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Unintended Health Consequences of Decreasing Unemployment Insurance Generosity During an Economic Recession

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Abstract

We exploit an unexpected labor market reform to estimate the effects of a significant *decrease* in unemployment insurance (UI) generosity during an economic recession. On July 13, 2012, the Spanish Government reduced the replacement rate from 60% to 50% after 180 days of UI benefit receipt for *all* spells beginning after July 14, 2012. Using rich linked administrative data and a difference-in-differences approach, we show that the decrease in UI generosity resulted in higher sickness absence rates, thereby reducing the previously documented government savings from this reform. Our findings suggest that both financial stress and moral hazard are possible mechanisms.

Keywords: Unemployment insurance, sickness absence, policy reform, financial stress

JEL classifications: D04, H55, I18, J22, J32, J65

Introduction

Unemployment insurance (UI) programs are an essential component of the safety net in many countries, offering an important buffer against negative income shocks (East & Simon, 2020). In the aftermath of the Great Recession in Europe, amidst the sovereign debt crisis, several countries *reduced* rather than increased their UI benefits (Rodríguez-Planas & Rebollo-Sanz, 2020). Reducing UI generosity during an economic recession presents a double-edged sword. On the one hand, cutting benefits might boost the job search efforts of affected workers, potentially increasing re-employment rates. On the other hand, the value of social insurance benefits is heightened during recessions, as recipients are more likely to exhaust their benefits without finding a job, face credit constraints, and experience declines in consumption (Domènech-Arumí & Vannutelli, 2023). These effects are likely to vary among workers with different levels of employability or job protection.

While the effects of UI generosity on labor market outcomes are well documented (Card et al., 2015; Katz & Meyer, 1990; Moffitt, 1985; Rodríguez-Planas & Rebollo-Sanz, 2020), we know very little about its potential effects on health.[1] This issue is nevertheless important for at least three reasons. First, it has substantial importance for policymaking, as it would help to more comprehensively evaluate past and potential UI reforms. For instance, the previously documented government savings from the reform we study (which decreased UI generosity, see Rodríguez-Planas & Rebollo-Sanz (2020)), would be reduced if the reform had also resulted in e.g. higher sickness absence rates. Second, it expands our understanding of the interrelations between individuals' health and labor market outcomes. Third, as less UI generosity typically leads to lower individual and household incomes, it can be also informative

[1] In terms of labor market outcomes, more generous unemployment benefits (UB) will produce a moral hazard cost if UB recipients reduce their search efforts and increase their unemployment duration (Katz & Meyer, 1990).

to the broader literature that studies the effects of income on health (Frijters et al., 2005; Smith, 1999).

The main contribution of this paper to these literatures is to provide the first causal evidence on the health effects of a significant *decrease* in UI generosity during a recession. On July 13, 2012, the Spanish Government unexpectedly announced a reduction in the replacement rate (RR) from 60% to 50% after 180 days of UI benefit receipt for *all* spells beginning from July 15, 2012, and onwards. The RR during the initial 180 days remained unchanged at 70%. Importantly, the implementation of the reform was clearly exogenous to the individuals and politically determined (cf. Rodríguez-Planas & Rebollo-Sanz, 2020). Here, we use this policy change in a difference-in-differences (DiD) approach, where we compare our outcome of interest from individuals eligible to be affected by the cut in the RR rate (our treatment group) before and after the reform to those individuals with similar potential UI benefit levels, but who were unaffected by the reform because they were entitled to no more than 180 days of UI benefits (our comparison group). We employ rich restricted-use linked administrative data from a large Spanish region (Catalonia), which besides complete working histories includes detailed medically certified sickness absences (SA) based on International Classification of Diseases (ICD) diagnosis codes, which we use as a measure of individuals' health.[2]

Another contribution of this paper is concerning a growing literature that studies how multiple social insurance programs affect workers' labor supply decisions (Inderbitzin et al., 2016; Leung & O'Leary, 2020), which differs from the larger literature that studies the isolated effect of a single program on labor supply, ignoring thus potential interactions with other social insurance programs.[3] Most of the studies documenting the interactions of UI with other social

[2] Throughout this paper, we use the terms sickness insurance (SI), sickness absence (SA), and (temporary) sick leave as synonyms.

[3] Some of these earlier studies have focused on policy changes in UI (Card et al., 2015; Katz & Meyer, 1990; Moffitt, 1985; Rodríguez-Planas & Rebollo-Sanz, 2020), SI (Johansson & Palme, 2005; Marie & Vall Castelló, 2023; Ziebarth, 2013) or disability insurance (DI) (e.g. Autor et al. 2016; French & Song, 2014).

insurance programs have focused on disability insurance (DI) (e.g., Inderbitzin et al., 2016; Lindner, 2016; Mueller et al., 2016; Petrongolo, 2009; Wise, 2016) and other programs relevant to older workers such as early and normal retirement (e.g., Gruber & Wise, 2004; García-Pérez et al., 2013). For example, in Europe, Inderbitzin et al. (2016) studied the pathways to (early) retirement among Austrian workers and found evidence of both program complementarity and substitution effects of extended UI benefits with DI benefits. Instead, in the US, Lindner (2016) and Mueller et al. (2016) found limited evidence that, respectively, UI benefits or the expiration of UI benefits affects the decision to apply for DI, showing little overlap between social security DI and UI recipients.

Only a few studies have explored the possible interactions of UI with sickness insurance (SI), which are two major social insurance programs among young and middle-aged workers.[4] Most of the studies examining the interactions between UI and SI are from Nordic countries (Hall & Hartman, 2010; Henningsen, 2008; Larsson, 2006). For instance, Larsson (2006) exploited a discontinuity in the institutional setting of UI and showed that sick reports in Sweden increase as the UI benefit expiration date approaches. She also provided evidence of an incentive effect on the sick report rate due to SI offering higher compensation than UI. Interestingly, the expiration effect, she argued, may actually "reflect unobserved impaired health (due to stress) related to UI benefit expiration" (p. 110). Limited information on the type of illness prevented her from investigating further this possibility.[5]

The last contribution of this paper is to explore the possible mechanisms behind the transitions from UI to SI. One new potential mechanism, alluded to in the previous Nordic studies, but not empirically tested for, is financial stress. A worsening subjective financial

[4] During 2012-2015—which includes our sample period—spending on public and mandatory private paid sick leave and unemployment cash benefits (unemployment compensation and severance pay) as a percentage of GDP was, respectively, 0.7 and 2.7 in Spain compared to 1.0 and 1.0 in OECD countries (see https://stats.oecd.org/).

[5] Hall & Hartman (2010) and Henningsen (2008) used correspondingly a reform that lowered SI benefits and one that extended the duration of unconditional UI to study its effects on the transitions from UI to SI. Both suggest "stress" as a potential channel for transitioning from UI to SI in their discussions, but do not investigate it.

strain is associated with an increased risk of general and major depression (Andreeva et al., 2015; Lorant et al., 2007). Perceived stress emerges, amongst others, from a "loss of control" (LOC) (Epel et al., 2018). In our setting, affected unemployed individuals were subject to about a 17% decline in their RR. In a context of an economic recession with unemployment rates (URs) reaching an unprecedented 26%, the actual loss or fear of losing a significant amount of income may well create a LOC feeling that results in financial stress, especially among workers with lower employability and less protected jobs. A second potential mechanism is moral hazard (e.g., Johansson & Palme, 2005). Because the RR from SI remained unchanged and relatively high (at 75% from day 21 onwards, see next section), the incentive to transition to SI (without being sick) increased after the reform among affected individuals. These two mechanisms are like two sides of the same coin and are thus not readily separable from each other. Our approach to this is to rely on indirect evidence. More precisely, we study if the reform effects vary by sub-types of SA according to the nature of the spell (stress-related or not), its length (short or long), and its timing (did it start immediately after entry into UI or not). Moreover, as the impact of UI on health may vary across, e.g., worker occupation, type of contract, and gender if such groups face different outside options or value work differently (Ahammer & Packham, 2023), we also consider these and other common observable characteristics in heterogeneity analyses.

As far as we know, only two recent studies have assessed the *causal* impact of UI generosity on health, and none of them focused on the effects of a pro-cyclical change in UI generosity. The first, by Kuka (2020), exploited plausibly exogenous changes in state UI laws in the United States (U.S.) to empirically estimate whether UI generosity mitigates any of the previously documented negative health effects of job loss. Her results showed that higher UI generosity increases health insurance coverage and utilization, with stronger effects during periods of high URs. During such periods, higher UI generosity also leads to improved self-reported health. She did not find any evidence, however, of an effect on risky behaviors or health conditions. The second, by Ahammer & Packham (2023), used linked administrative health and labor market data for Upper Austrian unemployed workers to assess the health effects of an extension in UI duration. Similar to Larsson (2006), they did not rely on a natural experiment but exploited a discontinuity in the UI scheme that extends UI benefits for workers aged 40 and older from 30 to 39 weeks. They showed that women eligible for an additional nine weeks of UI benefits fill fewer opioid and antidepressant prescriptions. Instead, for men there was little evidence of beneficial health effects of extending UI duration.[6]

Regarding the reform that we exploit in this paper, previous studies have investigated (only) its labor market effects. For instance, Rebollo-Sanz & Rodríguez-Planas (2020, RS-RP hereafter), using administrative social security data and a DiD approach, found that the 10 percentage points (p.p.) reduction in the RR from this reform increased workers' odds of finding a job by at least 41% relative to a control group. They attributed this effect to a reduction in moral hazard behavior among individuals affected by the reform through an increase in their job search efforts. They further found no evidence of an effect on wages, nor a worsening in post-non-employment job quality, suggesting that workers did not settle for worse job matches. At 15 months of follow-up, the reform had decreased UI expenditures by 16%, about one-half of which is explained by job seekers' behavioral changes. Following up on their findings, we also investigate the dynamic effects of the reform on employment and provide descriptive evidence on the pathways between UI and SI with a focus on employment and several social insurance schemes. Our results imply that their estimated government savings from this reform are likely overestimated, as they did not include an unintended increase in sickness absences.

[6] As in Ahammer and Packham (2023), we can isolate the health effects for a set of workers whose health insurance coverage is unaffected by job loss. Perhaps an advantage of our setting, in comparison to theirs, is that we exploit a change to the UI replacement rate which, compared to a change in UI benefit duration, is less likely to affect a worker's leisure time, and therefore doctor's visits and/or prescriptions for previously untreated illnesses.

In summary, we employ rich linked social security data on working histories and certified SA from Catalonia, the second largest region in Spain. Our identification strategy is similar to that of RS-RP and relies on a DiD approach to exploit the above-mentioned policy reform. Using a variety of specifications, we show that the reform increased the probability of having a certified SA episode by about 1-2 p.p., approximately doubling to tripling the probability of SA. Our analyses by subtypes of SA, population subgroups, and of potential dynamic reform effects suggest that the reform affected different individuals differently and that both financial stress and moral hazard could be possible mechanisms through which decreased UI generosity resulted in higher SA rates. Some of the more employable workers appear to have been "activated" early by the reform, finding a job before the actual reduction in their RR occurred. Conversely, some of those who transited to SI at the beginning of their UI spell may have been driven by moral hazard behavior. Finally, amid an economic recession with URs hitting an unprecedented 26%, the reform may have well worsened the health of some less protected or less employable workers through financial stress, increasing their likelihood of experiencing a stress-related or long SA. As we show, reducing UI generosity during an economic recession can have unintended effects on other social insurance schemes, such as SI. Considering such externalities is essential for good policymaking.

Institutional Background

Spain's Unemployment Insurance System

Regular UI benefits (*prestación por desempleo)* in Spain are contributory, not means-tested and taxable. To be eligible, a claimant must have contributed for a minimum of 360 days in the six years preceding the legal status of unemployment and fulfill some behavioral requirements. Differently from the U.S. (see Lindner, 2016), there are no monetary entitlement criteria, and no waiting period is required for UI benefit receipt. UI benefits in Spain initially replace 70%

of the reference earnings for up to 180 days and then 50% until benefit exhaustion. The reference earnings correspond to the average contribution base over the 180 days before UI start, with a floor and contribution ceiling (which were correspondingly at 944.40€ and 4,070.10 euros per month in 2019). The UI benefit is then limited by a maximum and a minimum that depend on the number of dependent children varying, e.g., from 80% to 175% of IPREM for workers without dependent children and from 107% to 225% of IPREM for workers with two or more dependent children.[7] The maximum and minimum are reduced in proportion to hours worked in the previous job in relation to the company's normal full working day. Finally, the total UI benefit duration increases stepwise with the contribution record starting at 120 days of benefit receipt for those who contributed 360 to 539 days up to 720 days for those who contributed 2160 days (i.e., fully) in the previous six years (see Figures A1 and A2, and OECD (2019) for more details).

Most workers who exhaust UI or are ineligible for it are entitled to "unemployment assistance" (UA, *subsidio de desempleo*), which is non-contributory, means-tested, but also taxable. The UA benefit is equal to 80% of IPREM (equivalent to about 22% of the average wage in 2019), with a minimum benefit duration of six months (OECD, 2019). Unemployed workers older than 45 can access UA benefits under less demanding requirements, for a longer period, and receive higher benefits if they have dependents.[8]

[7] IPREM *(Indicador Público de Renta de Efectos Múltiples)* is an index used for the calculation of social benefits, which in 2019 was equal to 537.84 euros per month or 7 529.76 euros yearly, including two bonus payments.
[8] To compute individuals' potential UI benefit duration, one must consider the most recent employment record since their last use of unemployment benefits, looking back up to a maximum of six years.

