

Unions as insurance: Employer–worker risk sharing  
and workers' outcomes during COVID-19

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by

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**Unions as insurance:  
Employer–worker risk sharing and workers’ outcomes  
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**Abstract**

We investigate to what extent workplace unionisation protects workers from external shocks as predicted by models of implicit contracts. Using the COVID-19 pandemic as a plausibly exogenous shock hitting the whole economy, we compare workers who worked in unionised and non-unionised workplaces directly before the pandemic in a difference-in-differences framework. We find that unionised workers were substantially more likely to remain working for their pre-COVID employer, at their pre-COVID workplace, in their pre-COVID job and to be in employment. This greater employment stability was not traded off against lower working hours or labour income.

**Keywords:** Unions; risk-sharing; implicit contracts; insurance effects; COVID-19

**JEL codes:** J51 (primary); I18, I19, J63 (secondary)

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## 1. Introduction

It has long been known that unions – like other labour market institutions – “serve an important function of social insurance” (Agell, 2002: 108) against labour market risks. Working in a unionised workplace not only offers higher wages and other benefits due to unions’ rent-seeking activities (Bryson, 2014), but also promises higher employment security if unions protect workers from external shocks.

Such protection from external shocks could arise through multiple channels. Several theoretical papers on implicit contracts argue that unions facilitate implicit risk-sharing agreements between workers and employers (Malcomson, 1983; Horn and Svensson, 1986; Hogan, 2001). The major empirical implication of this argument is that unions shield workers from external shocks by increasing their probability of staying at their current job. Another plausible union effect following an adverse shock may be that unions provide help to workers who were dismissed because of that shock, for example, through networks or training opportunities. This channel would imply that employment probabilities for union workers should recover faster after an adverse shock, resulting in better overall employment rates, but that they do not enjoy higher job stability in their pre-shock job. Discriminating between these two possible mechanisms requires data not just on individual employment outcomes but also information on the worker’s employer. Finally, the presence of a union might well influence the likelihood that a firm is affected by a shock in the first place, for example, by affecting management behaviour.

In this paper, we are interested in the first channel. We use the COVID-19 pandemic and the accompanying public health measures, such as closures of certain businesses, as an unexpected and severe shock that affected the whole economy and not just individual firms. Using this plausibly exogenous shock and data from Understanding Society – a large household panel – as well as the Understanding Society COVID-19 surveys, we then compare the outcomes of workers who worked in unionised workplaces and those in non-unionised workplaces directly before the pandemic in a difference-in-differences framework.

This design allows us to separate the protective effects of unions against an exogenous shock from the well-established general effect of unions on job separations: There is ample evidence that the presence of unions reduces voluntary turnover (Addison, 2020) – consistent with Freeman and Medoff’s (1984) exit–voice model of unionism – and some more limited evidence that unions reduce involuntary job separations (Lucifora, 1998; Knight and Latreille, 2000; Antcliff and Saunders, 2009; Goerke and Pannenberg, 2011; Pierse and McHale, 2015). And, related to these findings, it has also been found that unions increase the likelihood that job security guarantees are in place (White and Bryson, 2013).

Direct evidence on protective effects of unions via risk sharing is sparse as shocks that are sufficiently large for a union’s possible insurance function to become important and that are also truly exogenous are rare. There is some limited evidence that union presence reduces the responsiveness of wages of existing workers to firm-specific shocks (Cardoso and Portela, 2009; Guertzen, 2014). Even less is known about a protective effect in relation to job losses. The only direct evidence on this comes from Ivlen and Velizolis (2017) who find that in Central and Eastern European countries union members were less likely to lose their jobs during the Great Recession than non-members. Fewer dismissals among union members,

though, could just mirror lower involuntary job separations compared to non-members, along the lines of the previous paragraph, rather than unions shielding workers from the external shock caused by the Great Recession.

In addition, there is evidence from two papers that overall employment rates of union workers are higher during times of economic crisis. At the macro level, Bachmann and Frings (2021) find for the US and the EU member states that countries with higher union density experienced lower worker flows into unemployment and had lower unemployment rates during the Great Recession. Closely related to our setting, Han (2022) documents that union workers in the US had higher wages and employment rates than non-union workers during the COVID-19 pandemic. Yet, the evidence from these two papers cannot shed direct light on the protective effect of unions for job stability as effects on overall employment could be due to either of the two previously mentioned channels.<sup>1</sup>

Related to our findings, Fackler et al. (2021) report for Germany that plants with a works council, which is the German counterpart to the workplace union in other countries, had a significantly lower dismissal rate during the COVID-19 pandemic compared to plants without works councils. Their design, however, does not control for pre-existing differences in dismissal rates between these two types of plants. Hence, it remains unclear whether the fewer dismissals in co-determined plants in times of crisis reflect a level effect in dismissals that has also been found in normal times (Boockmann and Steffes, 2010; Hirsch et al., 2010; Grund et al., 2015). In relying on difference-in-differences as identification strategy our paper controls for any such level effects and thus allows us to answer whether unions protected workers from this external shock.

Against this background, our paper documents for the UK that workers in workplaces with a recognised union were significantly more likely to keep their job and to stay with their employer and with their workplace than were workers in non-unionised workplaces. We begin by estimating various specifications, starting from simple difference-in-differences and gradually incorporating various fixed effects: We incorporate individual fixed effects to control for the possible selection of workers into unionised workplaces. We also control for pre-COVID-tenure-by-year fixed effects to allow for systematic differences in average job duration, pre-COVID-industry-by-year fixed effects to control for the differential economic impact of COVID-19 across industries (see Braakmann et al., 2022, for a similar approach), pre-COVID-occupation-by-year fixed effects to account for the fact that job mobility has been influenced by occupational COVID-19 risk (Braakmann et al., 2022) and region-by-year fixed effects.

Across all specifications we find evidence that workers in unionised workplaces were protected during COVID-19: They were between 4.6 and 6.6 percentage points more likely to work for their pre-COVID employer, between 4.6 and 6.2 percentage points more likely to

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<sup>1</sup> Our paper also contributes to a recent literature trying to understand the economic shock caused by the COVID-19 pandemic (e.g., Chetty et al., 2020). Previous research focussing on affected individuals has, for example, investigated the role of COVID-19 mortality for occupational mobility (Braakmann et al., 2022), the switch to working from home (Adams-Prassel et al., 2022) or the consequences for the children of adversely affected workers (Hupkau et al., 2022). In contrast to this literature, our paper focusses less on the direct impact of COVID-19 and more on factors moderating the effects of this shock on individual workers.

work at their pre-COVID workplace and between 3.8 and 5.3 percentage points more likely to work in their pre-COVID job. We also find that they are between 1.3 and 2.2 percentage points more likely to be employed rather than unemployed or out of the labour force. There is no evidence that this greater employment stability is traded off against lower working hours or labour income – any differences between unionised and non-unionised workers in these outcomes are generally both small and statistically insignificant.

We then turn to various robustness checks. We estimate full event study specifications to check for differential pre-trends, conduct randomisation inference where we randomise a worker's pre-COVID union status 500 times and recalculate our estimates and, finally, conduct a formal sensitivity analysis to pre-trend violations proposed by Rambachan and Roth (2022). All of these confirm our main estimates.

In a next step, we investigate effect heterogeneity. We first focus on worker age and pre-COVID job tenure. We find that the protective effects of workplace unionisation are strongest for prime age workers between 30 and 50, followed by older workers, while there is little effect on younger workers. A similar pattern exists for pre-COVID tenure: There is no effect of workplace unionisation on workers with less than two years of tenure – the threshold at which most employment rights in the UK come into effect. Effects are strongest for workers with exactly two years of tenure prior to COVID, followed by workers with 3 or more years.

Subsequently, we investigate the role of the various non-pharmaceutical interventions that were implemented during 2020 and 2021 to reduce the spread of SARS-CoV-2. In the context of the UK these involved the closure of non-essential businesses and widespread work-from-home orders. An important factor is the existence of what is essentially a government insurance: Workers whose employers were affected by lockdowns and who could not work from home were offered government support (“furlough”) that paid 80% of their previous earnings (with the possibility for employers to pay the remaining 20%). Furlough was in effect from 2020 into the second half of 2021 and could in principle replace any insurance role that would otherwise be provided by workplace unions. A particular challenge in the UK context is that lockdown measures do not map neatly into occupations or industries but are a combination of the classification of the employer and the occupation of the worker. To address this problem, we rely on data from the Understanding Society Covid-surveys for 2020 that contain information on whether an individual was furloughed or worked from home in any of the waves. We find little effects of workplace unionisation on the outcomes of workers that were ever furloughed.

It could in principle also be possible that the protective role of workplace unions arises because they forced employers to be more flexible in terms of allowing workers to work from home. However, we find little evidence that effects differ between workers with different work-from-home arrangements.

Finally, we investigate the role of clinical vulnerability to COVID-19. Workers who are more vulnerable to COVID-19 faced stronger restrictions in terms of the work they could still do during the pandemic and could thus be more vulnerable to dismissals. Comparing effects for workers who were told by the National Health Service that they were vulnerable to COVID-19 with those for non-vulnerable workers we indeed find stronger protective effects of workplace unionisation on the former.

In a third step, we investigate effect heterogeneity by the size of the pre-COVID workplace. We find that effects across workplace sizes generally go in the same direction but are largest for workers in workplaces with more than 500 employees, followed by those with less than 50 employees and those with 50 to 499 employees. We also consider differences between private and other employers (public, charities) and find slightly larger effects outside of the private sector.

Finally, we examine auxiliary outcomes. First, we look at job-related expectations. We find some evidence that unionised workers' subjective assessment of their financial situation remained more positive during COVID-19 but find little effect on job satisfaction or financial expectations. Second, workplace unions in the UK are commonly involved in discussions around health and safety at work. In principle, this could mean that unionised workers experienced better health outcomes during COVID-19, for example, because of more stringent infection control measures at work. This could result either in objectively better health outcomes or in better mental health or higher satisfaction if workers feel better protected. However, given the widespread prevalence of COVID-19 in the UK and the possibility to infect oneself outside of work, it is also possible that the protective effect of workplace unionisation on health outcomes is negligible. We, in fact, find evidence in favour of the second possibility: Both objective and subjective health outcomes of unionised workers do not change differently from those of non-unionised workers.

The remainder of this paper is organised as follows: Section 2 presents background information on workplace unionisation in the UK, the Coronavirus pandemic and relevant institutional details such as the furlough scheme. Section 3 outlines data and estimation strategy. Section 4 presents results. Section 5 concludes.

