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The Impact of the Pandemic on Health-Related Inactivity and Benefit Claims

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Abstract

This paper examines whether the post-pandemic rise in health-related economic inactivity and benefit receipt in the United Kingdom was disproportionately concentrated among working-age adults classified as clinically vulnerable to COVID-19. Using longitudinal data from the UK Household Longitudinal Study covering 2009–2024, linked to the Understanding Society COVID-19 Study, we treat pre-existing clinical vulnerability as a marker of differential exposure to the health and behavioural consequences of the pandemic. We combine coarsened exact matching with pooled pre/post and two-way fixed-effects difference-in-differences specifications; a more demanding triple-differences design is used as an exploratory robustness check. The main difference-in-differences estimates indicate that clinically vulnerable individuals were 1.3–2.1 percentage points more likely to become inactive because of long-term sickness after 2020. Relative to pre-pandemic rates of around 5–6 per cent, this corresponds to an increase of roughly 20–40 per cent. The triple-differences estimates are positive but imprecisely estimated. We find no robust evidence of disproportionate increases in disability or incapacity benefit receipt. The results therefore suggest a persistent relative deterioration in labour-market participation among clinically vulnerable individuals, while the weaker relationship with benefit receipt is consistent with an important role for institutional processes and programme design.

JEL classification

J21, J14, I18

Keywords

economic inactivity, health, COVID-19, disability benefits

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1 INTRODUCTION

The COVID-19 pandemic generated simultaneous shocks to labour demand and labour supply across advanced economies. Mandated closures and abrupt changes in consumption caused large and uneven employment losses, disproportionately affecting younger, lower-paid and less securely employed workers (Adams-Prassl et al. (2020); Breunig et al. (2024)). Labour supply also fell as workers sought to avoid infection, provided care during school and care-home closures, or experienced deteriorating health. The disruption varied systematically with age, pre-existing health, education, occupation and the capacity to work remotely (Crossley et al. 2023). Clinically vulnerable individualsthose with conditions associated with a higher risk of severe COVID-19-were especially exposed (Abraham and Rendell (2023); Barrero et al. (2023)).

While employment recovered rapidly in most G7 economies, the United Kingdom followed a distinctive trajectory.[1] Since 2020, the UK has experienced a sustained rise in economic inactivity associated with long-term sickness. At the same time, disability- and incapacity-related benefit claims have risen by more than 50 per cent relative to pre-pandemic levels (DWP 2025). This reversal of the previous downward trend in health-related inactivity has raised concerns about persistent labour-supply scarring and about the interaction between health, social security and macroeconomic performance.

This paper asks whether the medium-run rise in health-related inactivity and benefit receipt was disproportionately concentrated among people classified as clinically vulnerable to COVID-19. The classification does not identify infection, long COVID or any single mechanism. Rather, it identifies a group that, because of pre-existing health characteristics and the associated official guidance, faced greater direct health risks and stronger incentives to shield or otherwise modify behaviour. Our estimand is therefore a *relative post-pandemic effect*: the change in outcomes among clinically vulnerable individuals compared with the change among otherwise comparable individuals classified as low risk.

[1] Amer-Mestre and Serrano-Alarcon (2025) nevertheless document a widespread increase in compensated absence from work because of illness across a number of countries.

We use longitudinal microdata from the UK Household Longitudinal Study (UKHLS), including observations through 2024, linked to the Understanding Society COVID-19 Study conducted between April 2020 and September 2021. Our empirical strategy combines coarsened exact matching (CEM) with pooled pre/post and two-way fixed-effects difference-in-differences (DiD) estimators. We also estimate an aligned-period DiD, comparing changes across two seven-year windows, and a triple-differences (DDD) specification that additionally differentiates by clinical vulnerability. Because the pandemic affected the comparison group as well as the clinically vulnerable group, the estimates should be interpreted as relative rather than total effects. Common adverse shocks to the low-risk group could attenuate the estimated difference, altlhough this is not guaranteed and we do not interpret the coefficients as formal lower bounds.

The paper makes two principal contributions. First, it extends existing UK evidence by examining a clinically defined measure of vulnerability, medium-run labour-market outcomes through 2024, and both disability and incapacity benefit receipt. Second, it illustrates the use of relative-exposure DiD designs in a setting where no population group was literally untreated, while also showing the limitations of more demanding aligned-period and DDD comparisons.