Spain's Sickness Insurance System

Benefits from sickness absence (SA), or temporary sick leave, in Spain compensate for the loss of income suffered by workers who are temporarily unable to work because of a common contingency (cc) (ordinary illness) or a work-related (professional) illness. Eligibility for either requires the claimant to be currently employed and in the case of SA due to cc to have contributed for a minimum of 180 days in the previous five years preceding the legal status of SA. In this paper, we focus on SA spells due to cc, which represent the vast majority of all new SA episodes.[9] For SA due to cc, there is a waiting period of three days and benefit receipt generally starts from day four on SA at 60% of the reference earnings.[10] The reference earnings correspond to the average daily contribution base in the previous month to SA start, with a contribution ceiling. In all SA cases, the employer bears the cost between days 4 to 15, and the National Social Security Insurance (NSSI) covers the cost from day 16 onwards up to a maximum of one year. This period can be extended by six additional months (for up to 545 days) if the NSSI determines the worker is expected to recover and return to work during this period. From day 21 on SA the replacement rate is at 75% of the reference earnings until benefit exhaustion (see Figures A1 and A2, and Seguridad Social (2021) for further details). Individuals who exhaust the extension of their SA benefit period or are not expected to recover and return to work by the Social Security can apply for DI benefits.[11]

SA episodes require certification from a physician from the beginning of a work-absence spell—which possibly reduces moral hazard (Johansson & Palme, 2005)—and include health care from the first day of absence. Health insurance coverage in Spain is unaffected by

[9] For instance, its incidence rate per 100 000 workers was of about 2500 in 2019 compared to 120 for work-related absences (Seguridad Social, 2023).
[10] Most workers and employers in Spain are also governed by collective bargaining agreements. These sometimes ensure benefit coverage for SA due to cc by the employer for the first three days and/or top-up payments to ensure a replacement rate of up to 100%. While many and very specific collective agreements exist in Spain, which we cannot map to the workers in our data set, the rules that govern those agreements are relatively constant over time and they are very unlikely to have changed at the time of the labor market reform that we study in this paper.
[11] As explained further below, we right-censor all individual spells at 17 months. Our follow-up is therefore too short to study transitions into DI. We leave this important issue for future studies.

job loss, as there is a statutory quasi-universal health care system (Flores, 2021). Importantly, all workers on SA during our sample period were subject to a "very tight monitoring system" (Marie & Vall Castelló, 2023, p.926).[12]

Transitions between Unemployment Insurance and Sickness Insurance

Workers on UI can transit directly to SA. If the SA spell is a "relapse" (i.e. a recurrence of an earlier diagnosed illness that required absence from work and occurs within 180 days of this earlier sickness spell), the new SA benefit level will equal the previous unemployment benefit (UB) level and SA benefit receipt will continue up to a maximum period, as explained above, even if the UI entitlement ends earlier. If the SA spell is not a relapse but rather a new SA episode, its benefit level will equal the previous UB level while the SA spell lasts, but only until the UB entitlement is exhausted. If the SA spell lasts longer than this, the SA benefit level will be adjusted to 80% of the monthly IPREM. The UB entitlement period is never extended (Seguridad Social, 2021). Thus, only the former group (those whose SA spell is a relapse) have a financial incentive to transit from UI to SA, especially if they are close to UB benefit exhaustion.[13]

In practice, when studying transitions to the first non-UI spell among UI recipients in our final sample (which includes the first 17 months since entry into UI, spanning from January 2012 to June 2014), we find that virtually none of them transit directly to SA (0.2%). Instead, most of them transit to employment (64%), and the rest to UA (16%) or remain on UI (19%) (see Figure A3). Nevertheless, among those who have a SA spell during our sample period,

[12] This was the result of reforms in the mid-2000s, which turned Spain into one of the OECD countries with the most elaborate monitoring system. For further details, see Marie & Vall Castelló (2023) and OECD (2010).

[13] While there are restrictions on dismissing workers on SA, in practice workers on SA can also transit directly to UI. Workers who are on a SA spell due to cc when their employment contract ends will continue receiving a SA benefit equal to the UB they are entitled to at job loss until the SA spell ends, with a maximum period as explained in the earlier subsection. This period will be subtracted from their UB entitlement period. If the SA spell ends before the exhaustion of the UB entitlement, the worker will be eligible to UB if the corresponding criteria are met. If the UB entitlement has been already exhausted, the worker will be eligible for UA (Seguridad Social, 2021).

one out of four come from UI (see Figure A4). Unfortunately, we do not have data on SA episodes before 2012 to identify unemployed individuals with a relapse for whom transiting to SA is financially more attractive compared to those who do not experience a relapse. Evidence for a period (2010-2014) that overlaps with ours shows that 4.8% of all SA spells in Spain were due to a relapse, 4.6% in the private sector (which is the one we focus on) and 5.1% in the public sector (Marie & Vall Castelló, 2023). Finally, and as shown below in the 'Results' section, our main findings are robust to dropping or controlling for those who were on SA when they entered UI.

The 2012 Labor Market Reform

In the aftermath of the Great Recession, the Spanish government reduced the RR from 60% to 50% after 180 days of UI benefit receipt for all spells starting after July 14, 2012. The RR during the initial 180 days remained unchanged at 70% (see Figure A2). The implementation of the reform was clearly exogenous to the individuals and politically determined. The reform was unexpected and was announced only four days before coming into force on July 15, 2012 (the actual details regarding the decline in the RR were made public for the first time on July 13, 2012). There is no evidence that the reform was endogenous in the sense of being a reaction to increasing URs as it decreased rather than increased UI generosity. Other EU countries back then reduced their UI benefits as well, following the recommendations of the European Commission which was concerned with avoiding a sovereign debt crisis at the EU level (see RS-RP). Circumstances in the U.S. were rather different, as during the Great Recession and its aftermath, maximum UI benefit receipt periods were extended (Mueller et al. 2016).

Unlike in many other settings, partial compliance is not an issue in our context since everyone entering UI after July 14, 2012, with a certain contribution record was or would have been unambiguously and comprehensively affected by the cut in UI generosity if s/he remained longer than 180 days on UI. One can also exclude that selection into or out of treatment drives the results, which is a central issue in other settings, for instance, when active labor market programs are evaluated.[14]

Data

Data sources

To examine how a reduction in UI generosity affects the probability of having a SA, we employ restricted-use linked register data from the Spanish Social Security (SS) system and detailed medical records on SA for the region of Catalonia (Spain). SS data are taken from the 2012-2015 annual waves of the Continuous Working Life Sample (in Spanish, *Muestra Continua de Vidas Laborales*, and hereafter MCVL) (Durán Heras, 2007). Each wave contains a random sample of 4% of all the individuals who contributed to the SS system during at least one day in the year of data extraction. This includes those who worked, were on UI, or received a contributory benefit, such as a DI or an old-age pension, as well as those who received a non-contributory benefit such as UA. A given wave of the MCVL does, therefore, not include individuals without any contact with the SS in that given year. While this may create some risks of sample selection bias, especially among women, immigrants, and young workers (García-Pérez et al., 2019), we minimize this potential selection effect by pooling data from four waves of the MCVL. That is, we cover every random person who had a relationship of at least one day with the SS administration during these four years. For that random person, we observe his/her full employment history from when s/he entered the labor market up until

[14] A more general labor market reform was introduced by the Spanish government on February 10, 2012, that affected collective bargaining agreements at the firm level and reduced dismissal costs for permanent workers (Real Decreto Ley 3/2012). As our inflows into unemployment span from January 1, 2012, to January 31, 2013 (see 'Sample Selection' section), this other reform affected most of our workers in the same way. RS-RP moreover show that inflows during January and the first ten days of February do not bias their estimates of the decline of the RR on employment by selecting only inflows into unemployment within three months of July 15, 2012. This result is consistent with the evidence of modest labor market effects of this reform, at least in the short term (OECD, 2013).

December 31, 2015. Records in the MCVL (as well as in the other administrative data set that we use) are at the spell level. For each (non-) employment spell we know the exact duration and several variables describing the characteristics of that spell.[15] For instance, for employment spells these characteristics include the occupational category, type of contract, type of workplace, type of employer, location of employer, and economic sector. There is also information on personal characteristics (age, gender, nationality, and level of education), as well as administrative information on earnings and a few health-related variables (spells of permanent sickness or disability, its severity, and the date of death without a medical diagnosis).

The advantage of our restricted-use version of the MCVL is that for every individual in the MCVL residing in Catalonia, and for the same period (2012-2015), we have their complete detailed medical records of SA due to cc provided by the Catalan Institute of Medical Evaluations (*Institut Català d'Avaluacions Mèdiques*, ICAM). Records from earlier (or later) years are not available. This information is also available at the spell level and includes mainly the starting and closure dates of each SA spell and medically certified diagnoses based on ICD codes (ICD-9 and ICD-10). Potential measurement error in our health variable because of recall bias, justification bias or reporting bias is thus less of an issue than in studies that rely on survey data (Flores & Kalwij, 2019).[16]

Sample selection

Our sample selection resembles that of RS-RP. We select all 20-to-52-year-olds full-time employees who became unemployed between January 1, 2012 and January 31, 2013, in

[15] In the MCVL there is a potential issue of measurement error in the number of employment and unemployment spells and their duration because firms can offer contracts for very short periods but subsequently recall workers. We treat overlapping job and unemployment spells in the MCVL as in García-Pérez (2008).
[16] There are two versions of the MCVL, with and without tax records. The linked one that we use in this paper is without tax records.

Catalonia and who had worked for at least 12 months within the previous six years (as otherwise, they would not be eligible for UI benefits). Those above age 52 are excluded because the reform we study was part of a broader package that included policy changes affecting UA in this age group; in particular, it increased the minimum eligibility age to receive UA from 52 to 55 (see Domènech-Arumí & Vannutelli, 2023). Similarly, we also drop those who were employed as public sector workers during our sample period, as at the time of this reform the government also reduced the generosity of their sick leave benefits, leaving however that of private sector employees unchanged (see Marie & Vall Castelló, 2023). Self-employed workers are not considered either as they were not eligible for UI benefits back then. The focus on workers displaced from full-time jobs is because the RR for part-time workers depends on the number of hours worked (see subsection 'Spain's Unemployment Insurance System') and the reform also modified how their RR was computed (RS-RP). We thus exclude individuals who before their UI spell in 2012 worked part-time, thereby considering up to six previous years for those who were continuously employed, or the time since their last UI spell if they had one (cf. RS-RP).[17] This selection yields 5,033 individuals, which we follow for up to 17 months (268,485 weekly observations).

While our (weekly) data goes until the end of 2015, we right-censor all individual spells at 17 months to allow for a broadly similar maximum follow-up to a SI reform that was passed on July 18, 2014. This policy reform modified the management and control of SA episodes during their first year of duration (BOE 2014). The latest unemployed individuals we include lost their jobs by the end o+f January 2013. This censoring creates a balanced sample in event time (and hence not in calendar time, see Figure A5). As shown, over long event time ranges, the sizes of the four groups we are comparing remain very stable, alleviating concerns that

[17] The fraction of part-time workers in Spain has been traditionally below 10% of the labor force. In 2012, it was at 14%, well below the EU-28 average of 19%, and the rate in France (18%), Germany (26%), or the Netherlands (49%), which has the highest rate in the EU (Eurostat, 2020).

compositional changes in our sample could be influencing our results (we return to this issue in the next section). Relatedly, nearly all individuals in our sample have a complete follow-up of 17 months (see Figure A6).

Similarly to RS-RP, we make a few additional sample adjustments. We exclude temporary layoffs, i.e. individuals who are recalled to their prior firm, as they may not be searching for a job and are less likely to be affected by the decline in the RR after six months on UI. This includes a special category of temporary layoffs termed *Expedientes de Regulación de Empleo* (*EREs*), who remained unaffected by the reform as their RR was not reduced after 6 months on UI benefits (in total, 452 individuals). Because temporary layoffs typically return to their employer, they are e.g. less likely to become stressed due to a potential future decline in UI benefits. Consistent with this interpretation, Inderbitzin et al. (2016) do find smaller effects of extended UI benefits ("REBP effects") when recalls are added to the sample. After a final small correction to exclude individuals with a very short initial UI spell who were working by the end of that same week (16 individuals), we ended up with a sample of 240,476 weekly observations for 4,567 individuals (3,618 individuals with an UI entitlement period of more than 180 days, and 949 individuals with just 120-180 days of UI entitlement). In our empirical analyses below, we keep only the first spell in the corresponding outcome variable that we are studying. In terms of missing values in our control variables, this affects mostly some of our worker characteristics at UI start. In our main analyses, we include a dummy variable to account for a certain worker characteristic being missing, but we also perform sensitivity checks where we exclude individuals with missing worker characteristics. For completeness, we note that we nevertheless lose two individuals when adding all our control variables to our empirical models. Summary statistics of our final sample for the two groups we are comparing before and after the reform are shown in Table A1.

Identification strategy

To estimate the causal effect of a reduction in the RR of UI benefits on SA, we leverage the quasi-experimental variation generated by the 2012 policy reform. Our identification strategy is similar to that of RS-RP and relies on a DiD approach. Besides static DiD models, we also estimate event study models to assess whether the effects of the reform are dynamic. In both analyses, identification comes from comparing the probability of having a SA for UI recipients who got displaced after July 14, 2012, and whose RR after 180 days of UI benefit receipt dropped from 70% to 50% to similar workers who lost their job before July 15, 2012, and whose RR after 180 days dropped from 70% to 60% only. Furthermore, to obtain more reliable estimates of the impacts of the reform (e.g. to avoid the risk that time-specific shocks may induce biases if one compares before versus after work absence behavior (Marie & Vall Castelló, 2023)), we include a comparison group of UI recipients with similar potential UI benefit levels, but who remained unaffected by the reform. These are workers who got displaced from their jobs at the same (i.e. either before or after the reform) and were entitled to at least 120 but no more than 180 days of UI benefit receipt. Their RR after 180 days on UI remained thus unaffected by the reform.