## **2. Background**

### *2.1 Workplace unionisation in the UK*

While our empirical design compares workers based on their pre-COVID unionisation status and thus abstracts from possible changes to workplace union recognition due to the pandemic, we briefly outline the process by which unions can become recognised in a specific workplace in the UK.

There are essentially two possibilities, voluntary and statutory recognition. In either case, the process begins with a written request for recognition by the respective union to an employer. This request must include the name of the union, define the employee group that will be represented by the union and state that the union is making the request under Schedule A1 of the Trade Union and Labour Relations (Consolidation) Act 1992. At this stage, the employer can accept the request and enter collective bargaining with the union.

Employers can also reject the initial request but enter (time-limited) negotiations with the union around questions such as which workers should be represented if the union was recognised or reject the request outright. In the latter case or if negotiations break down, the union can apply for statutory recognition to the Central Arbitration Committee (CAC). Statutory recognition will occur if the union satisfies the relevant requirements, specifically

that (1) they have sent the employer a copy of their application and any supporting documents, (2) they have at least 10% union membership among the employees they want to represent and (3) they have evidence that a majority of employees are in favour of recognition. Statutory recognition will fail if the union has unsuccessfully applied for recognition in the last 3 years, the union is not a certified independent union, another union is already recognised for the same employees in the same workplace or another union representing 10% of the employees has already applied to CAC. In addition, CAC can hold a ballot among the employees whether they want union recognition. This will usually occur either if union membership is less than 50% or if CAC believes that a ballot will help maintain good relations between employer and employees, there is evidence that a significant number of union members in the bargaining unit do not want the union to represent them or if there are concerns about why some members joined the union, for example, if they were pressured.

## *2.2 COVID-19 in the UK*

The UK was hit hard by COVID: At the end of 2021, it had experienced 2,627.45 deaths per million population – the 20<sup>th</sup> highest relative death count in the world – with a total of 177,376 confirmed deaths on 31<sup>st</sup> December 2021, the 8<sup>th</sup> highest count in the World (Mathieu et al., 2022). The COVID-19 pandemic in the UK essentially followed three waves: An initial wave from March to June 2020 (visible mainly in hospitalisation and deaths figures due to an undercounting of cases and a lack of testing during the early phase of the pandemic), a second wave – visible in all outcomes – in early 2021 (driven by the “Alpha” variant of the virus) and a third wave driven by the “Delta” and “Omicron” variants in the second half of 2021, when widespread vaccinations had led to a decoupling of case numbers and severe outcomes of the disease.

Following the decision by the World Health Organisation (WHO) to declare the COVID-19 outbreak as a global pandemic on 11<sup>th</sup> March 2020, the UK government enacted strict non-pharmaceutical policy interventions (NPIs). These included a full national lockdown from the 23<sup>rd</sup> March 2020 (ending in June 2020) with stay at home orders, school closures for all pupils but those of essential workers and vulnerable children, closure of most non-essential businesses, cancellation of public events, tight restrictions on gatherings (individuals were not allowed to meet individuals from outside their household from 23<sup>rd</sup> March), reduced public transport and restrictions on internal movements. The public was only allowed to leave their homes to shop for basic necessities, exercise outdoors once a day either alone or with members of the same household, for medical purposes or to provide care to vulnerable individuals and to travel to and from work where this was absolutely necessary and could not be done from home.

Following the initial lockdown, the constituent countries of the UK followed different timelines for the reopening of society. Focussing on England, the period from June 2020 first saw a gradual reopening of society, including the reopening of theatres and indoor hospitality from August (including the “Eat out to help out” scheme that subsidised food and drink in restaurants – see Fetzer, 2022, for an analysis of the link between this scheme and COVID-19 infections). Following a steady increase in COVID-19 infection rates and deaths, measures

were tightened again after the summer, leading to a 3-tier system of restriction based on local infection rates from the 14<sup>th</sup> October 2020 and a second four-week national lockdown from the 5<sup>th</sup> of November in England.<sup>2</sup> Following a slight reduction of restrictions over Christmas 2020, England went into a third lockdown on 6<sup>th</sup> January 2021. People again had strict orders to stay home, but nurseries remained open, the use of support bubbles continued, and some gatherings were excluded from the ban.

The mass COVID-19 vaccination rollout started in the UK on 8<sup>th</sup> December 2020. The vaccine rollout commenced in phases according to individual vulnerability and age. From 8<sup>th</sup> March 2021 the “roadmap out of lockdown” began, a phased lockdown exit strategy, designed to allow more individuals to receive their first COVID-19 vaccine, which slowly eased restrictions on 8<sup>th</sup> March, 29<sup>th</sup> March, 17<sup>th</sup> May and 19<sup>th</sup> July when all remaining legal restrictions were lifted<sup>3</sup> (Cabinet Office, 2021). No further lockdowns were implemented. At the end of 2021, 90.1% of the population aged 12 and over in the UK had received one vaccination, 82.5% a second vaccination and 59.3% a third dose.

### *2.3 Furlough and working from home*

As explained in Section 2.2, during 2020 and 2021, the UK Government implemented various lockdowns that closed down certain businesses at various points in time. While certain industries were always affected as a whole (e.g., hospitality or leisure where all businesses had to close during certain times or healthcare where businesses generally remained open), in many industries lockdown measures were a combination of the classification of the employer and the occupation of the worker. For example, plant operatives working for a manufacturing company could be considered essential workers if the company produced medical supplies, while administrative staff in the same company would likely be ordered to work from home. However, neither would be considered essential if working for a manufacturing company producing, say, fitness equipment or flatscreen televisions. Similarly, kitchen staff in a hospital would be essential workers who would be needed at their workplace while kitchen staff in hospitality premises would likely not be working.

Workers whose employers were affected by lockdowns and who could not work from home were offered government support (“furlough”, officially the Coronavirus Job Retention Scheme) that paid 80% of their previous earnings (with the possibility for employers to pay the

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<sup>2</sup> Tier 1: “Rule of six” and 10pm curfew for hospitality. Tier 2: No indoor gatherings but outdoor gatherings up to six people allowed. Tier 3: Strict ban on household mixing and closure of hospitality sector. Retail sector, schools and personal care remained open.

<sup>3</sup> Step 1 included school openings in England and two individuals being allowed to meet outdoors. Otherwise people were asked to remain at home. On 29<sup>th</sup> March outdoor gatherings of six individuals or two households and the reopening of outdoor sports facilities commenced and stay at home orders were lifted. Step 2 commencing on 12<sup>th</sup> April saw the reopening of non-essential retail, the food hospitality sector opened its doors for outdoor catering, outdoor venues and indoor leisure and recreational business as well as self-catering holiday accommodation. Step 3 increased the number of individuals for outdoor gatherings to 30 and indoor gatherings to follow the “rule of six”. Indoor trading for business reopened. Spectator sport was allowed for very large venues to allow up to 10,000 spectators (e.g., football). Step 4 commenced on 19<sup>th</sup> July (four weeks later than previously anticipated). This removed almost all legal requirements for social contacts in England and the remaining closed sectors reopened (e.g., nightclubs).



remaining 20%). The scheme was gradually phased out during the summer of 2021 and fully ended at the end of September 2021. In our context, the availability of furlough pay during 2020 and most of 2021 will likely have a dampening effect on any insurance function played by workplace unions. In a sense, furlough can be considered as a government provided insurance that protects workers against the adverse economic shock caused by COVID-19 that will likely substitute for any related role that would otherwise be played by workplace unions.

In addition, legislation mandated that workers who could work from home were obliged to do so. Working from home can in principle also protect workers from adverse employment consequences caused by the forced closure of their employer. Recent evidence has documented large within-occupation and within-industry differences in the percentage of work-tasks that can be done from home (Adams-Prassel et al., 2022), suggesting considerable heterogeneity depending on specific workplace arrangements. It is, in principle, possible that the presence of a workplace union influences these arrangements, for example, by increasing management trust that workers will still be productive.

### 3. Data

We mainly use Understanding Society, a household panel that replaced the British Household Panel Survey and has been running since 2009. The most recent data from wave 12 covers interviews until the end of 2021. Each wave covers approximately 40,000 households. Waves in Understanding Society cover two years each but fieldwork for consecutive waves overlaps so that individual respondents are usually interviewed annually. The data also contains the exact date of the interview. We focus on individuals who were employed in wave  $j$  – the last wave before COVID-19 – in either a unionised (6,911 individuals) or non-unionised workplace (7,574 individuals). Questions on workplace unionisation are not included in every wave, so we cannot study possible changes to union representation due to COVID-19. We then use data for these individuals from 2018 to 2021 (including data from waves  $i$  to  $l$ ), giving us a total estimation sample of 44,245 observations, 22,960 for individuals who worked in non-unionised workplaces prior to COVID-19 and 21,285 for individuals who worked in unionised workplaces.

In addition, we use data from the Understanding Society COVID-19 surveys. These interviewed a subset of individuals participating in Understanding Society in eight waves during 2020 and 2021. Each wave covered a different range of topics. We use data from all eight waves to gather information on topics such as clinical vulnerability to COVID-19, lockdown/furlough and working from home.

Our main focus is on six employment-related outcomes, specifically whether an individual works for their pre-COVID employer, at their pre-COVID workplace or in their pre-COVID job, whether an individual is employed (as opposed to being unemployed or out of the labour force), their working hours and their monthly gross labour income. The first three outcomes speak directly to the protective effect of unions via employer–worker risk sharing, while the other outcomes are informative about general worker outcomes as well as possible trade-offs between job stability and wages. Additionally, we also consider subjective employment related outcomes – an individual’s assessment of their financial situation, their

financial expectations and their job satisfaction – and health outcomes, specifically self-rated health, the presence of any health condition, three simple life satisfaction questions (with life in general, health and income) and two measures of anxiety based on the General Health Questionnaire (GHQ), a screening device for identifying minor psychiatric disorders in the general population. The GHQ consists of twelve questions with individual items asking about aspects such as sleep problems, feelings of worry or anxiety or concentration problems. We use a linear scale as well as a caseness measure used in clinical screening. The latter is a dummy variable that is “1” if four or more questions indicate a potential problem.

Table 1 provides descriptive statistics. The high percentage of employed individuals is a direct consequence of the sample selection criteria with everyone in the sample being employed in wave  $j$ .

(Table 1 about here.)

