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Regulated Earnings Security: The Relationship between Employment Protection and Unemployment Scarring over the Great Recession

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Abstract

The Great Recession raised the concern that employment protective institutions that are effective during macroeconomic stability might become counterproductive under growing macroeconomic volatility. We study this question by examining the relationship between employment protection legislation and unemployment scars on earnings in 21 countries during the period surrounding the Great Recession. We use harmonized work history data for 21 countries from 2004-2014 and combine propensity score matching and multilevel-regression to estimate how earnings losses due to unemployment vary with the strength of labor market regulation and over changing macroeconomic conditions. We find that unemployment scarring is lower in contexts with robust employment protection, both under positive and negative macroeconomic environments. We also show that economic downturns intensify unemployment scarring significantly more in countries with weak employment protection legislation, largely because long-term unemployment is more strongly penalized. Taken together, our study finds that the positive effects of employment protection for workers remain robust during economic downturns.

Keywords

unemployment, scarring effects, welfare regimes, labor market regulation, economic recession

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INTRODUCTION

The dramatic rise in unemployment during the Great Recession reinvigorated the debate about employment protection legislation (i.e., Countouris and Freedland, 2014; European Commission 2011a; Palier and Thelen 2010; Muffels, Crouch, and Wilthagen 2014). Employment protection legislation (EPL) sets standards for how workers can be fired and hired, by mandating severance payments, advanced notice of dismissal, or setting limits on contracts through temporary work agencies. EPL ranges in a continuum from low to high (or weak to strong), depending on the required costs and procedures involved in hiring and dismissing workers. Supporters of a strong employment protection legislation say that these regulations can successfully generate good quality jobs and shelter workers from severe economic uncertainty without undermining macroeconomic performance (Baccaro and Rei 2007; Backer et al. 2005; Bauer, Bender and Bonin 2007; Countouris and Freedland, 2014; Gangl 2006; Hastings and Heyes 2018; Howell and Rehm 2009; Howell et al. 2007; Vergeer and Kleinknecht 2012). Critics say that strong employment protection legislation blocks firms' flexibility and capacity to adapt to changing economic environments, slowing economic growth and innovation (Bauer, Bender, and Bonin 2007; Bernal-Verdugo, Furceri and Guillaume 2012; Bierhanzl 2008; European Commission 2002; 2007; 2011; 2012; IMF 2003; Kugler and Pica 2008). Critiques of EPL have been around for a long time (Palier and Thelen 2010), but have become more prominent in the context of the Great Recession with the premise that EPL might no longer be effective in a globalized and highly volatile macroeconomic environment (Countouris and Freedland, 2014; Hastings and Heyes 2018; Muffels, Crouch, and Wilthagen 2014). The current wave of criticism stresses that while EPL might have had positive equilibrium effects in previous industrial economies, the rigidities of these policies are increasingly disadvantageous in a context where constant flexibility and innovation is necessary, particularly during economic downturns (European Commission 2012; Hastings and Heyes 2018; Muffels, Crouch, and Wilthagen 2014).

Our paper intervenes in this debate by studying whether EPL accentuates the negative consequences of economic recessions for workers who lose their jobs. Building on previous literature on unemployment scarring on earnings (Farber 2005; Gangl 2006), we examine how the degree of EPL and variation in macroeconomic environments shape workers' post-unemployment earnings losses in the period surrounding the Great Recession. Unemployment scarring on earnings is a useful summary measure that captures how job losses affect the likelihood of re-employment, re-unemployment, post-unemployment job match and quality, and workers' overall exposure to economic uncertainty and volatility (Farber 2005). If critics are right, EPL will amplify the negative consequences of economic recessions on labor market conditions and worsen unemployment scarring on workers' earnings. In other words, earnings penalties to unemployment will increase during a recession more in a context with higher EPL. If EPL supporters are instead right, economic recessions will not worsen unemployment scarring more in contexts with stronger employment protection legislation in place.

Previous research on unemployment scarring on earnings examined variation across labor policy regimes and macroeconomic environments separately. Studies concerning labor policy regimes have largely focused on periods of economic stability or growth. These studies find that higher EPL is associated with longer unemployment duration (Gangl 2004a; 2004b) but smaller earnings scarring (Gangl 2006). Research on unemployment scarring across macroeconomic environments, on the other hand, has been largely single-country and not paid attention to labor market institutions such as EPL. Earlier studies in the US found that economic recessions do not substantially worsen earnings scarring (Farber 1997; 2005), but more recent studies find that economic recessions do worsen earnings scarring, showing that workers who lose jobs during a recession experience longer unemployment spells and greater earnings losses (Gangl 2006; Couch, Jolly, and Placzek 2011; Couch, Placzek, and Jolly 2010). Neither of these bodies of research has considered the interaction between labor market institutions and macroeconomic environment, thus leaving open the possibility that high-EPL's seemingly virtuous outcome to reduce unemployment earnings scarring might wash away in a context of growing macroeconomic volatility.

The interaction between EPL and macroeconomic shocks has been examined in an adjacent literature that focuses on aggregate-level unemployment rates, rather than unemployment scarring. Blanchard and Wolfers (2000) proposed the institution-shock framework to argue that the impact of shocks on unemployment rates varies across institutional environments. They used this model to explain changing disparities in unemployment rates between the US and European countries. This body of research finds that shocks increased unemployment rates *more* in contexts with strong EPL compared to other contexts (Bertola, Blau, and Kahn 2001; Blanchard and Wolfers 2000). More recent research, however, has disputed these findings and showed that they are very sensitive to model specification (Avdagic and Salardi 2013). Related studies on labor market flows, which examine mobility rates and typical length of employment and unemployment, also considered the interaction between labor market institutions and macroeconomic environment, finding that market flows are generally lower in contexts with high EPL and less sensitive to macroeconomic shocks (DiPrete and Nonnemaker 1997; DiPrete et al. 1997). While informative, the findings from this literature are inconclusive about how the interaction between EPL and macroeconomic environment can affect unemployment scarring on earnings. For instance, high EPL could worsen unemployment scarring through increased long-term unemployment, even if it does not lead to greater increases in the unemployment rate. Alternatively, high EPL might continue to protect workers from experiencing elevated earnings losses despite market flows being less responsive to macroeconomic shocks.

In this article we examine four possible pathways through which the interaction between EPL and macroeconomic environment can shape unemployment scarring on earnings: employer reluctance to hire, unemployment stigma, labor market segmentation, and wage dispersion. We employ harmonized individual-level work history data built from panel survey datasets covering 21 European and North American countries for the years 2004 to 2014 and we merge it with country-specific time-varying measures of EPL, macroeconomic environment, and other relevant context-level labor market institutions. Our analyses use difference-in-difference (DiD) propensity score matching to estimate unemployment earnings scarring, comparing earnings change between workers who experience job loss with earnings change among similar workers who do not experience job loss. We use multi-level linear regression models to estimate how context-level EPL and macroeconomic environment shape the magnitude of unemployment earnings scarring. Our paper makes three contributions to the existing literature. First, we offer an empirical test for the hypothesis that EPL's effectiveness at protecting workers might be contingent on positive macroeconomic environments. Second, we update existing research on earnings scarring across welfare regimes and across macroeconomic environments with new data and assess whether previous conclusions hold up. Third, we expand the number of countries covered in the analysis thus increasing variation in both institutional characteristics and macroeconomic environment.

The results show that EPL is effective at reducing unemployment earnings scarring even under negative macroeconomic conditions. Workers who lose jobs in countries with higher EPL experience smaller earnings losses than their counterparts in countries with weaker EPL, both in periods of economic growth and during economic downturns. Workers in countries with weaker EPL experience large increases in earnings scarring as macroeconomic conditions deteriorate. This pattern is not due to differences or differential change in unemployment duration or in the composition of the unemployed workforce, nor it is due to the interaction with other labor market institutions, such as unemployment benefit generosity. Instead, we find this pattern to be driven by substantially higher earnings penalties associated with long-term unemployment in contexts with weaker EPL. Economic recessions increase long-term unemployment across the board, but loss of earnings due to long-term unemployment is much higher in low-EPL contexts than in high-EPL contexts. Thus, contrary to critics of EPL, we find robust evidence that stronger EPL continues to protect workers from severe unemployment earnings scarring even in a context of growing macroeconomic volatility.

BACKGROUND

The literature on economic unemployment scarring shows that losing a job is associated with long-lasting declines in earnings, work quality, and often with unemployment re-incidence (Brand 2015; 2006; Farber 1993; Gangl 2004a; Ruhm 1991). Research on unemployment scarring on earnings shows that earnings losses are higher when workers take longer to find a job, when they switch jobs or occupational categories, and when workers are highly skilled or have tenure (Stevens 1997; Carrington and Zaman 1994; Farber 2005; Kletzer 1998; DiPrete and

Nonnemaker 1997). Studies show that both labor market institutions and macroeconomic environments can substantially accentuate or reduce unemployment scarring on earnings.

Research on labor market institutions has largely focused on unemployment insurance and EPL. Several studies find that both policies are associated with longer unemployment spells (Kugler and Pica 2008; Lalive 2007; OECD 2004; 2006), suggesting that this translates into greater unemployment scarring on earnings as well. But other studies find that both unemployment insurance benefits and EPL are associated with higher employment stability after job loss (Wulfgramm and Fervers 2015), better job matches (Gangl 2004b), and smaller earnings scarring (Gangl 2006; DiPrete and McManus 2000). Recent studies challenge some of these findings for EPL, showing that higher EPL is associated with stronger barriers to enter high quality jobs after unemployment (Dieckhoff 2011), particularly for marginalized workers (Kahn 2007).

Research focused on the macroeconomic environment shows that unemployment scarring on earnings varies across these contexts too. Recent studies estimate that long-term earnings losses increase between 2-4% when job losses occur in a context of economic recession (Couch, Jolly, and Placzek 2011; Couch, Placzek, and Jolly 2010; Davis and Von Wachter 2011). This recent set of studies contradicts previous research that had found no substantial differences in unemployment earnings scarring across periods of economic growth and recession (Farber 1993; 1997; 2005). It is still unclear if this discrepancy in results is indicative of a change in labor market dynamics or due to differences in identification and estimation approaches.

Both sets of literatures on labor market institutions and on macroeconomic environments show that unemployment scarring on earnings is shaped by the types of jobs that are lost, the typical length of unemployment, and the conditions of the jobs unemployed workers eventually find. On average, unemployment scarring is greatest when it affects workers with the best jobs and workers get much lower quality jobs after unemployment. Scarring is smallest when it is more likely to affect workers with lower quality jobs and workers can easily go back to similar jobs afterwards. Building on existing research we describe four major context-based processes that can shelter workers or make them more susceptible to unemployment scarring: employer reluctance to hire, unemployment stigma, labor market segmentation, and earnings dispersion.

Employer reluctance to hire

Standard economic theory argues that constraints on and costs of layoffs (i.e. high EPL) turn employers into conservative hirers and reduce economic dynamism (OECD 1999; 2004). When employers cannot fire workers at will, every hiring decision becomes potentially costly, and employers only hire when they absolutely need to. This line of argument is consistent with research finding that market flows are lower in contexts with stronger EPL (Bertola 1999; Bertola and Rogerson 1997; DiPrete et al. 2001; Layte et al. 2004; DiPrete et al. 1997), although some recent studies challenge these findings (Bauer, Bender, and Bonin 2007; Kugler and Pica 2008). This argument is also consistent with studies finding that unemployed workers take longer to find a job in a context with stronger EPL (Behaghel, Crépon, and Sédillot 2008; Bernardi et al. 2000; Machin and Manning 1999; Skedinger 2010). As longer periods of unemployment aggravate loss of human capital and deteriorate job search networks, they are expected to result in higher unemployment scarring on earnings too.