The papers closest to ours also use UKHLS data to examine labour-market behaviour during the pandemic. Richardson (2025) and Webb et al. (2025) focus on specific pre-existing health conditions, whereas we use the NHS-based classification of clinical vulnerability. This measure aggregates information about the risk of severe COVID-19 and was salient to respondents because vulnerable individuals received explicit guidance about their risk. Richardson (2025) finds that each additional pre-existing condition reduced the probability of working positive hours by 2-3 percentage points. Webb et al. (2025), using an explicit causal identification strategy, find that most long-term conditions were associated with lower employment or hours during the COVID-19 period but not before it. We extend this literature by focusing on outcomes after the acute phase of the pandemic and by combining matching with several DiD specifications.

A broader literature documents that adverse health shocks reduce labour supply through lower employment, increased sickness absence and earlier retirement (Bound and Burkhauser (1999); Currie and Madrian (1999); Lerner et al. (2004); Evans-Lacko et al. (2013); Trevisan and Zantomio (2016)). Estimating the effect of health on employment is complicated by measurement error, reverse causality and the influence of local labour-market conditions (Britton and French 2020). In the UK, health-related inactivity has long been concentrated in former industrial areas, where poor health, entrenched disadvantage and limited suitable employment reinforce one another (Beatty and Fothergill 2023). Reforms to incapacity benefits, changes in their value relative to unemployment-related support and tighter conditionality elsewhere in the benefit system have also altered incentives, although net flows onto and off health-related benefits changed only modestly and unevenly before the pandemic (Banks et al. (2015); Britton and French (2020); OBR (2024)).

The pandemic introduced additional channels linking health and employment. Post-COVID-19 condition can affect productivity and work capacity through fatigue, respiratory and musculoskeletal symptoms, sleep disturbance, cognitive impairment and mental-health problems (WHO 2025). Its prevalence is associated with sex, age, pre-existing conditions, poverty and education (Blanchflower and Bryson (2023); Calvo Ramos et al. (2024); Heald et al. (2025)); infection risk itself also varied with deprivation and occupation (Corradini et al. 2024). More generally, deteriorating mental health, disrupted healthcare and longer waits for diagnosis and treatment may have worsened disability and sickness absence (Banks and Xu (2020); Pierce et al. (2021); Fetzer and Rauh (2022); King's Fund (2024); Propper et al. (2020); NHS England (2024)). Dennett et al. (2025) provide evidence of persistent effects of COVID-related work absences on labour-force exits in the United States.[2] By contrast, Warner and Zaranko (2025) find no evidence that NHS waiting lists explain the rise in incapacity or disability benefit claims, although they do not examine economic inactivity and lack data on access to general practice.

[2] See also Amer-Mestre and Serrano-Alarcon (2025), who find that musculoskeletal and mental-health conditions predict increases in long-term sickness absence.

Disability insurance and related programmes provide protection against earnings risk but can affect labour-supply decisions (Autor and Duggan (2003); Autor and Duggan (2006); Maestas et al. (2013); Kostøl and Mogstad (2014)). If the pandemic impaired health, one might expect both lower employment and higher benefit receipt. The two outcomes need not move together, however, because eligibility rules, assessments, administrative delays and labour-market conditions mediate the relationship between health and claims. Some older workers may also have re-optimised the leisure-income trade-off following furlough and changing perceptions of risk (Boileau and Cribb 2022), although this cannot account for the full age and regional pattern of rising inactivity.

We test two primary hypotheses. H1 is that individuals classified as clinically vulnerable experienced larger post-2020 increases in inactivity due to long-term sickness than comparable low-risk individuals. H2 is that they also experienced larger increases in disability and/or incapacity benefit receipt.