We implement our event analyses by estimating an equation of the following type:

(1) $$y_{it} = \alpha + \beta D_i^T + \gamma D_i^{Post} + D_i^T \times D_i^{Post} \times \sum_{\mathrm{m}=1}^{M} \delta_m \mathbf{I}\,(t - t_i^* = m) + \gamma_m + controls + \varepsilon_{it}$$

where *i* denotes individuals and *t* stands for time, measured in weeks since entry into UI. The dependent variable y_{it} is a dummy, which equals one if an individual *i* has a SA (or another of the outcomes we study) in week *t*. D_i^T is a dummy that takes the value one if the individual is entitled to more than 180 days of UI benefits and thus potentially affected by the reform, and zero if the individual is in the comparison group of individuals with UI entitlements of 120-180 days. D_i^{Post} is a dummy equal to one if the worker entered unemployment after July 14, 2012, and zero if before July 15, 2012. γ_m are monthly event dummies and $\mathbf{I}(t - t_i^* = m)$ are the corresponding indicator variables (measuring time in months relative to an individual's month of entry into UI, t_i^*). Our parameters of interest are the dynamic treatment effects on the treated δ_m. These measure the change in our outcome variable during month *m* since UI start among affected individuals relative to similar individuals but who remained unaffected by the reform.[18]

The variables in $controls$ differ across specifications. Because calendar time varies substantially across event time (see Figure A5), we first include monthly calendar dummies. These are meant to capture any remaining seasonality and other general time effects in our outcome variable that are not related to the policy change. Next, in what we term our "Preferred" specification, we add dummies for the province, gender, and birth year, as well as nationality, occupational category, economic sector and type of contract defined at UI start to avoid potential problems of "bad controls". We then implement a specification similar to RS-RP which adds UI benefit entitlement at UI start (in months) and substitutes the monthly event and calendar dummies with a monthly time trend, quarterly dummies, and the quarterly GDP growth rate. In our last specification ("Full"), we add UI benefit entitlement at UI start to our "Preferred" specification. The inclusion of these individual-level variables (e.g. birth year, gender, economic sector, and occupation) allows us to control for potential labor force composition changes around the policy period.[19] We estimate equation (1) with a linear

[18] Based on a visual inspection of the data (see Figure A7), and to reduce the "multicollinearities abound" problem in our event study data structure (Miller, 2023), we impose a common pattern in event time for those who remained unaffected by the reform (i.e. those with 181+ days of UI entitlement who entered UI before the reform, and those with 120-180 of UI entitlement who entered either before or after the reform), though allowing for a shift in the event patterns for each of these groups. Nevertheless, in the robustness section, we test for the sensitivity of our dynamic treatment effects to these extra restrictions.

[19] One may also worry about potential compositional changes around the policy introduction due to variations in the aggregate unemployment rate (UR) that could change the kind of unemployed workers who start SA spells postreform (e.g., in worse health). We do not expect this to be especially relevant in our context. The UR attained unprecedented levels, ranging between 24% and 27%, during our sample period (2012T1-2014T2). It began at approximately 24% in 2012T1 for both men and women and remained within 10% of this value until 2014T2. The sole exception occurred in 2013T1, where the UR was 11% higher than in 2012T1 (data extracted from the INE database, the Spanish National Statistics Institute, at https://www.ine.es).

probability model and report heteroskedasticity-robust standard errors clustered at the individual level.[20]

In addition to event study analyses, we present static DiD estimates as a summary of the effect across all post-reform months and to perform sub-group analyses where we may lack statistical power to conduct appropriate event analyses. These are estimated using the same equation except that the event study interaction terms are replaced with a single interaction term denoting an individual entitled to more than 180 days of UI benefits who entered unemployment in the post-reform period, $D_i^T \times D_i^{Post}$. As this interaction term shows, it is not possible to include individual fixed effects in our empirical models (having an UB entitlement period of more than 180 days and having entered UI after the reform are both time-constant at an individual level). Furthermore, because we do not have medical records on SA prior to January 2012 (which is when the first unemployed individuals entered our sample), we cannot test for "pre-trends" in SA as suggested by Rambachan & Roth (2023).[21] Our analysis however does rely on the assumption that in the absence of the reform, the differences in SA between individuals entitled to more than 180 days of UI benefits and just 120-180 days would have remained constant.[22] Finally, as made clear above, we do not make any "forbidden comparisons" of treated to already-treated individuals (by comparing "late adopters" to "early

[20] Recent studies who assessed other related policy reforms in Spain, contemporaneous to ours, cluster at a different level (e.g. at the province-year-quarter level, see Domènech-Arumí & Vannutelli (2023)) and compute post estimation wild bootstrap p-values (MacKinnon & Webb, 2018; Roodman et al., 2019) that account for the small number of treated clusters. Doing so produces smaller standard errors (see 'Robustness and Placebo Tests' section). Instead, and besides the more conservative approach of clustering at the individual level (as in e.g. RS-RP), we also present Romano-Wolf p-values (Romano & Wolf, 2005; Clarke et al., 2020) when appropriate to test whether our estimates are sensitive to multiple hypothesis testing.

[21] Before their UI start, individuals are generally working and not on SA. However, one out of ten who experienced a SA spell in our final sample were on SA when they entered UI (RS-RP did not consider this possibility in their analyses). As shown in the 'Results' section, our main findings are robust to dropping or controlling for those who were on SA when they entered UI.

[22] We also assume that the reform had no causal effects before its implementation (no anticipatory effects), which is very plausible given the four-day lag between the very first announcement of the reform on July 11, 2012, by the Spanish Prime Minister, Mariano Rajoy, and its implementation on July 15, 2012 (see 'Institutional Background' section). Below, we conduct a placebo test to assess the validity of this assumption.

adopters") (see Goodman-Bacon, 2021), as our comparison group includes only never-treated individuals.

Results

Descriptive evidence

Descriptive evidence on the impact of the reform is given in Table A1 and in Figure A7. Panel A in Table A1 presents sociodemographic and pre-displacement job characteristics of UI recipients in the treatment and comparison groups before and after the reform. While there are several differences in the post-reform period between those affected by the reform and those not affected (e.g. in terms of occupational category and nationality at UI start, age, and gender), most of these differences existed before the reform and are thus "washed out" by our identification strategy, as shown in the last column. The only difference across time that remains is the increase in the share of women among those affected by the reform (the difference in the share of individuals aged 40 and older is statistically significant at the 10 percent level only). The subgroup analysis below explores whether our results hold across these different groups of displaced workers. As we will see, the reform increased the probability of having a SA among both, men and women.

Panel B in Table A1 reports the share of overall SA and employment spells in the first 17 months since UI start by treatment status and time of displacement. It also shows the corresponding numbers for the main subtypes of SA spells that we study. Before the reform, the share in SA for individuals in the comparison group was slightly larger (1.6%) than for those in the treatment group (1.1%), though the difference is not statistically significant. After the reform, this share more than doubled for those in the treatment group (2.8%) while it remains stable for those in the comparison group (1.5%). Overall, this results in an increase of 1.9 p.p. in the probability of having a SA spell that is statistically significant at the 1 percent level. Instead, for employment the overall reform effect is close to zero, as the existing differences before the reform remain rather stable afterwards.

Figure A7 shows that from the onset of entry into UI, the share in SA is around 2 p.p. higher among affected individuals (with UB entitlements of 181+ days who entered UI after the reform) compared to their unaffected counterparts (with similar UB entitlements but who entered UI before the reform). This difference widens until about month seven since UI start, shortly after the reform begins to take effect, and then slightly narrows to around 1.5 p.p. at the end of our follow-up. Conversely, there are minimal differences in the patterns of individuals in our comparison groups with shorter UB entitlement periods (see Figure A7).[23]

Main Results

We start this section with the results from our event study analysis of the effect of the reform on SA spells, which are shown as an event study plot in Figure 1. The coefficients and 95% confidence intervals of the interactions between the DiD estimator and dummies measuring the distance to UI start in months are displayed. 17 post-treatment effects are included, along with a full list of controls, as in our "preferred" specification of Equation (1). Standard errors are clustered at the individual level.

The effects of the reform on SA manifest already in the first month following UI start and last for at least one year. The point estimates suggest an increase in SA of about 2-3 p.p. during the first 9 months since UI start, which then slightly decreases to just below 2 p.p. after one year. The coefficients remain positive during the rest of the period, but the CIs are too large to rule out a zero effect. Nevertheless, we cannot reject the null hypothesis of homogeneous effects throughout our follow-up period (p-value=0.448).

[23] By calendar time, we observe an increase in the share of overall SA spells, which is driven by those lasting longer than 20 days and not by the short (1-3 days) and medium-length spells (4-20 days). Moreover, given the similarity in prevalence over time of the latter two, in what follows, in terms of duration, we will distinguish spells that last 1-20 and 21 days or more (see Figure A8).

<Insert Figure 1 about here>

A summary of the effects of the UI reform on SA is given in Table 1. The table shows the results from different specifications, starting from the simplest one with just the main effects end event time dummies (column 1) to others where we sequentially include calendar time dummies and other control variables (columns 2 and 3). Our preferred specification is that in column 3, which corresponds to that in equation (1). Column 4 presents a specification close to that in RS-RP (the main difference here is that instead of monthly calendar dummies, these authors include other controls/proxies for time and seasonality effects). Column 5 includes all the additional variables from the RS-RP specification that were not part of our preferred specification in column 3.

Our results indicate that the reform increased the probability of having a SA episode, regardless of the specification used. Compared to those unaffected by the reform, unemployed individuals whose UB were or would have been reduced exhibit approximately a 2 p.p. higher probability of having a certified SA episode. This effect on SA is, moreover, substantial in relative terms, representing approximately a tripling of the probability of SA, and is significant at the 1% level.

<Insert Table 1 about here>

Mechanisms: Financial Stress and Moral Hazard

Financial stress and moral hazard are potential mechanisms that may explain how a decline in UI generosity due to the 2012 reform increased the probability of having a SA spell. The empirical challenge is to disentangle these two mechanisms, and, to do so, we rely on indirect evidence. Specifically, we examine whether the effects of the reform vary by sub-types of SA, considering the nature of the spell (e.g. stress-related or not), its duration (short or long), and its timing (whether it began immediately after entry into UI or later). For instance, longer SA spells will typically imply a more severe illness, as they always require a confirmation from the initial certifying physician and, in some cases, a clinical confirmation by a specialist. Longer spells will thus be less affected by moral hazard behavior.[24] Regarding timing, if an immediate effect is observed, it *could* be argued that a rational, cost-based switch to SI is driving the shift.

Moral hazard would occur if some unemployed workers had adjusted their job-search effort and SA behavior in response to the decline in the generosity of the UI system (e.g., Hall & Hartman, 2010; Johansson & Palme, 2005). Individuals with a certain contribution record who entered UI after the July 2012 reform experienced a significant reduction in their RR, dropping from 70% to 50% after six months of UB receipt, compared to those who entered before the reform, whose RR decreased just from 70% to 60%. Since the RR from SI remained unchanged and relatively high, at 75% from day 21 onwards, the incentive for affected individuals to transition to SI (without being genuinely ill) increased after the reform.

Simultaneously, the substantial 17% decrease in the RR among affected individuals could have introduced an additional source of financial stress for this group with regard to the comparison group, whose RR remained unchanged after the reform. Medical literature indicates that anticipated income losses can be as stressful and detrimental to an individual's health as actual losses. The adverse effects of the RR decline were likely intensified by the context of record-high URs of around 25%, particularly among unemployed workers with low levels of employability or with less protected jobs. Online Appendix A.1 provides a detailed discussion of the medical evidence explaining how exposure to (financial) stress can deteriorate health (and eventually lead to SA take-up).

[24] In contrast, "short-term sick leave is mostly determined by flues and light illnesses that clearly leave more space for moral hazard, especially when monitoring is light" (Ziebarth, 2013, p.291.). Light monitoring, though, is unlikely in our setting, given the "very tight monitoring system" to which workers on SA were subject to during our sample period (Marie & Vall Castelló, 2023, p.926).

Measuring stress is inherently difficult due to its complex nature; it affects nearly all systems of the body and manifests differently across individuals (Epel et al., 2018; Fink, 2017). Additionally, its diagnosis relies often on patients' subjective assessment, which may allow for a moral hazard behavior. We attempt to overcome these issues by selecting *all* relevant ICD codes that are potentially caused or exacerbated by stress, which should also mitigate concerns about cherry-picking stress-related outcome variables. We further distinguish between those that are observable or measurable from those that are purely subjective (details on how we identify the stress-related ICD codes and on how we classify them into "measurable" and "non-measurable" are given in the online appendix). The share of measurable diseases, as it turns out, is too small to draw meaningful conclusions, but we also conducted separate analyses on subsets of what we think as more objective diseases that reinforce the evidence of an effect on SA through financial stress. Still, it is worth highlighting that while most of our stress-related diseases are to some extent subjective, they are all medically diagnosed, whereas the literature studying the effects on mental health has often relied on self-administered questionnaires that inquire about mental health problems (e.g. Braghieri et al., 2022).

Our analysis by subtypes of SA episodes, shown in Table 2, reveals that the overall effect on SA is driven by musculoskeletal diseases (MSD) and by stress-related diseases. These effects are statistically significant at the 5% level or less, and relatively large when compared to the corresponding pre-displacement mean of potentially affected individuals, particularly for stress-related diseases. Within the stress-related category, we observe an effect in the non-measurable (subjective) component but find no evidence of an effect in the measurable (objective) component, although our sample is probably underpowered to detect a difference in this regard. We do not observe any effects on other major groups of ICD codes, such as mental diseases, cardiovascular diseases (CVDs), and traumas (we return to this latter one below). However, we do find an effect on mental diseases when combined with diseases of the nervous system, which corresponds to a subcategory of non-measurable stress-related diseases.

<Insert Table 2 about here>

When distinguishing by the duration of the SA spell, we find that the overall effect of the policy reform is driven by longer SA spells, i.e. those lasting 21 days or more (see Table 3). This holds for overall SA spells, as well as for the sub-types which showed a significant effect in Table 2. Moreover, all the positive effects on long SA spells remain significant at the 5% level when accounting for multiple hypothesis testing, while the ones on MSD and stress-related diseases from Table 2 remain significant at the 10% level.

