited scope for hours or wage adjustments, generally strong employment-protection policies and comparatively low labour flow indicators. Thanks to an institutional discontinuity, the Italian context offers the possibility of measuring the causal effect of EPL – even if not specifically targeted to workers experiencing health deteriorations – on their labour market inclusion and DI participation.

Most of the exiting empirical studies on the labour consequences of health shocks focus on the US, Anglo-Saxon, Nordic countries and the Netherlands, a pattern that reflects the availability of appropriate data sources in these countries (e.g., Dobkin et al 2018, Au et al. 2005, Zucchelli et al. 2010, Heinesen and Kolodziejczyk 2013, Maczulskij and Böckerman 2019, García-Gómez et al. 2013). However, compared to others, these countries feature higher levels of job mobility,[3] more limited job protections and, in the case of the Netherlands and Nordic countries, more generous disability policies in terms of coverage and integration (see European Commission, 2009; OECD, 2016).[4] Along these dimensions, they differ remarkably from Southern European[5] countries like Italy.

Studying the Italian institutional context is possible thanks to the availability of a new administrative dataset, WHIP&Health, which covers the work and social security histories (1990–2012) of a random sample drawn from the Italian Social Security (INPS, *Istituto Nazionale della Previdenza Sociale*) archives, linked to individuals' hospital discharge records from all private and public hospitals. We study workers aged 18–64 and their response to acute forms of cardiovascular disease (CVD) experienced between 2003 and 2011. We measure the behavioural response in terms of employment, annual income from labour and receipt of disability insurance, for up to 9 years later, and extend previous evidence by also covering the channels of adjustment for those who maintain employment, i.e. the effect on hourly wage and working times.

Administrative data on hospitalisations overcome several measurement error challenges typically encountered with survey data and self-reported measures, spanning from recall to justification biases (Baker et al., 2004; Benitez-Silva et al., 2004; Jäckle and Himmler, 2010; d'Uva et al., 2008). Multiple reasons underpin the choice of focusing on acute CVD shocks. They are highly prevalent[6] and often lead to physical and mental impairments, limiting daily activities and the ability to work. In addition, the focus on acute CVD relates to the issue of health endogeneity. We consider unplanned hospitalisations for myocardial infarction (ischaemic heart disease) or stroke (cerebrovascular disease), the onset of which is clearly attributable to a specific, yet unpredictable, point in time. They offer a source of unexpected variation[7] in health because, although people may anticipate their risk of illness, they cannot anticipate the timing of actual shock occurrence. At the same time, the risk is well known to depend on established factors (Braunwald et al., 2015), which aids in selecting an appropriate comparison group. To this end, a wide observational time window allows a long history record, up to twenty years before the health shock occurrence, to be exploited.

Identification follows two approaches: the main one, used to study long-term responses in terms of Average Treatment Effect on the Treated (ATT), combines coarsened exact matching and entropy balancing procedures on individuals who are affected or not affected by a CVD shock at a particular point in time. The second approach, which can be applied only to short- to medium-run dynamics, exploits individuals who experience the same CVD shocks five years later as a control group that is plausibly less exposed to any unobserved heterogeneity remaining under the first approach. The two identification approaches deliver similar short- to medium-run adjustments to health shocks, which are robust to the inclusion of weights accounting for selective mortality. We then exploit a firm-size-related discontinuity in EPL under a RDD framework to measure their effect on workers' labour market inclusion and DI participation after the onset of a major health event.

Our work's first contribution is offering – through our results - a picture of permanent employment effects of health shocks experienced when working in a rigid labour market. In the context we study, the bulk of adjustments happen along the extensive margin, and become permanent, with no evident employment recovery mechanism. For workers who maintain employment, there appears to be very limited scope to adjust working times flexibly, or wages to lower productivity, two routes that, if viable, might cushion against the alternative of losing or leaving employment. Instead, previous studies in the US[8] (e.g. Charles 2003, Dobkin et al. 2018) found hours worked to be an important channel of adjustment to health shocks. In Italy, existing rigidities may force workers to withdraw from the labour force and enter the DI system as the only viable option to replace labour income. This condition becomes an absorbing state, especially in a sticky labour market featuring low hiring rates.

The second contribution of this work is the provision of new evidence on the value of EPL in raising the chances that a worker experiencing a health deterioration does not leave employment. Such evidence feeds into a well-developed literature on the causal effects of ELP on firms' and workers' outcomes, such as employment, wages, inflow and outflow from (un)employment (e.g. Miles, 2000; Autor et al., 2006; Hijzen et al., 2017; Boeri and van Ours, 2008, Boeri and van Ours, 2021) mental well-being and health (e.g., Caroli and Godard, 2016). However, to the best of our knowledge, the causal literature has so far overlooked the value of EPL for unhealthy workers, which bears important implications in

[3] The Netherlands is a notable exception, featuring comparatively low hiring rates: in 2006 the hiring rate for older workers (aged 55-64), measured as the percentage of employees with job tenure of less than one year, was 1.7 (against an OECD average of 9.2).

[4] According to OECD, 2003, disability *compensation* measures reflect coverage, minimum disability level, disability level for a full benefit, maximum benefit level, permanence of benefits, medical assessment, vocational assessment, sickness benefit level, sickness benefit duration and unemployment benefit level and duration. The *integration* dimension instead reflects coverage consistency, assessment structure, employer responsibility for job retention and accommodation, supported employment programme, subsidised employment programme, sheltered employment sector, vocational rehabilitation programme, timing of rehabilitation, benefit suspension regulations and additional work incentives.

[5] To our knowledge, country-specific evidence on Southern European countries includes only García-Gómez and Lopez Nicolás (2006) for Spain. It should be noted that Spain exhibits higher hiring rates than other Southern EU countries, in relation to the large use of temporary contracts. In 2006 the hiring rate for older workers was 7.7, while the same indicator was 4.0 for Italy, and the OECD average was 9.2 (Horwitz and Myant, 2015). Indeed, existing studies on Spain measure a larger effect of health deteriorations than found for other Southern European counties in comparative studies.

[6] Over the past 25 years, the incidence of CVD cases has increased in most European countries, including Italy. Data on the crude prevalence in 2015 depict an impressive situation, as more than 85 million people across Europe were living with CVD.

[7] For example, some authors focus on accidents (Dano, 2005; Halla and Zweimüller, 2013; Parro and Pohl, 2021), unplanned hospitalisations for a variety of health conditions (García-Gómez et al., 2013; Lindeboom et al., 2016), or the onset of certain kinds of major health shocks (Bradley et al., 2013; Coile, 2004; Datta Gupta et al., 2015; Jones et al., 2020; Smith, 1999, 2005; Trevisan and Zantomio, 2016).

[8] Information on hourly wages and hours worked are often not available (or exploited) in European studies, and therefore these channels of adjustment have not been thoroughly explored.

terms of (saved) social expenditure and (reduced) health and income inequalities transmission (as discussed in Reeves et al. 2014). This is important in light of theoretical and empirical works highlighting how the employment protective legislation specifically targeted to the disabled might bear adverse effects of reduced employment for the intendedly protected group (see Acemoglu and Angrist, 2001 and literature cited therein).[9]

These results are also relevant to the stream of literature which studies how different forms of public protection might *de facto* act as substitutes. So far, evidence has documented the use of DI as an insurance against non-health-related permanent shocks, as revealed by the increased DI applications/receipts visible under worsened economic conditions (see Black, Daniel and Sanders (2002) and more recently, Pasini and Zantomio (2013) and Maestas, Mullen and Strand (2015) on the Great recession in Europe and the US respectively). Here, we discuss how EPL might instead lower DI applications through an increase in the employment security of unhealthy workers.

The rest of the paper is organised as follows. Section 2 discusses the theoretical underpinnings setting the scene for the following empirical analysis. Section 3 describes the Italian labour market rigidities and the available DI insurance programmes. Section 4 illustrates data, sample selection and the identification approaches. Section 5 presents our findings, and finally, Section 6 discusses the results and concludes.

2. Theoretical underpinnings

According to theories of health capital (Grossman, 1972a, Grossman, 1972b), health deteriorations might reduce labour supply through several channels, including an increase in the fixed cost of going to work, a higher disutility of work and not least, updated expectations on the remaining lifespan (Finkelstein et al., 2013 [10]; Chang, 1991; Hamermesh, 1984.[11] In the absence of employment-contingent health insurance (e.g., in the US, see Bradley et al., 2013), one can unambiguously expect a reduction in optimal labour supply, potentially along both margins. Some workers' preferred choice might be to leave the labour market, while others – under lower fixed costs of working - might prefer to reduce labour supply along the intensive margin (i.e. hours worked). On the other hand, some individuals might be keen to maintain the same labour supply, even if faced with reduced hourly wage prospects resulting from decreased productivity.

Optimal workers' responses will be shaped by personal characteristics such as the extent of work ability loss brought by the health deterioration and relative preferences over income and leisure. However, also institutional factors contribute to shaping workers' labour supply re-optimization: for example, the availability and generosity of Disability Insurance (DI) programmes offering an earnings replacement. If DI eligibility requires leaving the labour market (as is often the case, the benefit being strictly targeted to those unable to work, or earnings-tested), DI programmes might be expected to contribute to a further reduction in optimal labour supply (Bound, 1989; Gruber, 2000; Chen and van der Klaauw, 2008; Maestas et al., 2013, amongst others). Other institutions, such as mandatory requirements for on-the-job accommodation for affected workers, might instead mitigate the preferred labour supply reduction (Burkhauser et al., 1995; Charles, 2005; Hill et al., 2016).

However, actual labour adjustment also reflects the employer's response to a worker's loss of productivity relatable to health deterioration. Productivity shocks lead firms to reduce their cost of employing labour, with possible channels including reductions in hourly remuneration, working times, or, ultimately, dismissals.[12] Again, and remarkably, firms' and employees' responses are shaped by institutions. Theory predicts that higher labour market rigidities entailed by labour market institutions increase the chance of frictions, leading to the dismissal of less productive workers. In more detail, the more limited the chances to adjust wages to lower productivities, or the higher the cost of employing part-time workers (in relation to the fixed-cost of hiring workers), the more likely employers' labour cost reduction will be achieved through reducing employment.

Typically, though, more rigid labour markets feature employment protection legislation (EPL) provisions which limit the chance of worker's dismissal through increased firing costs. Also, employees are more prone to invest in specific rather than general skills if expecting a longer tenure in the firm, which becomes more likely under stricter EPL. This makes the employee more productive, further reducing the risk of dismissal (Berton and Garibaldi, 2012). A vast literature agrees that stricter EPL reduces workers' turnover and the flow of workers in and out of (un)employment (see, e.g., Bertola and Rogerson, 1997; Hopenhayn and Rogerson, 1993; Bentolila and Bertola, 1990).

A stricter EPL can thus be expected to provide better employment protection to certain vulnerable groups such as workers in poorer health and disabled workers. Non-disability targeted EPL might unambiguously foster the labour market inclusion of workers experiencing a health deterioration, saving social welfare funds allocated to providing earnings replacement programmes (Cazes and Verick, 2013).

Overall, theoretical predictions lead us to expect that first, in a rigid labour market, health shock adjustments will arise in terms of reduced employment (rather than in terms of reduced hours or wages), to a larger extent than it would have resulted if these margins of adjustment were also viable. Second, in a rigid labour market with low turnover, we expect very limited chances for a worker who exits employment after a health deterioration to re-enter at a later stage. Third, workers enjoying stronger EPL legislation will experience, other things equal, higher chances of maintaining employment after a health deterioration, with respect to workers subject to milder institutional protection. Fourth, we expect a larger value of EPL for blue-collar workers engaged in more physically demanding jobs and tasks that are generally less amenable to accommodate health-related work limitations. In the following sections, we explore the empirical counterpart to such hypotheses.