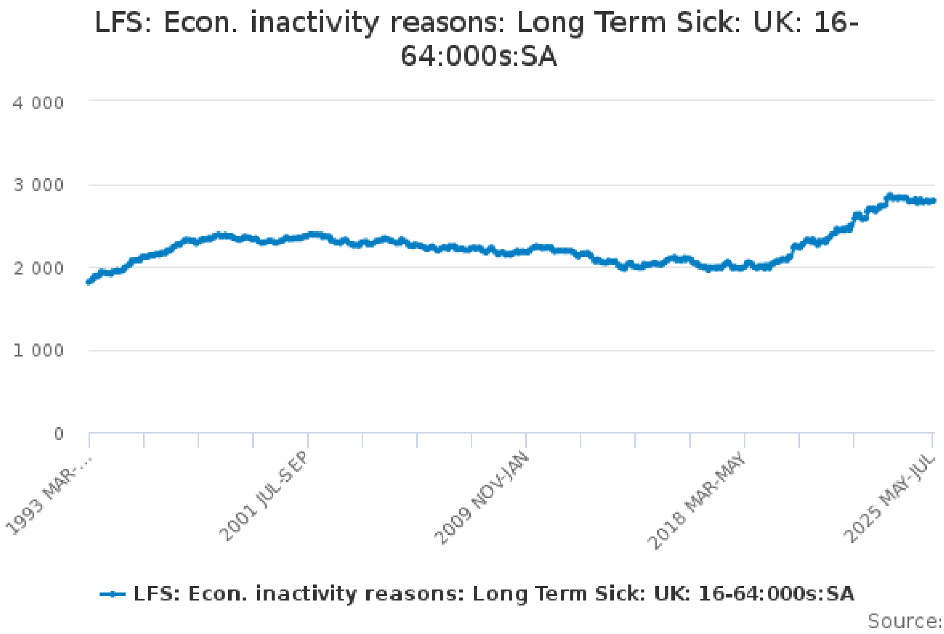
The remainder of the paper proceeds as follows. Section 2 summarises post-pandemic labour-market and institutional developments. Section 3 describes the data and variable construction. Section 4 sets out the empirical strategy. Section 5 presents the results. Section 6 discusses their interpretation, limitations and policy implications.

2 BACKGROUND: THE UK LABOUR MARKET AFTER THE PANDEMIC

Before 2020, economic inactivity in the UK had been trending downward, reflecting improvements in population health, rising educational attainment, greater female labour force participation, and disability benefit reforms (Banks et al. 2015). The pandemic precipitated a significant reversal, although data issues (in particular affecting the Labour Force Survey) make it difficult to quantify the precise impacts and timing (ONS 2025). By early 2024, the number of working-age adults inactive due to long-term sickness reached over 2.8 million as shown in Figure 1-the highest figure since comparable records began

(ONS 2025). Alternative estimates that attempt to correct for the difficulties in interpreting the LFS data also show a sharp rise post-pandemic, albeit of slightly smaller magnitude.

Figure 1



Source: ONS

This was despite a very strong recovery in the overall labour market after the pandemic, with labour shortages widely reported across a number of sectors, especially health and hospitality. In this respect, the UK experience was similar to that of other advanced economies, as illustrated by Figure 2. If anything, in some sectors shortages were further aggravated by Brexit (which meant that freedom of movement with EU member states ended in January 2021). However, the employment rate among the UK-born fell by about 1.5 percentage points compared to the pre-pandemic period, an effect partly offset in headline aggregates by strong post-pandemic immigration concentrated among high-participation groups, as immigration rules for non-EU origin migrants were loosened to address labour shortages and to compensate for the impact of Brexit (HMRC 2025). In contrast, inactivity fell in most other G7 economies; Germany's inactivity rate, for example, declined by roughly two percentage points (OECD 2025). This relative under-