#### 4. Empirical approach

Our empirical approach compares the outcomes of workers in unionised and non-unionised workplaces in a difference-in-differences framework. We fix the treatment assignment – the presence of a workplace union – in the last observed wave prior to COVID-19. While this choice is partially data driven as outlined in Section 3, it also allows us to abstract away from changes to workplace unionisation due to COVID-19 or workers moving into or out of unionised workplaces. Given that the onset of COVID-19 was an unanticipated shock, we can also rule out anticipation effects, such as selection of workers into unionised workplaces to benefit from higher protection during the pandemic.

We begin by estimating simple  $2 \times 2$  difference-in-differences and then gradually introduce a number of controls, such as individual fixed effects or interactions between pre-treatment characteristics and time. Our most comprehensive specification for the outcome  $Y_{it}$  of a worker  $i$  in a year  $t$  is

$$Y_{it} = \alpha_i + \gamma_t + \beta' age_{it} + \delta'(W_i \times \gamma_t) + \tau \times (Unionised_i \times post-COVID_t) + \varepsilon_{it}, \quad (1)$$

which includes an individual fixed effect  $\alpha_i$ , a year fixed effect  $\gamma_t$ , a set of age dummies ( $age_{it}$ ) and interactions between pre-COVID characteristics  $W_i$  and year dummies to allow for differential time-trends for workers who had different job tenures pre-COVID or who worked in different industries, occupations or regions. We cluster standard errors at the individual level, which is essentially the level at which the treatment is assigned.

There is a number of possible confounding influences that the interactions between pre-COVID characteristics  $W_i$  and year dummies control for. First, demand shocks due to lockdowns or behavioural changes can vary either at the industry level or possibly at the regional level, in particular during the time period when closures were tied to local infection rates. Following Braakmann et al. (2022), we control for these using interactions between a worker’s pre-COVID industry or region of residence and year dummies. Second, the evidence

in Braakmann et al. (2022) suggests that job mobility was influenced by occupation-specific mortality rates, which could lead to differential trends by occupation. To control for these, we include interactions between a worker's pre-COVID occupation and year dummies. Finally, we include interactions between a worker's pre-COVID tenure in their job and year dummies to control for possible differential job tenure between unionised and non-unionised workers.

We then explore the robustness of our estimates in three ways. Firstly, we estimate full event study specifications, where we estimate half-yearly effects relative to the second half of 2019. This gives us three pre-treatment-estimates to evaluate possible pre-trends and four post-treatment estimates to judge treatment effect dynamics. Given the annual frequency of interviews in Understanding Society each of these effects is estimated using a different composition of workers. However, Understanding Society randomises the timing of interviews, which limits the possibility of composition bias (see, e.g., Powdthavee et al., 2019, or Braakmann, 2021, for similar arguments). Secondly, we conduct a formal sensitivity analysis to common trends violations recently proposed by Rambachan and Roth (2022) that uses observed deviations from common trends in the pre-treatment period to bound the treatment effect in the face of post-treatment common trend deviations of different magnitudes. Finally, we conduct a placebo test by using randomisation inference. We randomise a worker's pre-COVID union status 500 times and recalculate the resulting placebo treatment effects.

In a next step, we turn to an investigation of treatment effect heterogeneity across three dimensions. Firstly, we explore heterogeneity by age and pre-COVID job tenure. We expect that the insurance function of unions plays a lesser role for young workers who arguably find it easier to find a new job after losing their old one. Similarly, we expect unions to focus more on workers with high job tenure whom we expect to form unions' main clientele. Moreover, employment protection in the UK (against unfair dismissal) is absent for workers with less than two years of job tenure, which will likely limit unions' influence on the employment security for these low-tenure workers.

Subsequently, we investigate the role of the various non-pharmaceutical interventions that were implemented during 2020 and 2021 to reduce the spread of SARS-CoV-2. We rely on data from the Understanding Society Covid-surveys for 2020 that contain information on whether an individual was furloughed or worked from home in any of the waves. Finally, we investigate the role of clinical vulnerability to COVID-19. Workers who are more vulnerable to COVID-19 faced stronger restrictions in terms of the work they could still do during the pandemic and could thus be more vulnerable to dismissals.

In a third step, we investigate effect heterogeneity by the size of workers' pre-COVID workplace. There are two countervailing influences workplace size may exert on the insurance function of unions. On the one hand, union recognition is more widespread and union density higher in large workplaces (Department for Business, Energy and Industrial Strategy, 2022) suggesting that unions are more powerful in large workplaces and consequently more capable of protecting workers from external shocks. On the other hand, small workplaces may be particularly vulnerable to external shocks because of their limited resources. Consistent with this view, Lai et al. (2016) find for the UK that small firms were more likely to lay off workers during the Great Recession and more vulnerable than big firms in general. Union presence may for this reason be particularly helpful to workers in small workplaces. We also consider differences between private and other employers (public, charities).

## 5. Results

### 5.1 Main results

Table 2 presents our main results. Column (1) is a simple  $2 \times 2$  difference-in-differences, column (2) replaces the treatment group dummy with individual fixed effects, column (3) adds workers age dummies, column (4) adds pre-COVID tenure-by-year interactions, columns (5) and (6) add respectively pre-COVID industry- and occupation-by-year interactions and column (7) combines all these controls plus region-by-year interactions. All estimates are generally very stable across specifications. The only controls that tend to reduce estimates in size are industry-by-year interactions. This result, however, is hardly surprising: Both lockdowns and behavioural changes by the population leading to changes in demand would likely be picked up by these interactions.

(Table 2 about here.)

Overall, we find evidence that workers in unionised workplaces were protected during COVID-19 in line with the arguments around employer–worker risk sharing: They were between 4.5 and 6.6 percentage points more likely to work for their pre-COVID employer, between 4.6 and 6.2 percentage points more likely to work at their pre-COVID workplace and between 3.8 and 5.3 percentage points more likely to work in their pre-COVID job. We also find that they are between 1.3 and 2.2 percentage points more likely to be employed, which is line with the general evidence on better outcomes of unionised workers during times of crisis such as Bachmann and Frings (2021) and Han (2022). There is no evidence that this greater employment stability is traded off against lower working hours or labour income – any differences between unionised and non-unionised workers in these outcomes are generally both small and statistically insignificant.

In Figure 1, we present full event study estimates where we estimate half-yearly effects relative to the second half of 2019: Pre-treatment effects tend to be small and centred around zero, which is supportive of the common trend assumption. Treatment effects also tend to be larger in 2021 than in 2020 for the first three outcomes – working for the pre-COVID employer, at the pre-COVID workplace or in the pre-COVID job. This pattern suggests that the protective effects of unions become more important during the final (third lockdown) in early 2021 and the more “normal” second half of that year, which may possibly reflect the accumulating nature of the three subsequent lockdowns. Furthermore, the second half of 2022 coincided with the fading out and the abolition of the furlough scheme in September, thereby plausibly lending a bigger role to the protection of workers through unions than in times when furlough pay was available. Effects on employment do not tend to vary much year-by-year and are only individually significant in the first half of 2020 – however, point estimates in later periods are generally of a similar size. For hours and labour income, there is little evidence for any effect at any point in time.

(Figure 1 about here.)

## 5.2 Robustness

In Figure 2, we report the result from the formal sensitivity analysis to common trend violations by Rambachan and Roth (2022). We present results for the whole post-treatment effect – combining 2020 and 2021 – as well as for each year separately. For each effect, we report the original 95% confidence interval – valid under the assumption of no common trend violation – as well as adjusted confidence intervals that allow for common trend violations equal to 0.1, 0.25, 0.5, 1, 1.5 and 2 times the magnitude observed during the pre-treatment period. Overall, the results suggests that our estimates are modestly robust to common trend violations. Where the original effect is significant, they typically survive common trend violations equal to 0.5 times the magnitude of those observed pre-treatment at the 5% level of significance. They also often survive slightly larger violations at the 10% level.

(Figure 2 about here.)

In Table 3, we report the results of our randomisation inference tests. We report the original effect ( $\tau(\text{obs})$ ), the number of the 500 placebo estimates that are larger in absolute value than the original estimate ( $\#\tau \geq |\tau(\text{obs})|$ ), the resulting  $p$ -value and its standard error. For the four outcomes where we observed a significant effect in Table 2, we typically find that these effects are the largest or the second-largest effect in the distribution of placebo effects, which supports our main findings. For hours and labour income, Table 3 also confirms that effects on these outcomes are generally insignificant.

(Table 3 about here.)

## 5.3 Heterogeneity

We present three sets of heterogeneity estimates, focussing first on worker age and pre-COVID job tenure in Table 4. We find that the protective effects of workplace unionisation are strongest for prime age workers between 30 and 50, followed by older workers, while there is little effect on younger workers. A similar pattern exists for pre-COVID tenure: There is no effect of workplace unionisation on workers with less than two years of tenure – the threshold at which most employment rights in the UK come into effect. Effects are strongest for workers with exactly two years of tenure prior to COVID, followed by workers with 3 or more years.

(Table 4 about here.)

In Table 5, we focus on the role of furlough, home-working and clinical vulnerability. We find little effects of workplace unionisation on the outcomes of workers that were ever furloughed, which supports the idea that furlough served as a government issued insurance to

workers that superseded possible effects of workplace unionisation. It could in principle also be possible that the protective role of workplace unions arises because they forced employers to be more flexible in terms of allowing workers to work from home. However, we find little evidence that effects differ between workers with different work-from-home arrangements.

Finally, we investigate the role of clinical vulnerability to COVID-19. Workers who are more vulnerable to COVID-19 faced stronger restrictions in terms of the work they could still do during the pandemic and could thus be more vulnerable to dismissals. Comparing effects for workers who were told by the National Health Service that they were vulnerable to COVID-19 with those for non-vulnerable workers we indeed find stronger protective effects of workplace unionisation on the former.

(Table 5 about here.)

In Table 6, we investigate effect heterogeneity by the size of the pre-COVID workplace. We find that effects are generally in the same direction but are generally largest for workers in workplaces with more than 500 employees, followed by those with less than 50 employees and those with 50 to 499 employees. We also consider differences between private and other employers (public, charities) and find slightly larger effects outside of the private sector.

(Table 6 about here.)

#### *5.4 Auxiliary outcomes*

In Table 7, we look at job-related expectations. We find some evidence that unionised workers' subjective assessment of their financial situation remained more positive during COVID-19, specifically workers in unionised workplaces are about 2 percentage points less likely to state that their financial situation is difficult, which is substantial given that the mean of this variable is 0.06. There is little effect on job satisfaction or financial expectations.