3. Institutional framework

3.1. The Italian labour market

Based on comparative labour market indicators over the period covered by our study, Italy features a highly regulated market in which firms are strictly limited in their ability to adjust hourly wages, require overtime work, make workers redundant, or engage in firm-level negotiations. The OECD *Strictness of Employment Protection* indicator (ranging from 0 to 5) scores Italy at 2.7, a value close to those of other Southern

[9] Acemoglu and Angrist (2001) consider the introduction of the Americans with Disabilities Act (ADA) – a law implemented in 1990 intended to protect people with disabilities from discrimination – and show ex-ante theoretically ambiguous effects on employment of the disabled, and empirically that the negative effect dominates. More recently, Kim and Rhee (2018) found that due to ADA regulation firms were less likely to fire workers but the hiring selection became stricter.

[10] By comparing older Americans with and without chronic illness, Finkelstein et al. (2013) infer that the marginal utility of consumption decreases with ill-health. If the marginal utility of leisure rises when health falls, the reservation wage rises, and labour-force withdrawal becomes more likely (O'Donnell et al., 2015).

[11] In the standard life cycle model of consumption with no bequest motive in which there is dissaving before death, a longer length of life is predicted to increase labour supply (and saving) at any given age (O'Donnell et al., 2015).

[12] Younger workers, who have accumulated less firm-specific human capital , or low-skilled workers, who can be more easily substituted, will thus be more likely to experience dismissal. Becker's (1964) model suggests that employers and employees face different incentives in training: the former avoid investing in general human capital that may benefit competitors through employees' mobility; the latter do not pay for training required for a specific firm.

European countries (e.g. Greece, 2.8) and higher than those for Anglo-Saxon countries (e.g. UK, 1.1) or OECD countries as a whole (2.08 in 2012). Employment protection legislation has historically been particularly high for workers in open-ended[13] contracts within medium and large companies (i.e. with more than 15 employees), whose dismissal was not allowed during the study period, and that represents the prevailing case in our data (see Section 4.3). In more detail, during the period we study, when unfairly dismissed, employees under open-ended contracts and working in a firm with more than 15 employees could ask to be reinstated and receive wages and social security contributions forgone in the period between the dismissal and the sentence. On the contrary, employees in firms with up to 15 employees[14] would be subject to the employer's decision concerning reinstatement or rather a severance payment ranging from 2.5 to 14 months; even under reinstatement, the worker would not receive forgone wages.

Besides employment protection, a high level of regulation is confirmed by the OECD *Trade unions and Collective Bargaining* indicators. The collective bargaining coverage rate was 80% in the 1998–2016 period, similar to Spain, Portugal and Greece before the crisis, against an OECD average of 33%. Although Italy has no legal minimum wage, it is de facto set through collective bargaining agreements sector-by-sector. The resulting compensation structure is particularly rigid[15] and displays a life-cycle profile of hourly wages that differs evidently from those of other countries (Contini, 2019). For many years, in fact, Italy has been the only European country in which remuneration did not decline at older ages[16] because of wages linked to seniority until retirement, especially in large firms and under prevailing open-ended contracts. Such rigidity could result in frictions that increase labour mobility and workers' reallocations; however, Italy is ranked at the bottom for hiring, separations and turnover (European Commission, 2009).

The majority of existing part-time work is involuntary: in 2018, the share of male voluntary part-timers as a percentage of total male employment was only 1.5%, increasing only slightly for older men aged 55 to 64 (3.1%) and in stark contrast to the corresponding OECD figures (7.5 and 7.1% respectively, according to OECD, 2019). Further evidence from Eurostat (2019) reveals that the prevalence of part-time contracts amongst male workers aged 45 or older who suffer health-related limitations is only 12%, a figure that places Italy in the penultimate position amongst EU28 countries.

3.2. Sick leave and disability insurance

Italian employees are entitled to receive sick leave for a maximum of 180 days per calendar year. After 180 days, the employer may rescind the contract if the employee does not return to work. However, if the employee returns to work, the employer cannot dismiss her based on health-related limitations unless proving (the difficult burden of proof being on the employer) that the health limitation and available tasks are such that no accommodation can be envisaged.

In case a health shock results in disability, mandatory employment quotas are provided for disabled workers[17] and apply in relation to firm size. The quota amounts to 7% for firms employing more than 50 employees, but decreases to 2 employees for firms with 36–50 employees and to only 1 for firms with 15- 35 employees. No quota applies in the case of businesses with less than 15 employees.

Income protection against health-related risk is offered through two disability schemes, which require at least five years of social security contributions paid and at least three in the five years preceding the application. The first one, called ordinary incapacity benefit (*Assegno ordinario di invalidità*), is a temporary benefit for certified mental or physical impairments reducing working ability by at least two-thirds. It is important to stress that this DI benefit is compatible with working activity,[18] a feature that distinguishes it from the prevailing case in other countries, i.e. a benefit incompatible with earning significant labour income (Low and Pistaferri, 2020). It lasts three years and can be renewed twice - upon a medical screening - before becoming permanent: it is then absorbed into the old-age pension when the claimant reaches the minimum age requirement. The second programme, called disability pension (*Pensione di inabilità*), is payable to claimants who present permanent and total inability to perform any kind of work and as such is not compatible with any work activity. The 100% incapacity required to be entitled is the highest even amongst Southern European countries, with Spain and Greece requiring 33% and 50%, respectively (MISSOC 2021).

The amount of both benefits is relatively generous, as it is computed according to the old-age pension formula. Under the first benefit, for example, a worker claiming at age 50, after thirty years of contributions, would be entitled to a gross replacement rate of about 60%.[19] The second benefit is more generous than the first because it entails the addition of a sizeable contributory bonus (Belloni and Maccheroni, 2013). However, while the Italian net replacement rate is in line with those prevailing in many other European countries,[20] the Italian DI scores worse in terms of achieved coverage (i.e. a 5.5% recipiency rate, as opposed to the 9.9, 8.7 and 7.9% rates achieved in Sweden, the Netherlands and Denmark respectively, according to Applica & Cesep & European Centre, 2007), plausibly in relation to the level of incapacity required for entitlement.

4. Empirical approach

4.1. Identification

Ideally, the causal effect of health deterioration would be measured as the difference in individual outcome $Y_{i,t}$ observed for individual i at time t simultaneously in two states of the world. In the first, the CVD shock event T occurs for individual i at time $\bar{t}$ ($T_{i,\bar{t}} = 1$), yielding outcome $Y^1_{i,t}$; in the other, it does not ($T_{i,\bar{t}} = 0$), yielding outcome $Y^0_{i,t}$. In that case, we could estimate the average treatment effects on the treated (i.e. on an individual who is affected by the CVD shock) $ATT_{\bar{t}+v}$ at time $\bar{t}+v$,

[13] According to the OECD (2016), the incidence of temporary work for those aged 55-64 was only 6.4% in 2006, decreasing to 5.8% in 2016, against corresponding OECD figures of 8.9% and 7.9%, respectively.

[14] These are widespread in the Italian productive panorama in comparison with other OECD countries.

[15] Devicienti et al. (2007) provide evidence of significant downward wage rigidity, with real rigidity prevalent over nominal rigidity. After 1991, Italy experienced a trend of declining union power and an increasing role of local wage-setting. Nevertheless, the influence of local wage bargaining has always been modest. Devicienti et al. (2007) report a wage drift of about 1%.

[16] Nordic European countries and the UK exhibit a peak of wages around 45 years old, whereas in Italy wages continue to raise until the worker is 60 years old.

[17] It is worth stressing that experiencing an acute CVD shock of the kind we consider increases the chances of developing a disability but does not necessarily result in disability.

[18] The benefit is reduced by 25% (50%) when labour income is greater than four (five) times the minimum pension (i.e. euro 26.676,52 or euro 33.345,65 in 2019).

[19] In the period covered by our study, the prevailing old-age pension schemes were a more generous defined benefit scheme that applied to workers with more than 18 years of contributions paid by the end of 1995, and a less generous mixed (defined benefit/notional defined) contribution scheme paid to other (broadly, younger) workers.

[20] The net replacement rate (RR) of a long-term disability benefit for a 55-year-old individual (with a 100% incapacity to work) as of 2005 is above 70 percent in Belgium, the Netherlands and Spain, between 60-70 in Italy, Sweden, Austria and Luxemburg, lower than 60 in Portugal, France, Malta and others (Palme et al., 2009).

that is, v years after the CVD shock, as:

$$E\left[Y^1_{i,\bar{t}+v} - Y^0_{i,\bar{t}+v} \mid T_{i,\bar{t}} = 1\right] = E\left[Y^1_{i,\bar{t}+v} \mid T_{i,\bar{t}} = 1\right] - E\left[Y^0_{i,\bar{t}+v} \mid T_{i,\bar{t}} = 1\right]$$

In practice, though, an individual will only experience–and be observed–in one state, so the two potential health states ($T_{i,\bar{t}} = 1,\ T_{i,\bar{t}} = 0$) and their corresponding labour outcomes ($Y^0_{i,t},\ Y^1_{i,t}$) are never simultaneously observed. We model the counterfactual unobserved outcome under the assumption of unconfoundedness, or conditional independence (Rosembaum and Rubin, 1983). In our context, the assumption can be formulated as:

$$\left(Y^0_{i,t}, Y^1_{i,t}\right) \perp T_{i,\bar{t}} \mid \left(W_i,\ X_{i,\bar{t}-s}\right)\ s = 1 \ldots \ldots S,$$

where W_i is the individual time-invariant characteristics and $X_{i,\bar{t}-s}$ is the time-varying ones, including labour, social insurance and health histories (indicative of individuals' underlying CVD risk), observed s years before the shock and up to past time S. Under unconfoundedness, conditioning on the observables W_i *and* $X_{i,\bar{t}-s}$ makes both potential outcomes independent with respect to the treatment status, and the conditional probability of experiencing an acute CVD shock in $\bar{t}$ as good as random. The assumption would be violated if unobservables systematically differed between individuals who experience the $T_{i,\bar{t}} = 0$ state and those who experience the $T_{i,\bar{t}} = 1$ state. Therefore, while untestable, its credibility relies on the scope of the available data, a point to which we come back after presenting our data in more detail.

A second assumption for identification requires some overlap in the distribution of observables W_i *and* $X_{i,\bar{t}-s}$ between individuals who experience the health shock and those who do not, so the conditional treatment probability for both groups is:

$$0 < pr(T_{i,\bar{t}} = 1 | W_i = w,\ X_{i,\bar{t}-s} = x) < 1$$

Under both assumptions - that is, under strong ignorability (Rosenbaum and Rubin, 1983) - the $ATT_{\bar{t}+v}$ at time $\bar{t} + v$ - that is, v years after the CVD shock, denoted by $\tau_{\bar{t}+v}$- is identified as:

$$\tau_{\bar{t}+v} \equiv E[Y^1_{i,\bar{t}+v} - Y^0_{i,\bar{t}+v} | W_i = w,\ X_{i,\bar{t}-s} = x]$$

$$\equiv E[Y^1_{i,\bar{t}+v} | W_i = w,\ X_{i,\bar{t}-s} = x] - E[Y^0_{i,\bar{t}+v} | W_i = w,\ X_{i,\bar{t}-s} = x].$$

4.2. Data

WHIP&Health is an administrative dataset that combines the work, social insurance and health histories of a 7% random sample of workers covered by the INPS, that is, all private-sector workers except agriculture. The first component, the Work Histories Italian Panel (WHIP), which covers from 1990 to 2012, is a rich employer-employee database of detailed information about each period of employment (e.g. starting and ending dates, qualification, sector of activity, firm identifier, firm dimension, region of employment, labour income, type of contract). Additional information includes other types of work periods (i.e. self-employment or atypical work) and non-work periods like unemployment. Information on receipt of a variety of social security programmes—including temporary and permanent disability programmes, unemployment, and old-age pensions—is also available, although not the amount received.

Demographic information covers the years of birth and death, place of birth, and gender.