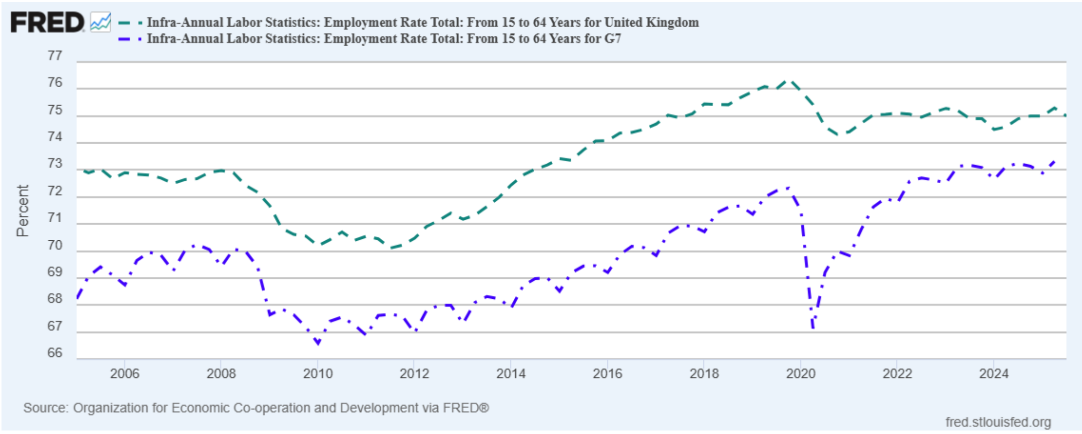


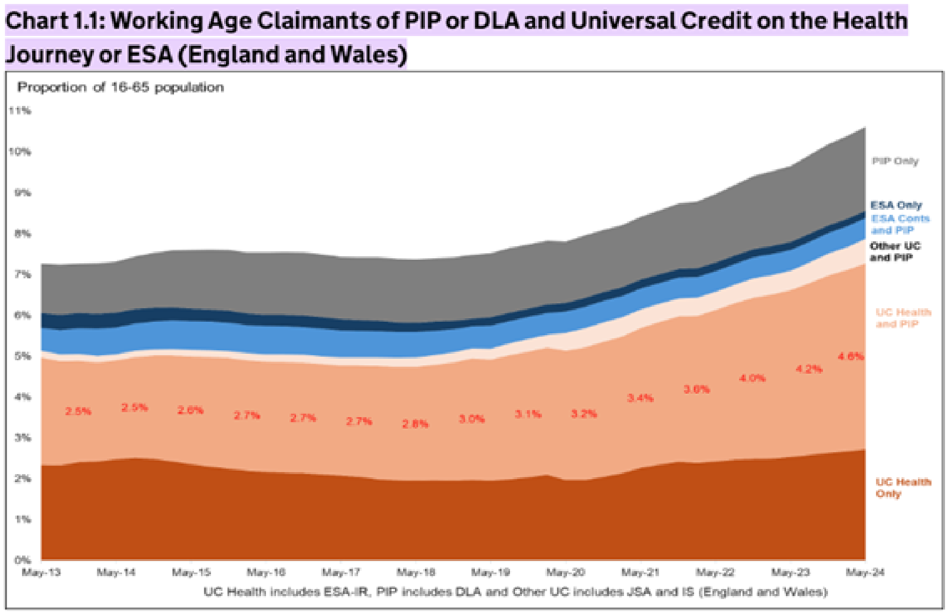
Figure 2
UK and G7 employment rates, 2005–2025

performance has persisted: the UK's employment rate, which was about 4 percentage points above the G7 average in 2019, is now just 2 percentage points higher, the lowest gap on record.

The UK welfare system mediates the relationship between health, employment and benefit entitlement primarily through two programmes. Incapacity benefits – historically Invalidity Benefit and ESA, now largely integrated as Limited Capability for Work elements within Universal Credit (UC) – support those not working who, via the Work Capability Assessment, are deemed to have limited or no capacity to work. Disability benefits – Disability Living Allowance (DLA) and its successor Personal Independence Payment (PIP) – provide non-means-tested transfers to offset the additional costs of disability and are awarded based on functional limitations, regardless of employment status. Individuals may receive either or both benefits; the mapping between labour market status and benefit receipt is complex because eligibility, assessment outcomes, and means-testing interact, so this summary is something of an oversimplification.

Since 2020, claims for health-related benefits have risen substantially, while other claims-in particular, non-health-related claims for UC - have been relatively flat, although there has been a slight rise since 2023 as the labour market has weakened. Approximately 4.2 million working-age adults – about 10% of the population – now receive at least one such benefit, more than 50% above pre-pandemic levels (DWP 2025). Growth has been broad-based but especially pronounced for PIP among younger cohorts, with mental health and learning disabilities comprising large shares of new awards; in levels, however, most claimants remain aged 40–64. Incapacity claims reflect a mix of long-standing musculoskeletal conditions and growing mental health issues. As well as the direct and indirect impacts of the pandemic on health, potential explanations for this rapid growth include the financial and institutional incentives described above that make incapacity and disability benefits more attractive relative to unemployment benefits, as well as the move to online-only assessment for most claimants (Latimer et al. 2024). These patterns raise classic questions in the disability insurance literature about the roles of underlying health, labour market conditions, and programme design (Autor and Duggan (2003); Autor and Duggan (2006); Bound and Burkhauser (1999)).