Because economic recessions increase economic uncertainty, they are also likely to increase employer reluctance to hire, regardless of the institutional environment. Studies show that increases in long-term unemployment were widespread and long-lasting during the Great

Recession (Kroft et al. 2016), indicating that employers hesitated to open new positions. However, it is unclear if employers in different institutional environments would react similarly to a shock in economic uncertainty. This perspective raises the possibility that a negative macroeconomic environment might further exacerbate employer reluctance to hire in contexts with higher EPL, producing an echo effect that stalls economic dynamism and worsens long-term unemployment and its associated earnings penalties. It is also possible, however, that when employer reluctance to hire is already high, the added effect of macroeconomic uncertainty might be smaller or not substantially different from its effect in other contexts.

Unemployment stigma

Signal theory argues that scarring occurs because employers rely on signals to choose their workers and unemployment is seen by employers as a negative sign about workers' productivity. In their seminal work, Gibbons & Katz (1991) showed that workers who lost jobs in mass layoffs had smaller earnings scarring than those who lost jobs due to regular dismissals, arguing that only in the latter case was unemployment used as a sign about workers' quality. This theory also poses that the higher the uncertainty and costs to hiring decisions, the more likely employers are to use signals and thus to discriminate against unemployed workers. Indeed, several studies have found evidence in this direction (Canziani and Petrongolo 2001; Gangl 2004b; Holden and Rosén 2014; Kugler and Saint- Paul 2004). For instance, Gangl (2004) finds that workers with long unemployment spells are penalized more severely in protected jobs both in the US and Germany, and Kugler and Saint Paul (2004) find that US states with higher firing costs are associated with lower re-employment probabilities for unemployed workers. A recent study challenges this hypothesis showing that workers who were laid off are not more likely to get temporary contracts than those who lost jobs in plant closures (Biegert and Kühhirt 2018). This perspective suggests that unemployment stigma is worse in contexts with higher EPL due to the higher costs of hiring decisions, but it is unclear how macroeconomic uncertainty might interact with unemployment stigma. Macroeconomic uncertainty could exacerbate unemployment stigma in contexts with higher EPL but, because the levels of stigma might be already relatively higher in those contexts, it is very plausible that unemployment stigma is more cyclical in contexts with lower EPL. Employers in contexts with lower EPL have fewer constraints on setting wages and more discretion in evaluating worker's characteristics (including unemployment history), which may come into play more strongly in a context of high macroeconomic uncertainty and increase unemployment scarring on earnings.

A different approach on unemployment stigma links its prevalence to social and cultural norms. Researchers show that labor institutions shape the extent to which unemployment is perceived and experienced as the individual's fault (Newman 2013; Sharone 2013). Sharone (2013), for instance, argues that prevailing cultural norms make US workers more likely to blame themselves for job losses than Israeli workers. Further, political economy scholars suggest that EPL is tied to cultural commitments to full-time employment that accentuate structural rather than individual blame for unemployment (Tahlin 2013). This argument suggests that unemployed workers might be less severely discriminated against in contexts where unemployment is less likely to be seen as the fault of an individual, and that in such contexts unemployed workers, even if long-term unemployed, might be less likely to accumulate negative effects of employer discrimination.

Labor market segmentation

The labor market segmentation approach emphasizes how labor market structure shapes which workers are most likely to lose jobs. Scholars argue that labor market segmentation reduces overall unemployment scarring on earnings by concentrating unemployment risks among the contingent and often low-skilled workforce (Esping-Andersen 2000; Kletzer and Fairlie 2003; Gangl 2006). Low-wage workers who lose a job are more likely to find a job that pays more or less the same as their previous job and experience generally low unemployment scarring on earnings. At the same time, labor market segmentation protects workers with good jobs through provisions that make their dismissal costlier, and through sectoral boundaries that protect benefits and returns to skills provided that unemployed workers can manage to remain in the same sector after unemployment (Sorensen 2000; Estevez-Abe, Iversen, and Soskice 2001; Weeden 2002). Because higher EPL is tightly connected to segmented labor markets (Biegert 2017), several scholars argue that this compositional effect is what maintains low unemployment scarring on earnings in these contexts (Gangl 2006).

This approach raises the possibility that economic shocks could weaken the protection that “insider” workers enjoy as companies are forced to restructure, resulting in higher unemployment scarring on earnings. An economic downturn could also lead to more workers crossing sectoral and occupational boundaries even though the penalties to this form of mobility are typically greater in segmented labor markets, thus increasing earnings scarring (Bertola and Rogerson 1997; Cha and Morgan 2010). This set of processes seem plausible particularly during recessions that involve major economic restructuring and displace entire sectors of the economy, e.g. construction sector in US, Spain, and Estonia during the Great Recession (Tahlin 2013). This pattern suggests that economic recessions might worsen unemployment scarring on earnings more in a context with stronger EPL than in contexts with weaker EPL.

An alternative expectation would indicate that “insider” workers might remain protected even during mass layoffs in economic downturns. In fact, “insider” workers might be more protected during a recession than equivalent high-wage and high-tenure workers in contexts with weak employment protection. This is consistent with research showing that highly skilled workers are more protected from unemployment risks in countries with higher EPL, at least during contexts of economic growth (DiPrete et al. 1997; DiPrete and McManus 2000).

Earnings dispersion

Unemployment scarring on earnings is partly a function of wage inequality in the labor market, with greater disparity increasing the potential for elevated scarring on earnings. Scholars argue that EPL is often connected to reduced wage dispersion and higher wage floors through diffuse institutional mechanisms related to other labor market legislation, such as minimum wage or unionization (Gangl 2006; Biegert 2017). Previous studies suggest that wage dispersion is one potential explanation for the smaller unemployment scarring on earnings in contexts with higher EPL (Gangl 2006).

Researchers suggest that EPL mechanisms can limit the extent to which companies can resort to lowering wages to adjust to negative macroeconomic conditions (Behaghel, Crépon, and Sédillot 2008; Bernardi et al. 2000; Machin and Manning 1999; Skedinger 2010). This reasoning implies that economic downturns might exacerbate earnings dispersion more in contexts with weaker employment protections and potentially accentuate unemployment scarring on earnings as a result. On the other hand, studies have shown that wage inequality grew substantially during the recent Great Recession in contexts with robust EPL too (Grusky, Western, and Wimer 2011). This suggests that the protective effect of compressed wage dispersion associated with higher EPL might disappear in a context of economic volatility and no longer protect workers from severe earnings losses.

Hypotheses

The previous discussion summarizes four set of processes that can shape unemployment scarring on earnings. These four types of mechanisms are linked to different theoretical traditions (the reluctance to hire and the uncertainty-related unemployment stigma processes are common in mainstream economic, and the labor segmentation, inequality, and culture-related unemployment stigma are common in institutional economics or sociology), but they are not mutually exclusive and several mechanisms could be empirically operating at the same time. Our discussion has centered on describing the implication of each of these mechanisms for the relationship between unemployment scarring on earnings and EPL, macroeconomic conditions, and the interaction between the two.

This discussion presents various processes whereby negative macroeconomic conditions could be expected to increase unemployment scarring more in contexts with lower EPL than in contexts with higher EPL, as well as various processes whereby the negative macroeconomic conditions could be expected to increase unemployment scarring more in contexts with higher EPL than contexts with lower EPL. We summarize these expectations in two hypotheses:

H1: Economic recession will worsen unemployment scarring on earnings more in contexts with high EPL. This outcome could result because:

- EPL's higher costs to hiring and firing will accentuate *employer reluctance to hire* and result in longer unemployment spells and higher earnings losses.
- EPL's higher costs to hiring and firing will accentuate *unemployment stigma* and result in longer unemployment spells and higher earnings losses.
- Increased layoffs of "insider" workers and/or increased mobility across sectors and industries in *segmented labor markets* contexts with high EPL will increase the prevalence of large earnings losses.
- Increased *earnings dispersion* in contexts with robust EPL will increase the prevalence of large earnings losses.

H2: Economic recession will worsen unemployment scarring on earnings more in contexts with weak EPL. This outcome could result because:

- *Employer reluctance to hire* will increase more in lower EPL contexts than in higher EPL contexts where this reluctance is already high.
- EPL is associated with stronger barriers to employers' ability discriminate based on workers' (un)employment history as well as with cultural values that lower *unemployment stigma* and mitigate large penalties associated with long-term unemployment.
- EPL continues to protect "insider" workers in *segmented labor markets* while high-skilled and high-paid workers in weak EPL contexts face increased risks of losing their job, thus increasing the prevalence of large earnings losses.
- Increased *earnings dispersion* in contexts with weak EPL will increase the likelihood of large earnings losses.

DATA, MEASURES, AND METHODS

Data

We use panel data for workers' employment and earnings history in 21 countries for the years 2004 to 2014. We harmonized five major panel surveys: the US Survey of Income and Program Participation (SIPP), the European Union Statistics of Income and Living Conditions (EU-SILC), the German Socioeconomic Panel (GSOEP), the British Household Panel Study (BHPS), and the Understanding Societies Survey (UKHLS). All these are household surveys containing the most high-quality longitudinal information on work and employment in the United States and Europe. Because there are some differences in survey design, we harmonized all datasets to reflect the EU-SILC design that offers the maximum common denominator. The EU-SILC has a four-year rotating panel structure and conducts interviews once per year. Respondents report monthly employment information and annual earnings from the year prior to the interview. The Online Appendix includes more detailed information about the harmonization steps.

Our sample is comprised of 130,414 workers ages 16 to 60 and employed at the time of the first interview and report positive earnings for the year before the first interview. This means that, like other studies (Gangl 2006; Farber 2005), our sample represents adults who are already attached to the labor market. Our treatment group is made up of workers who lose jobs between the second and third interview and our control group is made up of workers who remain employed in that period. This identification choice allows us to have a treatment group for whom we observe earnings around two years prior to job loss and earnings from jobs after unemployment. Based on prior research finding that unemployed workers' earnings start declining right before job loss (Stevens 1997; Ruhm 1991), we want to match the treated and control groups on worker and job characteristics over a year before job loss. See the Online Appendix for more information about the construction of the analytic sample.

Measures

Job losses are transitions from employment to unemployment. We identify these shifts in employment status using respondents' monthly records on economic activity. Employment status is defined as having a job even if not currently at work, this definition makes sure that workers on holidays or on leave are classified as employed. A job loss is identified when respondents move from having a job to not having a job, which could be due to layoffs, end of contract, or quitting a job, our data cannot distinguish between these various forms of job loss. Our measure includes job losses that result in at least one month of unemployment and it includes unemployment spells of varying duration. This means that our treated group includes workers who lose jobs and regain employment anytime between one month after the job loss to more than one year after the job loss.