The health component is drawn from the hospital discharge records (or SDO, i.e., *Schede di dimissione ospedaliera*) registry, provided by the Italian Ministry of Health, that collects information on all types of hospitalisations between 2001 and 2012. Variables include the main and the secondary diagnoses according to the ICD codes (ICD-IX); the year and month of hospitalisation; the type of dismissal, which allows deaths that occurred while in the hospital to be identified. Thus, we are able to identify unplanned hospitalisations related to an acute CVD shock that does not result in death in the same year: ICD-IX codes 410–414 (ischaemic diseases), 430–434 and 436–437 (cerebrovascular diseases). To strengthen the case for regarding the timing of a shock as unanticipated, we disregard both unplanned hospitalisations that are related to other major conditions the onset of which is not related to a specific point in time, and unplanned hospitalisations following injuries or accidents whose conditional risk distribution is not as easily traceable to observable risk factors.

4.3. Research design and sample selection

For the assumption of unconfoundedness to be credible, as much previous labour and health history information must be observed as possible. At the same time, we aim at evaluating the effect in the longer term. To balance the two, we place the time window of CVD shocks in the years 2003–2011 so we can observe up to s=20 years of previous labour and social insurance history (i.e. for individuals who experienced the CVD shock in 2011, with WHIP variables dating back to 1990), along with up to $v = 9$ years of labour outcomes past the health shock year (i.e. for individuals who experience the CVD shock in 2003, with WHIP variables observable up to 2012). Fig. 1 clarifies the time window covered by WHIP and SDO components of WHIP&Health, and how we exploit them.

The sample for analysis consists of men and women aged between 18 and 64 years old who were employed as blue- or white-collar workers between 2003 and 2011. The upper age limit corresponds to the year prior to the statutory retirement age in the analysed period. We exogenously set the upper age bound as commonly done in the related literature to avoid potentially endogenous sample selectivity induced by individual retirement decisions.[21] Because of unobserved heterogeneity concerns, we also restrict the sample to individuals continuously employed[22] in the four years before the treatment year $\bar{t}$ and who did not experience an acute CVD shock in the two previous years (i.e. $\bar{t} - 1;\ \bar{t} - 2$). We exclude cases with missing or inconsistent information on relevant variables.

The resulting sample consists of 911,863 individuals: amongst them, the 8586 individuals who do experience an acute CVD shock between 2003 and 2011 represent a very large 'treated' subsample.[23] While they might have experienced recurrent CVD events within the treatment window, we consider only the first shock observed within the 2003–2011 window as the reference shock. In line with the national and international statistics (see Wilkins et al., 2017), most cases involve ischaemic diseases (72.2%), and only about one in five (27.4%) are cerebrovascular events. It is worth stressing that WHIP is representative of workers and for this reason, larger firms are overrepresented in the data. Therefore, most workers in our sample are employed in medium and large companies (about 66% of workers) and hired under open-ended contracts (more than 92% of workers) and, hence subject to a higher level of EPL.

Table 1a and 1b show descriptive statistics for our working sample. In addition to basic demographics and health history variables, they include a large set of retrospective labour and social security history variables that reconstruct workers' past for up to 20 years. We derive multiple summary indicators of labour market trajectories, as well as time-and job-specific characteristics for previous employments to reduce the influence of time-varying unobservables to the extent they are correlated with observed confounders. We also include time-specific lagged outcomes, which addresses potential bias from time-invariant unobservables (O'Neill et al., 2016). Other unobserved heterogeneity concerns might arise from, for example, a lack of available information on genetic

[21] In our data, age 65 is a mass retirement point, although many individuals leave work earlier through early retirement schemes (in which case, we consider them as 'not in employment' in later analysis).

[22] This condition results in dropping a minor proportion of individuals in our sample.

[23] The sample of treated individuals counts 88.6% men and 11.4% women; 66.3% are blue-collar and 33.7% are white-collar workers.

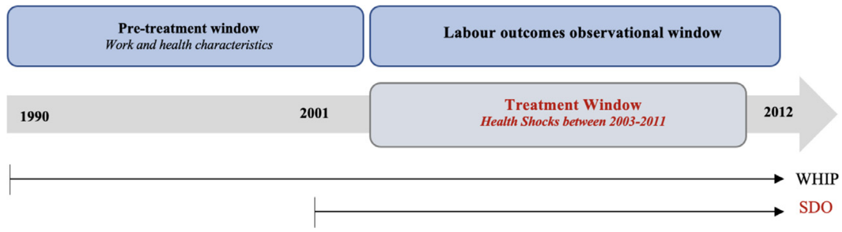


Fig. 1. Dataset time coverage and related identification strategy.

Table 1a
Descriptive Statistics.

	Treated		Controls	
Variable	*Mean*	*Sd*	*Mean*	*Sd*
Year (of CVD shock, for the treated)	2007	2.547	2007	2.563
Age (when the CVD shock occurs, for the treated)	50.75	7.236	40.64	9.385
Woman	0.114	0.318	0.332	0.471
Area of birth (north)	0.358	0.479	0.446	0.497
Area of birth (centre)	0.146	0.354	0.152	0.359
Area of birth (south or islands)	0.411	0.492	0.273	0.445
Area of birth (abroad)	0.085	0.279	0.129	0.335
Born in a developing country	0.075	0.264	0.113	0.317
Ever hospitalised for CVD until $(\bar{t}-1)$	0.040	0.195	0.002	0.043
Days spent in hospitals for a CVD shock until $(\bar{t}-1)$	0.437	3.105	0.018	0.610
Ever hospitalised for *other type of diseases* until $(\bar{t}-1)$	0.425	0.494	0.343	0.475
Days in hospitals for *other type of diseases* until $(\bar{t}-1)$	4.391	13.14	2.498	8.778
Hospitalisations for *other types of diseases* in $(\bar{t}-1)$	0.180	0.547	0.111	0.418
Days in hospitals for *other types of diseases* in $(\bar{t}-1)$	0.867	4.358	0.451	3.185
Ever received *ordinary invalidity benefits* until $(\bar{t}-1)$	0.301	1.621	0.032	0.506
# weeks on sick leave until $(\bar{t}-1)$	15.53	24.22	8.775	15.88
Years of LM activity until $(\bar{t}-1)$	15.17	3.938	12.95	4.747
# employment contracts until $(\bar{t}-1)$	17.14	5.026	15.35	5.736
% years as an employee/total worked until $(\bar{t}-1)$	97.00	11.13	98.00	8.539
# involuntary job losses until $(\bar{t}-1)$	0.183	0.477	0.190	0.467
# employer changes until $(\bar{t}-1)$	2.321	1.696	2.363	1.701
# contracts as a blue-collar until $(\bar{t}-1)$	10.23	7.616	7.779	6.970
# contracts as a white-collar until $(\bar{t}-1)$	4.534	6.893	4.613	6.471
Permanent contracts/total contracts until $(\bar{t}-1)$	93.29	20.06	88.78	24.82
% full-time contracts/total contracts until $(\bar{t}-1)$	0.295	0.456	0.230	0.421
Ever in *cassa integrazione guadagni* until $(\bar{t}-1)$	0.408	1.302	0.428	1.336
Unemployment benefits received until $(\bar{t}-1)$	0.408	1.302	0.428	1.336
Whether received unemployment benefits in $(\bar{t}-1)$	0.039	0.193	0.060	0.237
Years self-employed/total years worked until $(\bar{t}-1)$	3.403	12.75	2.029	9.615
Days self-employed until $(\bar{t}-1)$	200.9	782.8	121.79	605.0
Years as an atypical worker/total worked until $(\bar{t}-1)$	1.436	7.502	1.742	7.583
# contracts as atypical worker until $(\bar{t}-1)$	0.235	1.444	9.213	1.146

Source: WHIP&Health.

or behavioural risk factors (e.g. smoking, eating habits, physical activity) that are correlated with labour market outcomes, a point to which we return in Section 4.5. However, our results would not be invalidated if, besides genetic invariance over time, these behaviours were stable over time: in this case, their effect would be purged via the inclusion of lagged outcomes.

4.4. Implementation of main identification strategy for long term dynamics

Before any adjustment in composition, the distribution of characteristics varies between treated and control individuals (standardised percentage bias and p-values on mean difference are shown in Appendix Tables A1.a and A1.b), revealing selection in experiencing CVD shocks. Individuals who experience an acute CVD shock are on average older, have poorer previous health outcomes and have significant differences in labour market outcomes, possibly related to their different age distribution.

Following Ho et al. (2007), we compute ATTs by combining preprocessing procedures to balance the distribution of observed confounders through matching techniques, with later parametric estimation conducted on matched samples. Matching removes systematic links between treatment assignment and confounders that would cause bias and reduces the risk of model dependence in the following parametric estimates. Parametric estimation contributes to remove bias from imbalances remaining after matching, an occurrence that is not uncommon when non-exact matching procedures are used.[24]

As in Jones et al. (2020), and for similar reasons, we implement the balancing adjustment in two steps: coarsened exact matching (CEM) (Iacus et al., 2011) along a limited set of basic confounders, and entropy balancing matching (EB) (Hainmueller, 2012) on the full set of observed potential confounders. CEM and EB balancing adjustments are preferred to other commonly employed matching procedures (like propensity score matching or inverse probability weighting) because they both impose ex-ante a balancing target to be reached, and as such tend to achieve a better balancing of observed confounders, functional to minimizing avoidable bias. CEM performs an exact matching between

[24] This double-step procedure allows ATTs that are robust to model misspecification to be obtained. This two-step approach is regarded as doubly robust because consistency requires only that either the parametric component or the nonparametric component is consistently estimated (Ho et al., 2007).

Table 1b
Descriptive Statistics.

	Treated		Controls	
Variable	*Mean*	*Sd*	*Mean*	*Sd*
Weeks on sick leave ($\bar{t}$−1)	1.458	4.219	0.819	2.877
Paid weeks ($\bar{t}$−1)	49.36	8.723	47.71	10.85
Working under a permanent contract ($\bar{t}$−1)	0.048	0.213	0.080	0.271
Years under the same employer ($\bar{t}$−1)	10.38	7.742	7.631	6.750
Area of work (North) ($\bar{t}$−1)	0.536	0.499	0.613	0.487
Area of work (Centre) ($\bar{t}$−1)	0.186	0.389	0.192	0.394
Area of work (South or islands) ($\bar{t}$−1)	0.278	0.448	0.194	0.396
Area of work (Abroad) ($\bar{t}$−1)	0.001	0.029	0.001	0.027
Job qualification (blue-collar) ($\bar{t}$−1)	0.662	0.473	0.597	0.490
Job qualification (white-collar) ($\bar{t}$−1)	0.261	0.439	0.344	0.475
Job qualification (manager) ($\bar{t}$−1)	0.057	0.228	0.049	0.18
Job qualification (director) ($\bar{t}$−1)	0.020	0.141	0.010	0.099
Firm size (0 to 15 employees) ($\bar{t}$−1)	0.280	0.449	0.342	0.474
Firm size (16 to 250 employees) ($\bar{t}$−1)	0.384	0.486	0.365	0.481
Firm size (more than 250 employees) ($\bar{t}$−1)	0.336	0.472	0.293	0.455
Sector: Agriculture ($\bar{t}$−1)	0	0	0.0003	0.019
Sector: Manufacturing ($\bar{t}$−1)	0.408	0.492	0.431	0.495
Sector: Construction ($\bar{t}$−1)	0.115	0.319	0.096	0.297
Sector: Mineral extraction ($\bar{t}$−1)	0.007	0.083	0.003	0.056
Sector Energy ($\bar{t}$−1)	0.020	0.141	0.013	0.111
Sector Trade ($\bar{t}$−1)	0.123	0.328	0.169	0.375
Sector Food and hotel services ($\bar{t}$−1)	0.039	0.193	0.050	0.219
Sector Transports ($\bar{t}$−1)	0.105	0.306	0.066	0.249
Sector Finance services ($\bar{t}$−1)	0.173	0.378	0.158	0.364
Sector Real estate services ($\bar{t}$−1)	0.007	0.084	0.007	0.083
Sector Public services ($\bar{t}$−1)	0.004	0.061	0.007	0.085
Annual earnings ($\bar{t}$−1)	30,921	25,780	26,002	89,230
Annual earnings ($\bar{t}$ − 2)	30,458	24,879	25,512	89,097
Annual earnings ($\bar{t}$−3)	29,939	23,913	24,876	89,020
Annual earnings ($\bar{t}$−4)	29,087	23,922	23,672	89,507
Hourly wage ($\bar{t}$−1)	15.91	12.05	14.16	43.11
Hourly wage ($\bar{t}$ − 2)	15.60	11.85	13.87	43.05
Hourly wage ($\bar{t}$−3)	15.44	11.13	13.56	43.05
Hourly wage ($\bar{t}$−4)	15.2	11.19	13.23	44.14
Full-time ($\bar{t}$−1)	0.919	0.273	0.867	0.339
Full-time ($\bar{t}$ − 2)	0.922	0.269	0.872	0.334
Full-time ($\bar{t}$−3)	0.923	0.267	0.876	0.330
Full-time ($\bar{t}$−4)	0.920	0.271	0.877	0.328
Ordinary Incapacity benefit ($\bar{t}$ − 1)	0.046	0.210	0.010	0.075
Ordinary Incapacity benefit ($\bar{t}$ − 2)	0.040	0.196	0.005	0.068
Ordinary Incapacity benefit ($\bar{t}$ − 3)	0.036	0.185	0.004	0.062
Ordinary Incapacity benefit ($\bar{t}$ − 4)	0.030	0.170	0.003	0.056