<Insert Table 3 about here>

Heterogeneity

One natural follow-up question to the earlier findings is: Do the measured health effects for affected unemployed workers vary by observable worker characteristics? Earlier studies have argued that the impact of UI on health may vary across worker occupations or gender if such groups face different outside options or value work differently (Ahammer & Packham, 2023). This section conducts various heterogeneity analyses in terms of individuals' pre-displacement job characteristics and sociodemographic traits. The analyses are carried out both on overall SA spells and on sub-types for which we found significant reform effects in the previous analyses. Here, we exclude individuals with missing worker characteristics at UI start (or missing sociodemographic information), but as shown below in the 'Robustness and Placebo Tests' section, doing so does not change the main findings of the paper. The results are given in Tables 4 and 5.[25]

[25] The linked version of the MCVL that we use in this paper does not include tax records. This prevents us from exploring whether the reform effects are e.g. larger among low-income individuals. Instead, we use the occupational category (rather than education) as a proxy for income or, more generally, socioeconomic status. We note that education, while recorded in the MCVL, is not regularly updated and subject to systematic measurement

In terms of occupational category, we distinguish between those who worked on a "Skilled, non-manual", "Skilled, manual", "Unskilled, non-manual", and "Unskilled, manual" job at UI start (columns 1 to 4 in Table 4). The increase in SA spells is driven by workers with intermediate occupational categories, that is, skilled manual workers and unskilled non-manual workers. The effects among skilled manual workers are particularly large and broad across all SA subtypes, both in absolute and relative terms, while for unskilled non-manual workers we find an effect on stress-related SA spells. There is no or very limited evidence of an effect among workers with extreme occupational categories, i.e., those who worked as skilled non-manual and unskilled manual workers before job loss.

By economic sectors (columns 5 to 7 in Table 4), we find evidence of relatively large effects in the construction and services sectors. Interestingly, the effects in the construction sector are driven by musculoskeletal diseases, while in the services sector they are driven by stress-related diseases. Finally, when distinguishing between individuals with different levels of job protection, i.e., with a permanent and non-permanent contract at job loss (columns 8 and 9 in Table 4), we find a consistent effect among those with lower job protection both on overall SA spells and on each of the subtypes we study. Among those with a permanent contract at job-displacement, we also find an effect on stress-related SA.

<Insert Table 4 about here>

Our last heterogeneity analyses by sociodemographic traits in Table 5 show that the effect of the reform is driven mostly by men (column 1), though there is also an effect on stress-related SA among women (column 2), by natives (column 5), and by those under age 40 only (column 3), as there is very limited evidence of an effect among those aged 40 and older (column 4).

error. For instance, the level of education is likely underestimated for native individuals who never changed their municipality of residence (Gonzalez & Ortega, 2011).

<Insert Table 5 about here>

Additional outcomes: Employment

We also looked at the effects of the reform on employment, motivated by the earlier work of RS-RP and because employment, in our setting, could be a pathway through which the effects of the reform on SA operate (as shown in Figure A4, most individuals on SA transitioned from employment).

Our employment results are shown in Figure 2 and in Table A2. Consistent with the findings in RS-RP, we find that the reform had a positive effect on employment in the short run (Figure 2) and an overall zero effect in the longer run (Table A2). The results from our event study model show that the overall zero reform effect on employment is driven by initial positive effects occurring before month six, when the RR decreases, which turn into negative effects afterward. Thus, it appears that some of the unemployed workers were "activated" early by the reform and found a job before the actual reduction in their RR occurred (perhaps via increased job search effort).

<Insert Figure 2 about here>

Robustness and Placebo Tests

We conducted various robustness and placebo tests to corroborate the validity of our main findings. First, despite the restrictions on dismissing workers on SA, workers on SA can transit directly to UI (see 'Institutional Background' section), and in our sample some individuals were indeed on SA when they entered UI. As shown in Table 6, the effect of the reform drops from about 2 p.p. to just below 1 p.p. when we control for being on SA at UI start (Robu1) or drop those individuals who were on SA when they entered UI (Robu2). Yet, both effects remain significant at the 5% level or less. Second, one may question the inclusion of back pain (BP) diagnoses—the main category of musculoskeletal diseases and around 13% of all SA spells in our sample. BP is more difficult to be objectively diagnosed and earlier studies found its prevalence to be sensitive to changes in benefit entitlement among DI claimants in both the U.S. (Deshpande and Li, 2019) and the Netherlands (Godard et al., 2022). We therefore excluded all BP diagnoses from our sample. Doing so reduces somewhat the effect of the reform to 1.6 p.p., though it remains significant at the 5% level (Robu3). Third, some worker characteristics (especially occupation at UI start were missing for a non-negligible share of individuals in our treatment group (much less so for those in the comparison group, see Table A1). We do not have an explanation for this, but excluding those with missing worker characteristics at UI start does not lower the size or statistical significance of our estimate of the reform effect (Robu4). Fourth, we also changed the level at which standard errors are clustered and clustered them at the province-year-quarter level (cf. Domènech-Arumí & Vannutelli, 2023) and computed post estimation wild bootstrap p-values (MacKinnon & Webb, 2018; Roodman et al., 2019) that account for the small number of treated clusters. As shown, this does not reduce the statistical significance of our estimated reform effect (Robu5).[26]

<Insert Table 6 about here>

We also conducted placebo tests. Methodologically, we have relied on the assumption that, in the absence of the reform, the differences in SA between the treated and comparison groups would have remained constant. As this assumption is not testable, we carried out a placebo test where we used a different fictitious policy date (May 1, 2012) and included only workers displaced between January and August 2012 for the analysis. Doing so delivers a

[26] Finally, we also tested the sensitivity of our dynamic treatment effects to our assumption of common patterns in event time for those who remained unaffected by the reform (i.e. those with 181+ days of UI entitlement who entered UI before the reform, and those with 120-180 of UI entitlement who entered either before or after the reform). This assumption which still allows for different *levels* of the patterns between the three groups was meant to reduce the "multicollinearities abound" problem in our event study data structure (Miller, 2023). Nevertheless, allowing, for instance, as well for a differential pattern in event time for those with entitlements of 181+ days who entered UI before the reform leaves our empirical results unchanged (see Figure A9). We also note that the dynamic treatment effects would become somewhat larger and statistically more significant if we excluded all our controls other than the event time dummies (results available upon request).

precise zero coefficient on the effect of the reform on SA (Placebo1). This further strengthens our assumption of no anticipatory behavior during the months preceding the reform.[27] As a final placebo test, we explored the effects of the reform on *Traumas*, which include "Injuries, poisonings, and certain other consequences of external causes" that we argue can be considered as exogenous to an individual's behavior. As shown in column 4 in Table 2, the point estimate is again close to zero and statistically insignificant.

Discussion and conclusions

Making social insurance schemes, such as UI, less generous during an economic recession can have unintended health consequences. In this paper, we exploited the variation induced by an unanticipated labor market reform in Spain in 2012 to provide the first causal evidence on the health effects of a pro-cyclical change in UI generosity. The reform was motivated by the necessity to cut government spending and comply with the fiscal consolidation efforts required by the European Commission during the European sovereign debt crisis (Domènech-Arumí & Vannutelli, 2023; RS-RP). Nevertheless, while increasing employment in the short run (though not in the longer run), we found that the decrease in UI generosity also caused an unexpected increase in SA episodes of about 0.7 to 2 p.p., representing an increase of 1.7 to 3 times in the probability of SA (depending on the specification). Our analyses by subtypes of SA revealed that the overall effect on SA was driven by illnesses lasting longer than 20 days, by those related to stress, and not (only) by common and more subjective SA spells such as those due to back pain. In terms of population subgroups, we found that the effects were driven largely by male, native, younger workers, with an intermediate occupational category and a non-

[27] Because our SA data starts in January 2012, we cannot use workers displaced, say, one year before the reform, as often done in studies using a DiD approach. Nevertheless, to further test the robustness of our estimates to potential anticipatory effects, we implemented various donut specifications leaving out 2, 4, 6, and 8 weeks around the policy change. The results are given in Table A3 and show that our main results are not sensitive to the donut size choice.

permanent contract at job displacement. When exploring the potential dynamic reform effects, we found that they became apparent already in the first month following UI start and lasted for at least one year.

Our findings suggest that the reform affected different individuals differently and that both financial stress and moral hazard could be possible mechanisms through which decreased UI generosity resulted in higher SA rates. Some of the more employable workers could have been "activated" early by the reform, as they found a job before the actual reduction in their RR occurred. Some of those who transited to SI at the beginning of their UI spell could have been driven by a moral hazard behavior. Finally, amid an economic recession, with URs hitting an unprecedented 26%, workers with more job uncertainty and lower levels of employability may have experienced a worsening in their health due to financial stress, thereby increasing their likelihood of suffering a stress-related and long SA spell.[28]

Are the health effects of decreasing and increasing UI generosity symmetric? Do counter- and pro-cyclical changes to UI generosity impact health differently? Answering these questions is important for policymakers. Kuka (2020) showed that a higher UI generosity, in terms of a higher RR, led to improved self-reported health during periods of high URs, when U.S. states typically extend UI payments. Ahammer & Packham (2023) found that extended UI benefit duration had a beneficial effect mostly on women's mental health. Because their data span from 2003 to 2013 it is not clear though if this represents a pro- or a counter-cyclical change in UI generosity. In conjunction with these studies, our findings suggest that the effects of decreasing and increasing UI generosity on health are to some extent symmetric and that both counter- and pro-cyclical changes to UI generosity will impact individuals' health.

[28] While all workers on SA during our sample period were subject to a "very tight monitoring system" (Marie & Vall Castelló, 2023, p.926), this is especially true for longer SA spells, as they always require a confirmation from the initial certifying physician and are thus less prone to moral hazard behavior.

Considering how multiple social insurance programs affect workers' labor supply decisions is important (Inderbitzin et al., 2016; Leung &O'Leary, 2020). Somewhat unexpectedly, we found the reform effects to be driven by those below age 40 and not by those aged 40+. One possible explanation is that the 40+ transited to other social insurance programs such as the special UA scheme for 45+ (see 'Institutional Background' section). Our sample size was unfortunately too small to separately study the transitions into this special scheme. We leave this important issue, along with the analysis of transitions into DI, for future studies.

Social insurance programs should be designed jointly when they address problems that are inherently related, as happens with unemployment and bad health, which are typically tackled with UI and SI, respectively. Our unintended UI reform effect on health appeared even in a country (Spain) with a universal healthcare system, where health insurance coverage is unaffected by job loss. Our findings thus also yield important policy implications for discussions on optimal UI determination in the presence of relatively generous safety net programs.

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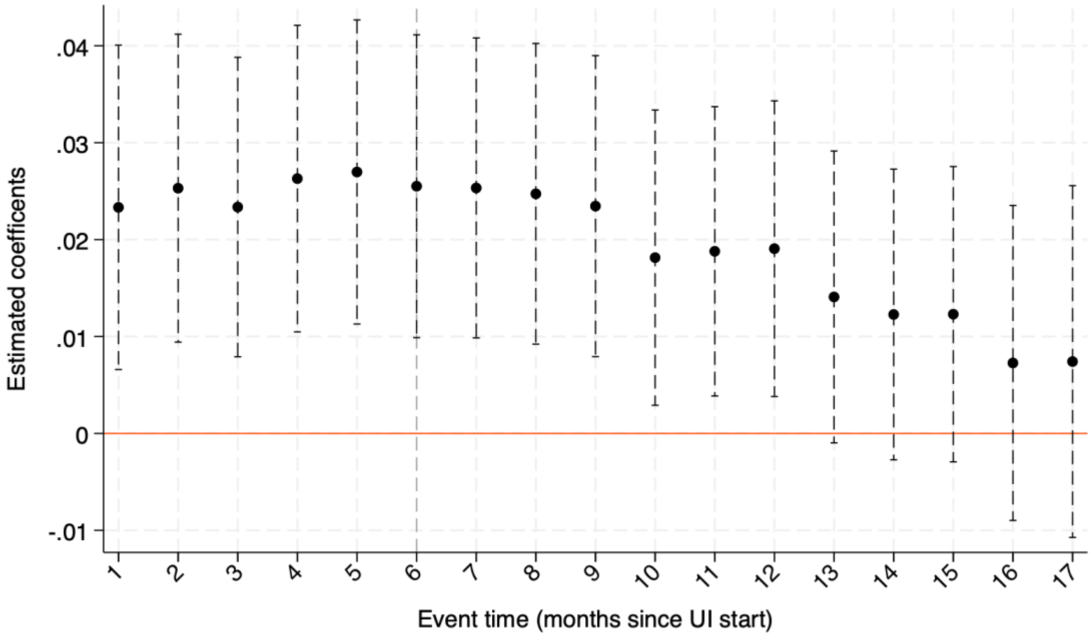
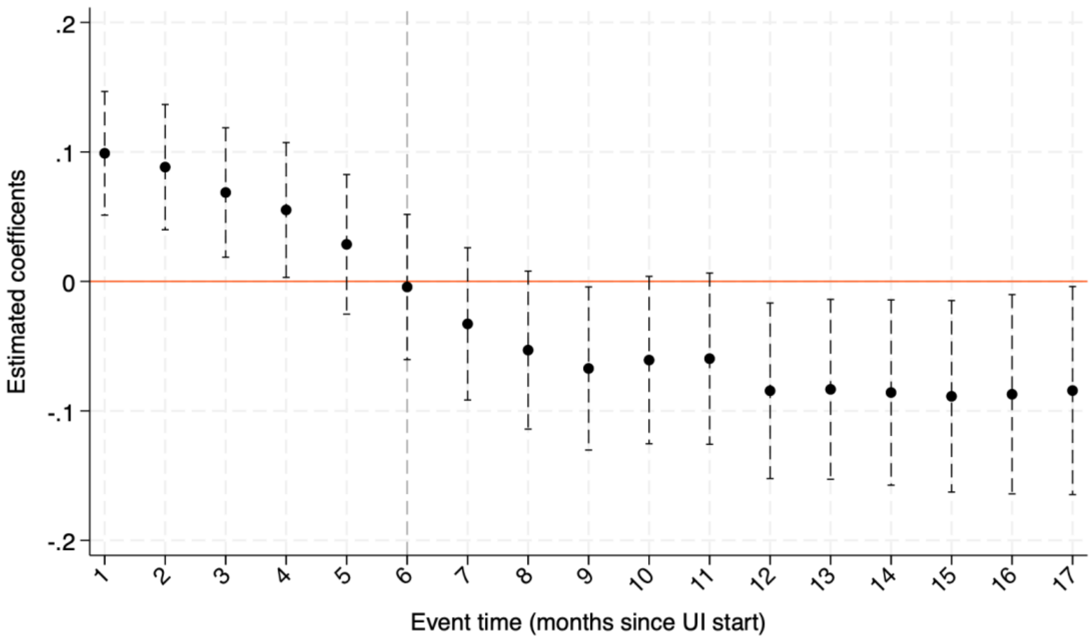
Figures and Tables

Figure 1: Event study analysis of the effect of the reform on SA episodes (0-1)



Notes: The figure shows the coefficients and 95% CIs for the interactions between the DiD estimator and monthly dummies measuring the distance to UI start. Only the first SA spell is considered, later spells are excluded from the analysis, and all individuals are censored at 17 months of follow-up. A full list of controls is included as in our "preferred" specification of Equation (1). Standard errors are clustered at the individual level. The dashed line shows when the actual reduction in the RR would happen (i.e., after 180 days on UI benefit receipt). We do not reject the null hypothesis of homogeneous reform effects in event time (*p*-value=0.448).