Monthly earnings are estimated using the worker's annual earnings reports divided by the number of months in employment in that year. Earnings are harmonized to 2005 EUR. All earnings measures are logged, thus a change in this earnings measure can be interpreted as percentage change. Ideally, we would prefer to calculate hourly wages, but the EU-SILC does not include information on usual work hours corresponding to the income reference period. Our measure is also imperfect because it does not allow for a perfect fit between jobs and wages, e.g. a worker who switches jobs during the year will be given the value of the average wage instead of the wage corresponding to each job. While better information is available in some panel surveys (SIPP, GSOEP, BHPS), detailed information is not available in the EU-SILC. We adjusted the analytic design to this feature of the data so that it does not pose a problem for our analyses. The unit of analysis in our study is the year and our measures of pre- and post-unemployment wages are taken from separate calendar years before and after unemployment. This guarantees that our wage measure does not average over pre- and post- unemployment jobs. See Online Appendix section "Analytic Sample" for more details on the construction of this variable.

Employment Protective Legislation (EPL) is a country-level time-varying measure that captures the rigidity of employment regulations. We use the OECD synthetic index of strictness of employment protection in individual and collective dismissals. The index compiles information on three main dimensions: procedures and costs involved in individual dismissal of workers on regular contracts, additional costs for collective dismissals, and regulation of temporary contracts[1]. In our sample this index ranges from 0.25 to 3.21, with higher values indicating stronger employment protection. For instance, a country with high penalties for firing senior workers will score higher than a country with low penalties for firing senior worker, all else being equal. It is important to note that measuring employment protective legislation in a single index necessarily simplifies the existing policies and does not capture all variations and dimensions of this body of social policy. This measure, for instance, does not distinguish between temporary and permanent workers. Notwithstanding these caveats, the measure we use is the best harmonized synthetic measure to compare countries in the relative strength of employment protection.

[1] For more information see: http://www.oecd.org/employment/emp/oecdindicatorsofemploymentprotection-methodology.htm

Unemployment Rate Change (UR) is the country-level time-varying measure that we use to capture the macroeconomic environment[2], with growing levels of unemployment indicating a negative macroeconomic environment and declining levels of unemployment indicating an improving and positive macroeconomic environment. We use Eurostat statistics on year-to-year changes in the annual average unemployment at the country level. The main substantive reason to use the unemployment rate as a macroeconomic indicator is to measure directly the state of the labor market, so that our estimates compare workers in different institutional contexts but similar labor market conditions. Additionally, using changes in the unemployment rate as our key indicator means that our estimates set aside the macro-level relationship between EPL, economic recession, and unemployment rate explored in adjacent literatures (i.e., Blanchard and Wolfers 2000; Avdagic and Salardi 2013).

Other individual-level variables. Our models include standard control variables for workers' human capital, occupation and job characteristics. Age is coded as a continuous variable. Education level is summarized in three categories (1 = high school or less; 2 = post-secondary no college degree; 3 = college degree and above). Work hours are coded as a continuous variable, ranging from 1 to 80 hours per week. Job tenure is a dummy variable which indicates whether the worker had the job for over a year. Occupation specific characteristics are measured using dummy variables for each of the ISCO-08 single-digit occupations.

Other country-level control variables. Our models also include controls for country-level characteristics that could confound the relationships of interest. Drawing on previous research on the institutional policies that correlate with EPL and with the consequences of job loss (Gangl 2006; Biegert 2017), we include measures for unemployment insurance benefits (UI) and union

[2] In sensitivity analyses reported in Table 4 we confirm the robustness of our conclusions to an alternative indicator of macroeconomic environment measuring change in GDP indexed at pre-recession levels.

density (UD). We use OECD data to construct both measures. Including a measure of unemployment insurance benefits is crucial because previous research has shown it can prolong the duration of job search (Gangl 2004b; 2004a) and it is a common policy instrument in contexts that also have higher EPL. Failing to control for unemployment insurance benefit generosity could lead our employment protection measure to pick up this correlation and over-estimate its correlation with unemployment duration and earnings scarring. A similar logic motivates the inclusion of union density. Higher union density correlates with EPL and is related to labor market processes that can result in lower unemployment scarring, such as collective wage agreements that constrain employers' ability to make wage offers dependent on previous (un)employment history.

Separating the independent effects of different labor market institutions is challenging because certain combinations of policies are more common than others and because policies have different effects in different contexts (Hall and Thelen 2008; Hall and Soskice 2004). A reasonable estimation requires, for instance, sufficient variation in unemployment benefits across contexts that have similar levels of EPL. Table 1 provides a summary of all key macro-level variables. The country with the highest EPL score is the Czech Republic at 3.21, while the United States has the lowest score at 0.26. The average change in unemployment rates is positive, denoting general increases in unemployment rates across this period. Variation in unemployment insurance generosity (UI) ranges from a high of 6.47 in Denmark to a low of 1.13 in Poland, whereas variation in union density (UD) ranges from a high of 73% in Sweden to a low of 8% in France. Figure 1 illustrates the changes in the unemployment rate for our sample of countries between 2003-2014. This figure shows that although the vast majority of countries experienced substantial increases in the unemployment rate during the Great Recession, countries were not all hit equally hard nor exactly at the same time or for the same length of time. It is due to this pattern of heterogeneity that it is particularly appropriate to use country-specific time-varying measures to identify levels of labor stress (in our case the unemployment rate) instead of relying on cruder measures such as pre-/post-recession dummies.

Methods and analysis plan

We combine difference-in-difference (DiD) propensity score matching with multi-level regression to model how institutions and macroeconomic conditions shape the consequences of job loss. Our analysis involves three steps: 1) balancing our treated and control sample with propensity score matching to estimate DiD within-person changes in monthly earnings associated with job loss (this is our measure of unemployment scarring on earnings), 2) modeling the relationship between unemployment scarring on earnings across EPL and macroeconomic environments, and 3) examining the mechanisms that drive this relationship.

The goal of the first step is to obtain an estimate about the amount of monthly earnings workers lose as a result of job loss. Following common practice in this literature (i.e., Gangl 2006), we use propensity score matching to balance the distributions of treatment and control groups to obtain a causal estimate of the consequences of job loss. Given that we base the analysis on panel data, we are able to employ a difference-in-differences (DiD) matching estimator that conditions the analysis on all (observed or unobserved) stable characteristics of individual respondents (see Heckman, Ichmura and Todd 1997; 1998). The dependent variable in our analysis is the log monthly earnings change observed for individual respondents between time points T1 and T3 (survey waves 2 and 4), and we construct the DiD estimate of earnings loss associated with job loss by comparing earnings change among workers who lost their jobs between T1 and T2 (survey waves 2 and 3) to the counterfactual earnings change estimated for the matched sample of workers without the experience of job loss between T1 and T2, thus workers who have otherwise similar characteristics to those in the treated group. The propensity score model includes the following variables (all referring to the time of the first interview except when noted otherwise): potential years of experience, gender, highest level of education, logged monthly earnings in the year before the first interview, weekly hours of work, occupational level, and job tenure. The propensity score model is stratified by country and by year, this means that workers who lose a job in Germany in 2004 can only be matched with workers who do not lose a job in Germany in 2004. We employ Kernel matching algorithm, which uses inverse weight probabilities to match the control sample with the treatment group (for similar applications see Gangl 2006; Gebel 2009). The Online Appendix and Table S2 present more details about the propensity score model and matching quality statistics. Our results are robust to alternative propensity score matching algorithms (e.g. nearest neighbor), and we discuss these results in the additional sensitivity tests section below.

In the second step we use three-level HLM regression models to analyze how unemployment scarring on wages varies across contexts. To accommodate the nested structure of our data, we use three-level models with individuals nested in countries and countries nested in years, thus the models include random intercepts at the country and country-year levels (Schmidt-Catran and Fairbrother 2016). These random intercepts cluster standard errors at the country and country-year levels, allowing for observations from the same country and country-years to share more random error than observations from different countries and/or country-years (Gelman and Hill 2007). Our data fulfills the requirement of a cluster-level sample size above 10 deemed necessary to estimate context-level effects in linear regression models (Bryan and

Jenkins 2015). And our models also include random slopes at the country level for all individual-level variables in the model (Heisig and Schaeffer 2015; Bryan and Jenkins 2015). The basic structure of this model can be formalized as follows,

$$\hat{\delta}_{icy} = \gamma_0 + \boldsymbol{\gamma}_r \boldsymbol{Z}_{rcy} + \boldsymbol{\gamma}_{kc} \boldsymbol{X}_{kicy} + u_{cy} + v_c + r_{icy}$$

where $\hat{\delta}_{icy}$ is the individual-level treatment effect estimate, i.e. the logged difference between the observed and the counterfactual monthly earnings change estimate obtained from the DiD propensity score matching, γ_0 is the overall mean intercept, γ_r is a vector of *r* coefficients for context-level variables (Z) such as employment protection legislation (EPL), unemployment rate change (UR), or the interaction between the two (EPL*UR). γ_{kc} is a vector of *k* regression coefficients for individual-level variables (X) that are allowed to vary across countries (random slopes). u_{cy} and v_c are country-year and country random intercepts, respectively, and r_{icy} is the individual-level error term.

We estimate three sets of models. The baseline model provides the estimate for the average penalty across all countries. The second set of models analyzes how this penalty is shaped by context-level characteristics, importantly by EPL, macroeconomic environment, and their interaction. The third set of models, also the third step in our analysis, examines various mechanisms through which EPL and macroeconomic environment are expected to shape unemployment scarring on earnings. These models successively add controls for: (1) individual-level worker characteristics to capture the *labor market segmentation* processes that shape the composition of job losses[3], (2) unemployment duration to capture the compositional implications

[3] Some of the control variables entered in this model are also variables included in the propensity score model. While propensity score model step aims to obtain an average treatment effect (i.e., a weighted regression estimate based on the matched treatment and control sample), these subsequent regression models aim to describe the distribution of treatment effects, or treatment heterogeneity, across individual characteristics (Xie et al. 2012)

of the *reluctance to hire* and uncertainty-related *unemployment stigma* mechanisms, (3) the interaction between unemployment duration and EPL to capture uncertainty-related and cultural-related *unemployment stigma* mechanisms, and (4) the GINI coefficient to capture *earnings dispersion* mechanisms. These models examine whether any of the mechanisms laid out above mediates the macro-level associations between unemployment scarring on earnings, EPL, and macroeconomic environment. For instance, if EPL is associated with lower earnings scarring because it concentrates unemployment risks among low-skilled workers, controlling for cross-country and over-time differences in the composition of unemployed workers should partly mediate the correlation between EPL and earnings penalty. Similarly, if negative macroeconomic environments increase unemployment scarring on earnings because they prolong unemployment duration, controlling for this variable should mediate this correlation.

Taken together, the analysis provides a comprehensive picture to assess whether and how EPL interacts with the macroeconomic environment. Employment protection regulations will prove robust if they succeed in protecting workers similarly well in contexts of both high and low unemployment. Employment protection regulations will be counterproductive if they fail to protect workers in a context of increased macroeconomic volatility and elevated unemployment.

RESULTS

Table 2 shows descriptive statistics for our analysis pooled sample and by country. Our sample includes 130,414 workers 16 to 60 years of age at the time of the first interview, and we observe 5,944 job losses between focal interviews 2 and 3 (5% of the sample). Out of these job losses, 71% find a job before the end of the observation window and we can thus observe their post-unemployment wage. A little over half of our sample are women, workers' average age is 42 and the average monthly wage in the first observation is 1913 Euros.