(Table 7 about here.)

Finally, we investigate unionisation insurance effects of a different kind. Specifically, workplace unions in the UK are commonly involved in discussions around health and safety at work. In principle, this could mean that unionised workers experienced better health outcomes during COVID-19, for example, because of more stringent infection control measures at work. This could result either in objectively better health outcomes or in better mental health or higher satisfaction if workers feel better protected. However, given the widespread prevalence of COVID-19 in the UK and the possibility to infect oneself outside of work, it is also possible that the protective effect of workplace unionisation on health outcomes is negligible. In Table 8, we, in fact, find evidence in favour of the second possibility: Both objective and subjective health outcomes of unionised workers do not change differently from those of non-unionised workers.

(Table 8 about here.)

## 6. Discussion and conclusion

The onset of the COVID-19 pandemic in 2020 and the accompanying public health measures aimed at reducing the spread of SARS-CoV-2 posed a strong, wholly unanticipated external shock to businesses that not only experienced a widespread drop in demand but also severe interruptions in their operations. In this paper, we considered the role of unions in protecting individual workers from losing their job following this truly exogenous shock faced by their employers – a mechanism predicted by models of implicit contracts, where unions can facilitate employer–worker risk sharing. It has long been acknowledged that unions not only serve as a device for rent extraction through monopolising labour, but also – like other labour market institutions – “serve an important function of social insurance” (Agell, 2002: 108) against labour market risks. Therefore, we tested whether the presence of a workplace union mediated the adverse effects of the COVID-pandemic on individual workers.

Using UK data from the Understanding Society survey and relying on a difference-in-differences approach that compares the evolution of labour market outcomes of workers who worked at workplaces with a recognised union pre-COVID and non-unionised workers and controls for a rich set of fixed effects we saw that union presence safeguarded workers against losing their employment in all specifications: Workers at unionised workplaces were 4.5 to 6.6 percentage points more likely to stay with their pre-COVID employer, 4.6 to 6.2 percentage points more likely to work at their pre-COVID workplace and 3.8 to 5.3 percentage points more likely to work in their pre-COVID job. We found no evidence that unionised workers in return experienced lower working hours or labour incomes. All these results survived a series of robustness checks that scrutinise the common trend assumption.

Event study estimates showed that the protective effect was strongest at the end of our observational window in 2021 when no further lockdown took place and the furlough scheme, which had offered generous government support, had been abandoned. We further found effect heterogeneities that point at the absence of any insurance effect for young workers aged below 30, newly hired workers with a job tenure below two years and workers on the furlough scheme. Moreover, the protective effect of unions was biggest for workers who were clinically vulnerable to COVID-19 and workers at either small or large pre-COVID workplaces. We found no effect on health outcomes or job-related expectations with the only exception that workers at unionised workplaces considered their financial situation less often difficult.

Overall, our results are clearly indicative that workplace unions protect workers from the employment risks posed by external shocks faced by their employers in general and mediate the adverse effects of the COVID-pandemic on individual workers in particular. The presence of a workplace union thus not only insures workers against involuntary job separations during normal times but also in times of economic crisis. In a recent survey, Addison (2020: 1) concedes that “[d]eclining union power would not be an overwhelming cause for concern if not for rising wage inequality and the loss of worker voice”. In light of our findings, we think adding the loss of unions’ insurance function to the list seems justified.

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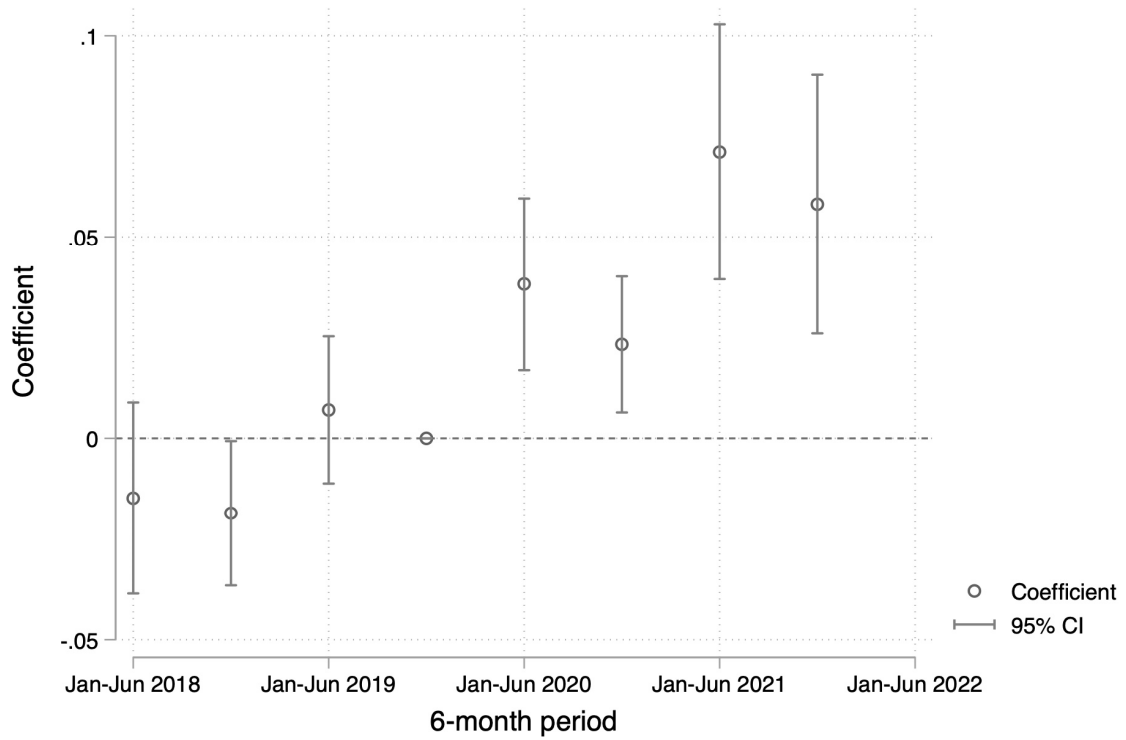


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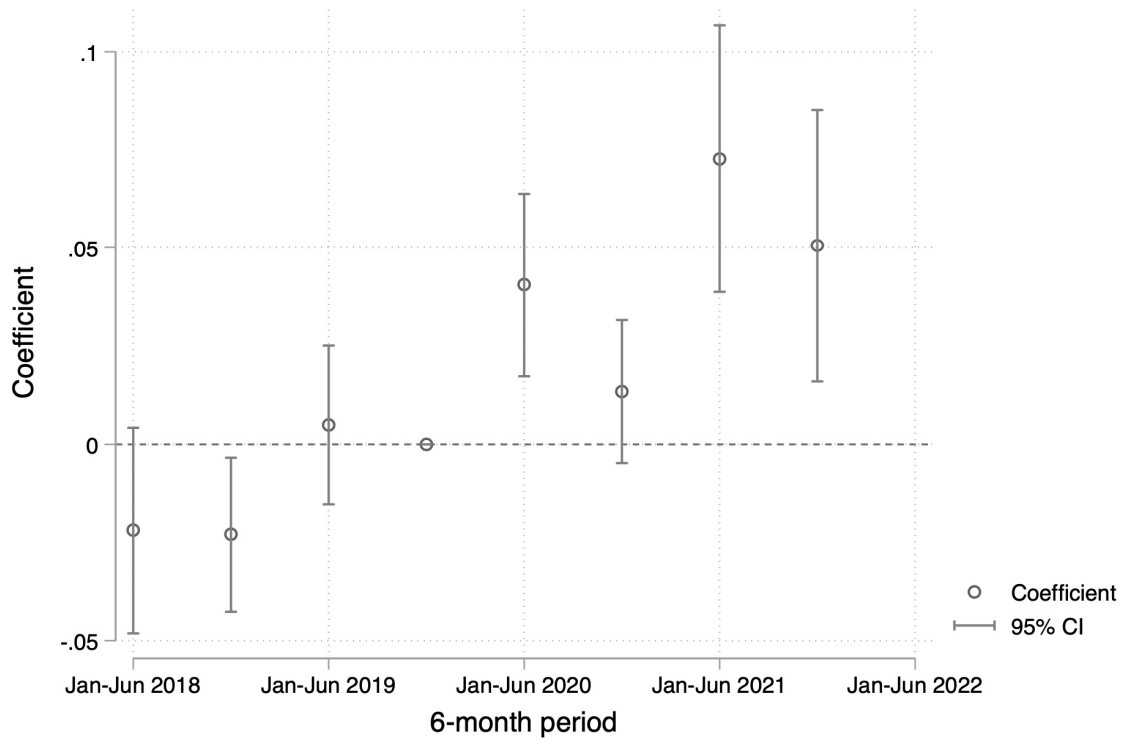
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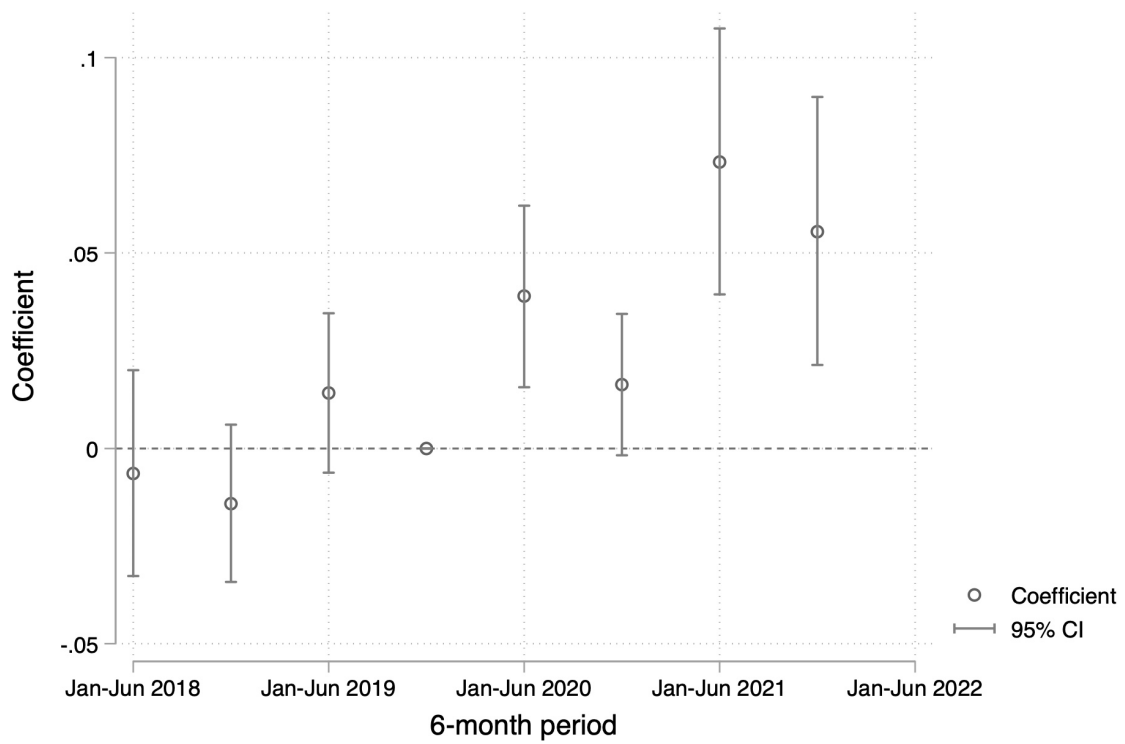
**Figure 1: Event study estimates**  
*Panel (a): Works for pre-Covid employer*