Source: WHIP&Health.

treated and control individuals and holds the important Monotonic Imbalance Bouding property (Iacus et al., 2011) of reducing the imbalance in selected variables (while implementing common support on these) without worsening the balancing in other variables, a disadvantage that that other procedures based one propensity score estimations might entail.[25] However, a greater number of variables involved in CEM or a finer coarsening applied to non-dichotomous variables results in a higher proportion of cases discarded because no exact match is found. To avoid the loss of observations that would result from exact matching on all covariates, we include in CEM only a few basic major confounders.[26]

To remove imbalances that remain in the larger set of potential confounders, we then apply EB matching on the CEM-retained samples of treated and control individuals. The EB procedure reweights observations such that variables' distribution satisfy a set of specified moment conditions (Hainmueller et al., 2012), imposing ex-ante a desired level of sample moment adjustment. We impose a first moment condition on the extended set of variables obtaining a remarkable overlap, as shown in Appendix Tables A1.a, A1.b (first moment) and A2.a-A2.b (for higher moments). In the pre-processed samples, the *bias,* measured as the standardised percentage difference in means between treated and matched controls, is reduced to zero for all variables with a few exceptions, where it does not exceed -0.2.

Lack of bias in observables does not address the chance of remaining bias stemming from unobservables, particularly the time-varying (given the inclusion of lagged outcomes). Time-varying unobservables would presumably emerge in detectable differences in pre-shock outcomes between treated and matched controls, however no difference is detectable in the four years before $\bar{t}$ (Figs. A1.a and A1.b in Appendix), suggesting

[25] Also, CEM accounts for variables' interactions and nonlinearities. In practice, the CEM algorithm stratifies the sample by subsets of coarsened variables values (or exact variable values in the case of dichotomous variables or if no coarsening is applied). Strata that lack at least one treated and one control unit are dropped; retained individuals are attributed a weight that accounts for the different number of treated and control individuals in each matched stratum.

[26] In CEM, we include (as *uncoarsened*, thus exactly matched) the year of treatment, age, gender, whether the individual had experienced an acute CVD shock, the type of occupation (blue *vs* white-collar), whether the individual worked under a part-time or a full-time contract, and whether the individual was under a fixed-term or open-ended contract as of $\bar{t}$ -1. Two other variables included in CEM are *coarsened* instead: firm size (coarsened into three groups of values: 0-15/16-250/250+ employees) as of $\bar{t}$-1, and region of work, coarsened to a geographical area indicator (north-east, north-west, centre, south and islands).

that the matching procedure has balanced the distribution of pre-shock outcomes with their observed and unobserved determinants.

Finally, we estimate parametric models for each outcome (OLS or probit, according to the continuous or binary nature of each outcome), on a selection of demographic, health and labour history variables. We obtain ATTs by predicting the counterfactual outcome for each treated unit and integrating the difference between the observed outcome and the predicted counterfactual outcome over the sample distribution of treated individuals' covariates. Following suggestion by Ho et al. (2007), as also discussed in Jones et al. (2020), we use standard methods to compute standard errors for inference on the ATTs derived from the parametric regression models estimated on the matched data (with appropriate weights). This is because matching through CEM and EB is not seen as an estimation technique, but rather a pre-processing step to reduce covariates' imbalance and as such only affects the data by balancing on the confounders. The selection of the preferred matching procedure favours the one producing maximum balance, with the others – discarded – not playing a role in inference.[27]

4.5. Alternative identification strategy for medium-run dynamics: exploiting individuals who suffered from a health shock later

One potential limitation of the identification strategy stems from concerns about the role of any time-varying confounder unobserved, particularly if uncorrelated with the observed characteristics included in the matching adjustment. In our setting, the availability of health risk indicators relevant to the treatment assignment appears limited when contrasted with the wide array of labour market information. As a result, one might still expect some systematic differences between treated and controls after matching, such as in relation to health risk knowledge, health-related behaviours like smoking and, more generally, expectations about the distribution of future outcome dynamics, including the occurrence of a CVD shock, all of which may be correlated with labour market behaviour.

An approach that the programme evaluation literature proposes and uses convincingly in diverse contexts—see, for example, Fadlon and Nielsen (2021) for an application in the same thematic area—is that of using as a comparison group the set of units that undergo treatment later in time. In our setting, this approach corresponds to individuals who experience an acute CVD shock a few years later and so might be expected to be similar in terms of unobservables, also in light of the unpredictable timing of CVD shocks. This identification strategy comes at the cost of shortening the time horizon over which the effect can be credibly measured, i.e. the time between the year in which treated individuals experience the shock and the later year in which the controls do. The longer this time is set, the wider the scope for dissimilarity in unobservables between treated and matched control units becomes.

In light of such a trade-off, and our data's time coverage, we exploit individuals who experienced the same shock five years later and obtain estimates for ATTs up to three years later, the fourth being excluded as corresponding to the year prior to health shock occurrence in the control group. For this reason, the approach is presented as an alternative for the medium run results, useful in gauging the robustness of the main identification approach in the shorter run.[28] Appendix Figs. A2.a and A2.b report the post-matching average outcomes for treated and controls, substantially aligned in the pre-shock years up to year $\bar{t}-1$.

A caveat to the use of this strategy in the present context is the possibility that CVD shocks could be reducing labour market activity in the years leading up to the shock, e.g. in relation to a declining health trajectory, with symptoms from narrowing or clogged arteries reducing the ability of workers to exert themselves. If present, such suppression of workers' activity in the comparison group approaching the CVD shock might generate a downward bias in the estimated ATT, with respect to the main identification strategy.

4.6. Selective mortality

CVD is amongst the leading causes of death in developed countries, including Italy.[29] Between 30% and 40% of fatal events in the age range 35–64 occur right after the symptoms start and before the individual reaches the hospital (Ministry of Health, 2010). Our analysis uses only individuals who survive to leave the hospital they first entered when they experienced the acute CVD. Observing the year of death allows us to exclude from the estimation of $ATT_{\bar{t}+v}$ (i.e. $\boldsymbol{v}$ years after the acute CVD shock) individuals who died by $\bar{t}+v$.

However, this approach is not sufficient to address the chance of bias from selective mortality which would result in underestimation of $ATT_{\bar{t}+v}$, an issue that becomes more relevant in long-term analyses. To detect the presence of selective mortality patterns, we estimate $ATT_{\bar{t}+v}$ for the death outcome to determine whether the acute CVD shocks we study increase the probability of later death. Evidence shown in Appendix Fig. A3 signals a differential probability of death, conditional on the individual surviving until she leaves the hospital, that is significant in $\bar{t}+2$. However, if the individual survives until then, we do not detect a significantly different mortality risk.[30] Based on these considerations, it seems unlikely that the mortality-based selectivity issue will bias our findings. Still, in recognition of the potential threat, we exploit the mortality information to derive mortality weights and assess the sensitivity of our results to their inclusion,[31] with results discussed in Section 5.

4.7. Identifying the Role of EPL

To identify the role of EPL in mediating the labour market consequences of health deteriorations, we exploit a sharp firm-size related discontinuity in employment protection which applied until 2012[32] to

[27] So the set of covariates and preprocessing weights can be considered fixed, as in standard regression approaches it is usual that covariates are assumed fixed and exogenous, and no additional correction to standard errors is envisaged.

[28] Due to our data time coverage and the new identification strategy based on the selection of individuals who experience the acute CVD shock *five* years later, the treated group only refers to the years 2003-2006. In this way, the corresponding control individuals are those who experience the shock between 2008-2011. Despite the dramatic drop of observations, the assignment to this alternative control group may reduce the need to adjust for confounders, particularly as far as health characteristics are concerned. As a result, the matching algorithm is simplified and adapted such that CEM involves only year of the treatment, age, gender, type of occupation and whether the individual had experienced an acute CVD shock. After CEM, EB involves all remaining variables (as listed in Table 1a and 1b) with the addition of interaction terms between the variables maintained in CEM and other basic confounders previously included in CEM but excluded in this round. In this way, it is possible extend balancing to variables' interactions and co-moments, beyond the univariate distributions of separate confounders.

[29] For men in particular, CVD diseases are the most common cause of death for those under age 65 in Europe (31% of deaths), compared to about 22% of deaths that are related to cancer.

[30] This result is not necessarily at odds with epidemiologic findings for the general population (e.g., Taylor et al., 2019), considering that our results refer to workers who did not have a CVD shock in the prior two years and are obtained by exploiting an appropriately selected control group that faces a comparable risk of experiencing the same shock.

[31] Assuming that mortality is selective on observables, mortality weights are obtained by estimating a binary model for probability of death, regressed on the same confounders that we controlled for in the main analysis. Weights are given by the inverse of the estimated probability of survival and are integrated into our main ATT estimation procedure.

[32] The 2012 Monti-Fornero reform significantly lowered the firing restrictions that previously applied to medium and large companies. Previously, EPL was based on Article 18 of the Workers' Statute (Law No. 300 of May 20, 1970). The Fornero-Monti reform came into force in July 2012, rewrote in total article

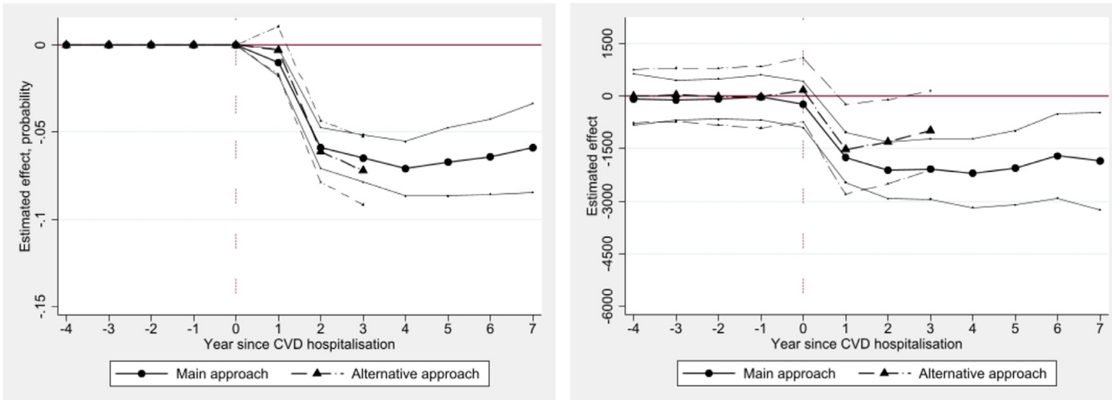


Fig. 2. ATT by year since CVD hospitalisation: Probability of Employment and Probability of receiving a DI benefit.