We begin this section assessing the quality of our data and sample to examine the questions of interest. If economic recessions tighten the labor market, we should observe more unemployment events and longer unemployment spells in our sample. Figure 2 offers a descriptive picture by plotting unemployment events and duration by macro-level change in the unemployment rate. These summary estimates are computed by collapsing the data by country and year and estimating the percent of workers who experience an unemployment event and the average cumulative duration of unemployment. We observe a clear positive relationship between the unemployment rate measure and the incidence and duration of unemployment in our sample. Where the unemployment rate is rising, we observe a greater proportion of our sample experiencing job loss and longer exposure to unemployment. The regression analyses presented next will formally examine how the macroeconomic environment and EPL shape the earnings consequences associated with those job losses.

Table 3 presents regression results for our main models. We start reporting the estimated average unemployment scarring on earnings in our sample. This coefficient should be interpreted as the average earnings loss among workers who lost jobs in this period; more specifically, the difference between within-person earnings change among workers who lost jobs and within-person earnings change among similar workers who did not lose jobs. We find that workers lose about 11% of earnings due to unemployment; their earnings change would have been 11% higher had they not experienced job loss. This estimate is comparable to what previous studies have found (e.g. Gangl, 2006).

Model 2 adds the two key variables of interest, EPL and UR, and Model 3 adds the interaction term that tests whether the effectiveness of EPL changes under different macroeconomic conditions. Consistent with previous research, we find that EPL protects workers' earnings losses. A one-unit increase in the EPL scale (EPL scale median is 2, min 0.26 max 3.3), lowers earnings loss by 3%. This translates into a substantial drop from 18% to 15% if we move from 0 to 1 on the EPL scale. Workers who lose jobs in countries with robust employment protection experience lower unemployment scarring than workers who lose jobs in countries with little employment protection. In Model 2 the coefficient for unemployment rate is initially not statistically significant, but Model 3 suggests this is because its effect systematically varies across countries. Model 3 shows that rising unemployment worsens earnings losses especially in weakly regulated labor markets, where a one-unit increase in the unemployment rate is increasing workers earnings losses by as much as 3.5% in the most liberal environment in the sample (in our sample the lowest EPL level is 0.26 and the model shows the effect when EPL level is set to 0), but this cyclical effect declines in magnitude the more regulated the national labor market.

Model 3 shows that the rate at which macroeconomic conditions affect unemployment scarring is moderated by countries' EPL level. We find that unfavorable macroeconomic conditions increase unemployment earnings scarring *more* in countries with weaker employment protections than in countries with stronger employment protections. This result is consistent with the idea that EPL continues to perform well and to protect workers from severe earnings scarring even under deteriorating macroeconomic conditions. This result does not support the critical approach suggesting that EPL is no longer effective in contemporary economies exposed to growing macroeconomic volatility.

It could be that results in Model 3 are biased because they do not account for country differences in unemployment insurance policies (UI) and unionization (UD), both variables that have been previously shown to affect unemployment outcomes (i.e., Gangl 2006). In Model 4 we add UI and UD as controls and find that EPL and UR main effects and interaction remain largely intact. This bolsters our confidence in the results. Model 4 also shows that neither UI or UD appear to be associated with unemployment scarring. This is inconsistent with previous studies, which show that unemployment insurance reduced unemployment scarring (Gangl, 2006). We examined this finding further and concluded that this discrepancy is likely due to the fact that our data represents monthly wages, instead of hourly wages. In supplementary analyses with a subsample where hours of work are available, we find that unemployment insurance is associated with lower unemployment scarring as previous studies have shown. Sensitivity analyses assessing the robustness of our findings to different matching specifications and methods also confirm our findings. Models including country-fixed effects to assess the sensitivity of our results to unobserved fixed heterogeneity at the country-level also corroborate our findings. We further discuss these results in the additional sensitivity tests section below. In all these analyses we find that EPL lowers unemployment earnings scarring and that negative macroeconomic conditions increase earnings scarring *more* in contexts with weak employment protection.

Figure 3 illustrates the interaction between EPL and UR, comparing high- and low-EPL contexts. We select two cutoff points to present the results, the low-EPL scenario represents the lowest EPL level observed in our dataset corresponding to the US (EPL = 0.26) and the high-EPL scenario corresponds to one standard deviation above the mean (EPL = 2.90). In countries with robust EPL, unemployment scarring is largely insensitive to changes in macroeconomic conditions. By contrast, in countries with weaker EPL, unemployment scarring is cyclical and becomes larger as macroeconomic conditions deteriorate. In a country with weak EPL it makes a big difference whether workers lose a job in a context of rising unemployment or not. In a country with robust EPL, the penalty to unemployment does not substantially change when macroeconomic conditions deteriorate.

Why is unemployment scarring worse in negative macroeconomic conditions in countries with weak employment protection? Table 4 presents results for the four mechanisms discussed above: reluctance to hire, stigma (uncertainty-related and culture-related), labor market segmentation, and wage dispersion. We begin testing the *labor segmentation mechanism*, which concerns differences in the composition of the unemployed workers across countries that vary by EPL and the possibility that macroeconomic shocks would shift the composition of unemployed workers differently in contexts with strong and weak EPL. For instance, if strong EPL continues to protect “insider” workers in a context of recession because its associated firing costs remain high, a negative macroeconomic environment might increase job losses among highly-skilled and high-wage workers relatively more in contexts with weak EPL than in contexts with strong EPL, resulting in greater deterioration of unemployment scarring on earnings in contexts with weak EPL than in contexts with strong EPL. Model 5 tests for this explanation by adding controls for the characteristics of the jobs that are lost including skill level, occupation, job tenure, and work hours. The results show that differences in composition do not explain why unemployment scarring deteriorates more in countries with low employment protection; EPL and UR main coefficients and the interaction remain largely unaltered. If anything, the interaction is slightly strengthened after controlling for these compositional differences. It is possible in principle that our covariate controls are insufficiently detailed to capture some more nuanced patterns and that we fail to capture how specific compositional shifts play a role in deteriorating unemployment scarring in a context of weak EPL. However, as our empirical results rest on a DiD matching estimator that controls for both observed and unobserved time-constant individual characteristics in a very general way, it seems fair to argue that systematic bias in time-varying unobservables, i.e. in covariates not already incorporated in the analysis, would have to be on some very particular empirical pattern to overturn our fundamental conclusions. Naturally, we have no way of ascertaining more than data limitations permit, and we emphasize that it is possible in principle that some time-varying unmeasured characteristic could affect our inferences on the interaction between EPL and UR. But given the safeguards already implemented in our hierarchical DiD design, we would argue that unobserved compositional mechanisms are very unlikely the primary driver of the interaction between EPL and macroeconomic environment reported here.

A second plausible mechanism is related to behavioral responses to job loss from employers that prolong unemployment duration and exacerbate unemployment scarring on earnings (Stevens, 1997). Both the *reluctance to hire* approach as well as the *uncertainty-related unemployment stigma* approach suggest this process. If negative macroeconomic environments prolong unemployment duration more in contexts with weak EPL than in contexts with strong EPL, either because of reluctance to hire or because uncertainty-related unemployment stigma being more elastic and increasing relatively more in those contexts, this could explain the greater deterioration of scarring on earnings in contexts with weak EPL. Although long-term unemployment is typically associated with contexts with robust EPL, recent research shows that during the Great Recession long-term unemployment increased across a wide variety of countries (Kroft et al 2016). Model 6 examines these possibilities by adding a control variable for cumulative unemployment duration that captures both length of unemployment and re-unemployment incidence. We find that the interaction coefficient between EPL and UR loses statistical significance and drops in size, suggesting that cumulative unemployment plays a role in this interaction. The coefficient for cumulative unemployment shows that workers with greater exposure to unemployment also experience greater earnings scarring.

To disentangle whether cumulative unemployment mediates or moderates the relationship between EPL and UR, Model 7 adds an interaction term between EPL and cumulative unemployment. Mediation would imply that the interaction is mainly produced through a compositional effect, i.e., a larger increase in long-term unemployment associated with negative macroeconomic conditions in contexts with low-EPL because unemployed workers become relatively less likely to be rehired in low-EPL countries (either related to *reluctance to hire* or *uncertainty-related unemployment stigma* reducing hiring rates and increasing unemployment length). The interaction term addresses the additional possibility that EPL moderates the relationship between long-term unemployment and unemployment scarring by producing higher penalties to long-term unemployment in low-EPL contexts. This could result from either *uncertainty-related unemployment stigma* mechanisms lowering wage offers to unemployed workers and/or the *cultural environment* with lower unemployment stigma. EPL can safeguard workers against elevated earnings losses, even when they remain unemployed for a long time, by constraining employers' bandwidth to set individual wages and discriminate or stigmatize based on workers' employment history. This is consistent with prior research finding that EPL increases unemployment spell duration but results in better job matches (Gangl, 2004b). Consistent with a moderation mechanism, we find that cumulative unemployment is associated with higher scarring in contexts with weak EPL. This result suggests that the reason why cumulative unemployment explains the interaction between EPL and UR is because the greater prevalence of long-term unemployment associated with periods of high unemployment are more negatively penalized in labor markets with weak EPL than in contexts with robust EPL.

Model 8 examines the final mechanism concerning *earnings dispersion*, the idea being that higher inequality increases the likelihood of elevated earnings scarring. We find that general wage compression does not change the observed patterns, and it does not notably change the interaction between cumulative unemployment and EPL. Consistent with previous research (Gangl 2006), this results suggest that it is not general wage compression in the labor market whereby EPL lowers earnings scarring among the unemployed, but, as discussed before in conjunction with Model 6, the fact that stricter EPL indirectly prevents employers to penalize workers for unemployment spells when making wage offers (or offering job conditions more broadly) at reemployment.

Figure 4 illustrates the finding that penalties to cumulative unemployment duration produce a large difference across EPL contexts. We compare workers with short and long cumulative unemployment durations in contexts with low- and high-EPL and across the business cycle. Among workers with short unemployment exposures, increasing unemployment rates worsen unemployment scarring similarly in contexts with weak and robust EPL. Among workers with long exposure to unemployment, however, deteriorating macroeconomic conditions augment earnings scarring in countries with weak EPL but not in countries with robust EPL.

Additional sensitivity tests

We test the sensitivity of our results to alternative measures of macroeconomic conditions and matching estimators. Table 5 replicates key findings substituting the macroeconomic indicator for a measure of change in GDP indexed to pre-recession levels (Models 9 and 10) and using nearest neighbor matching instead of Kernel matching (Models 11 and 12). Our conclusions are robust to these sensitivity analyses using different measurement and matching specifications. Table 5 shows robust evidence of the interaction between EPL and macroeconomic environment, showing that the association between unemployment scarring and macroeconomic environment is stronger in contexts with weak EPL. It also shows robust evidence that cumulative unemployment interacts with EPL and plays a key role explaining the interaction between EPL and macroeconomic environment. Both alternative specifications provide robust evidence that penalties to long-term unemployment are larger in contexts with weak EPL. Once cumulative unemployment is added to the models, the interaction between EPL and the macroeconomic environment is no longer statistically significant.

In supplementary analyses available in the Online Appendix, we tested the sensitivity of our findings to survey design and interview timing (Table S3), we included country fixed effects to examine sensitivity to country-level unobserved fixed heterogeneity (Table S4), and we re-ran the analyses excluding key countries from our analysis sample (i.e. US and Germany) (Table S5). With few minor discrepancies in tests of statistical significance, all these results replicate the substantive patterns presented here and confirm the robustness of our findings. The results show that the interaction between cumulative unemployment and EPL is key to explain the negative interaction between EPL and macroeconomic environment; in other words, that unemployment scarring in deteriorating macroeconomic conditions is worse in a context with weak EPL and that this is because long-term unemployment is more strongly penalized in these contexts.