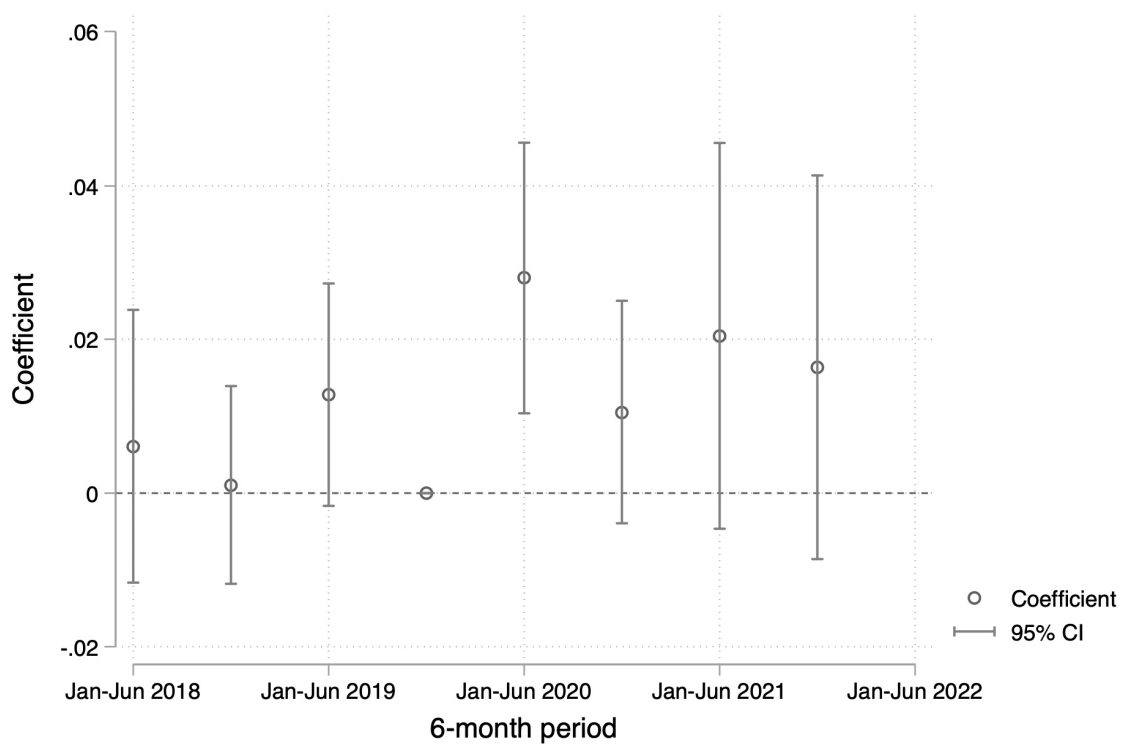
*Panel (b): Works at pre-Covid workplace*