Figure 2: Event study analysis of the effect of the reform on employment (0-1)



Notes: The figure shows the coefficients and 95% CIs for the interactions between the DiD estimator and monthly dummies measuring the distance to UI start. Only the first employment spell is considered, later spells are excluded from the analysis, and all individuals are censored at 17 months of follow-up. A full list of controls is included as in our "preferred" specification of Equation (1). Standard errors are clustered at the individual level. The dashed line shows when the actual reduction in the RR would happen (i.e., after 180 days on UI benefit receipt).

Table 1: Difference-in-differences estimates on SA episodes (0-1)

	SA (0-1) (1)	SA (0-1) (2)	SA (0-1) (Preferred) (3)	SA (0-1) (RS-RP) (4)	SA (0-1) (Full) (5)
Treated (0-1) x Post-reform (0-1)	0.020***	0.019***	0.020***	0.021***	0.020***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Treated (0-1), Post-Reform (0-1)	X	X	X	X	X
Dummies for months since UI start	X	X	X		X
Dummies for months to UI reform		X	X		X
Dummies for province, gender, birth year, and nationality, type of contract, economic sector and occupational category (at UI start)			X	X	X
Monthly time trend, quarterly dummies, GDP growth rate (quarterly)				X	
UI benefit entitlement at UI start (in months)				X	X
R-squared	0.005	0.007	0.016	0.014	0.016
Y-Mean	0.018	0.018	0.018	0.018	0.018
Y-Mean (Pre-T)	0.010	0.010	0.010	0.010	0.010
Respondents	4567	4567	4565	4565	4565
Observations	228798	228798	228656	228656	228656

Notes: Difference-in-differences estimates on overall SA episodes (0-1). Individuals are followed during 17 months since UI start. Only the first SA spell is considered, later spells are excluded from the analysis. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: *** p<0.01 ** p<0.05 * p<0.10.

Table 2: Difference-in-differences estimates on subtypes of SA episodes (0-1)

	Mental (g5) (1)	CVDs (g4-g9-g11) (2)	MSK-CT (g13) (3)	Traumas (g19) (4)	Stress-related (5)	Non-measurable stress (6)	Measurable stress (7)	Mental/ nervous (8)
Treated x Post-reform	0.0049*	0.0024	0.0106**	0.0033	0.0124***	0.0105***	0.0019*	0.0050**
	(0.0028)	(0.0016)	(0.0046)	(0.0024)	(0.0035)	(0.0033)	(0.0012)	(0.0022)
MHT-adjusted *p*-values	0.279	0.279	0.079	0.279	0.055	0.067	0.279	0.171
R-squared	0.011	0.008	0.009	0.019	0.012	0.010	0.011	0.012
Y-Mean	0.0033	0.0015	0.0060	0.0033	0.0058	0.0052	0.0006	0.0023
Y-Mean (Pre-T)	0.0014	0.0008	0.0026	0.0028	0.0026	0.0025	0.0000	0.0009
Respondents	4565	4565	4565	4565	4565	4565	4565	4565
Observations	228656	228656	228656	228656	228656	228656	228656	228656

Notes: Difference-in-differences estimates on subtypes of SA episodes (0-1) using our "preferred" specification of Equation (1). Individuals are followed during 17 months since UI start. Only the first SA spell is considered, later spells are excluded from the analysis. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: *** $p<0.01$ ** $p<0.05$ * $p<0.10$. The analysis on *Traumas* serves as a placebo test. The first row in the bottom panel shows Romano-Wolf *p*-values for each outcome to test whether our estimates are sensitive to multiple hypothesis testing (MHT).

Table 3: Difference-in-differences estimates on subtypes of SA episodes (0-1) by spell length

	1-20 days	>20 days	MSK-CT (g13) & >20 days	Stress-related & >20 days	Non-measurable stress & >20 days	Mental/nervous & > 20 days
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post-reform	-0.0004	0.0207***	0.0106**	0.0123***	0.0104***	0.0049**
	(0.0007)	(0.0070)	(0.0046)	(0.0035)	(0.0033)	(0.0022)
MHT-adjusted *p*-values	0.591	0.019	0.031	0.016	0.025	0.122
R-squared	0.001	0.018	0.010	0.013	0.010	0.013
Y-Mean	0.0017	0.0164	0.0057	0.0054	0.0048	0.0022
Y-Mean (Pre-T)	0.0016	0.0086	0.0023	0.0021	0.0021	0.0008
Respondents	4565	4565	4565	4565	4565	4565
Observations	228656	228656	228656	228656	228656	228656

Notes: Difference-in-differences estimates on subtypes of SA episodes (0-1) using our "preferred" specification of Equation (1). Individuals are followed during 17 months since UI start. Only the first SA spell is considered, later spells are excluded from the analysis. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: *** $p<0.01$ ** $p<0.05$ * $p<0.10$. The first row in the bottom panel shows Romano-Wolf *p*-values for each outcome to test whether our estimates are sensitive to multiple hypothesis testing (MHT).

Table 4: Difference-in-differences estimates on overall and selected SA episodes (0-1) by job characteristics at UI start

	Skilled, non-manual (1)	Skilled, manual (2)	Unskilled, non-manual (3)	Unskilled, manual (4)	Industry/ Energy (5)	Construction (6)	Services (7)	Permanent (8)	Non-permanent (9)
SA (0-1)	0.0292	0.0380***	0.0062	0.0373*	0.0182	0.0272**	0.0217**	0.0194	0.0230**
	(0.0192)	(0.0120)	(0.0175)	(0.0219)	(0.0181)	(0.0128)	(0.0105)	(0.0148)	(0.0113)
R-squared	0.036	0.038	0.022	0.060	0.040	0.068	0.018	0.023	0.025
Y-Mean	0.0070	0.0235	0.0167	0.0249	0.0188	0.0168	0.0186	0.0197	0.0145
Y-Mean (Pre-T)	0.0101	0.0101	0.0101	0.0101	0.0101	0.0101	0.0101	0.0101	0.0101
Musculoskeletal (0-1)	0.0235	0.0175**	0.0003	0.0121	0.0168*	0.0183**	0.0089	0.0092	0.0137**
	(0.0186)	(0.0069)	(0.0142)	(0.0097)	(0.0090)	(0.0088)	(0.0074)	(0.0118)	(0.0064)
R-squared	0.049	0.030	0.018	0.062	0.058	0.047	0.009	0.014	0.022
Y-Mean	0.0031	0.0087	0.0063	0.0042	0.0058	0.0069	0.0058	0.0068	0.0040
Y-Mean (Pre-T)	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026
Stress-related (0-1)	0.0220	0.0166**	0.0149**	0.0109	0.0136*	0.0022	0.0165***	0.0199**	0.0137**
	(0.0187)	(0.0068)	(0.0073)	(0.0093)	(0.0076)	(0.0051)	(0.0055)	(0.0078)	(0.0058)
R-squared	0.042	0.026	0.028	0.060	0.044	0.023	0.016	0.017	0.026
Y-Mean	0.0038	0.0072	0.0073	0.0035	0.0055	0.0031	0.0068	0.0064	0.0044
Y-Mean (Pre-T)	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026
> 20 days (0-1)	0.0270	0.0382***	0.0072	0.0406*	0.0204	0.0263**	0.0220**	0.0200	0.0231**
	(0.0191)	(0.0120)	(0.0175)	(0.0219)	(0.0180)	(0.0128)	(0.0105)	(0.0148)	(0.0113)
R-squared	0.046	0.039	0.024	0.065	0.044	0.071	0.020	0.024	0.029
Y-Mean	0.0055	0.0220	0.0150	0.0232	0.0172	0.0155	0.0168	0.0182	0.0124
Y-Mean (Pre-T)	0.0086	0.0086	0.0086	0.0086	0.0086	0.0086	0.0086	0.0086	0.0086
Respondents	519	1466	1155	559	917	878	2635	3286	1192
Observations	28376	80393	65073	29648	40726	46788	134289	163750	60413

Notes: Difference-in-differences estimates on overall and subtypes of SA episodes (0-1) by job characteristics at UI start using a similar specification to our "preferred" one of Equation (1). Individuals are followed during 17 months since UI start. Only the first SA spell is considered, later spells are excluded from the analysis. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: *** p<0.01 ** p<0.05 * p<0.10.

Table 5: Difference-in-differences estimates on overall and selected SA episodes (0-1) by sociodemographic traits

	Men	Women	< age 40	Age 40+	ESP	Other EU	Non-EU
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SA (0-1)	0.0200**	0.0233	0.0232***	0.0185	0.0185**	0.0079	0.0348*
	(0.0081)	(0.0151)	(0.0087)	(0.0131)	(0.0087)	(0.0089)	(0.0180)
R-squared	0.021	0.032	0.023	0.012	0.018	0.025	0.078
Y-Mean	0.0170	0.0204	0.0182	0.0177	0.0196	0.0038	0.0146
Y-Mean (Pre-T)	0.0101	0.0101	0.0101	0.0101	0.0101	0.0101	0.0101
Musculoskeletal (0-1)	0.0139***	0.0022	0.0094	0.0128*	0.0097*	0.0039	0.0142
	(0.0044)	(0.0122)	(0.0062)	(0.0072)	(0.0055)	(0.0037)	(0.0108)
R-squared	0.015	0.020	0.010	0.014	0.011	0.036	0.073
Y-Mean	0.0059	0.0063	0.0062	0.0057	0.0062	0.0013	0.0073
Y-Mean (Pre-T)	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026
Stress-related (0-1)	0.0106***	0.0161**	0.0161***	0.0077*	0.0121***	0.0028	0.0109
	(0.0040)	(0.0078)	(0.0053)	(0.0047)	(0.0038)	(0.0036)	(0.0087)
R-squared	0.013	0.026	0.016	0.008	0.014	0.034	0.075
Y-Mean	0.0044	0.0089	0.0071	0.0039	0.0065	0.0013	0.0036
Y-Mean (Pre-T)	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026
> 20 days (0-1)	0.0201**	0.0241	0.0238***	0.0184	0.0190**	0.0077	0.0352*
	(0.0081)	(0.0150)	(0.0087)	(0.0131)	(0.0087)	(0.0088)	(0.0179)
R-squared	0.023	0.036	0.025	0.013	0.019	0.029	0.083
Y-Mean	0.0155	0.0183	0.0163	0.0164	0.0178	0.0029	0.0135
Y-Mean (Pre-T)	0.0086	0.0086	0.0086	0.0086	0.0086	0.0086	0.0086
Respondents	3180	1385	2718	1847	3722	286	557
Observations	157468	71188	137202	91454	188017	14279	26360

Notes: Difference-in-differences estimates on overall and selected SA episodes (0-1) by sociodemographic traits using a similar specification to our “preferred” one of Equation (1). Individuals are followed during 17 months since UI start. Only the first SA spell is considered, later spells are excluded from the analysis. Age in columns 3 and 4 is defined at UI start. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: *** p<0.01 ** p<0.05 * p<0.10.

Table 6: Robustness and placebo tests

	SA (0-1) (Preferred)	SA (0-1) (Robu1)	SA (0-1) (Robu2)	SA (0-1) (Robu3)	SA (0-1) (Robu4)	SA (0-1) (Robu5)	SA (0-1) (Placebo1
Treated x Post-Reform	0.020***	0.008**	0.007**	0.015**	0.024***	0.020***	-0.005
	(0.007)	(0.003)	(0.003)	(0.007)	(0.008)	(0.004)	(0.007)
Wild bootstrap *p*-value						0.002	
R-squared	0.016	0.596	0.007	0.017	0.018	0.016	0.013
Y-Mean	0.018	0.018	0.007	0.015	0.019	0.018	0.012
Y-Mean (Pre-T)	0.010	0.010	0.010	0.010	0.010	0.010	0.010
Respondents	4565	4565	4474	4541	3578	4565	2872
Observations	228656	228656	226149	228056	196894	228656	146022

Notes: Difference-in-differences estimates on the probability of having a SA episode (0-1) using our "preferred" specification of Equation (1). Individuals are followed during 17 months since UI start. Only the first SA spell is considered, later spells are excluded from the analysis. "Preferred" is taken from Table 1 and serves as our baseline specification. "Robu1" controls as well for being On SA at UI start (0-1). "Robu2" instead drops those with On SA at UI start = 1. "Robu3" instead drops those with Back pain = 1. "Robu4" instead drops those with any missing job characteristic at UI start from those considered in Table 4. "Robu5" clusters the standard errors at the province-quarter-year level and computes the wild bootstrap p-value that accounts for the small number of treated clusters. "Placebo1" uses a different fictitious policy date (May 1, 2012) and includes only workers displaced between January and August 2012 for the analysis. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level (except in "Robu5") and shown in parentheses. Significance levels: *** p<0.01 ** p<0.05 * p<0.10.