Source: WHIP&Health.

Notes: ATTs: point estimates (connected lines) and 95% confidence intervals (dashed lines); marginal effects are reported; by sample selection all individuals were employed in the year of the shock and the previous 4 years, so the average difference between treated and controls is zero, and ATT is not estimated in that time range.

unfairly dismissed workers in open-ended contracts. The same discontinuity has been previously exploited by a few studies on Italy seeking to identify the causal effect of EPL on dismissals (Boeri and Jimeno, 2005; Kugler and Pica, 2008), turnover (Hijzen et al., 2017), and employer provided training (Bratti et al., 2021).

As explained in Section 3, during the period we study, legal ELP provision resulted in higher firing costs for firms with more than 15 employees. As discussed in Bratti et al. (2021), the *de jure* discontinuity was further amplified in facts by long duration of labour trials in the country (Gianfreda and Vallanti, 2017), duration variability across regions, and further uncertainty related to regional variability in judges' decisions, overall leading to significantly higher firing costs for firms above the threshold. However, firing legislation is not the only institution that changes discontinuously at the 15 employees cut-off: others include the possibility for firms with more than 15 employees to access a short term programme funding employment in times of restructuring (in relation to economic crisis or insolvency, so called '*Cassa Integrazione Guadagni Straordinaria*', or CIGS); the right to form Work Councils based on the Workers' Statute (Cardullo et al., 2020); and provisions for mandatory employment quotas for disabled workers, which do not apply to firms with less than 15 employees. All of these policy discontinuities can be expected at best to raise - as the firing legislation does - the employment protection and labour integration opportunities for unhealthy workers. Our analysis can be regarded as capturing the combined causal effect of this "bundle" of protective labour market institutions, acknowledging that it would not be possible to separately measure each component–specific causal effect. It is very likely, however, that what we identify is mainly the effect of higher firing costs in bigger firms. Indeed, the CIGS is temporary and concerns collective dismissals, while the health shock may occur in another period. Moreover, in the firm size bandwidths we consider in the empirical analysis, firms above the threshold are required to hire only one disabled worker.

To estimate the causal effect of EPL on shocked workers' outcomes, we adopt an RDD framework which follows Bratti et al. (2021). The analysis is restricted to workers under open-ended contracts (95% of our sample), to which the discontinuity applies. As discussed in Section 4.4, after matching, we estimate parametric models. Here, these are estimated using local regressions (with bandwidths of 6 to 25 employees and 11 to 20 employees). The parametric specification is augmented with a dichotomous indicator equal to one if the worker is employed in a firm above the 15 employees threshold and two polynomials (above the threshold and below the threshold) in firm size, normalised to the threshold. Both the dichotomous indicator and the two polynomials are allowed to vary for treated and control individuals.

The estimated coefficient on the dichotomous "above-threshold" indicator for treated individuals will capture the causal effect of protective institutions on workers hit by an acute CVD shock, with reference to workers hired by firms in a neighbourhood of the 15 employees discontinuity. This is a local average treatment effect (LATE) estimation, with identification relying on a standard continuity assumption (Imbens and Lemieux, 2008). In particular, one concern would be endogenous sorting – e.g. based on health trajectories - of workers in firms of larger or smaller size invalidating identification. We provide evidence in support of our strategy through a set of balancing tests for relevant covariates around the cut-off, which are reported in Appendix Table A.3. Reassuringly, all the available workers' health indicators (past hospitalizations, sick leave periods, CVD shock onset and the duration of shock-related hospitalization for treated individuals) display no discontinuity around the threshold. Overall, we do not detect any evidence of threshold assignment manipulation. Finally, it worth stressing that indeed, more generally, firms of larger size feature a wider scope of organizational practices that encourage unhealthy and disabled workers' inclusion, such as workplace training, accommodation of disability, and reallocation to new tasks or branches in the firm (e.g. Bassanini et al., 2007). However, to the extent that these do not change discontinuously at the more-than-15-employees cut-off, they would not threaten identification.

5. Results

We present results in terms of estimated ATTs ($\hat{\tau}_{\bar{t}+v}$) and corresponding relative size effect (RSE), computed as the ratio of each ATT $\hat{\tau}_{\bar{t}+v}$ to the mean of the contemporaneous counterfactual outcome $Y^0_{i,\bar{t}+v}$ in the matched controls sample. Results are derived under the two identification approaches. However, the discussion focuses on the main one as the alternative provides similar results, confirmed when accounting for selective mortality through appropriate weighting (with results reported in Appendix, Fig. A4).

5.1. Employment, Employment income, and receipt of DI

Fig. 2 reports results for the probability of employment (i.e. working as an employee) and annual income from employment, while Table 2

18 of the Workers' Statute, providing different regulations for different types of dismissal. The most relevant novelty concerned the possibility for a firm with more than 15 employees to dismiss workers for economic reasons. In this type of dismissal, the employee cannot claim his or her job back and has only the right to an indemnity ranging from 12 to 24 months of salary, the sum being decided by a court.

Table 2
Employment-related unconditional outcomes: RSE.

Time	Probability of employment	Annual income from employment	Probability of receiving DI
$\bar{t}$	-	-0.80	235.60
N. treated	-	8495	8495
$\bar{t}$ +1	-1.04	-6.32	340.36
N. treated	8478	8478	8478
$\bar{t}$+2	-7.01	-8.22	344.40
N. treated	7364	7364	7461
$\bar{t}$+3	-8.25	-8.70	355.52
N. treated	6278	6278	6469
$\bar{t}$+4	-9.78	-9.87	342.57
N. treated	5209	5209	5479
$\bar{t}$+5	-9.86	-9.90	337.87
N. treated	4159	4159	4514
$\bar{t}$+6	-10.09	-8.82	340.78
N. treated	3157	3157	3549
$\bar{t}$+7	-9.89	-10.07	335.14
N. treated	2208	2208	2595

Source: WHIP&Health.
Notes: By sample selection all individuals were employed in $\bar{t}$, so the probability of employment in that year is 1 by construction. Hence, the relative size effect cannot be measured. The relative size effects refer to the main approach.

shows RSE values (ATT figures are available in Appendix, Table A4). CVD shocks reduce workers' employment. Loss of employment begins the year after the shock, peaks four years after the shock, reaching an ATT of -7.1 percentage points, and only a minor recovery after that. In terms of RSE, the size of the reduction in the probability of employment is mainly constant from $\bar{t}+4$ onwards, ranging from 9 to 10%. Loss of employment entails an immediate and substantial loss of income, which persists over time, with the RSE reaching in $\bar{t}+7$ the 10% of the earnings those workers would have obtained in the absence of the shock.[33]

Table A5 in Appendix shows (on the left-hand side) a broader measure of labour market activity, which includes self-employment and atypical work on top of employment, allowing for possible transitions out of (wage) employment. Results are not statistically different from those obtained on employment. This result might appear to be at odds with the argument of individuals' being "pushed" into self-employment by a lack of opportunities as employees (e.g., Blanchflower and Oswald, 1998) or health-related limitations (e.g., Zissimopoulos and Karoly, 2007). However, in the Italian context, our finding might be explained by the health-related protection granted to employees, which may lower the incentive to switch to other forms of work that, although more flexible, grant lower protection.

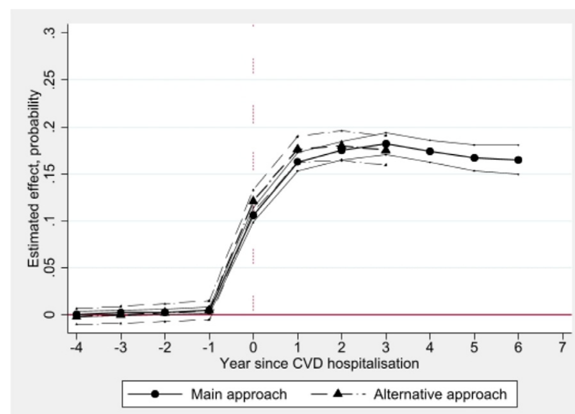


Fig. 3. ATTs by year since CVD hospitalisation: Probability of receiving a Disability Insurance benefit.
Source: WHIP&Health.
Notes: ATTs: point estimates (connected lines) and 95% confidence intervals (dashed lines); marginal effects are reported.

Fig. 3 shows how Disability Insurance offers protection against the potential loss of earnings. The ATT for DI receipt (see also Table A5 in Appendix, right-hand side) amounts to almost 16 percentage points in the year past the shock and remains higher than 15 percentage points after that. The RSE on DI (Table 2, third column) are always higher than 200%. It is worth stressing that more than 90% of treated individuals who enter DI after the shock receive the temporary benefit, i.e. the one compatible with work activity.

An important economic question concerns the overall degree of financial insurance against health shocks' consequences. To this end, we construct a binary indicator to identify individuals who are both out of work and, at the same time, neither receiving DI nor early retirement schemes, relevant as another social insurance route of exit from

[33] To place them in the international context, our RSE results can be compared with García-Gómez et al. (2013) and Fadlon and Nielsen (2021), two studies that are also based on administrative data and analyse the full range of workers' ages but cover other European countries (the Netherlands and Denmark, respectively). Our employment results for Italy are actually similar to those obtained by García-Gómez et al. (2013) for the Netherlands. For men's employment, they report a small RSE in $\bar{t}+1$, increasing to 7.2 percent in the following year and showing no recovery afterward. Our effect in $\bar{t}+2$ is slightly higher (8.8 percent), but this is consistent with the different type of shock García-Gómez et al. (2013) consider, i.e. a wider category of acute hospital admission; in addition to diseases of the circulatory system, they include external causes of injury, digestive system hospitalisations, and others. They perform a sensitivity analysis by specific type of health shock and report that the negative impact of heart attacks and strokes is the strongest among the causes of hospitalisation they analysed. The impact on receipt of DI is also comparable, with their RSE in the range of 300–350 percent, which is consistent with greater access to DI in the Netherlands with respect to Italy. According to Trevisan and Zantomio (2016), the disability recipiency rate in 2005 was 8.7% in the Netherlands and 5.5% in Italy. While one might have expected stronger short-term employment effects in the Netherlands in the light of prevailing disability policies, it should be noted that García-Gómez et al. (2013) consider a wider set of health conditions (including potentially milder ones) resulting in hospitalization. Fadlon and Nielsen (2021) find a stronger negative RSE for employment in the short run in Denmark, ranging from -12 percent in $\bar{t}+1$ and increasing to -17 percent in $\bar{t}+3$, an increase that is similar to our findings. Also, they document a stronger decline in (unconditional) annual earnings in the first year after the shock (-15 percent versus our -12 percent), increasing to -19 percent (versus our -11 percent) two years later, paralleling the results for employment. Their evidence however does not extend beyond the third year following the shock occurrence, which would have been informative on the chances of re-entry in the future.