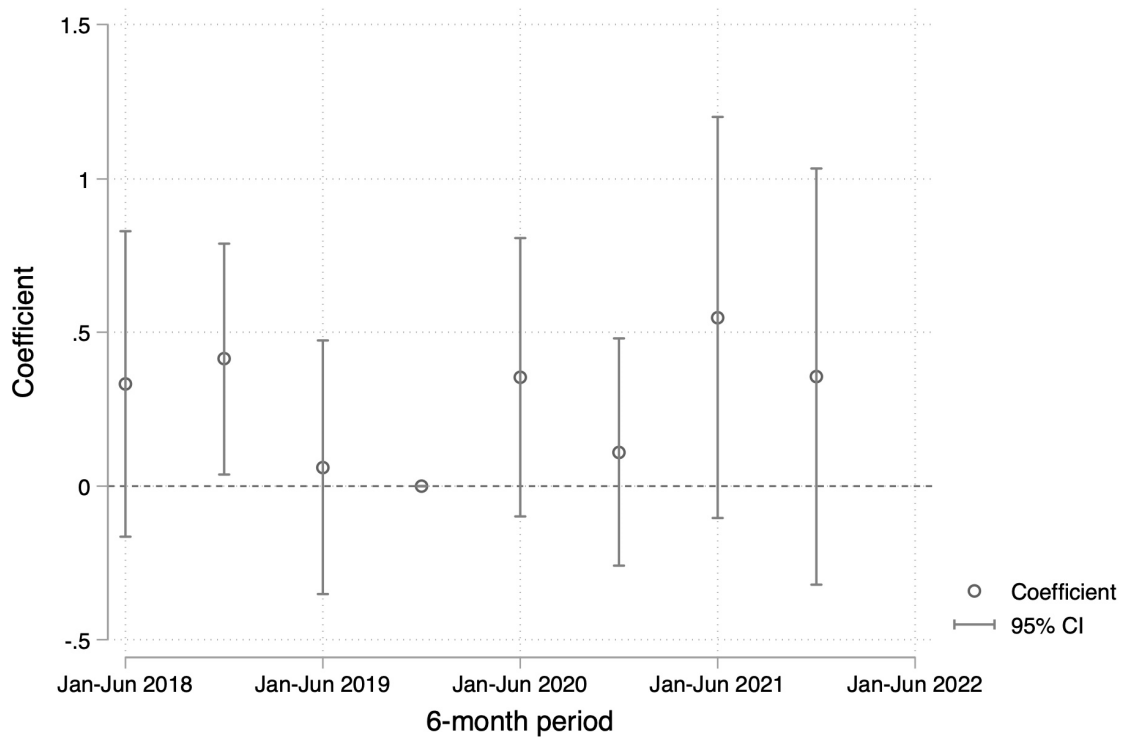
Panel (c): Works in pre-Covid job



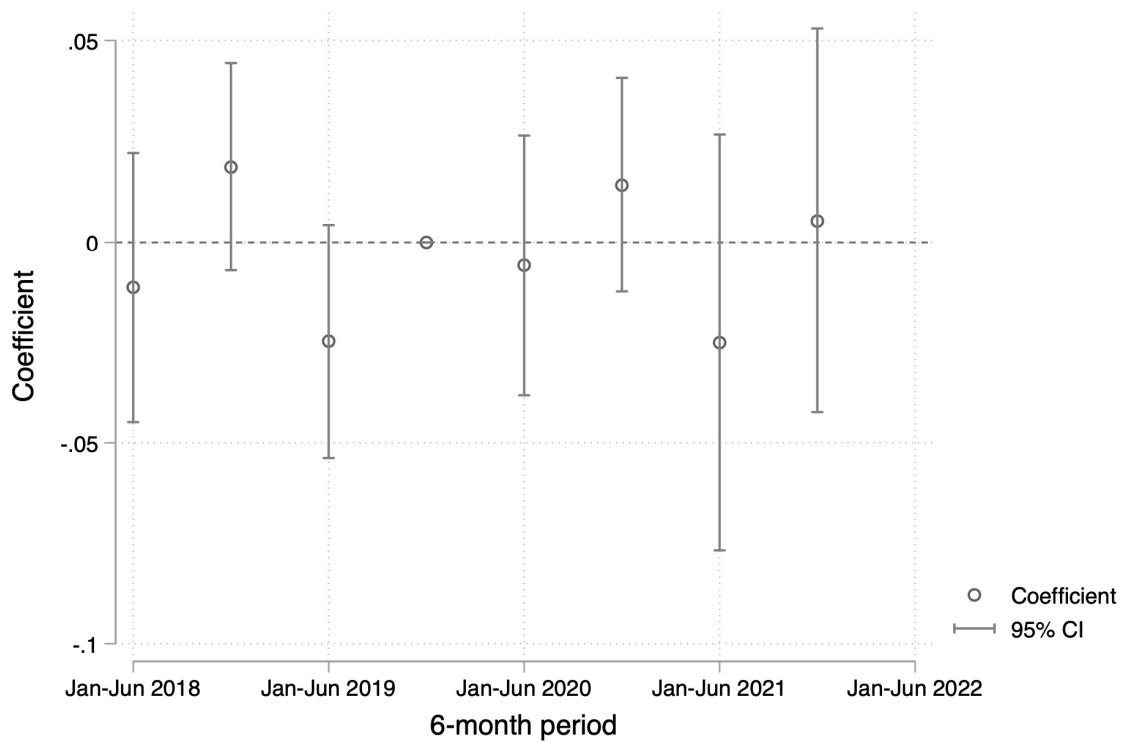
Panel (d): Employed



Panel (e): Weekly working hours

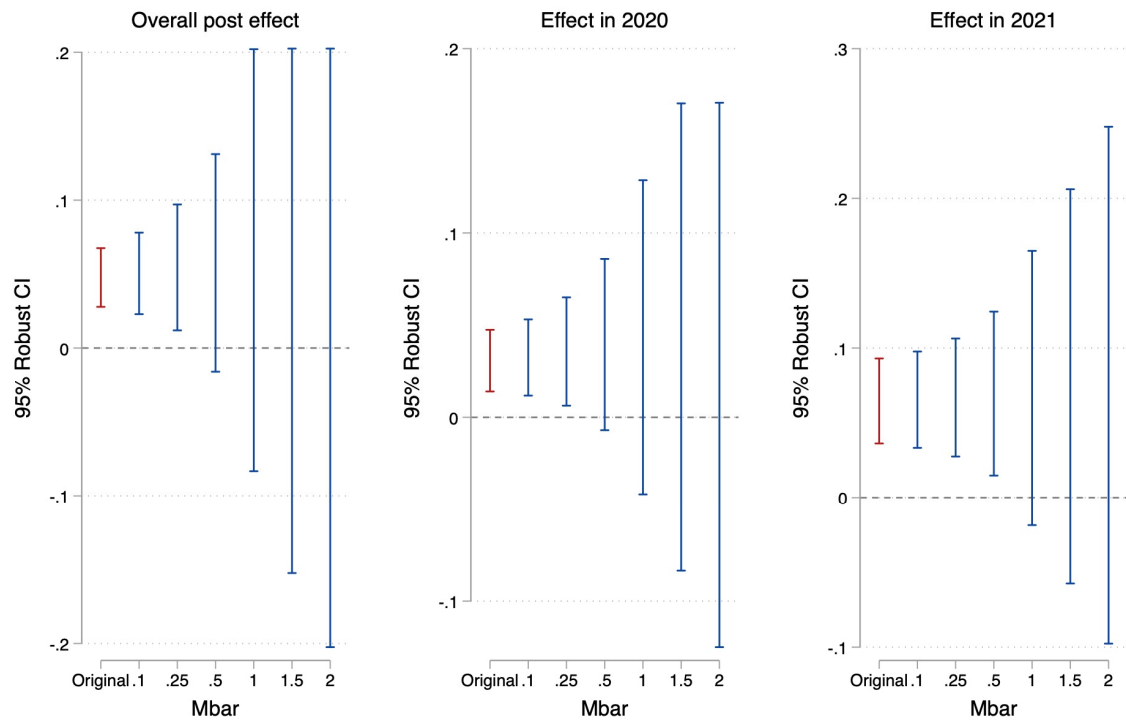


Panel (f): Ln(monthly gross labour income)

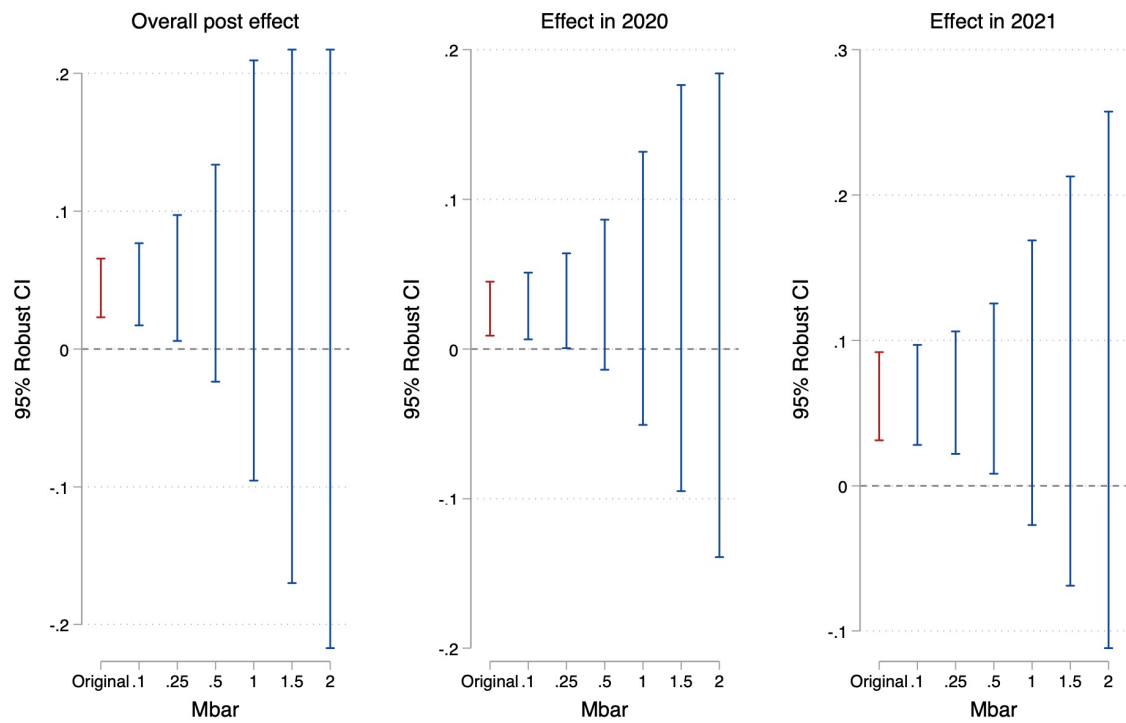


**Figure 2: Sensitivity to common trend violations**

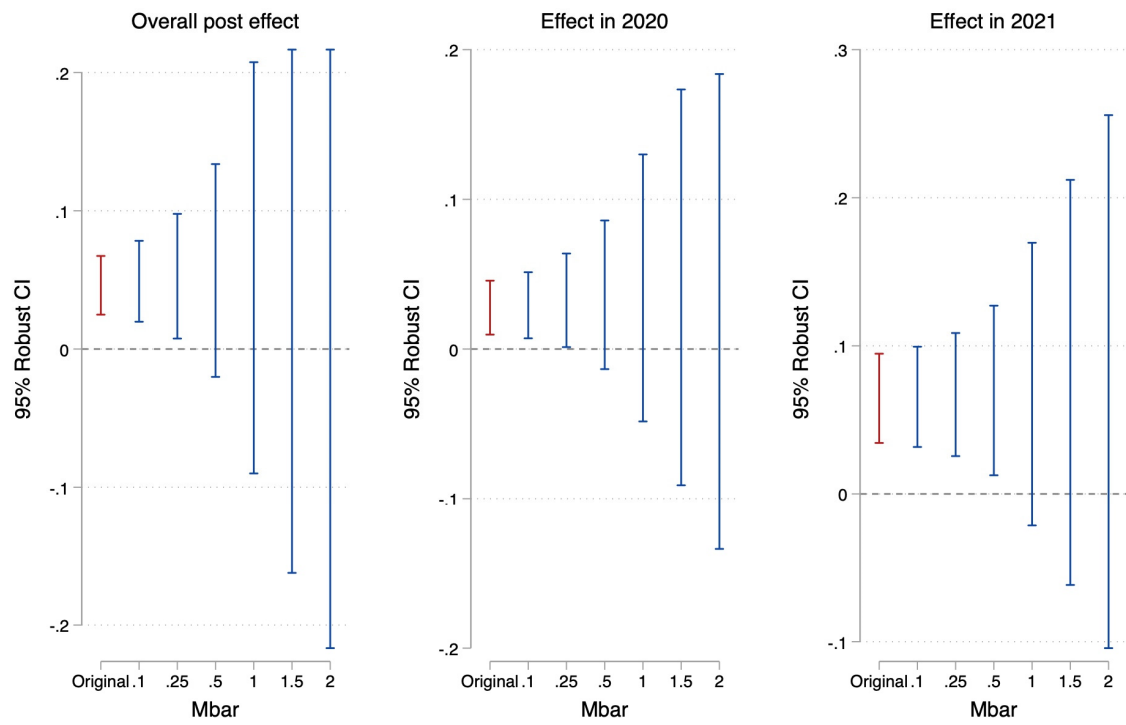
*Panel (a): Works for pre-Covid employer*



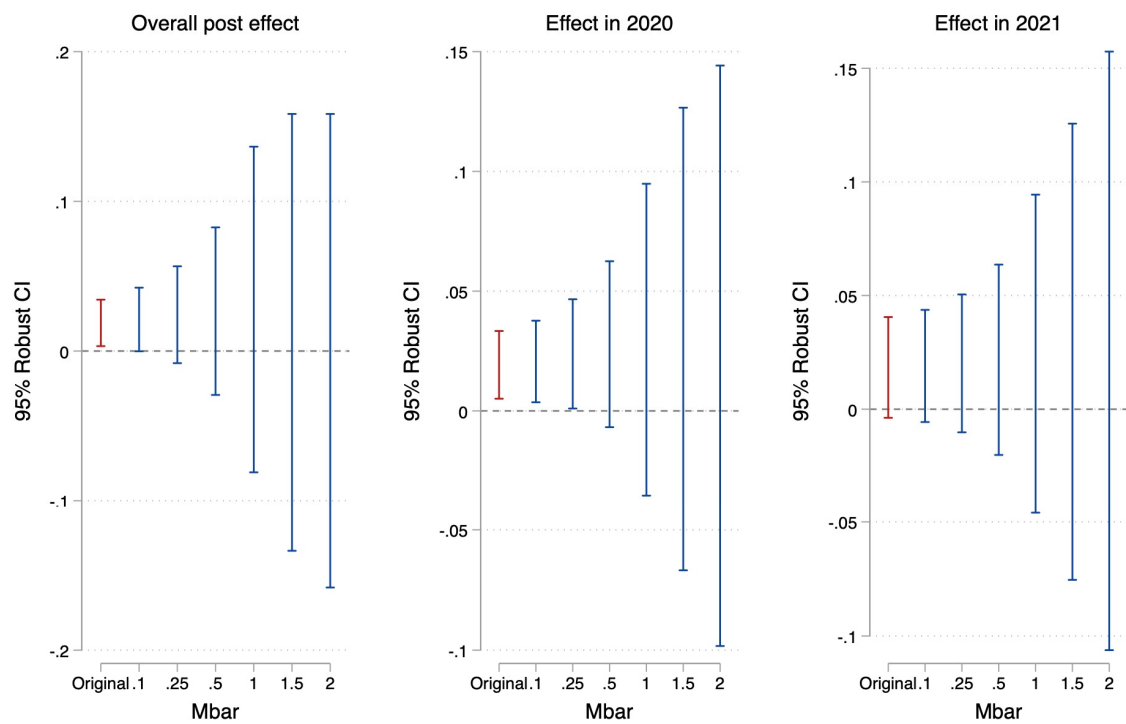
*Panel (b): Works at pre-Covid workplace*