“Unintended Health Consequences of Decreasing Unemployment Insurance Generosity During an Economic Recession”

Manuel Flores, Fernando G. Benavides and Laura Serra-Saurina

Web Appendices A and B

NOT FOR PUBLICATION

A.1 Identifying stress-related ICD codes

This section describes how we created a list of ICD codes that are potentially caused or exacerbated by stress. In addition to consulting with physicians, we reviewed the literature on this topic, which, as discussed below and summarized in Table A4, is very extensive.

Involuntary job loss can cause financial stress, especially when it results in increased economic hardship (Pearlin, 1989). Both actual and subjective financial strain are associated with worse health (Andreeva et al., 2015). Job loss operates as a "stressor" that evokes a "stress response", i.e., a cognitive, emotional and/or biological reaction (Epel et al., 2018). For instance, in a widely cited paper, Lorant et al. (2007) found that a worsening subjective financial strain was significantly associated with an increased risk of general and major depression.[29] Perceived stress emerges, amongst others, from a "loss of control" (LOC) (Epel et al., 2018).

In our setting, treated unemployed individuals experienced after the reform an inevitable 16% decline in their replacement rate (RR) from unemployment insurance (UI). Losing a job during an economic recession, with unemployment rates (URs) reaching an unprecedented 26%, likely creates a LOC feeling, especially among less employable and less protected workers. We hypothesize that the decline in UI generosity due to the 2012 labor market reform created additional financial stress, deteriorating the health status of some affected unemployed workers compared to similar unemployed workers who remained unaffected. This effect was likely driven by those with more job uncertainty and lower levels of employability.

One major challenge to our analysis relates to the "lack of consistency and thoroughness in stress measurement" (Epel et al., 2018, p.146). First, measuring stress is inherently complex due to its multi-level nature; stress affects virtually all systems of the body (American Psychological

[29] See Guan et al. (2022) for a review and additional references on the effects of financial stress on depression.

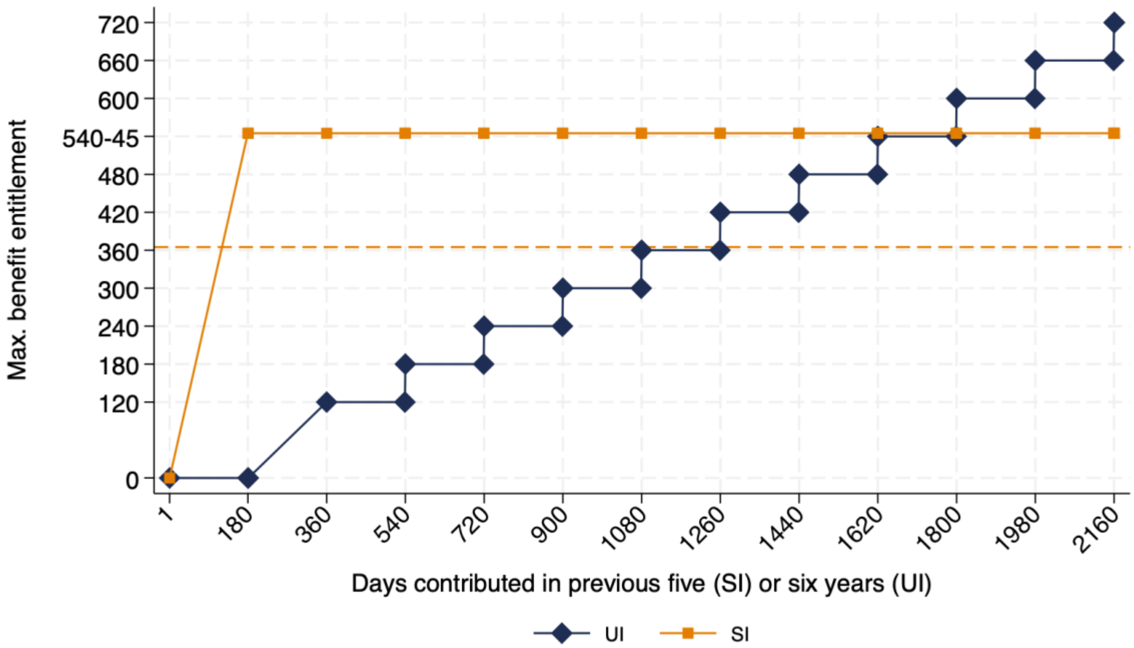
Association, 2023; Epel et al., 2018; Fink, 2017). Therefore, no single indicator suffices; instead, a multiplicity of health outcomes must be considered when measuring the effects of stress on health. Second, stress manifests differently across individuals. While two individuals may not differ in their overall vulnerability to certain stressors, they can differ in their vulnerability to particular outcomes (for instance, women may be more prone to depressive symptoms, while men may be more likely to abuse alcohol, see Pearlin (1989)). Third, individual contextual factors, from genes to socio-economic environment, along with protective factors like social support, can attenuate or exacerbate the effects of exposure to stressors on health (Cohen and Willis, 1985; Epel et al., 2018; Pearlin, 1989). Most of these factors are typically unobservable to empirical researchers. Finally, in settings like ours, not all health problems caused by stress will result in a medically certified sickness absence (SA) spell, but likely only the most severe ones.

All in all, these earlier issues imply that it is important to allow for heterogeneous effects across sub-groups of individuals and to consider a full range of relevant stress-related health outcomes. Considering a full range is also important because we have only one ICD code per SA episode. Finally, while we have access to restricted-use linked register data, this does not cover the full population but rather a representative sample. Therefore, sampling error remains a potential concern for diseases with a very low prevalence.

Based on a review of the literature and consultation with physicians, we selected all relevant ICD codes that are potentially caused or exacerbated by stress. Altogether, these account for about one-third of all SA spells. We differentiated between those diseases that are observable or measurable (about 2% of all spells) and those that are not (about 31% of all spells). The full list of ICD-10 and their corresponding ICD-9 codes, along with relevant references, is provided in Table A4.

A.2. Additional Figures and Tables

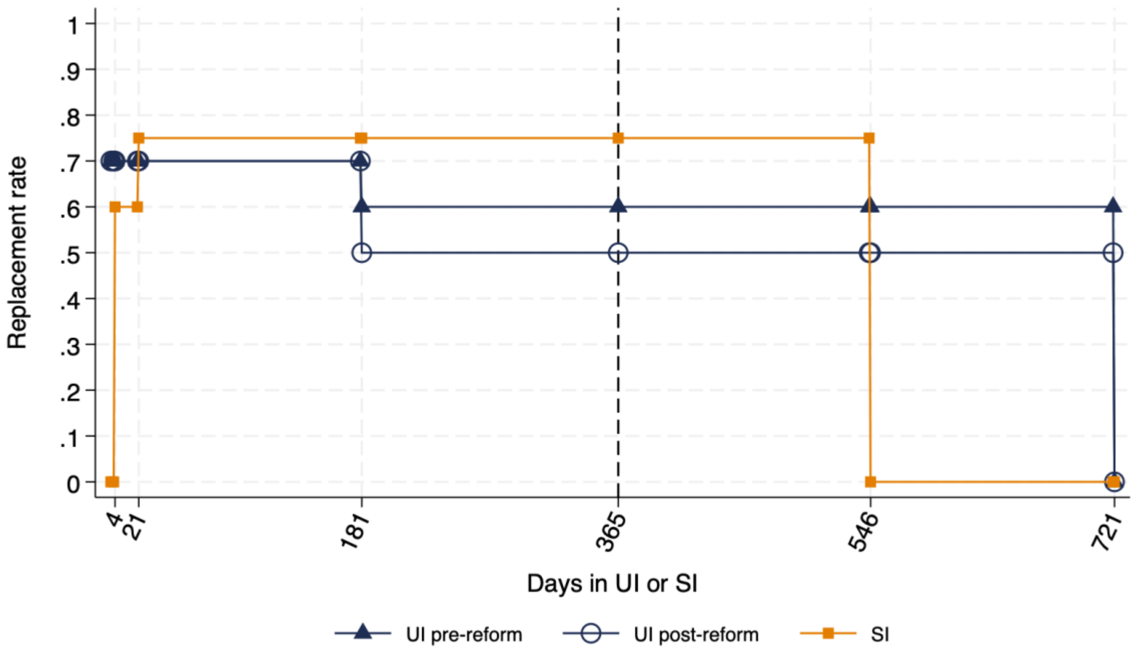
Figure A1: Benefit duration from UI and SI in Spain by contribution record



Notes: The figure shows the maximum duration of entitlements to unemployment insurance (UI) and sickness insurance (SI) in Spain as a function of the number of days contributed in the previous six or five years, respectively. As shown, UI benefit duration increases stepwise with the contribution record, instead SI benefit duration remains flat.

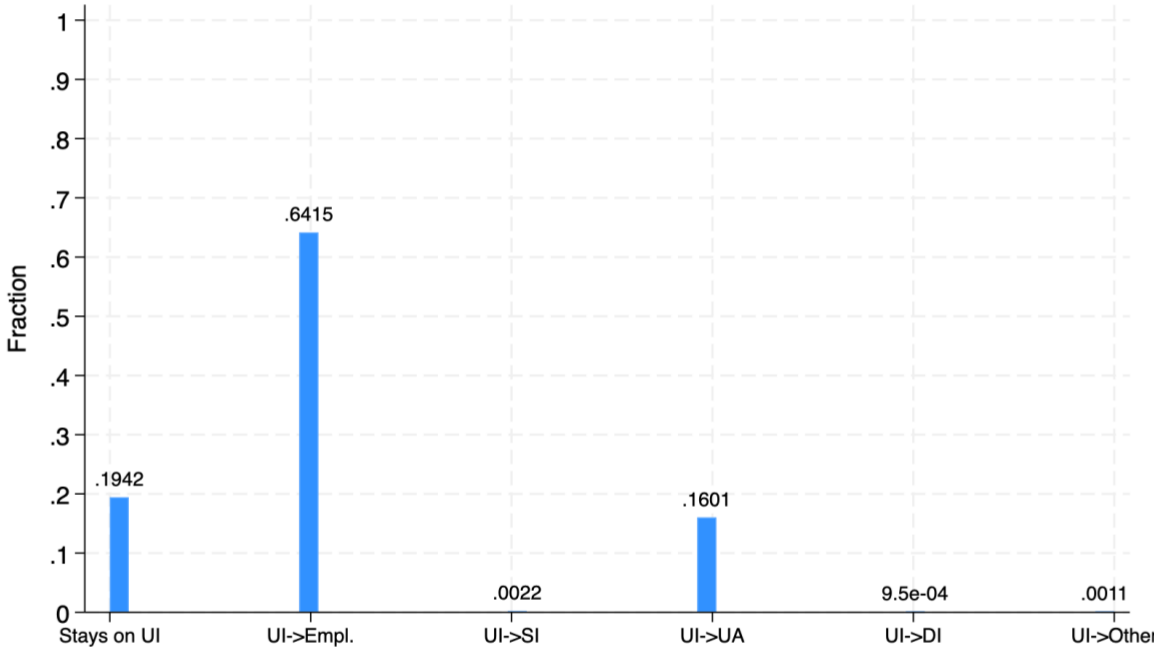
Source: Authors' elaboration based on OECD (2019) and Seguridad Social (2021).

Figure A2: Generosity of SI benefits and of UI benefits before and after the 2012 reform



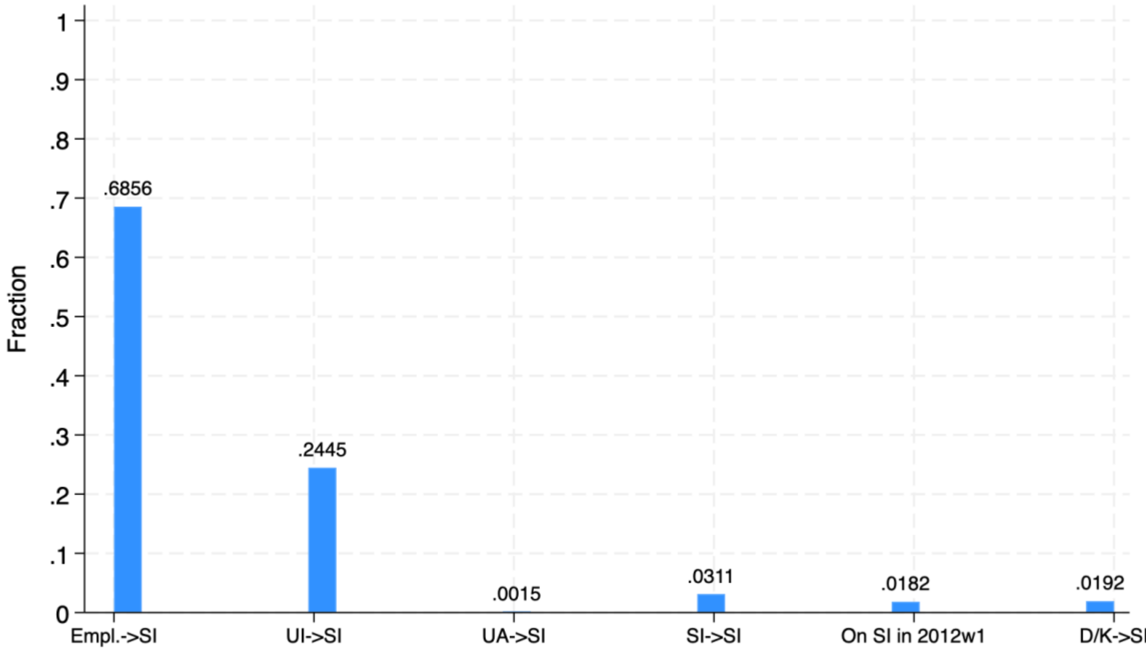
Notes: The figure shows the replacement rates of unemployment insurance (UI) benefits before and after the 2012 reform and of sickness insurance (SI) benefits in Spain as a function of the number of days in UI and SI, respectively. As shown, the 2012 reform decreased the generosity of UI benefits in Spain, while that of SI remained unchanged (among private sector workers).
Source: Authors' elaboration based on OECD (2019) and Seguridad Social (2021).