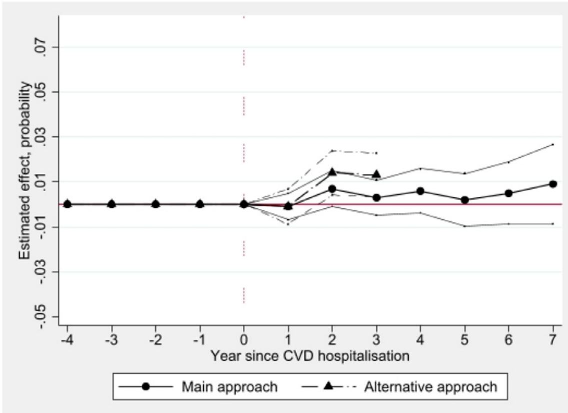


Fig. 4. ATTs by year since CVD hospitalisation:
probability of being out of work and not receiving DI nor early retirement scheme.
Source: WHIP&Health.
Notes: ATTs: point estimates (connected lines) and 95% confidence intervals (dashed lines); marginal effects are reported. By sample selection all individuals were employed in the year of the shock and the previous 4 years, so the average difference between treated and controls is zero, and ATT is not estimated in that time range.

the labour market.[34] The ATT (Fig. 4) is generally not statistically different from zero during the observational period (except for a very tiny positive effect in a few years close to the time when the temporary DI comes to the first renewal phase). Such a result suggests that workers are generally insured – in terms of income receipt, as in our data we cannot measure individuals' DI replacement rates –against the consequences of major health events, either through labour inclusion or through accessing social insurance. In other words, health deteriorations do not cause workers to lose entirely sources of income, which is reassuring evidence about the protective role achieved by labour market and social insurance institutions as a whole for workers in our sample.

5.2. Heterogeneous treatment effects

Figs. 5a, 5b, 6a, 6b display heterogeneity in ATT by gender and for white versus blue collars respectively, with corresponding estimates reported in Appendix Tables A6 and A7. It is important to stress that significance of heterogeneous effects is hampered by the underlying sample composition, featuring a predominant proportion (61%) of male blue-collar workers.

Employment and earnings effects are negative for both men and women but not statistically different by gender.[35] In terms of point estimate, the size of the employment reduction shrinks for women in the longer run, which might be explained by an earlier female exit from the labour market applying also in the control group. Instead, the ATT on DI receipt is significantly (at least 4 percentage points) larger for male workers, which might be more likely to seek additional financial resources in relation to their breadwinning role; this result is not new in the DI participation literature (see, e.g., Pasini and Zantomio, 2013).

Health deterioration risk is particularly salient for blue-collar workers, potentially more prone to CVD shocks and exposed to detrimental consequences (Baigi et al. 2001; Won et al., 2013), both because employed in more physically demanding tasks and because more easily substitutable. The expectation of significantly worse consequences for blue collar workers emerges in terms of employment. We detect a significantly higher employment reduction (four to six percentage points, in terms of ATT, see Appendix Table A7) in $\bar{t}+2$ and $\bar{t}+3$ for blue-collar workers. As to earnings, only blue-collars suffer a systematically significant reduction. Also, DI receipt is always significantly higher for blue-collars, with the ATT difference between the two occupational groups ranging from 8 to 13 percentage points. All in all, heterogeneity by the occupational group suggests that blue-collar workers are more prone to employment and earnings loss, in line with other studies finding similar gradients for the same occupational groups (Halla and Zweimüller, 2013), or other socioeconomic indicators such as income (García-Gómez et al., 2013) and education (Lundborg et al., 2015, Trevisan and Zantomio, 2016).

In terms of income protection, the more adverse labour outcomes of blue collars might be compensated by their higher DI receipt. For this reason, it is interesting to consider again the possibility of a heterogeneous causal effect on the chance of remaining 'uncovered by income sources' i.e. neither working nor in receipt of social insurance. We do detect a significant increase in the probability of being 'uncovered' in $\bar{t}+2$ for blue-collar workers only: the effect, although seemingly small (0.9% percentage points increase) and losing significance in other years, in terms of RSE amounts to the 18% of the contemporaneous counterfactual outcome.[36]

Further heterogeneity results by age[37] and shock type[38] are in line with findings from previous studies and available upon request from the authors.

5.3. Other margins of adjustment

Figs 7a–7c report results for outcomes observed conditional on remaining in employment: annual income from employment, the probability of being employed full-time (versus part-time) and hourly wages[39] (corresponding RSE values are available in Table 3). As evident in Fig. 7a, workers that continue employment after an acute CVD shock

[34] In Italy, social insurance routes of exit from the labour market, other than DI, are available such as early retirement programmes like the so-called seniority pension widely exploited by employees ineligible for DI (like the individuals in our control group). DI is by far more generous than early retirement schemes for those who have a choice.

[35] Similar employment effects by genders were also reported by Coile (2004) and Trevisan and Zantomio (2016), while other works detect larger employment reductions for women (e.g. García-Gómez et al. 2013).

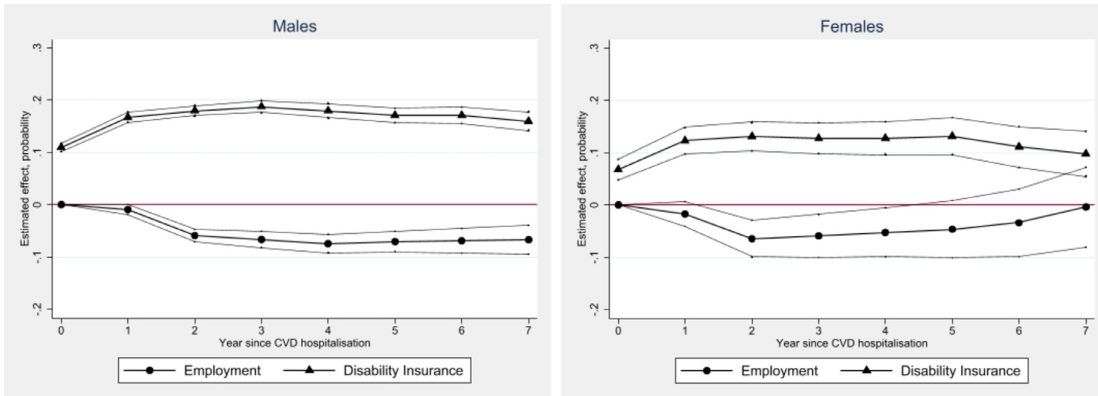
[36] Results available upon request from the authors.

[37] Older individuals might be less attached to the labour market because of available routes of permanent exit from the labour market, such as early retirement or disability pensions. Human capital destruction following a shock is also likely to be higher for older workers (García-Gómez et al., 2013), who may also experience more severe shocks. In a model of human capital formation, investments in the health-specific human capital that supports returning to work may be more attractive to younger individuals, given expected earnings-related returns over a longer time horizon (Charles, 2003). A larger reduction in employment for older worker has emerged in a few previous studies (e.g. Jones et al., 2020) and is confirmed in ours, with the RSE for workers aged above the median of 52 years old two to three times larger than that for younger workers.

[38] We distinguish ischemic heart diseases from cerebrovascular diseases, the latter of which is often a more severe condition that leads to greater impairment. As in Trevisan and Zantomio (2016), we find that cerebrovascular leads to a larger reduction in employment and labour income than ischemic heart diseases do. Here, the long-run evidence confirms that the difference is persistent over time. Heterogeneity by shock type in our sample can barely be traced to differences in shock-specific age at onset, as the median age of onset for the two conditions are similar—52 and 51—suggesting that age and the type of shock are broadly independent dimensions of heterogeneity.

[39] We compute hourly wages by combining information on labour income, paid weeks and the type of work (part-time or full-time). The WHIP data does not have the number of hours worked, but we do have the distribution of hours worked for male blue-collar workers from the EU QLFS data. We find that this distribution is highly concentrated around two mass points: 20 hours for part-timers and 40 hours for full-timers (with no dispersion in the latter case, which is consistent with legal provisions). In computing the hourly wage, we attribute 20 hours of work to part-time contracts and 40 hours to full-time contracts. More than 94% of prevalent annual contracts in our data are full-time.

a: ATTs by *gender* since the CVD hospitalisation:
Probability of Employment and Probability of receiving a DI benefit



b: ATTs by *gender* since the CVD hospitalisation:
Unconditional Annual Income from Employment

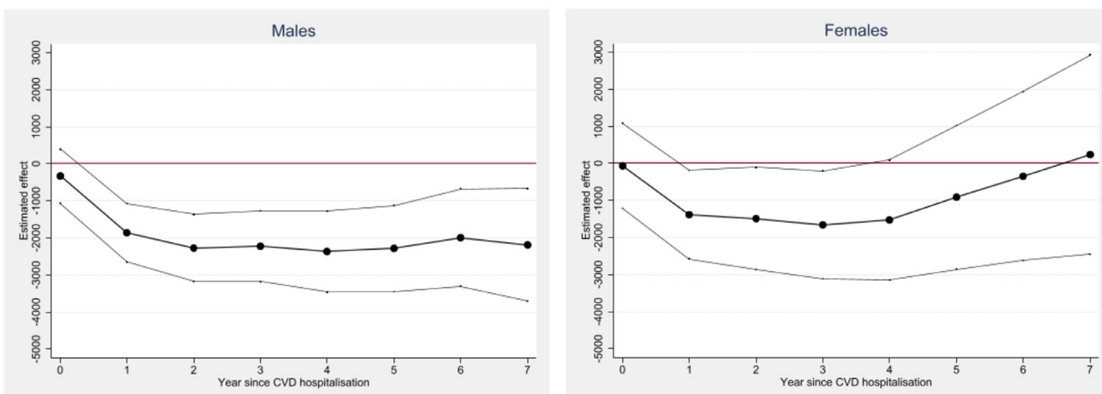


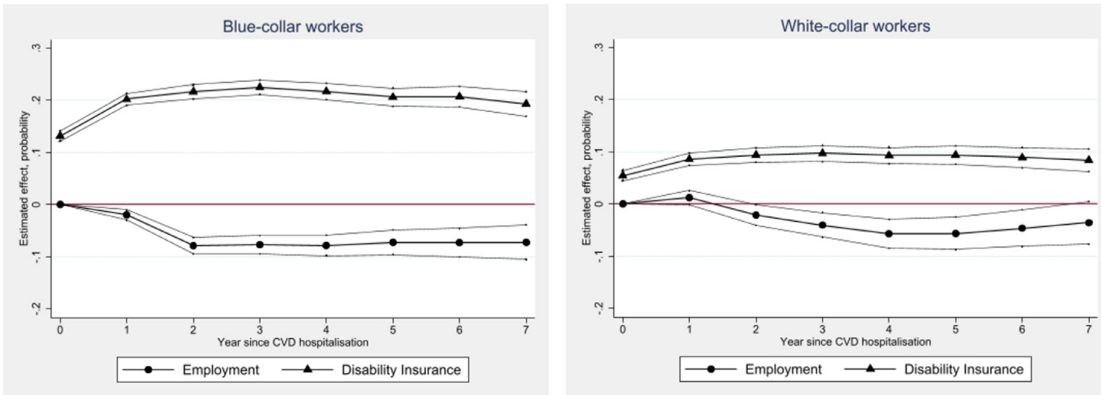
Fig. 5. (a): ATTs by *gender* since the CVD hospitalisation: Probability of Employment and Probability of receiving a DI benefit.
(b) ATTs by *gender* since the CVD hospitalisation: Unconditional Annual Income from Employment.
Source: WHIP&Health.
Notes: ATTs: point estimates (connected lines) and 95% confidence intervals (dashed lines); marginal effects are reported.

Table 3
Employment-related conditional outcomes: RSE.