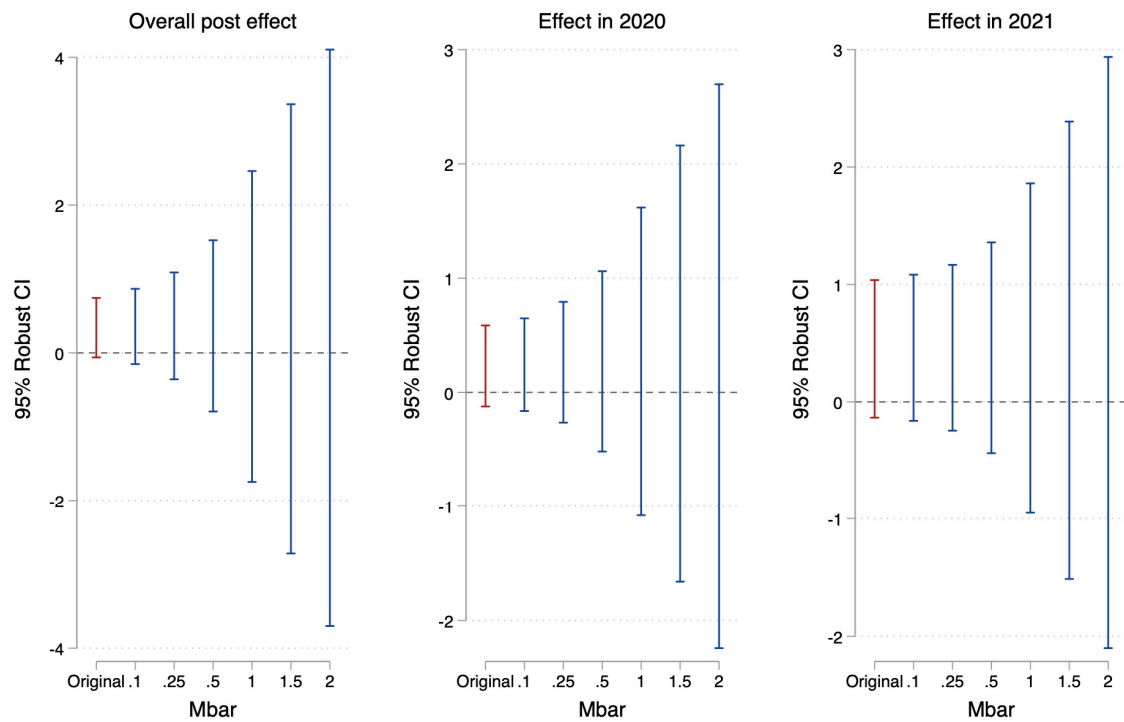
Panel (c): Works in pre-Covid job



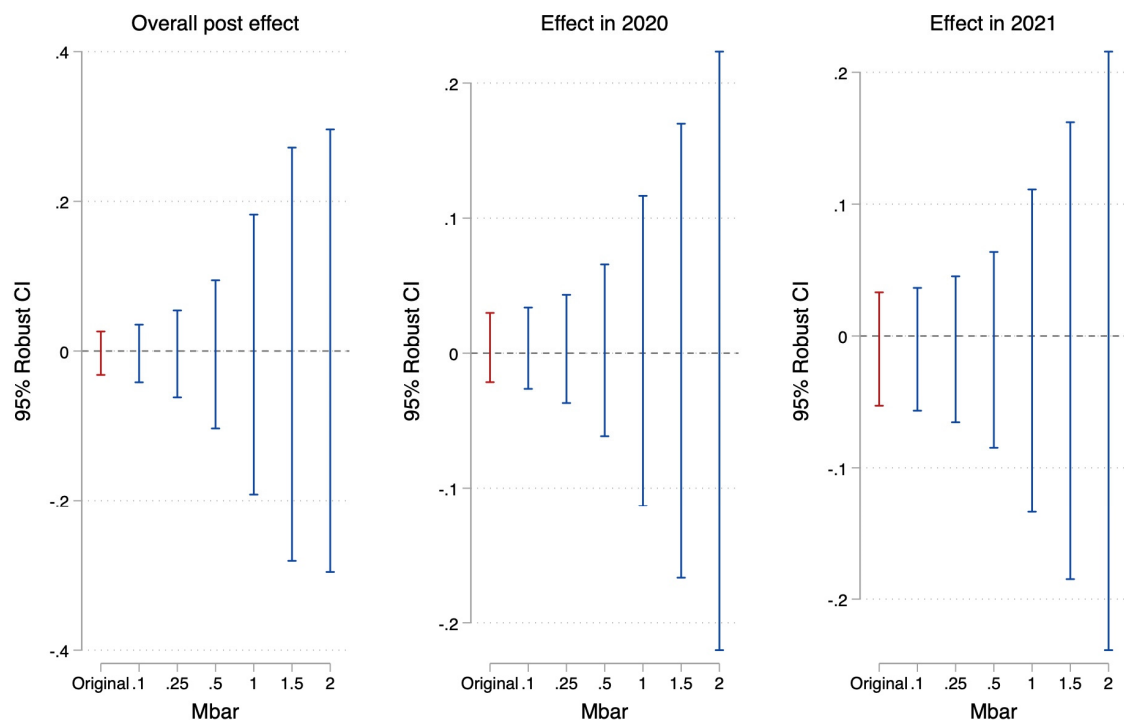
Panel (d): Employed



Panel (e): Weekly working hours



Panel (f): Ln(monthly gross labour income)



Notes: The figures show sensitivity estimates based on relative magnitude restrictions as proposed by Rambachan and Roth (2022). Presented are the original estimate as well as estimates sensitive to violations of the post-treatment common trend equal to 0.1, 0.25, 0.5, 1, 1.5 and 2 times the maximum observed deviation in the pre-treatment period.



**Table 1: Descriptive statistics**

	Overall		Employed in ... pre-COVID			
	Mean	Std.dev.	non-unionised workplace		unionised workplace	
	Mean	Std.dev.	Mean	Std.dev.	Mean	Std.dev.
Employed	0.94	0.24	0.93	0.26	0.95	0.21
Actual weekly hours	33.4	10.2	33.6	11.0	33.2	9.3
Ln(monthly gross labour income)	7.56	0.74	7.50	0.80	7.62	0.67
Works for pre-COVID employer	0.88	0.33	0.85	0.36	0.91	0.29
Works at pre-COVID workplace	0.85	0.35	0.83	0.38	0.88	0.32
Works in pre-COVID job	0.85	0.35	0.83	0.38	0.88	0.33
Age	44.7	12.7	43.8	13.3	45.6	12.0
Tenure (pre-COVID)	2.68	0.95	2.58	0.99	2.78	0.89
Current financial situation:						
Comfortable	0.27	0.45	0.27	0.45	0.28	0.45
Doing alright	0.46	0.50	0.45	0.50	0.46	0.50
Quite/very difficult	0.06	0.24	0.07	0.25	0.06	0.23
Expects worse financial situation	0.12	0.32	0.11	0.32	0.12	0.33
Job satisfaction	5.4	1.3	5.4	1.3	5.3	1.3
Self-rated health						
Excellent	0.11	0.32	0.12	0.32	0.11	0.31
Good	0.37	0.48	0.37	0.48	0.38	0.48
Fair	0.13	0.33	0.13	0.33	0.13	0.33
Poor	0.02	0.14	0.02	0.13	0.02	0.14
Any health condition	0.26	0.43	0.24	0.43	0.27	0.44
GHQ caseness	0.19	0.39	0.18	0.39	0.20	0.40
GHQ linear	11.4	5.4	11.3	5.3	11.6	5.4
Satisfaction with						
Life	5.2	1.3	5.1	1.4	5.2	1.3
Health	4.9	1.5	5.0	1.5	4.9	1.5
Income	4.8	1.5	4.8	1.5	4.8	1.5
Observations	44,245		22,960		21,285	

*Notes:* Sample comprises 2018 to 2021 observations for individuals who in wave  $j$  were employed in either a unionised (6,911) or non-unionised workplace (7,574).

**Table 2: Main estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel (a): Works for pre-COVID employer</i>							
Unionised workplace × post-COVID	0.060***	0.065***	0.060***	0.065***	0.045***	0.066***	0.046***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)	(0.006)	(0.008)
Observations <sup>1</sup>	44245	43531	43527	43526	43518	43526	43517
<i>Panel (b): Works at pre-COVID workplace</i>							
Unionised workplace × post-COVID	0.057***	0.062***	0.057***	0.061***	0.046***	0.062***	0.047***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)
Observations <sup>1</sup>	44245	43531	43527	43526	43518	43526	43517
<i>Panel (c): Works in pre-COVID job</i>							
Unionised workplace × post-COVID	0.049***	0.053***	0.048***	0.053***	0.038***	0.053***	0.039***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)
Observations <sup>1</sup>	44245	43531	43527	43526	43518	43526	43517
<i>Panel (d): Employed</i>							
Unionised workplace × post-COVID	0.020***	0.021***	0.020***	0.022***	0.013**	0.022***	0.015**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.006)
Observations <sup>1</sup>	44245	43531	43527	43526	43518	43526	43517
<i>Panel (e): Weekly working hours</i>							
Unionised workplace × post-COVID	-0.114	-0.100	0.030	0.075	0.105	0.094	0.108
	(0.156)	(0.119)	(0.119)	(0.118)	(0.148)	(0.120)	(0.150)
Observations <sup>1</sup>	40616	39228	39224	39223	3921	39223	39214
<i>Panel (f): Ln(monthly gross labour income)</i>							
Unionised workplace × post-COVID	-0.030***	-0.010	0.003	0.003	0.005	0.004	0.005
	(0.011)	(0.008)	(0.008)	(0.008)	(0.010)	(0.008)	(0.010)
Observations <sup>1</sup>	41653	40493	40489	40488	40479	40488	40478
Treatment group FE	Yes	No	No	No	No	No	No
Year FE	Yes	Yes	Yes	No	No	No	No
Individual FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	No	No	Yes	Yes	Yes	Yes	Yes
Pre-COVID tenure × year FE	No	No	No	Yes	Yes	Yes	Yes
Pre-COVID industry × year FE	No	No	No	No	Yes	No	Yes
Pre-COVID occupation × year FE	No	No	No	No	No	Yes	Yes
Region × year FE	No	No	No	No	No	No	Yes

*Notes:* Coefficients, standard errors adjusted for clustering at the individual level in parentheses. \*\*\*/\*\*/\* denote statistical significance on the 10%, 5% and 1% level respectively. <sup>1</sup> Observations are effective sample sizes excluding singleton observations for the respective set of fixed effects.