Figure A3: Transitions from initial UI spell to employment and other social insurance schemes



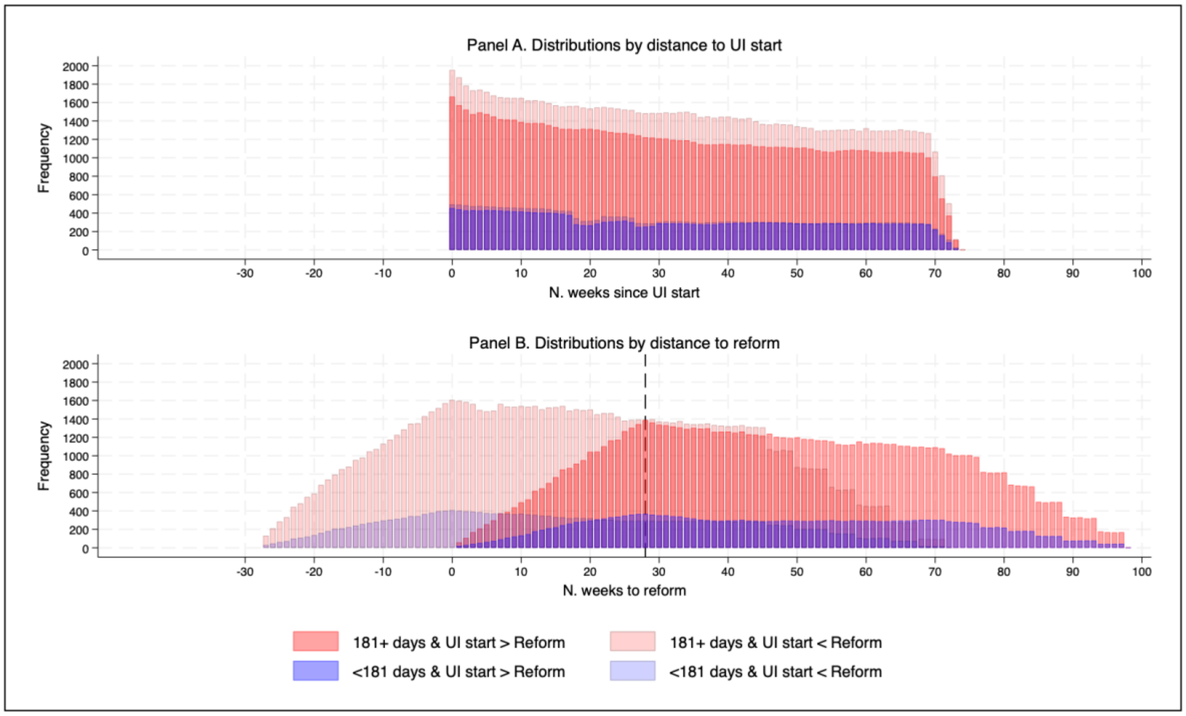
Notes: The figure shows the transitions from the initial UI spell (as a share) to employment and other social insurance schemes in our final sample with individuals censored at 17 months.

Figure A4: Pathways to first SA spell



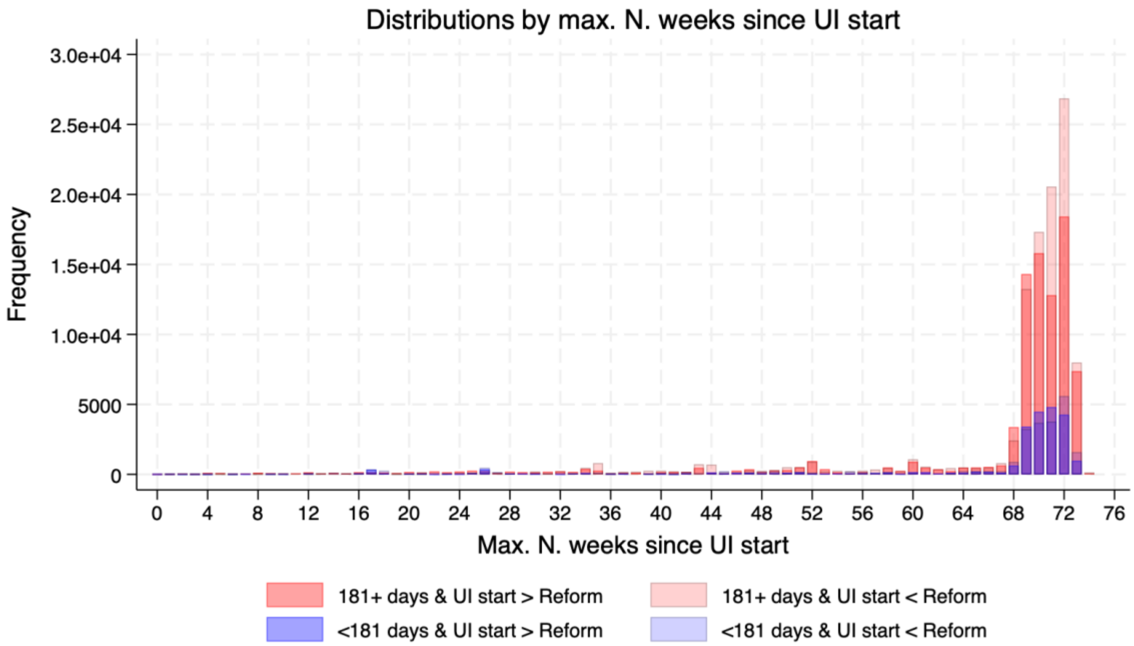
Notes: The figure shows the pathways to the first sickness absence (SA) spell as a share in our final sample, conditional on having a SA spell during the 17 months of follow-up. The pathway "D/K→SI" includes individuals with intermittent affiliations and a "hole" in the previous week of their first SA spell. The pathway "On SI in 2012w1" includes individuals who lose their job and start a SA spell in the first week of our calendar sample period.

Figure A5: Distribution of individuals in event time (Panel A) and calendar time (Panel B) by UB entitlement length and UI start date



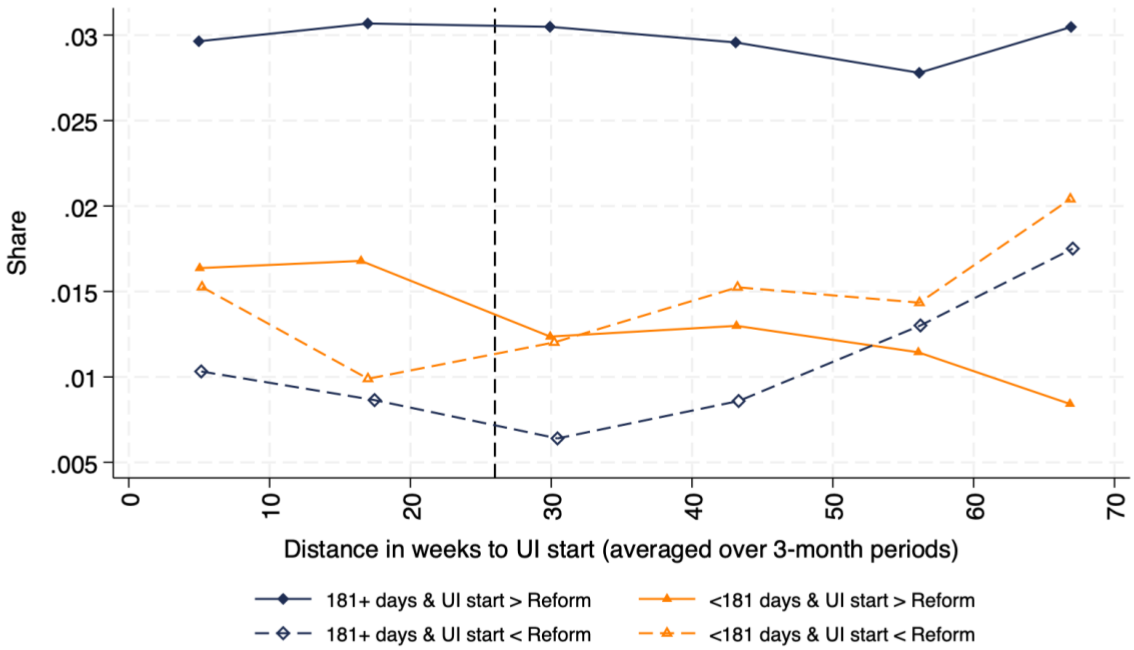
Notes: The figure shows the distribution of individuals in our final sample in event time (Panel A) and calendar time (Panel B) by unemployment benefit (UB) entitlement length and unemployment insurance (UI) start date. All individuals are censored at 17 months. Using our "estimation samples" for sickness absence (SA) spells, where we consider correspondingly only an individual's first SA spell, produces a very similar figure.

Figure A6: Distribution of individuals by maximum weeks of follow-up, UB benefit entitlement length and UI start date



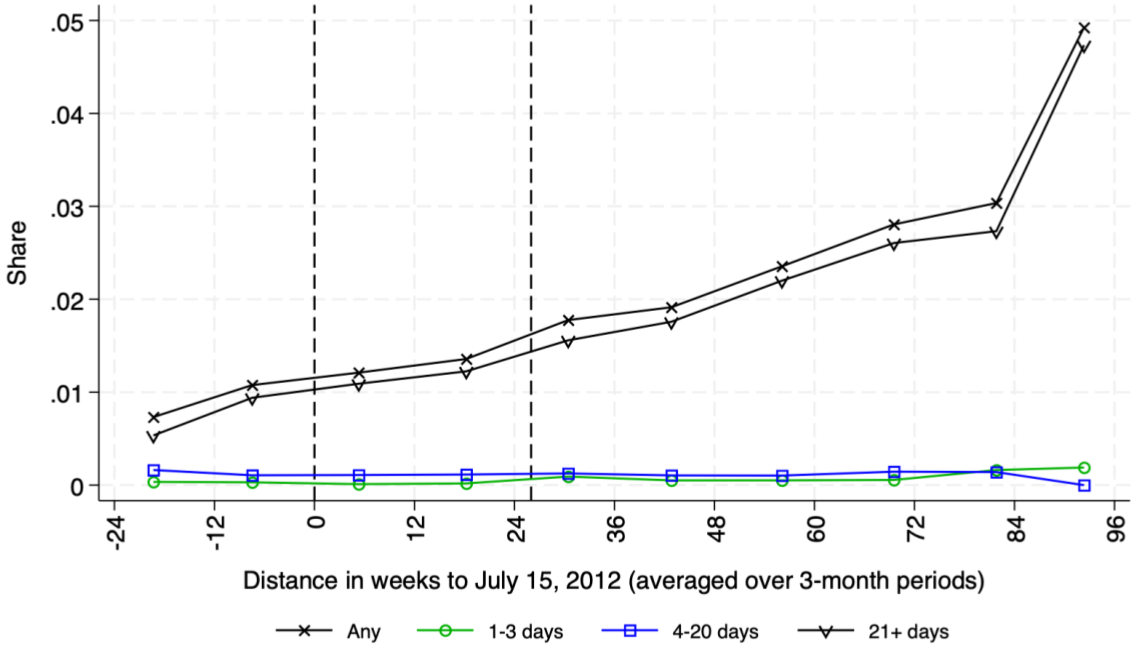
Notes: The figure shows the distribution of individuals in our final sample by the maximum number of weeks of follow-up, unemployment benefit (UB) entitlement length and unemployment insurance (UI) start date. All individuals are censored at 17 months.

Figure A7: Share in SA spells by distance to UI start, UB benefit entitlement length and UI start date



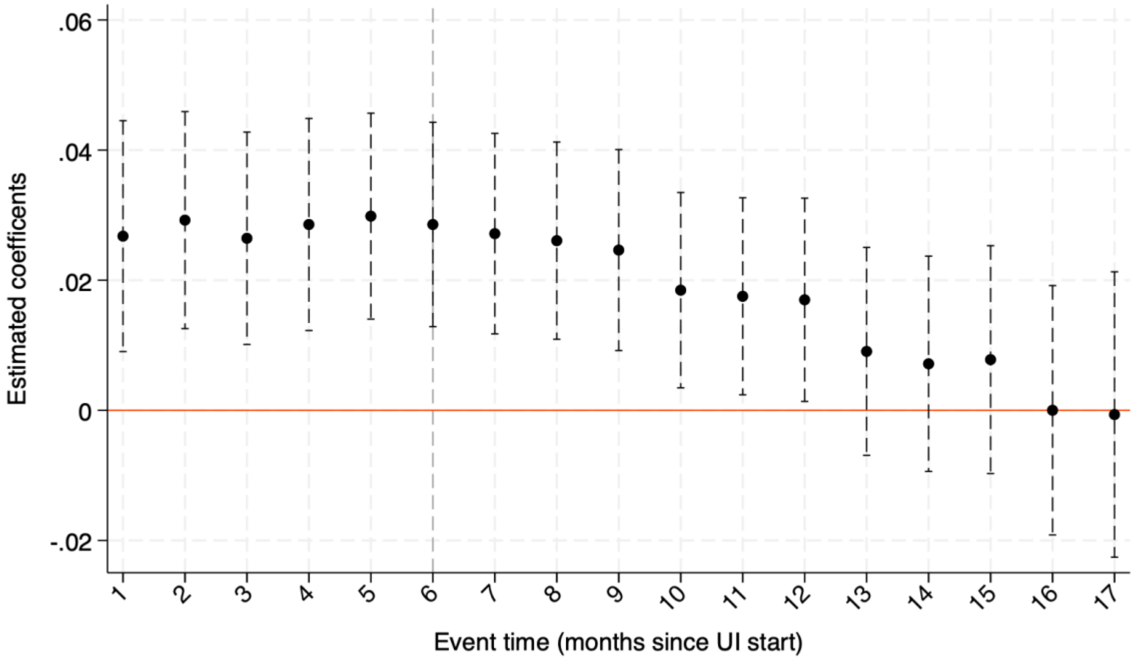
Notes: The figure shows the probability of having a sickness absence (SA) spell in our final sample by event time (distance to unemployment insurance, UI, start), unemployment benefit (UB) entitlement length and UI start date. Only the first SA spell is considered, later SA spells are excluded from the analysis, and all individuals are censored at 17 months. Distance to UI start is measured in weeks but averaged over three-month periods. The dashed vertical line indicates when the 2012 UI reform began to take effect (after 180 days on UI).