Time	Annual income from employment	Probability of being employed full-time	Hourly wage
$\bar{t}$	-0.80	-0.07	-1,22
N. treated	8495	8495	8495
$\bar{t}$ +1	-6.54	-0.70	-2.14
N. treated	7680	7680	7614
$\bar{t}$+2	-3.94	-1.01	-1.05
N. treated	5808	5808	5733
$\bar{t}$+3	-2.93	-0.88	-0.49
N. treated	4520	4520	4454
$\bar{t}$+4	-1.50	-0.84	-0.94
N. treated	3419	3419	3376
$\bar{t}$+5	-0.89	-1.05	-0.06
N. treated	2528	2528	2496
$\bar{t}$+6	-0.24	-0.91	0.12
N. treated	1785	1785	1765
$\bar{t}$+7	-2.53	-1.00	-1.68
N. treated	1180	1180	1163

Source: WHIP&Health.
Notes: The relative size effects refer to the main approach. Drops of treated individuals in outcome “hourly wage” due to missing values in the number of paid weeks.

a: ATT by *occupation groups* since the CVD hospitalisation: probability of employment and probability of receiving a DI benefit



b: ATTs by *occupation groups* since the CVD hospitalisation: Unconditional annual income from employment

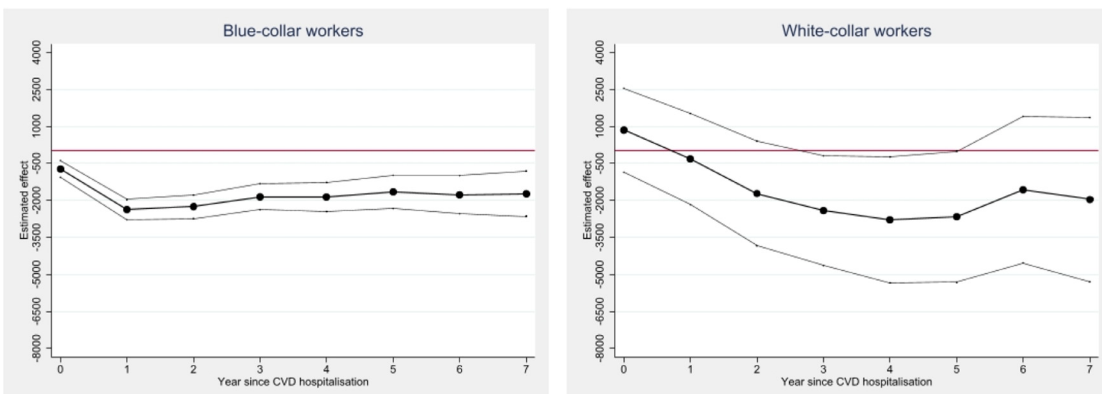


Fig. 6. (a) ATT by *occupation groups* since the CVD hospitalisation: probability of employment and probability of receiving a DI benefit.
(b) ATTs by *occupation groups* since the CVD hospitalisation: Unconditional annual income from employment.
Source: WHIP&Health.
Notes: ATTs: point estimates (connected line) and 95% confidence intervals (dashed lines). Marginal effects are reported in panel (a).

suffer significant losses in earnings only in $\bar{t}+1$, perhaps related to the sick leave period, where the wage replacement granted through sick pay may be incomplete. But clearly, the exit from employment explains the sizeable quantitative difference between the RSE measured on all shocked workers (Table A4) versus those who maintain employment (Tables A8 and A9).

Figs. 7b and 7c shed light on the other available channels of adjustment. The probability of working full time (versus part time) is substantially unaltered; in only a few years, the ATT of full-time employment is significant and negative, yet very small (detailed values in Appendix, Table A8). The number of previous studies that considered hours adjustments is definitely smaller than works studying employment effects; generally, they report a reduction in the number of hours worked by those who maintain employment (Dobkin et al., 2018, Cai et al., 2014, Charles, 2003, Pelkowski and Berger, 2004). However, these works considered the case of the US and Australian workers, embedded in more flexible labour markets than the one we study. Here, the lack of hours adjustment is not surprising because voluntary part-time work is uncommon in Italy, particularly amongst the blue-collar male workers, forming the major component of our sample. At the same time, we do not detect systematic wage adjustments (Fig. 7c and Appendix Table A9), consistent with the overall downward wage rigidity institutional scenario.[40] Again, the even fewer previous works that studied wage adjustment found –in the US – some evidence of a reduction in wages (e.g. Charles, 2003, Pelkowski and Berger, 2004).

Yet another adjustment mechanism is the transition to another employer, motivated by the search for tasks that accommodate a disability, even accepting a lower pay than under the previous employer. However, it does not appear as an adjustment channel actually pursued by Italian workers (see Fig. A5 and Table A9 in Appendix).

5.4. *Adjustment in the long(er) run*

A general limitation of previous literature is that results are typically confined between one and three years after a health shock occurs, leaving unanswered the substantive questions that could only be addressed over the longer term, for example about the chances of later reintegration into the labour market.[41] This is due to a combination

[40] The positive wage effect arising under the alternative approach in t+3 might reflect a worsening labour market trajectory for those in the control groups approaching their health shock.

[41] With very few exceptions (all of which are still restricted to Nordic and Continental European countries). These include Lundborg et al. (2015) who

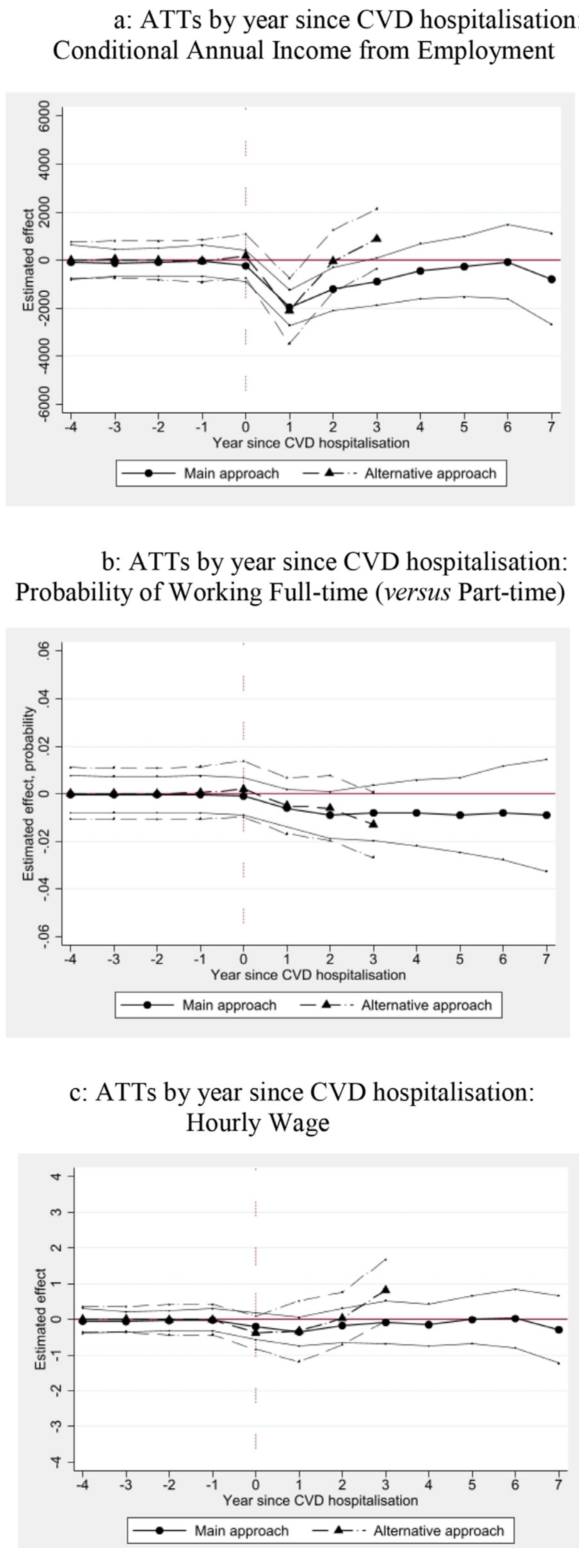


Fig. 7. (a) ATTs by year since CVD hospitalisation: Conditional Annual Income from Employment.
(b) ATTs by year since CVD hospitalisation: Probability of Working Full-time (versus Part-time).
ATTs by year since CVD hospitalisation: Hourly Wage.
Source: WHIP&Health.
Notes: ATTs: point estimates (connected line) and 95% confidence intervals (dashed lines) Marginal effects are reported in panel (b).

of data availability and identification strategy requirements, where the latter rely on pre-shock labour and health histories, resulting in a reduced observational window for analysing post-shock dynamics. In this way the picture remains partial on cumulative effects that fully transpire only over time. First, because later health improvements or the development of new disability-specific human capital are more likely to emerge in the medium to long run (Charles, 2003)[42]. Second, because the institutional environment is important as short run exits from the labour market could be *temporary* or become *permanent* in the longer run. Table 4 reports results for employment, annual labour income and receipt of DI up to $\bar{t}+8$ and $\bar{t}+9$, which can be estimated only for workers who experienced CVD shocks in 2003–2004 (for $\bar{t}+8$) and 2003 (for $\bar{t}+9$). We report RSE to enhance comparability across results obtained from these restricted subsamples (corresponding ATTs are shown in Appendix Table A10). Overall, results highlight the long-term persistence of effect for all outcomes. It is interesting to note that the effects in $\bar{t}+9$ (i.e. 2012) deviate from those in previous years/periods: the relative reduction in employment probability jumps to 16% (from a reduction of 9.57% in 2011). While we cannot rule out the chance of effect specific to the ninth year after the shock, the 2012 evidence fits the important legislative change of the Monti-Fornero reform of labour law, which significantly reduced firing restrictions in medium-sized and large firms. Such result is consistent with the evidence on the protective value of EPL we document in the next section.

5.5. *The role of employment protective institutions*

Table 5 reports estimates on the causal effect of EPL on the employment, labour income and DI participation of employees hit by acute CVD shocks. In the first six columns, we report results adopting the 6 to 25 firm size bandwidth; in the remaining six, results adopting the stricter 11 to 20 bandwidth. The marginal effect on "*Treated: above*" captures the LATE of interest.

Estimates on employment show that employment protective institutions do increase the likelihood for shocked individuals to retain employment both one and two years past the health shock occurrence. The effect of protective institutions appears sizeable, spanning from a 7 to a 13 percentage points higher probability of continuing employment after experiencing an acute CVD shock if enjoying stronger employment protection. The corresponding effect on labour income is systematically positive, although significance is achieved only under the 11 to 20 employees bandwidth.

We perform a set of robustness checks. Results are confirmed when estimated (Table A11 in Appendix) on the subsample of treated individuals (instead of comparing them with matched controls, as in Table 5). We also run a doughnut regression, where we drop firms with 14, 15 or 16 employees, to tackle possible firm self-sorting on each side of the discontinuity. Results (last two columns, Table 6) are confirmed, although the reduced sample size results in a loss of significance in t+2. We then perform two falsification tests, where the analysis is repeated under placebo firm size discontinuities set at 10 or 20 employees: in this case (first four columns, Table 6), reassuringly, the marginal effect on the treated individuals hired in firms just above the placebo thresholds is never significant.

Results are also confirmed – with larger magnitude - when adopting higher polynomials of the second and third order. Interestingly, we fail to detect any local effect of EPL on DI participation (Table 5). One might have expected a decrease in DI receipt for those facing higher EPL, with DI being possibly regarded as a form of employment replacement for

study effects up to nine years after a health shock in Sweden; García-Gómez et al. (2013) and Dano (2005) up to six years in the Netherlands and Denmark respectively; Halla and Zweimüller (2013) up to five years in Austria. Overall, the evidence points to short run-effects that persist over time.

[42] In the US, Charles (2003) observes that the immediate reduction in earnings has been followed by a recovery since the first two post-onset years.

Table 4
Long(er) term unconditional employment-related outcomes: RSE.

	CVD shock experienced in 2003/2004			CVD shock experienced in 2003		
Time	Probability of employment	Annual income from employment	Probability of receiving a DI benefit	Probability of employment	Annual income from employment	Probability of receiving a DI benefit
$\bar{t}$	-	-0.13	351.52	-	-1.45	385.52
N. treated	-	1711	1711	-	810	810
$\bar{t}$+1	-0.30	-3.78	468.46	-0.84	-5.71	483.56
N. treated	1707	1707	1707	808	808	808
$\bar{t}$+2	-6.05	-6.55	452.29	-4.99	-9.88	469.90
N. treated	1675	1675	1698	796	796	804
$\bar{t}$+3	-8.66	-8.34	440.60	-9.36	-9.45	430.76
N. treated	1643	1643	1695	779	779	802
$\bar{t}$+4	-9.55	-10.95	415.13	-8.26	-12.75	436.78
N. treated	1603	1603	1688	763	763	797
$\bar{t}$+5	-11.47	-11.87	393.39	-10.08	-14.09	399.44
N. treated	1560	1560	1684	743	743	796
$\bar{t}$+6	-10.37	-12.76	375.39	-9.23	-18.92	377.36
N. treated	1513	1513	1680	719	719	794
$\bar{t}$+7	-11.23	-12.85	368.45	-10.63	-19.28	390.74
N. treated	1438	1438	1671	679	679	789
$\bar{t}$+8	-10.60	-12.76	347.86	-9.57	-19.03	367.96
N. treated	1338	1338	1667	626	626	788
$\bar{t}$+9	-	-	-	-16.15	-22.99	363.61
N. treated	-	-	-	584	584	784

Source: WHIP&Health.
Notes: Relative effects are reported (corresponding ATT are reported in Table A10); by sample selection all individuals were employed in $\bar{t}$, so the probability of employment in that year is 1 by construction. The relative size effects refer to the main approach.