**Table 3: Randomisation inference, 500 replications**

	(1)	(2)	(3)	(4)
	$\tau(\text{obs})$	$\#\tau \geq  \tau(\text{obs}) $	$p$	$SE(p)$
<i>Panel (a): Works for pre-COVID employer</i>				
Unionised workplace $\times$ post-COVID	0.046	0	0.000	0.0000
<i>Panel (b): Works at pre-COVID workplace</i>				
Unionised workplace $\times$ post-COVID	0.047	0	0.000	0.0000
<i>Panel (c): Works in pre-COVID job</i>				
Unionised workplace $\times$ post-COVID	0.039	0	0.000	0.0000
<i>Panel (d): Employed</i>				
Unionised workplace $\times$ post-COVID	0.015	1	0.002	0.0020
<i>Panel (e): Weekly working hours</i>				
Unionised workplace $\times$ post-COVID	0.108	196	0.392	0.0218
<i>Panel (f): Ln(monthly gross labour income)</i>				
Unionised workplace $\times$ post-COVID	0.005	271	0.542	0.0223

Notes: Based on 500 replications. Unionised workplace status in wave  $j$  (membership in the treatment group) randomly assigned to individuals.

**Table 4: Heterogeneity I: Age and tenure**

	(1)	(2)	(3)	(4)	(5)	(6)
	Below 30	Pre-COVID-age 30 – 50		Above 50	Pre-COVID-tenure Less than 2 2 years 3 and more years	
<i>Panel (a): Works for pre-COVID employer</i>						
Unionised workplace × post- COVID	0.012 (0.023)	0.065*** (0.011)	0.033** (0.014)	0.018 (0.029)	0.090*** (0.022)	0.043*** (0.009)
Observations <sup>1</sup>	7069	21160	15203	6230	6466	30727
<i>Panel (b): Works at pre-COVID workplace</i>						
Unionised workplace × post- COVID	-0.003 (0.024)	0.069*** (0.011)	0.030** (0.014)	0.016 (0.029)	0.087*** (0.023)	0.047*** (0.009)
Observations <sup>1</sup>	7069	21160	15203	6230	6466	30727
<i>Panel (c): Works in pre-COVID job</i>						
Unionised workplace × post- COVID	-0.011 (0.024)	0.060*** (0.012)	0.026* (0.014)	0.003 (0.029)	0.077*** (0.023)	0.040*** (0.009)
Observations <sup>1</sup>	7069	21160	15203	6230	6466	30727
<i>Panel (d): Employed</i>						
Unionised workplace × post- COVID	-0.011 (0.017)	0.028*** (0.007)	-0.002 (0.012)	0.003 (0.021)	0.036** (0.016)	0.012* (0.007)
Observations <sup>1</sup>	7069	21160	15203	6230	6466	30727
<i>Panel (e): Weekly working hours</i>						
Unionised workplace × post- COVID	0.730 (0.495)	-0.177 (0.197)	-0.011 (0.243)	0.843* (0.511)	0.452 (0.425)	-0.117 (0.165)
Observations <sup>1</sup>	6036	19642	13445	5077	5740	28283
<i>Panel (f): Ln(monthly gross labour income)</i>						
Unionised workplace × post- COVID	-0.007 (0.029)	-0.000 (0.012)	0.012 (0.018)	0.001 (0.034)	0.009 (0.028)	0.003 (0.011)
Observations <sup>1</sup>	6365	20189	13837	5400	5993	28978
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-COVID tenure × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-COVID industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-COVID occupation × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region × year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients, standard errors adjusted for clustering at the individual level in parentheses. \*/\*\*/\*\* denote statistical significance on the 10%, 5% and 1% level respectively. <sup>1</sup> Observations are effective sample sizes excluding singleton observations for the respective set of fixed effects.

**Table 5: Heterogeneity II: Furlough, home-working and clinical vulnerability**

	(1)	(2)	(3)	(4)	(5)	(6)
	Ever furloughed	Working from home		Never	Clinically vulnerable to COVID-19	
		“Always” for at least 1 wave	Can work from home		Yes	No
<i>Panel (a): Works for pre-COVID employer</i>						
Unionised workplace × post- COVID Observations <sup>1</sup>	0.003 (0.022) 5995	0.037** (0.015) 11837	0.044*** (0.013) 16926	0.041*** (0.015) 7537	0.064*** (0.020) 7621	0.043*** (0.012) 17561
<i>Panel (b): Works at pre-COVID workplace</i>						
Unionised workplace × post- COVID Observations <sup>1</sup>	0.007 (0.023) 5995	0.030* (0.016) 11837	0.040*** (0.014) 16926	0.042*** (0.016) 7537	0.061*** (0.021) 7621	0.041*** (0.013) 17561
<i>Panel (c): Works in pre-COVID job</i>						
Unionised workplace × post- COVID Observations <sup>1</sup>	-0.002 (0.023) 5995	0.025 (0.017) 11837	0.034** (0.014) 16926	0.035** (0.016) 7537	0.044** (0.021) 7621	0.039*** (0.013) 17561
<i>Panel (d): Employed</i>						
Unionised workplace × post- COVID Observations <sup>1</sup>	0.008 (0.014) 5995	0.015 (0.010) 11837	0.012 (0.008) 16926	0.008 (0.008) 7537	0.028* (0.016) 7621	0.005 (0.009) 17561
<i>Panel (e): Weekly working hours</i>						
Unionised workplace × post- COVID Observations <sup>1</sup>	0.902** (0.400) 5553	-0.055 (0.292) 11217	0.183 (0.241) 16026	0.028 (0.295) 7099	0.820** (0.354) 6767	-0.393* (0.231) 16176
<i>Panel (f): Ln(monthly gross labour income)</i>						
Unionised workplace × post- COVID Observations <sup>1</sup>	-0.012 (0.033) 5710	-0.001 (0.016) 11474	0.010 (0.014) 16401	-0.030 (0.023) 7291	0.003 (0.025) 6963	-0.013 (0.016) 16604
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-COVID tenure × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-COVID industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-COVID occupation × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Region × year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Coefficients, standard errors adjusted for clustering at the individual level in parentheses. \*/\*\*/\*\* denote statistical significance on the 10%, 5% and 1% level respectively. <sup>1</sup> Observations are effective sample sizes excluding singleton observations for the respective set of fixed effects.

**Table 6: Heterogeneity III: Pre-COVID workplace size and private vs. other employers**

	(1)	(2)	(3)	(4)	(5)
	Pre-COVID workplace size			Private vs. other companies	
	<50 employees	50 – 499 employees	500+ employees	Private	Other (public, charities)
<i>Panel (a): Works for pre-COVID employer</i>					
Unionised workplace × post-COVID Observations <sup>1</sup>	0.051*** (0.013) 18371	0.028** (0.013) 14700	0.054*** (0.019) 9626	0.030*** (0.010) 27168	0.047*** (0.015) 16294
<i>Panel (b): Works at pre-COVID workplace</i>					
Unionised workplace × post-COVID Observations <sup>1</sup>	0.056*** (0.014) 18371	0.027* (0.014) 14700	0.069*** (0.021) 9626	0.033*** (0.011) 27168	0.049*** (0.016) 16294
<i>Panel (c): Works in pre-COVID job</i>					
Unionised workplace × post-COVID Observations <sup>1</sup>	0.044*** (0.014) 18371	0.021 (0.014) 14700	0.062*** (0.021) 9626	0.025*** (0.011) 27168	0.033*** (0.016) 16294
<i>Panel (d): Employed</i>					
Unionised workplace × post-COVID Observations <sup>1</sup>	0.018* (0.010) 18371	0.007 (0.009) 14700	0.008 (0.013) 9626	0.007 (0.007) 27168	0.023*** (0.011) 16294
<i>Panel (e): Weekly working hours</i>					
Unionised workplace × post-COVID Observations <sup>1</sup>	0.272 (0.253) 16252	-0.224 (0.252) 13395	0.254 (0.348) 8940	0.197 (0.191) 24265	-0.353 (0.291) 14899
<i>Panel (f): Ln(monthly gross labour income)</i>					
Unionised workplace × post-COVID Observations <sup>1</sup>	0.013 (0.017) 16864	0.003 (0.015) 13795	-0.018 (0.021) 9119	0.004 (0.013) 25122	-0.018 (0.018) 15303
Individual FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID tenure × year FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID industry × year FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID occupation × year FE	Yes	Yes	Yes	Yes	Yes
Region × year FE	Yes	Yes	Yes	Yes	Yes

*Notes:* Coefficients, standard errors adjusted for clustering at the individual level in parentheses. \*\*\*/\*\*/\* denote statistical significance on the 10%, 5% and 1% level respectively. <sup>1</sup> Observations are effective sample sizes excluding singleton observations for the respective set of fixed effects.

**Table 7: Job-related expectations**

	(1)	(2)	(3)	(4)	(5)
	Current financial situation			Expects worse	Job
	Comfortable	Doing alright	Quite/very difficult	financial situation	satisfaction
Unionised workplace × post-COVID	-0.000 (0.008)	0.007 (0.011)	-0.019*** (0.005)	0.004 (0.007)	0.003 (0.027)
Observations <sup>1</sup>	43517	43517	43517	43517	40084
Individual FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID tenure × year FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID industry × year FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID occupation × year FE	Yes	Yes	Yes	Yes	Yes
Region × year FE	Yes	Yes	Yes	Yes	Yes

*Notes:* Coefficients, standard errors adjusted for clustering at the individual level in parentheses. \*/\*\*/\*\* denote statistical significance on the 10%, 5% and 1% level respectively. <sup>1</sup> Observations are effective sample sizes excluding singleton observations for the respective set of fixed effects.

**Table 8: Health outcomes**

	(1)	(2)	(3)	(4)	(5)
<i>Panel (a): Self-rated health and health conditions</i>					
	Self-rated health				Any health condition
	Excellent	Good	Fair	Poor	
Unionised workplace × post-COVID	-0.001 (0.006)	-0.001 (0.010)	-0.000 (0.007)	0.004 (0.003)	-0.004 (0.008)
Observations <sup>1</sup>	43517	43517	43517	43517	43517
<i>Panel (b): Mental health and satisfaction</i>					
	GHQ			Satisfaction with	
	Caseness	Linear	Life	Health	Income
Unionised workplace × post-COVID	0.009 (0.009)	0.063 (0.104)	0.013 (0.025)	-0.021 (0.029)	0.013 (0.028)
Observations <sup>1</sup>	42295	42295	42516	42537	42520
Individual FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID tenure × year FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID industry × year FE	Yes	Yes	Yes	Yes	Yes
Pre-COVID occupation × year FE	Yes	Yes	Yes	Yes	Yes
Region × year FE	Yes	Yes	Yes	Yes	Yes

*Notes:* Coefficients, standard errors adjusted for clustering at the individual level in parentheses. \*/\*\*/\*\* denote statistical significance on the 10%, 5% and 1% level respectively. <sup>1</sup> Observations are effective sample sizes excluding singleton observations for the respective set of fixed effects.



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