DISCUSSION

The Great Recession renewed interest in critiques of EPL, arguing that EPL curtails much needed flexibility necessary to adjust to an increasingly volatile macroeconomic environment, hurting workers' economic prospects as a result. This paper has focused on unemployment scarring on earnings to examine this claim. By studying how unemployment scarring on earnings varies across EPL and macroeconomic environments, we update results from previous research that analyzed these two context-level variables separately and provide a novel test about the interaction between the two. Contrary to critics of EPL, we find that negative macroeconomic conditions worsen unemployment scarring on earnings more in contexts with weak EPL, while workers in contexts with robust EPL remain protected. Our research also confirms previous studies showing that unemployment scarring on earnings is smaller in contexts with EPL and higher under negative macroeconomic conditions. Taken together, our study finds no evidence that EPL is detrimental for workers, neither in a context of economic growth, nor in a context of macroeconomic turbulence.

Our results show that severe penalties to long-term unemployment are a central mechanism worsening unemployment scarring during economic recessions in contexts with weak EPL. We find that earnings scarring for long-term unemployed workers is much higher in contexts with weak EPL. Thus, although the Great Recession increased the prevalence of long-term unemployment across the board (Kroft et al. 2016), only in contexts with weak EPL did unemployed workers experience large increases in earnings losses and earnings scarring. To the extent of our knowledge, no prior research has directly reported systematic variation in penalties to long-term unemployment across policy contexts. While research has focused on how EPL generates barriers to re-employment (Dieckhoff 2011), only a few studies have emphasized that this delay does not come with increases in earnings loses (Gangl 2006; 2004a). The finding that EPL is associated with lower unemployment scarring for those who experience long-term unemployment is consistent with approaches that emphasize both structural and cultural features of EPL, in particular the constraints on employers' ability to set individual wages and discriminate based on workers' prior work history as well as a cultural environment that lowers unemployment stigma. All of these structural components offer plausible explanations for how EPL lowers earnings scarring even among the long-term unemployed and in a situation of macroeconomic uncertainty.

Although critics of EPL stress the potentially adverse effects of employment rigidity in a context of macroeconomic volatility (i.e., European Commission 2012), the finding that workers fare worse under market volatility in contexts with weak EPL is entirely consistent with research reporting greater market exposure and vulnerability under liberal policy regimes (DiPrete et al. 1997). Our results add to the skepticism that scholars raise about mainstream economic policy lines critical of EPL without robust evidence (i.e., Hastings and Heyes 2018; Avdagic and Salardi 2013). Some mainstream economic theory frames unavoidable tradeoffs between economic performance and market coordination institutions, often ignoring evidence about multiple equilibrium regimes that provide a more complex picture about socioeconomic outcomes (Estevez-Abe, Iversen, and Soskice 2001). This speaks to the importance of conceptual frameworks that integrate multiple processes and examine the interrelationship between them (Gangl 2006, Biegert, 2017). The four mechanisms analyzed here are interrelated and are not mutually exclusive. Employer reluctance to hire or uncertainty-related unemployment stigma, for instance, are related to labor market segmentation in that hiring costs will systematically differ between insider and outsider workers. Wage inequality, too, can shape employer hiring costs assessments and hiring decisions. Our analysis has sought to provide a comprehensive framework and operationalize each mechanism separately, but future research should further examine the relationship between these mechanisms.

There are several limitations to the analyses presented here. First and foremost, our analyses can only speak to unemployment earnings scarring in the short-term and among attached workers who lose jobs. This limited scope excludes labor market entrants and long-term earnings scarring, both of which may have different implications for the interaction between EPL and macroeconomic environments. Data limitations, in particular the four-year rotating panel structure, makes it impossible for us to examine long-term earnings scarring. Prior research found that countries differ more in short-term penalties than in long-term penalties, with long-term penalties attenuating the cross-country variation (Gangl 2006; DiPrete and McManus 2000). This suggests that our conclusions might not change dramatically if we included long-term scarring. More research is necessary to investigate how long-term penalties vary with the macroeconomic environment. Similarly, more research is needed to investigate how the interaction between EPL and macroeconomic environment operates for labor market entrants. Second, our research design prioritized coverage (countries and years) and this comes with costs for measurement precision. We estimate monthly earnings measures from annual earnings reports and this introduces measurement error into our estimates. We face similar challenges to measure job characteristics, like detailed occupation or tenure length. Data availability also limits the kinds of macro-level variables we can incorporate in our models; for instance, we are unable to use more specific macroeconomic indicators, such as vacancy rates, because they are not available for most countries and years in our dataset. These measurement limitations can have a negative impact on both our matching analysis as well as on our regression analyses. While improvements in these measurement issues would definitively refine our estimates, sensitivity tests with alternative measurement and specification make us confident that our findings are robust. Also, because we implement a DiD matching estimator, our analyses control for both observed and unobserved fixed individual characteristics and these measurements limitations only apply to time-varying characteristics.

The results in this article have implications for contemporary debates about labor market institutions and economic performance. We find robust evidence that EPL lowers unemployment earnings scarring both in a context of economic downturn as well as in a context of economic growth. These findings challenge critics' hypothesis that EPL would amplify the negative consequences of economic downturns on workers, and favors continued support for EPL. It is possible, however, that EPL amplifies other negative consequences of economic downturns that are not examined here. Future research should investigate the interaction between labor market institutions and macroeconomic environment for additional populations and outcomes to contribute to this debate.

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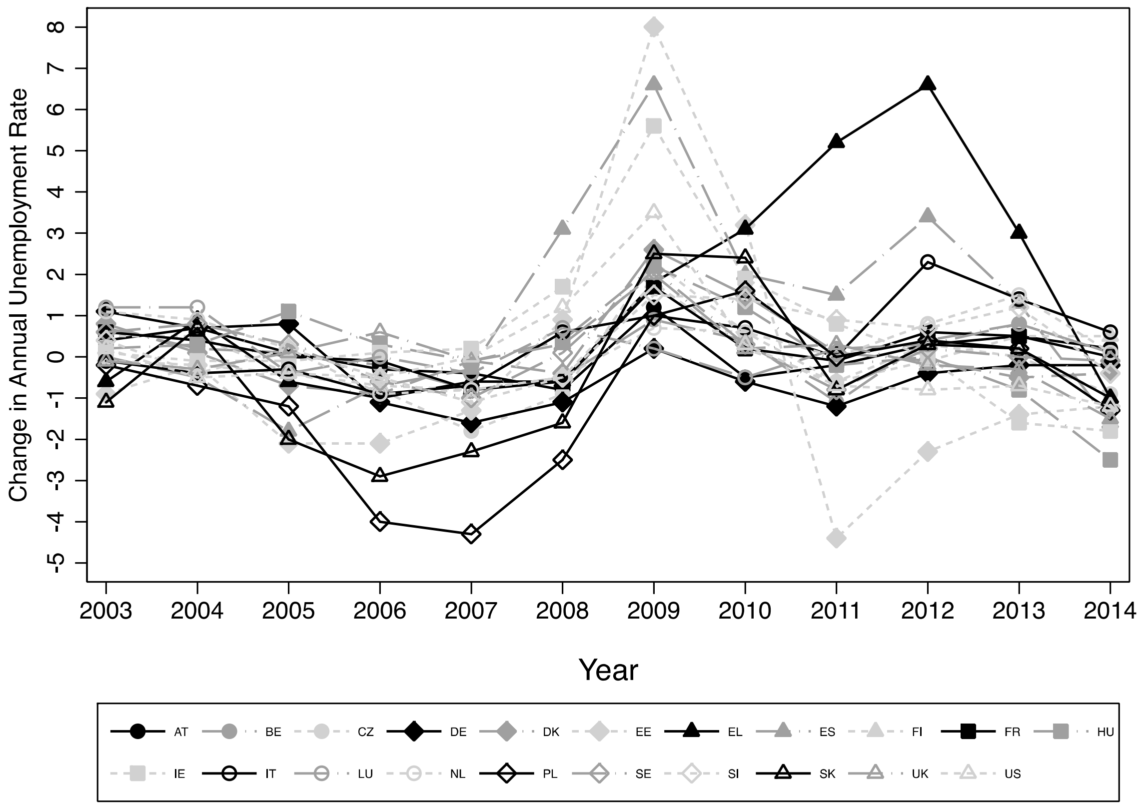
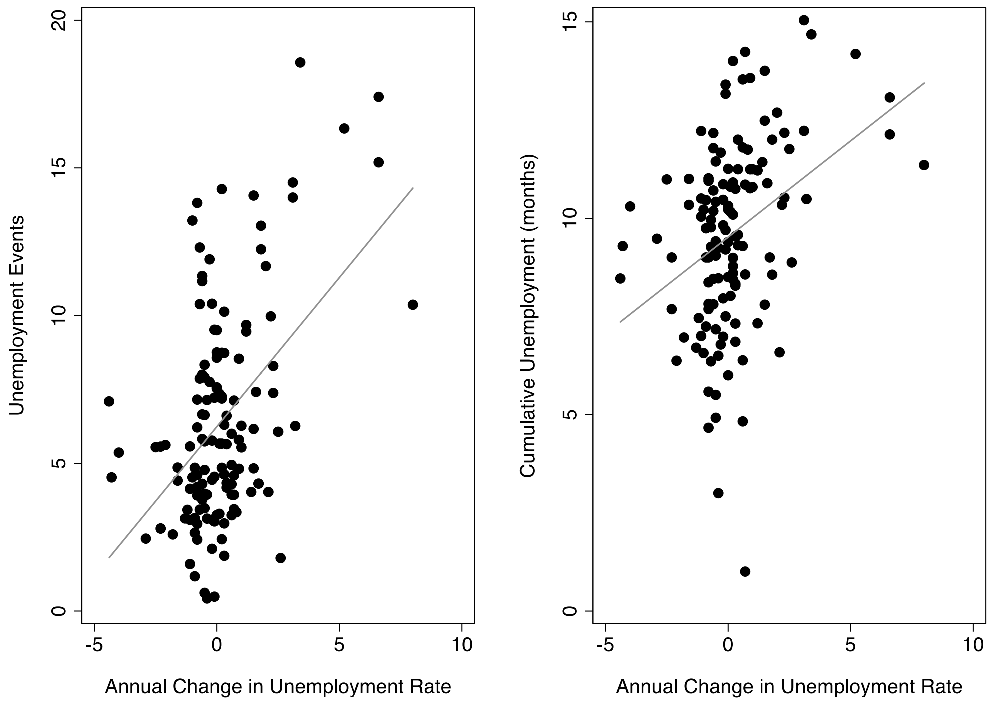
Figure 1. Changes in Annual Unemployment Rate around the Great Recession