Table 5
RDD analyses based on firm dimension (more than 15-employees cut-off) – treated vs controls group.

	Employment		Labour Income		Disability		Employment		Labour Income		Disability	
	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$
Treated: Above	0.073**	0.092**	3140	3959	0.028	-0.008	0.082*	0.132**	6736**	7418**	0.042	0.030
	(0.031)	(0.040)	(2021)	(2678)	(0.028)	(0.030)	(0.042)	(0.056)	(2873)	(3590)	(0.039)	(0.043)
Treated: Firm Size	0.0001	-0.008**	54.06	58.29	-0.005	-0.0002	-0.003	-0.019*	-662.2	-357.5	-0.002	-0.004
	(0.003)	(0.004)	(150.8)	(168.1)	(0.003)	(0.003)	(0.007)	(0.011)	(488.4)	(533.6)	(0.009)	(0.010)
Treated: Above*FirmSize	-0.008	0.005	-372.1	-525.5	-0.001	-0.004	-0.007	0.008	-540.7	-1081	-0.011	-0.011
	(0.005)	(0.007)	(295.8)	(387.9)	(0.005)	(0.005)	(0.014)	(0.019)	(884.5)	(945.5)	(0.015)	(0.016)
With matched controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
With additional covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(11–20)	(11–20)	(11–20)	(11–20)	(11–20)	(11–20)
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Pol. interaction	Above	Above	Above	Above	Above	Above	Above	Above	Above	Above	Above	Above
Tot. Observations	470,449	415,336	470,449	415,336	469,633	415,879	225,286	199,161	225,286	199,161	224,904	199,362
(N. treated)	1801	1555	1801	1555	1800	1583	873	767	873	767	872	775
R-squared	0.069	0.087	0.212	0.178	0.240	0.229	0.078	0.088	0.214	0.180	0.244	0.248

Source: WHIP&Health.
Notes: Covariates include year, sector, and area of work fixed effects.

less protected workers (Low and Pistaferri, 2020). This is rejected in our sample. In other words, the probability of DI participation does not appear to respond to EPL protection in a way that would possibly suggest moral hazard in claiming behaviour. Somehow, this result is not entirely surprising within the Italian institutional scenario, where receiving DI is compatible with work activity.

Finally, Table 7 displays the RDD baseline specification results for white- and blue- collars respectively.[44] Results clearly show that the protective role of EP institutions is key for blue collar workers' labour inclusion, inducing an increase of between 10 and 12 percentage points in the probability of remaining employed. On the contrary, no significant effect is detected for white-collar workers, plausibly less vulnerable to loss of employment in the light of higher chances of accommodation to non- physically-demanding tasks. Such heterogeneity stresses the value of EPL in mitigating the additional layer of income inequality arising because blue collar workers are more exposed to acute CVD shocks and their income consequences.

Finally, it is interesting to observe how, in Table 7, we do detect a local significant and sizeable (12 percentage points) effect of EPL on DI receipt in the case of white-collar workers only. The negative sign indicates that, for workers hired in firms around the discontinuity neighbourhood, the probability of DI participation by white collars does respond to EPL, even if employment is not reduced. In a scenario of compatibility between employment and DI receipt, our result suggests that more educated individuals (white-collars) might be more informed about social provision opportunities and better able to navigate the claiming system (see Mullen and Staubli, 2016, for evidence on white collars' higher elasticity of DI claiming to benefit generosity). This might contribute to exacerbating inequalities in the financial consequences of health deterioration.

[44] The very limited female workers sample size does not allow conducting RDD heterogeneity analyses by gender.

Table 6
RDD - Robustness checks for the probability of employment –treated vs control group.

	Fake 10		fake 20		Donut	
	Employment		Employment		Employment	
	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$
T: Above	-0.011	-0.011	0.056	-0.010	0.075^{*}	0.044
	(0.025)	(0.034)	(0.055)	(0.068)	(0.042)	(0.054)
T: Firm Size	0.007	-0.007	0.003^{*}	0.0003	-0.001	-0.004
	(0.007)	(0.010)	(0.002)	(0.002)	(0.004)	(0.005)
T: Above*FirmSize	-0.005	0.010	-0.025^{*}	0.009	-0.006	0.005
	(0.008)	(0.011)	(0.015)	(0.020)	(0.007)	(0.009)
Matched controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional covariates	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear
Pol. interaction	Above	Above	Above	Above	Above	Above
Tot. Observations	470,449	415,336	470,449	415,336	396,733	350,097
(N. treated)	1801	1555	1801	1555	1522	1311
R-squared	0.066	0.086	0.068	0.085	0.085	0.081

Source: WHIP&Health.
Notes: Covariates include year, sector, and area of work fixed effects.

Table 7
RDD analyses based on firm dimension (more than 15-employees cut-off) – treated vs controls group.

	Blue-collar workers						*White-collar workers*					
	Employment		Labour Income		Disability		Employment		Labour Income		Disability	
	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$	$\bar{t}+1$	$\bar{t}+2$
Treated: Above	0.096^{**}	0.118^{**}	2111^{*}	1735	0.050	0.024	-0.029	-0.018	7228	12,595	-0.049	-0.123^{**}
	(0.037)	(0.045)	(1156)	(1298)	(0.032)	(0.034)	(0.051)	(0.076)	(7778)	(11,279)	(0.050)	(0.061)
Treated: Firm Size	-0.0002	-0.007^{*}	10.47	-3.111	-0.008^{**}	-0.0004	0.002	-0.002	135.3	196.9	0.005	0.009
	(0.003)	(0.004)	(118.1)	(134.2)	(0.003)	(0.002)	(0.005)	(0.007)	(548.7)	(604.3)	(0.005)	(0.006)
Treated: Above*FirmSize	-0.012^{**}	0.002	-96.59	10.73	0.001	-0.004	0.014	0.008	-1208	-2338	-0.007	-0.0003
	(0.006)	(0.008)	(207.1)	(240.2)	(0.006)	(0.006)	(0.008)	(0.013)	(991.9)	(1423)	(0.009)	(0.011)
With matched controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
With additional covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)	(6–25)
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Pol. interaction	Above	Above	Above	Above	Above	Above	Above	Above	Above	Above	Above	Above
Tot. Observations	400,967	355,116	400,966	355,116	400,399	355,828	69,482	60,190	69,482	60,220	69,234	60,051
(N. treated)	1416	1228	1416	1228	1416	1254	385	327	385	327	384	329
R-squared	0.077	0.099	0.158	0.1508	0.2433	0.2331	0.078	0.069	0.144	0.122	0.260	0.241

Source: WHIP&Health.
Notes: Covariates include year, sector, and area of work fixed effects.

6. Conclusions

In this work, we offer new evidence on how labour market institutions shape the labour response to acute cardiovascular shocks in the long run in a highly regulated labour market. Using Italian administrative data that covers the history of employment, social security and hospitalizations of workers, we exploit several identification strategies to remove bias from observable and unobservable confounders and obtain causal evidence which is novel with respect to the institutional setting covered, the longer time horizon, the adjustment channels considered, and the explanatory mechanisms detected.

We acknowledge that the evidence we offer is subject to limitations. First, it concerns only individuals who experience acute CVD conditions, although other types of health deteriorations might affect workers as well. Despite confirming the reasons underlying the choice, we acknowledge it might limit external validity; plausibly, effects could be smaller for workers hit by milder and less disabling conditions. Second, while using administrative data presents major advantages, it also has drawbacks in terms of limited coverage of relevant topic areas, which restricts the range of observed confounders we could exploit for identification and hampers the scope for further heterogeneity analyses. Finally, while results on hours and wage adjustments expand knowledge on possible routes of adjustment, these are obtained on the selected subsample of workers who maintain employment despite the health shock, against a comparison group of individuals who maintain employment while not exposed to a health shock, and as such could be biased by compositional changes.

Bearing these limitations in mind, our results, robust across identification strategies and to the inclusion of selective mortality weights, consistently reveal that in a rigid and highly regulated labour market the bulk of response to health shocks emerges along the extensive margin, in terms of employment exits, starting from the short run. The significant reductions in employment and labour income persist over time, with no evidence of re-entry in the longer run, suggesting that employment exit might become an absorbing state. Neither do transitions to less demanding jobs generally offer a viable route of adjustment in the medium to long term. The dynamic pattern of response over nine years shows a corresponding and persistent earnings loss.

Unlike previous findings in the US, frictions find very little room for adjustment along the hours or wage margins, which might have otherwise buffered the decrease in employment. In Italy, while remaining at work might be problematic for individuals if they cannot reduce hours worked, firms tend to avoid offering part-time options, which entail lower productivity (e.g. in relation to the fixed cost of hiring a new worker) and ultimately higher costs when there is no chance of compensating less-productive individuals by adjusting their wages (Devicienti et al., 2015). In this respect, a promising policy recommendation would be providing incentives for firms to agree on voluntary part-time work to reconcile work and health-related limitations (Devicienti et al., 2015). Acting on the wage mobility side appears to be a less viable option, at least in the short term, given the extensive role currently played by collective bargaining in the country.

Application for a disability pension might remain the only available alternative to continuing the pre-shock employment.[45] The social insurance system partially compensates the earnings loss. We observe a sizeable increase in DI receipt, but importantly, no evidence suggests moral hazard in claiming behaviour, particularly regarding blue-collar workers hired in firms close to the 15 employees cut-off. Overall, we fail to detect a significant increase in the likelihood of being out of work and not receiving social insurance support, suggesting that Italian workers, in the years spanning from 2003 to 2011, were generally covered against the financial consequences of acute CVD shocks, either though maintained employment or through DI receipt.

In this respect, general EPL appears to play a crucial role in fostering the labour market inclusion of shocked workers, bearing an insurance value which causally emerges in the RDD analysis. We offer new evidence on the value of employment protective institutions not targeted at unhealthy workers in strengthening their labour inclusion, in particular for blue-collar workers. While somehow hinted by some previous studies (Reeves et al., 2014; Schuring et al. 2020), we provide causal evidence on this. The result we obtain is relevant in the light of the ambiguous or negative employment effect that has been detected in relation to disability-targeted protective legislation such as the US Americans with Disabilities Act or the UK Disability Discrimination Act (see O'Donnell et al., 2015 for a review documenting lack of evidence on a positive employment effect of disability targeted legislation) and bears potential implications for the optimal design of effectively inclusive legislation.

Additionally, on top of the (avoided) fiscal cost of financing public transfer programmes, the labour inclusive effect of EPL is important because there is more income production (which can be substituted by DI receipt) in labour activity. Despite income protection, leaving employment permanently means losing well-being opportunities in terms of self-esteem, motivation, sense of purpose, and social interactions (e.g. Hackett et al., 2012; Spelten et al., 2002; Vestling et al., 2013). All in all, our analysis suggests that the consequences of the more recent – past the years' span we study - labour market reforms lightening employment protection might have contributed to exacerbating income and wellbeing inequalities along workers' life cycle.

Data Availability

The authors do not have permission to share data.

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Supplementary materials

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