Figure A8: Share in SA spells by length of the spell and distance to 2012 UI reform



Notes: The figure shows the probability of having a sickness absence (SA) spell in our final sample by calendar time (distance to 2012 UI reform) for SA spells of any length, 1-20, 21-60 or 61+ days. The 1st vertical line shows when the UI reform act was passed (i.e. Jul 15, 2012) and the 2nd line indicates when the reform began to take effect (180 days later). Only the first SA spell is considered, later SA spells are excluded from the analysis. All individuals are censored at 17 months. Distance to the 2012 UI reform is measured in weeks but averaged over three-month periods.

Figure A9: Event study analysis of the effect of the reform on SA episodes (0-1) allowing for a differential pattern in event time for those with entitlements of 181+ days who entered UI before the reform



Notes: The figure shows the coefficients and 95% CIs for the interactions between the DiD estimator and monthly dummies measuring the distance to UI start. Only the first SA spell is considered, later spells are excluded from the analysis, and all individuals are censored at 17 months of follow-up. Besides the full list of controls from our "preferred" specification of Equation (1), this model also includes interactions between D^T (being entitled to more than 180 days of UI benefits) and monthly dummies measuring the distance to UI start. Standard errors are clustered at the individual level. The dashed line shows when the actual reduction in the RR would happen (i.e., after 180 days on UI benefit receipt). We do not reject the null hypothesis of homogeneous reform effects in event time (*p*-value=0.214).

Table A1: Pre- and post-displacement descriptive statistics for treated and comparison groups (shares and percentage point differences)

	Post-reform			**Pre-reform**			DiD
	Treated	Comparison	Diff	Treated	Comparison	Diff	
Panel A. Pre-displacement characteristics							
Female (0-1)	.347	.272	0.075*** (0.025)	.281	.276	0.004 (0.023)	0.071*** (0.034)
Spanish (0-1)	.855	.706	0.149*** (0.020)	.844	.669	0.175*** (0.020)	-0.026 (0.028)
Other EU (0-1)	.055	.072	-0.017 (0.012)	.06	.089	-0.030*** (0.013)	0.012 (0.018)
Non-EU (0-1)	.09	.221	-0.132*** (0.017)	.096	.242	-0.146*** (0.016)	0.014 (0.024)
Age 40+ (0-1)[a]	.431	.333	0.098*** (0.026)	.432	.272	0.159*** (0.025)	-0.061* (0.036)
Skilled, non-manual (0-1)	.133	.064	0.069*** (0.017)	.119	.073	0.046*** (0.016)	0.023 (0.023)
Skilled, manual (0-1)	.294	.401	-0.107*** (0.025)	.3	.421	-0.121*** (0.023)	0.014 (0.034)
Unskilled, non-manual (0-1)	.263	.248	0.015 (0.023)	.252	.226	0.027 (0.022)	-0.011 (0.032)
Unskilled, manual (0-1)	.094	.23	-0.136*** (0.017)	.09	.246	-0.156*** (0.016)	0.020 (0.024)
Occupation N/A (0-1)	.215	.057	0.158*** (0.020)	.238	.035	0.204*** (0.020)	-0.045 (0.028)
Permanent (0-1)	.829	.298	0.531*** (0.021)	.832	.295	0.537*** (0.020)	-0.006 (0.029)
Non-permanent (0-1)	.158	.697	-0.539*** (0.020)	.141	.681	-0.540*** (0.019)	0.000 (0.028)
Contract duration N/A (0-1)	.013	.004	0.008 (0.005)	.027	.024	0.002 (0.008)	0.006 (0.010)
Industry/Energy	.199	.169	0.030 (0.021)	.219	.165	0.055*** (0.020)	-0.024 (0.029)
Construction	.174	.206	-0.032 (0.020)	.192	.244	-0.052*** (0.020)	0.021 (0.029)
Services	.606	.599	0.007 (0.026)	.552	.559	-0.006 (0.025)	0.014 (0.036)
Primary or N/A	.021	.026	-0.005 (0.008)	.037	.033	0.004 (0.009)	-0.010 (0.012)
Barcelona (0-1)	.778	.73	0.048*** (0.022)	.754	.701	0.052*** (0.022)	-0.005 (0.031)
Girona (0-1)	.088	.105	-0.018 (0.015)	.094	.106	-0.011 (0.015)	-0.006 (0.021)
Lleida (0-1)	.043	.055	-0.012 (0.011)	.049	.051	-0.002 (0.011)	-0.009 (0.016)
Tarragona (0-1)	.091	.11	-0.019 (0.015)	.103	.142	-0.039*** (0.016)	0.020 (0.022)

Table A1: Continued

	Post-reform			Pre-reform			DiD
	Treated	Comparison	Diff	Treated	Comparison	Diff	
Panel B. Post-displacement health and employment[b]							
Sickness absence (0-1)	.028	.015	0.013*	.011	.016	-0.005	0.018***
			(0.007)			(0.004)	(0.008)
Musculoskeletal (0-1)	.01	.003	0.007	.003	.008	-0.005***	0.012***
			(0.004)			(0.002)	(0.005)
Stress-related (0-1)	.01	.001	0.009***	.002	.004	-0.002	0.011***
						(0.001)	(0.004)
> 20 days (0-1)	.025	.012	0.014*	.009	.014	-0.005	0.019***
			(0.007)			(0.003)	(0.008)
Employed (0-1)	.176	.34	-0.164***	.167	.31	-0.143***	-0.021
			(0.015)			(0.015)	(0.021)
Sample size	1664	456		1953	492		4565

Notes: "Differences" columns display a two-sample *t* test. Based on final sample with individuals censored at 17 months since UI start. Standard errors are in parenthesis. Significance levels: *** p<0.01 ** p<0.05 * p<0.10.

[a] In our empirical models, rather than controlling for age, we include dummies for each birth year. We show here descriptives by age (the share of individuals aged 40 or older), as in the main paper we also explore whether the effects of the reform vary across this age categorization.

[b] Because the comparisons in this table rely on the same sample, they are not restricted to the first SA or employment spell, respectively, as done in the main paper. This explains the small differences in the sample means of SA and employment in the pre-reform periods with respect to those in Table 1 and Table A2, respectively.

Table A2: Difference-in-differences estimates on employment (0-1)

	Emp. (0-1) (1)	Emp. (0-1) (2)	Emp. (0-1) (Preferred) (3)	Emp. (0-1) (RS-RP) (4)	Emp. (0-1) (Full) (5)
Treated (0-1) x Post-reform (0-1)	-0.006	-0.010	-0.012	-0.015	-0.013
	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)
Treated (0-1), Post-Reform (0-1)	X	X	X	X	X
Dummies for months since UI start	X	X	X		X
Dummies for months to UI reform		X	X		X
Dummies for province, gender, birth year, and nationality, type of contract, economic sector and occupational category (at UI start)			X	X	X
Monthly time trend, quarterly dummies, GDP growth rate (quarterly)				X	
UI benefit entitlement at UI start (in months)				X	X
R-squared	0.105	0.107	0.147	0.137	0.150
Y-Mean	0.210	0.210	0.210	0.210	0.210
Y-Mean (Pre-T)	0.172	0.172	0.172	0.172	0.172
Respondents	4567	4567	4565	4565	4565
Observations	210561	210561	210419	210419	210419

Notes: Difference-in-differences estimates on overall employment episodes (0-1). Individuals are followed during 17 months since UI start. Only the first employment spell is considered, later spells are excluded from the analysis. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: *** p<0.01 ** p<0.05 * p<0.10.

Table A3: Difference-in-differences estimates on SA episodes (0-1) with different donut sizes

	No donut	Excl. -/+2 weeks	Excl. -/+4 weeks	Excl. -/+6 weeks	Excl. -/+8 weeks
	(1)	(2)	(3)	(4)	(5)
Treated x Post-reform	0.020***	0.020***	0.023***	0.024***	0.020**
	(0.007)	(0.007)	(0.008)	(0.008)	(0.009)
R-squared	0.016	0.017	0.018	0.020	0.022
Y-Mean	0.018	0.018	0.019	0.019	0.018
Y-Mean (Pre-T)	0.010	0.010	0.010	0.010	0.010
Respondents	4565	4257	3962	3685	3365
Observations	228656	213867	198578	183822	168290

Notes: Difference-in-differences estimates on overall SA episodes (0-1) with different donut sizes, from 0 (no donut, "Preferred" from Table 1) to 8 weeks (i.e., excluding those who entered UI -/+8 weeks to the reform date), using our "preferred" specification of Equation (1). Individuals are followed during 17 months since UI start. Only the first SA spell is considered, later spells are excluded from the analysis. Pre-T refers to those in the treatment group who entered UI in the pre-reform period. Standard errors are clustered at the individual level and shown in parentheses. Significance levels: *** p<0.01 ** p<0.05 * p<0.10.

Table A4: ICD10 code lists that are potentially caused or exacerbated by stress

Group	Included diseases	ICD10 codes	Corresponding ICD9 codes	Observable or measurable	References
V Mental and behavioral disorders	Mental and behavioral disorders due to psychoactive substance use	F10–F19	291, 292, 303–305	No	Pearlin (1989), Thern et al. (2017)
	Mood [affective] disorders	F30–F39	296, 300.4, 311	No	American Psychological Association (2023), Guan et al. (2022), Thern et al. (2017)
	Neurotic, stress-related and somatoform disorders	F40–F48	300*, 306, 308, 309*	No	American Psychological Association (2023), Thern et al. (2017)
VI Diseases of the nervous system	Migraine	G43	346	No	American Psychological Association (2023), Vives-Mestres et al. (2021)
	Tension-type headache	G44.2	339.1	No	Loder & Rizzoli (2008)
	Sleep disorder	G47	327*, 780	No	Sleep Foundation (2023)
IX Diseases of the circulatory system	Hypertensive diseases	I10–I16	401–405	Yes	American Psychological Association (2023)
	Ischemic heart diseases	I20–I25	410–414	Yes	American Psychological Association (2023), Kivimäki et al. (2002), Rozanski et al. (1999)
	Cerebrovascular diseases	I60–I69	430–434, 435.9 436, 437*, 438	Yes	American Psychological Association (2023), Kivimäki et al. (2002)
XI Diseases of the digestive system	Crohn disease	K50	555	Yes	Gao et al. (2018)
	Ulcerative colitis	K51	556	Yes	American Psychological Association (2023), Gao et al. (2018)
	Irritable bowel syndrome	K58	564.1	No	American Psychological Association (2023), Balestrieri et al. (2023), Black et al. (2022), Gao et al. (2018), Kahn-Boesel et al. (2022)
	Gastro-oesophageal reflux disease without oesophagitis	K21.9	530.8	No	Wickramasinghe et al. (2023)
	Functional dyspepsia	K30	536.8	No	De la Roca-Chiapas et al. (2010)

Table A4: Continued

XII Diseases of the skin and subcutaneous tissue	Dermatitis and eczema	L20–L30	690, 691, 692*, 693	Yes	Arndt et al. (2008), Zhang et al. (2024)
	Psoriasis	L40	696*	Yes	Rousset & Halioua (2018), Zhang et al. (2024)
	Urticaria and erythema	L50–L54	695*	Yes	Zhang et al. (2024)
XIII Diseases of the musculoskeletal system and connective tissue	Dorsalgia (back pain)	M54	723*,724*	No	American Psychological Association (2023), de Porras et al. (2017)
	Soft tissue disorders (muscles)	M62, M79.0-2, M79.6-7	728, 729.1-2, 729.5	No	American Psychological Association (2023), de Porras et al. (2017)
XIV Diseases of the genitourinary system	Endometriosis	N80	617	Yes	Reis et al. (2020)
	Premenstrual tension syndrome	N94.3	625.4	No	Alshdaifat et al. (2022), Coyne et al. (1985)
	Female infertility	N97	628	No	Rooney & Domar (2018)
XVIII Symptoms, signs, and abnormal clinical and laboratory findings, not elsewhere classified	Abdominal and pelvic pain	R10	789*	No	Grundy et al. (2019)
	Symptoms and signs involving emotional state	R45	799.2	No	American Psychological Association (2023)
	Headache	R51	784.0	No	American Psychological Association (2023)
	Pain, not elsewhere classified	R52		No	American Psychological Association (2023)
	Malaise and fatigue	R53		No	American Psychological Association (2023)
	Syncope and collapse	R55		Yes	Hainsworth (2004)

Notes: The codes were selected based on a review of the literature and consultation with physicians. In particular, we wish to thank Dr. María Bermúdez, a specialist in Internal Medicine and Gastroenterology from the University Hospital Germans Trias i Pujol (Badalona, Spain), for her advice in generating the list of included codes.

*Does not include all subcodes.

Source: 2015 version ICD10 codes, available at https://icd.who.int/browse10/2015/en#/.

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