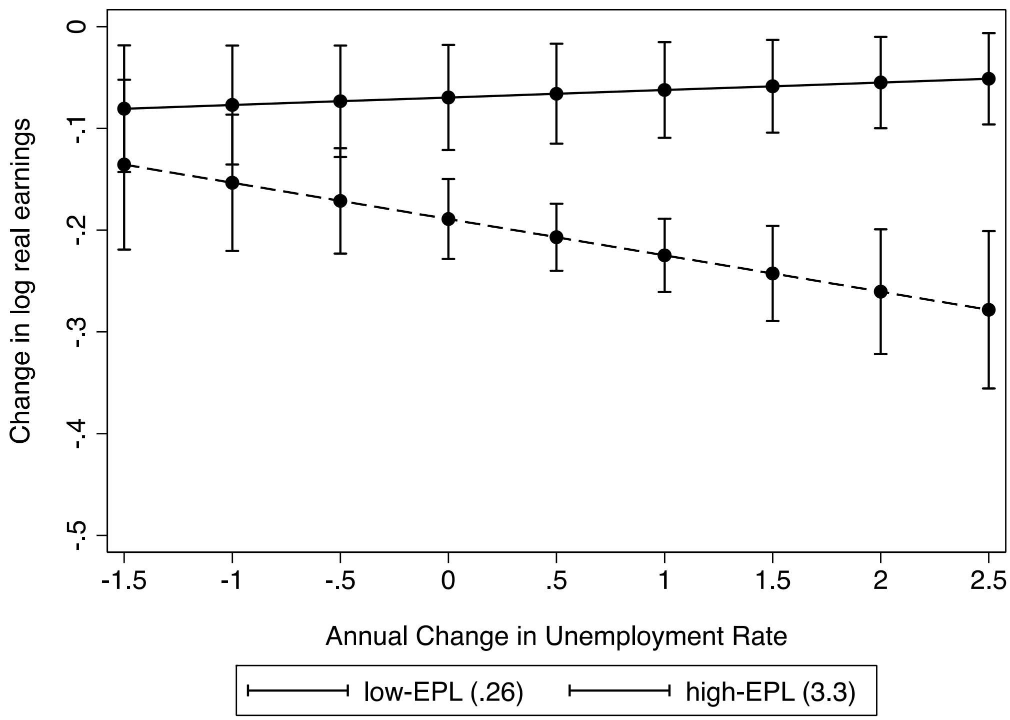
Notes: Changes in annual unemployment rate measure year-to-year differences in annual unemployment rate. For instance, a value of 1 indicates that the unemployment rate is one percentage point higher than in the prior year.
Source: OECD Statistics

Figure 2. Unemployment events and duration in our analysis sample by business cycle (UR)



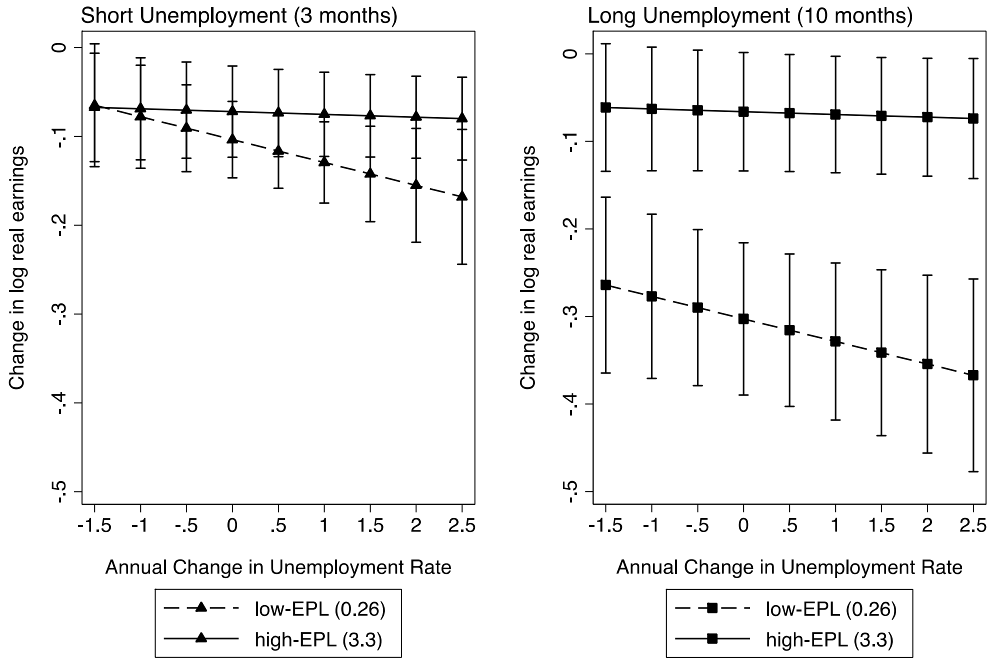
Notes: These summary estimates are computed by collapsing the dataset by country and year and estimating the percent of workers who experience an unemployment event and the average cumulative duration of unemployment.
Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Figure 3. Predicted earnings losses by employment protection legislation (EPL) and business cycle (UR)



Notes: The low-EPL scenario represents the lowest EPL level observed in our dataset corresponding to the US (EPL = 0.26) and the high-EPL scenario corresponds to the highest EPL level observed in our dataset corresponding to CZ (EPL = 3.3).
Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Figure 4. Predicted earnings losses by employment protection legislation (EPL), unemployment rate (UR) and unemployment cumulative duration



Notes: The low-EPL scenario represents the lowest EPL level observed in our dataset corresponding to the US (EPL = 0.26) and the high-EPL scenario corresponds to the highest EPL level observed in our dataset corresponding to CZ (EPL = 3.3).
Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Table 1. Descriptive statistics for key macro-level variables

	(1) UR Unemployment Rate (annual change)		(2) EPL Employment Protective Legislation		(3) UI Unemployment Insurance Generosity		(4) UD Union Density	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
POOLED	0.04	1.56	2.05	0.90	2.74	1.39	25.03	16.01
AT	-0.08	0.60	2.37	0.00	4.87	0.36	30.21	2.04
BE	-0.16	0.66	1.92	0.07	3.34	0.12	54.34	0.47
CZ	-0.54	1.05	3.21	0.12	2.85	0.12	18.88	1.53
DE	-0.72	0.56	2.68	0.00	4.38	0.26	19.82	1.47
DK	0.49	1.23	2.13	0.00	6.47	0.32	68.57	1.51
EE	0.52	3.92	2.62	0.32	1.52	0.07	8.53	1.09
EL	1.69	2.58	2.80	0.00	1.19	0.14	23.87	0.25
ES	2.47	2.23	2.36	0.00	3.63	0.05	16.13	1.26
FI	-0.14	0.78	2.17	0.00	4.97	0.22	69.86	0.93
FR	0.27	0.62	2.42	0.04	4.44	0.23	7.68	0.07
HU	0.48	0.76	2.00	0.00	2.75	0.24	15.65	1.35
IE	0.10	0.08	1.38	0.08	3.36	0.41	34.05	0.82
IT	0.34	1.01	2.76	0.00	3.53	0.10	34.25	0.79
LU	0.03	0.35	2.25	0.00	3.59	0.10	36.26	1.58
NL	0.06	0.66	2.86	0.03	3.91	0.30	19.85	0.82
PL	-1.18	2.26	2.23	0.00	1.13	0.09	16.29	1.56
SE	0.11	1.04	2.61	0.00	2.41	0.20	73.99	2.93
SI	0.49	0.94	2.65	0.00	3.04	0.17	30.10	5.07
SK	-0.87	1.93	2.22	0.00	1.37	0.01	20.16	2.69
UK	0.10	0.31	1.26	0.00	1.58	0.03	27.19	0.30
US	0.05	0.38	0.26	0.00	1.19	0.02	11.93	0.05

Sources: OECD Statistics, Eurostat Statistics, and UvA ICTWSS database

Notes: (1) UR uses Eurostat statistics on annual changes in the aggregate country-level unemployment rate, the mean reports the average annual change in the unemployment rate between 2003-2014 in each country; (2) EPL uses OECD Statistics on strictness of employment protection individual and collective dismissals; (3) UI is an index computed using OECD Statistics data on unemployment insurance coverage (UCOV) and spending on unemployment benefits (UBEN) and on a subset of active labor market policies that focus on income protection (ALMPT), the index is calculated as follows UCOV*(UBEN+ALMPT)*10/2; (4) UD uses UvA ICTWSS database on union density calculated as the net union membership as a proportion of wage and salary earners in employment.

Table 2. Sample Descriptive Statistics

	N	T	Women	Age	Education	Monthly arnings at T1	Monthly earnings at T3
POOLED	130414	5944	0.56	42.02	3.02	1913.12	2099.06
AT	4384	266	0.53	41.71	3.10	2553.52	2841.06
BE	4577	105	0.51	41.87	3.25	2671.78	2906.84
CZ	9137	273	0.54	42.53	3.07	671.27	792.49
DE	14060	469	0.47	42.35	2.96	2418.11	2655.02
DK	2348	39	0.54	45.04	3.26	3678.96	3972.07
EE	4081	182	0.64	42.89	3.25	571.04	659.73
EL	3329	311	0.45	40.42	2.90	1415.82	1443.01
ES	6155	546	0.57	42.61	2.91	1708.49	1822.79
FI	4255	277	0.53	41.31	3.31	2731.43	3009.30
FR	1540	53	0.62	42.67	3.10	2023.46	2177.24
HU	6832	458	0.64	42.36	3.08	481.72	520.70
IE	387	17	0.62	44.35	3.01	2707.54	3030.28
IT	8866	362	0.50	42.51	2.75	1863.68	1968.14
LU	1236	32	0.53	41.21	2.75	3432.14	3702.46
NL	6108	129	0.54	43.19	3.22	2868.97	3090.51
PL	8731	370	0.61	40.98	3.12	604.46	695.58
SE	3114	121	0.52	40.55	3.25	2492.13	2684.75
SI	2868	70	0.93	42.31	3.16	1399.76	1563.58
SK	5688	161	0.52	40.97	3.16	513.96	629.33
UK	13122	179	0.57	41.41	2.66	2538.36	2764.42
US	19596	1524	0.59	41.98	3.02	2555.95	2797.09

Sources: Survey of Income and Program Participation (SIPP), European Union Survey of Income and Program Participation (EU-SILC), British Household Panel Survey (BHSP), UK Understanding Societies (UKUS), German Socioeconomic Panel (GSOEP)
Notes: Education variable is measured in four categories; 1 = less than high-school; 2 = high-school; 3 = post-secondary education, non-tertiary; 4 = college or above.

Table 3. Associations between unemployment scarring, employment protection legislation (EPL), and business cycle (UR).

VARIABLES	Model 1	Model 2	Model 3	Model 4
Constant	-0.114***	-0.192***	-0.186***	-0.170***
	(0.0156)	(0.0110)	(0.00773)	(0.0158)
EPL		0.0374***	0.0354***	0.0436***
		(0.00830)	(0.00725)	(0.0160)
UR		-0.00256	-0.0388**	-0.0398*
		(0.00365)	(0.0183)	(0.0209)
EPL##UR			0.0154**	0.0157*
			(0.00736)	(0.00854)
UI				-0.00299
				(0.0156)
UD				-0.00106
				(0.00112)
Random intercepts	Yes	Yes	Yes	Yes
Observations	130414	130414	130414	130414
Number of groups	21	21	21	21

Notes: The dependent variable is the estimated individual-level treatment effect from the DiD propensity score matching algorithm, expressed as the difference in logged earnings change between T1 and T3 between the treatment and control group.

Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Determinants of unemployment scarring, structural and individual-level mechanisms

VARIABLES	Model 5	Model 6	Model 7	Model 8
Constant	-0.259***	-0.102**	-0.0853**	-0.129**
	(0.0376)	(0.0453)	(0.0422)	(0.0624)
EPL	0.0392**	-0.00166	-0.0185	-0.00937
	(0.0171)	(0.0139)	(0.0151)	(0.0286)
UR	-0.0468**	-0.0298*	-0.0313*	-0.0327*
	(0.0192)	(0.0181)	(0.0174)	(0.0194)
EPL##UR	0.0169**	0.00857	0.00940	0.0117
	(0.00769)	(0.00698)	(0.00674)	(0.00785)
UI	-0.00524	-0.00999	-0.00560	-0.00198
	(0.0161)	(0.0155)	(0.0143)	(0.0150)
UD	-0.000992	-0.00147*	-0.00146*	-0.000795
	(0.00124)	(0.000759)	(0.000858)	(0.00131)
Cumulative unemployment		-0.00757*	-0.0309***	-0.0317***
		(0.00399)	(0.00514)	(0.00406)
##EPL			0.0107***	0.0114***
			(0.00253)	(0.00224)
Wage inequality				0.00421
				(0.00450)
Education				
Secondary	0.0158	0.0148	0.0136	0.0154
	(0.0233)	(0.0258)	(0.0254)	(0.0260)
College	0.0299	0.0282	0.0280	0.0294
	(0.0206)	(0.0222)	(0.0218)	(0.0213)
	0.0811	0.0753	0.0754	0.0758
	(0.177)	(0.183)	(0.182)	(0.176)
Work hours	0.00015**	0.000145**	0.000148**	0.000170**
	(7.43e-05)	(7.37e-05)	(7.40e-05)	(7.59e-05)
Job tenure	-0.000165**	-0.000154**	-0.000156**	-0.00017**
	(6.86e-05)	(7.33e-05)	(7.38e-05)	(7.09e-05)
Women	0.0133	0.00963	0.0103	0.0123
	(0.0156)	(0.0168)	(0.0166)	(0.0166)
Random intercepts	Yes	Yes	Yes	Yes
Random slopes	Yes	Yes	Yes	Yes
Observations	130414	130414	130414	130414
Number of groups	21	21	21	21

Notes: The dependent variable is the estimated individual-level treatment effect from the DiD propensity score matching algorithm, expressed as the difference in logged earnings change between T1 and T3 between the treatment and control group. All models control for dummy variables for single-digit isco occupation codes

Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Sensitivity tests using alternative measures of macroeconomic environment and alternative matching algorithm

VARIABLES	Model 9 Macroeco indicator = GDP change indexed Matching = Kernel	Model 10 Macroeco indicator = GDP change indexed Matching = Kernel	Model 11 Macroeco indicator = UR_CH Matching = nearest neighbor	Model 12 Macroeco indicator = UR_CH Matching = nearest neighbor
Constant	-0.142***	-0.158***	-0.173***	-0.167***
	(0.0247)	(0.0588)	(0.0161)	(0.0514)
EPL	0.0419***	0.00418	0.0418**	0.00725
	(0.0157)	(0.0225)	(0.0164)	(0.0218)
UR	-0.0256***	-0.00740	-0.0385*	-0.0240
	(0.00944)	(0.00841)	(0.0212)	(0.0171)
EPL##UR	0.00526*	0.00379	0.0153*	0.00676
	(0.00314)	(0.00281)	(0.00863)	(0.00682)
UI	-0.0125	-0.00234	-0.00140	-0.000940
	(0.0172)	(0.0138)	(0.0162)	(0.0139)
UD	-0.00113	-0.000643	-0.000929	-0.000728
	(0.00108)	(0.000839)	(0.00113)	(0.000786)
Cumulative unemployment		-0.0301***		-0.0303***
		(0.00538)		(0.00555)
##EPL		0.0102***		0.0103***
		(0.00254)		(0.00259)
Wage inequality		0.00825**		0.00825***
		(0.00334)		(0.00303)
Education				
Secondary		0.0193		0.0190
		(0.0276)		(0.0274)
College		0.0291		0.0305
		(0.0227)		(0.0225)
Work hours		0.000114		0.000130**
		(7.49e-05)		(6.55e-05)
Job tenure		0.0708***		0.0715***
		(0.0230)		(0.0231)
Women		0.0111		0.0111
		(0.0170)		(0.0171)
Random intercepts	Yes	Yes	Yes	Yes
Random slopes	No	Yes	No	Yes
Observations	130414	130414	130414	130414
Number of groups	21	21	21	21

Notes: The dependent variable is the estimated individual-level treatment effect from the DiD propensity score matching algorithm, expressed as the difference in logged earnings change between T1 and T3 between the treatment and control group. All models control for dummy variables for single-digit isco occupation codes.
Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014
Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

PROPOSED ONLINE APPENDIX

DATA HARMONIZATION AND ANALYSIS SAMPLE

Our analyses use harmonized panel data from five major household surveys: the US Survey of Income and Program Participation (SIPP), the European Union Statistics on Income and Living Conditions (EU-SILC), the German Socioeconomic Panel (GSOEP), the British Household Panel Survey (BHPS), and the Understanding Societies Survey (UKHLS). We selected this set of panel data surveys because they contain the most high-quality longitudinal information on workers income and employment trajectories covering a large number of countries.

These five longitudinal surveys are remarkably similar. They are all nationally representative probability random samples of households that collect information on households' sociodemographic characteristics, employment, and economic conditions. The basic follow-up rules are also the same across surveys. There are, however, three important differences in the structure of these longitudinal surveys that we harmonized to build our analysis sample: a) panel length and sample rotation, b) interview schedule, and c) reference period for income and employment data.

The EU-SILC and SIPP have a sample rotation panel structure and each respondent is followed for a maximum of 4 to 6 years. The GSOEP, BHPS, and UKHLS have a simple longitudinal structure and each respondent is followed for the entire duration of the survey. The GSOEP is now one of the longest running longitudinal survey in Europe and the original sample has now been followed for over 30 years. We harmonized the panel length and sample rotation structure across surveys. We adapted all surveys to follow the EU-SILC four-year rotating panel structure, which is the most constringent structure and thus offers a maximum common denominator template. We did this in two ways. First, for surveys with rotating panels where respondents are eligible to be followed for more than 4 waves (this applies to some EU-SILC samples and the SIPP), we restricted all respondents to four observations only. Second, for longer surveys without rotating panels, we created a sample rotation structure. This applied to the GSOEP and BHPS, we did not do this for the UKHLS because this survey only included four waves of data at the time of this study. To replicate the EU-SILC overlapping sample rotation, we split the sample into equal rotation groups and assigned them different start dates (4 rotation groups for GSOEP and 2 for BHSP). When the rotation sample ends after four waves, respondents' observations are reused for new rotation samples. For instance, GSOEP rotation group 1 starts in 2004 and is followed until 2007 and rotation group 2 starts in 2005 and is followed until 2008; respondents in rotation group 1 and rotation group 2 can enter new rotation groups after 2007 and 2008, respectively.

The interview schedule also varies across these longitudinal surveys. The SIPP has a quarterly data collection and the remaining surveys follow an annual interview schedule. Annual interviews in the remaining surveys are spread out across the year, typically each rotation group has a different interview date (i.e. rotation group 1 is interviewed in the first quarter of the year and rotation group 2 is interviewed in the second quarter of the year). We harmonized the SIPP to mirror the other surveys by collapsing the quarterly data into an annual file, utilizing the quarterly data to construct annual measures on employment and income corresponding to the other surveys. We randomly assigned respondents to rotation groups with set interview calendars as above, so that the annual interview is that in the first quarter of the year for about a quarter of respondents, that in the second quarter of the year for about a quarter of respondents, and so on and so forth.

Surveys also vary in the reference period they use to collect information about employment and labor income. The EU-SILC collects income and employment calendar information for the year prior to the interview and collects current employment information as well. The remaining surveys collect employment calendar information prior to the interview and current employment information as well, but the income information is only collected with reference to the current year or month. We harmonized the datasets so that the information about employment and income was adequately aligned. This essentially means anchoring the beginning of the rotation panel to the second interview (instead of the first interview), so that information from the first interview mirrors the EU-SILC collection of income and employment information for the year prior to the interview. This harmonization step was already taken in consideration in the construction of the rotation group structure described above.

Analytic sample

We construct a sample of workers who are at risk of losing their job during the second and third interview. To identify this analytic sample, we begin with a core sample of men and women ages 16-60 at the beginning of the panel and select those who are employed at the time of the first interview, report labor earnings for the year prior to the interview, and have employment calendar information for the year prior to the interview. The analytic sample includes labor income and monthly employment calendar information for four consecutive years, the year before the first interview (Tm1), the year after the first interview (T1), the year after the second interview (T2), the year after the third interview (T3). Table S1 shows how the sample size changes with each condition in each country.

The treatment group is identified as the subset of workers who are employed at T1 and who lose their job between T1 and T2 (or between survey waves 2 and 3). This includes workers who report being without a job for at least one month between T1 and T2, without conditioning on employment status at T2 (i.e. the treatment group includes workers who at T2 are employed, or remain unemployed, or went back to school). In order to compute earnings losses, the treatment group is further constraint to include those who report labor earnings between T2 and T3. This step excludes workers who report no labor income between T2 and T3, these are workers who have been continuously unemployed since they lost their job between T1 and T2, workers who experienced subsequent job loss and resulted in non-employment for the full year between T2 and T3, or workers who dropped out of the labor market and became inactive (i.e. went back to school).

The control group is identified as the subset of workers who were employed at T1 and who did not report any spell of unemployment between T1 and T2. This can include workers who changed jobs without experiencing unemployment. In order to compute earnings change, the control group is further constrained to include those who report labor earnings between T2 and

T3 as well. This step excludes workers who experienced a full year of non-employment between T2 and T3. This step does not exclude workers who experience unemployment spells between T2 and T3 as long as these do not result in the full year of unemployment and thus zero annual labor earnings.

Employment status is identified as respondents who have a job even if they are not currently at work. This means that workers who are on holidays or on leave are classified as employed. Job loss is identified as respondents who move from having a job to not having a job, this could be due to layoffs, end of contract, or quitting a job, our measurement cannot distinguish between these forms of job loss. Monthly earnings are calculated using annual labor income reports divided by the number of months employed in that year. From this, we compute the dependent variable for our Difference-in-Differences (DiD) matching estimator as the difference between respondents' logged monthly earnings at T3 **–** respondents' logged monthly earnings at T1.

We ran supplementary analyses to confirm that our findings are robust to research design decisions. First, we ran analyses with controls for survey type to control for heterogeneity in measurement and design. Our results are also robust to this specification. Second, we ran analyses including controls for month of interview to address the possibility that interview schedule heterogeneity could systematically bias our estimates. Our results are robust to this specification. See Table S3 and supplementary sensitivity tests section below.

PROPENSITY SCORE MATCHING

We use propensity score methods to match workers who lose a job between T1 and T2 interview with similar workers who do not lose a job between T1 and T2. The propensity scores estimate the likelihood of treatment, based individuals' prior characteristics. Once we obtain the scores, we use Kernel matching algorithm to find the control group for the treatment group. Kernel matching algorithm uses all units of the control group and applies inverse weighting based on the distance in terms of the propensity score. Both the estimation of the propensity scores as well as the Kernel matching are stratified by country and year.

We use a logit model to estimate the propensity score. This model includes the following sociodemographic and economic characteristics at the time of the first interview: potential years of experience (continuous), gender, highest level of education (3 categories), logged monthly wage for the year before the first interview, weekly work hours, occupational level (6 categories), and job tenure (at the current job for more than one year).

The goal of propensity score matching is to balance the treatment and control group variables that correlate with both the likelihood of treatment and the outcome interest. With a dataset of 21 countries and treated units in multiple years per country, this amounts to 132 propensity score models and finding a common model that balances all 132 runs is challenging. The propensity model we used is the one that minimizes the median standardized bias for most countries and years.

Table S2 presents matching quality statistics by country. The difference between the initial number of observations and the observations that remain in the area of common support shows that our matching strategy does generally a good job at including the vast majority of units in the treatment group. The difference between the median standardized bias before and after matching shows that our model also does relatively well at balancing on key variables included in the propensity score model. The final median standardized bias is generally around or below the commonly used 5% rule of thumb, except for a few cases when the it is around 6% (Rubin, 2006). A closer examination of those cases showed that the higher levels of bias were localized in a few units. Robustness checks confirmed that dropping those units does not change the results of the analysis.

SUPPLEMENTARY SENSITIVITY TESTS

Supplementary analyses assess the robustness of our results to a number of potential confounders.

Table S3 examines the sensitivity of our results to differences in survey design by including control variables for each survey we use, the reference category if EU-SILC. The GSOEP and SIPP controls are statistically significant and positive, but the overall result patterns remain unchanged. The main effects and interaction patterns of EPL, UR and the interaction between the two are the same as in the main analyses. Model S2 replicates the finding showing the interaction between cumulative unemployment and EPL. Models S3-S4 add a variable to control for the month of the interview to address sensitivity to interview calendar. The results do not substantially change. Alternative specifications of the interview month variable (i.e. categorical specification) show similar results.

Table S4 examines the sensitivity of our findings to country-level unobserved heterogeneity. Models S5-S6 show that our key findings are robust to fixed unobserved heterogeneity at the country level. These results are obtained from HLM models that include country fixed effects and robust standard errors clustered at the country level.

Table S5 examines the sensitivity of our findings to excluding key countries from our analysis (i.e. US and Germany). Models S7-S8 replicate the results excluding Germany and Models S9-S10 replicate the results excluding the US. These sensitivity tests confirm that both the macro-level and the micro-level patterns and interactions are robust even after excluding either one of these two groups of potentially influential observations.

Table S1. Analytic Sample Selection

	(1) Individuals ages 16-60 at first interview	(2) Individuals in (1) who report being employed at first interview and positive earnings for the year prior to the first interview	(3) Individuals in (3) with complete information at T1, T2, and T3	(4) Individuals in (3) who experience job loss related unemployment between T1 and T2	(5) Individuals in (3) who do not experience job loss related unemployment between T1 and T2
AT	7448	4970	4384	266	4118
BE	7848	5064	4577	105	4472
CZ	16011	10178	9137	273	8864
DE	22182	15208	14060	469	13591
DK	3192	2573	2348	39	2309
EE	6513	4557	4081	182	3899
EL	9674	3981	3329	311	3018
ES	12548	7169	6155	546	5609
FI	7216	4764	4255	277	3978
FR	2511	1740	1540	53	1487
HU	13607	7878	6832	458	6374
IE	863	439	387	17	370
IT	19894	10014	8866	362	8504
LU	2145	1371	1236	32	1204
NL	9140	6566	6108	129	5979
PL	20294	9740	8731	370	8361
SE	4226	3396	3114	121	2993
SI	4822	3180	2868	70	2798
SK	10045	6234	5688	161	5527
UK	21996	14467	13122	179	12943
US	31000	21805	19596	1524	18072

Data sources: EU-SILC, SIPP, GSOEP, BHPS, UKHLS.

Notes: (3) Complete information means non-missing values in all variables included in the analysis and observed with positive earnings at T3. This last criteria is necessary to calculate the dependent variable, earnings change.

Table S2. Propensity Score Matching Quality Statistics

	Treatment Group		Control Group		Median Standardized Bias	
	All	In common support	All	In common support	Before Matching	After Matching
AT	266	235	4118	3165	33.3	5.8
BE	105	105	4472	4470	35.8	5.1
CZ	273	201	8864	6844	31.8	6.7
DE	469	397	13591	11785	22.7	4.5
DK	39	39	2309	2309	16.7	4.4
EE	182	181	3899	3802	18.1	2.3
EL	311	304	3018	3016	23.3	6.2
ES	546	535	5609	5473	22.3	4.3
FI	277	238	3978	3477	21.3	6.4
FR	53	50	1487	1487	21.3	5.6
HU	458	448	6374	6036	37.2	3.3
IE	17	15	370	366	20.1	6.3
IT	362	306	8504	7721	25.6	6.5
LU	32	31	1204	1186	17.9	4.7
NL	129	129	5979	5952	19.9	6.5
PL	370	368	8361	8361	29.9	3.9
SE	121	120	2993	2993	32.2	4.3
SI	70	67	2798	2798	44.5	5.0
SK	161	133	5527	4106	27.4	4.7
UK	179	179	12943	12582	11.8	6.8
US	1524	1504	18072	17934	14.6	1.2

Notes: The Median Standardized Bias presents the overall median of mean standardized bias corresponding to each variable included in the propensity score model. The variable-specific mean standardized bias is calculated as the difference in means between the treatment and the control group, divided by the standard deviation in the treated group. *Data sources*: EU-SILC, SIPP, GSOEP, BHPS, UKHLS.

Table S3. Sensitivity analyses to survey design

VARIABLES	Model S1	Model S2	Model S3	Model S4
Constant	-0.311***	-0.245***	-0.329***	-0.269***
	(0.112)	(0.0931)	(0.102)	(0.0844)
EPL	0.0805**	0.0376	0.0738**	0.0415
	(0.0361)	(0.0292)	(0.0335)	(0.0283)
UR	-0.0412*	-0.0254	-0.0497*	-0.0319
	(0.0242)	(0.0180)	(0.0296)	(0.0217)
EPL##UR	0.0171*	0.00809	0.0204*	0.0107
	(0.00972)	(0.00687)	(0.0121)	(0.00858)
UI	0.0113	-0.0131	0.0190	-0.0136
	(0.0207)	(0.0131)	(0.0213)	(0.0127)
UD	-0.000756	-0.000322	-0.00109	-0.000368
	(0.00121)	(0.000877)	(0.00125)	(0.000813)
Cumulative Unemployment		-0.0278***		-0.0273***
		(0.00691)		(0.00754)
##EPL		0.00927***		0.00872**
		(0.00303)		(0.00352)
Education				
Secondary		0.0169		0.0199
		(0.0275)		(0.0278)
College		0.0302		0.0287
		(0.0227)		(0.0218)
Work hours		0.000114		8.35e-05
		(7.18e-05)		(8.18e-05)
Job tenure		-0.000124*		-8.37e-05
		(7.28e-05)		(7.91e-05)
Women		0.0117		0.00744
		(0.0177)		(0.0150)
Interview month			0.00529	0.00494
			(0.00380)	(0.00321)
Survey				
GSOEP	-0.00892	0.101***	0.00328	0.141***
	(0.0227)	(0.0246)	(0.0249)	(0.0262)
BHPS	0.0289	-0.0191	0.00755	-0.0337
	(0.0632)	(0.0419)	(0.0589)	(0.0450)
UKHLS	0.0874	0.0521	0.0791	0.0499
	(0.0590)	(0.0434)	(0.0531)	(0.0434)
SIPP	0.127	0.151**	0.118	0.153**
	(0.0990)	(0.0642)	(0.0881)	(0.0642)
Random intercepts	Yes	Yes	Yes	Yes
Random slopes	Yes	Yes	Yes	Yes
Observations	130414	130414	130414	130414
Number of groups	21	21	21	21

Notes: The dependent variable is the estimated individual-level treatment effect from the DiD propensity score matching algorithm, expressed as the difference in logged earnings change between T1 and T3 between the treatment and control group. All models control for dummy variables for single-digit isco occupation codes

Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table S4. Supplementary sensitivity tests with country fixed effects

VARIABLES	Model S5 country FE	Model S6 country FE
Constant	-0.235**	-0.185
	(0.109)	(0.113)
EPL	0.226***	0.118**
	(0.0544)	(0.0503)
UR	-0.0492**	-0.0327
	(0.0220)	(0.0200)
EPL##UR	0.0172*	0.0101
	(0.00959)	(0.00851)
UI	0.00819	0.0172
	(0.0441)	(0.0394)
UD	-0.000302	0.00135
	(0.00845)	(0.00807)
Cumulative Unemployment		-0.0317***
		(0.00405)
##EPL		0.0111***
		(0.00219)
Education		
Secondary		0.0196
		(0.0286)
College		0.0275
		(0.0226)
Work hours		0.000126*
		(7.22e-05)
Job tenure		-0.000132*
		(7.29e-05)
Women		0.0113
		(0.0171)
Random intercepts	No	No
Random slopes	No	No
Observations	130414	130414
Number of groups	21	21

Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table S5. Supplementary sensitivity tests to excluding key countries from the sample

VARIABLES	Model S7 w/o DE	Model S8 w/o DE	Model S9 w/o US	Model S10 w/o US	Model S11 w/o UK	Model S12 w/o UK
Constant	-0.168***	-0.0824*	-0.425***	-0.252	-0.196***	-0.112*
	(0.0173)	(0.0440)	(0.144)	(0.261)	(0.0238)	(0.0680)
EPL	0.0393**	-0.0209	0.285***	-0.0408	0.0347***	-0.0294
	(0.0181)	(0.0152)	(0.0733)	(0.0448)	(0.0108)	(0.0195)
UR	-0.0421*	-0.0306	-0.108*	-0.0512	-0.0374**	-0.0268
	(0.0223)	(0.0194)	(0.0576)	(0.0422)	(0.0177)	(0.0226)
EPL##UR	0.0170*	0.00994	0.0441*	0.0190	0.0147*	0.0101
	(0.00916)	(0.00763)	(0.0234)	(0.0167)	(0.00766)	(0.00895)
UI	-0.000891	-0.0185	0.131*	0.00179	0.0175	0.0185
	(0.0188)	(0.0159)	(0.0691)	(0.0205)	(0.0261)	(0.0316)
UD	-0.000991	-0.000870	0.000752	-0.00118	-0.00102	-0.00115
	(0.00118)	(0.000899)	(0.00934)	(0.00132)	(0.000721)	(0.00115)
Cumulative Unemployment		-0.0317***		-0.113***		-0.0324***
		(0.00497)		(0.0433)		(0.00396)
##EPL		0.0118***		0.0392**		0.0117***
		(0.00243)		(0.0186)		(0.00214)
Education						
Secondary		0.0154		0.0428*		-0.00180
		(0.0293)		(0.0223)		(0.0235)
College		0.0287		0.0317		0.0193
		(0.0241)		(0.0298)		(0.0218)
Work hours		0.000162**		0.000141		0.000189**
		(7.91e-05)		(9.79e-05)		(8.01e-05)
Job tenure		-0.000154**		-0.000138		-0.000195**
		(7.78e-05)		(0.000101)		(7.60e-05)
Women		0.0751***		0.0248		0.0108
		(0.0248)		(0.0157)		(0.0170)
Random intercepts	Yes	Yes	Yes	Yes	Yes	Yes
Random slopes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	116354	116354	110818	110818	117292	117292
Number of groups	20	20	20	20	20	20

Notes: The dependent variable is the estimated individual-level treatment effect from the DiD propensity score matching algorithm, expressed as the difference in logged earnings change between T1 and T3 between the treatment and control group. All models control for dummy variables for single-digit isco occupation codes.

Source: EU-SILC, BHPS, UKHLS, GSOEP, SIPP, 2004-2014

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1