Figure 3



Source: DWP (2025)

3 DATA

We use the UK Household Longitudinal Study (UKHLS), covering 2009–2024, linked to the Understanding Society COVID-19 Study conducted between April 2020 and September 2021. UKHLS is a stratified, multi-stage probability survey that follows households and individuals over time and records detailed information on employment, income, health and demographic characteristics. The COVID-19 Study collected additional high-frequency information on shielding, clinical vulnerability and other pandemic-specific experiences. Linking the two sources allows us to observe labour-market and benefit outcomes before, during and after the acute phase of the pandemic. The principal clinical-vulnerability specification uses data through 2024. The aligned-period designs retain the equal seven-year windows 2010–2016 and 2017–2023.

Our sample includes individuals aged 18–65 who are observed at least once before 2020 and at least once afterwards. We exclude retirees, full-time students, those on parental leave and those recorded as inactive for reasons unrelated to health. UKHLS longitudinal weights are used to adjust for the survey design and observed attrition, and interview months are aligned to calendar years. The estimation samples differ across outcomes because the relevant benefit variables are not available or reported for every respondent in every wave.

We construct three binary outcome measures. First, inactivity due to long-term sickness or disability is defined using the self-reported labour-market status variable (*jbstat*, category 8), excluding retirement and other forms of non-health inactivity. Second, disability benefit receipt is defined as reported income from Personal Independence Payment (PIP) or Disability Living Allowance (DLA), using variables *nfh41*, *pbnft14*, *bendis5*, *nfh10* and *pbnft3*. Third, incapacity benefit receipt is defined as either receipt of Universal Credit (*benbase4*) alongside reported long-term sickness or disability, or receipt of Employment and Support Allowance (*bendis2*).

These measures are self-reported and are subject to measurement error. For example, otherwise similar older respondents may describe themselves as retired or as long-term sick, while the available Universal Credit variables do not identify the health element perfectly. Some respondents receiving a health-related UC element may not report themselves as long-term sick, and the converse may also occur. Such errors may attenuate the estimates if they are approximately unrelated to treatment and time, but this cannot be assumed in all cases.

The main treatment indicator uses the clinical-vulnerability classification (*clinvulndv*) collected in the COVID-19 Study. The classification follows NHS guidance and is based on pre-existing conditions associated with a higher risk of severe illness, including conditions that led some individuals to be advised to shield. We classify respondents at *medium or high risk* as the treatment group and respondents at *low risk* as the control group. For the longitudinal analysis, this classification is treated as a time-invariant marker of relative pandemic vulnerability and is linked to the respondent's observations before and after 2020. In our data, approximately a quarter of the observations are recorded as at medium or high risk.

Clinically vulnerable respondents are older, more likely to be female and in worse baseline health than low-risk respondents. The matching procedure described below is intended to improve overlap on these observed characteristics; it does not eliminate possible differences in unobserved, time-varying determinants of the outcomes.

4 EMPIRICAL STRATEGY

Our empirical analysis uses three complementary designs. Model 1 compares changes after 2020 between clinically vulnerable and low-risk respondents. Model 2 compares the late-period change in a seven-year window spanning the pandemic with the corresponding change in an earlier seven-year window. Model 3 combines the two comparisons in a triple-differences specification. Models 1 and 2 provide the principal estimates; Model 3 imposes a more demanding identifying assumption and is treated as an exploratory robustness exercise. All outcomes are binary and the reported specifications are linear probability models. Standard errors are clustered at the individual level, and longitudinal survey weights are combined with the CEM weights described below.

4.1 Identification and conceptual framework

COVID-19 and the resulting economic and social disruptions affected the entire population, so there is no literally untreated group. The strategy instead exploits *relative exposure*: respondents classified as clinically vulnerable were, ex ante, more likely to experience serious health consequences and to alter their behaviour in response to the pandemic. The treatment indicator therefore captures differential exposure associated with clinical vulnerability rather than infection itself.

The principal identifying assumption for Model 1 is that, in the absence of the pandemic, the outcomes of clinically vulnerable and low-risk respondents would have followed parallel trends after conditioning on the matching variables and fixed effects. The dynamic profiles provide a diagnostic for this assumption, but cannot prove it. Because low-risk respondents were also affected by infection, healthcare disruption and macroeconomic shocks, the coefficients estimate the widening of the gap between risk groups rather than the total effect of the pandemic on either group. If, as seems plausible, non-vulnerable groups also experienced adverse shocks – through COVID infection, healthcare disruption, or macroeconomic headwinds – our relative DiD and DDD coefficients will be attenuated toward zero, although the direction of any bias is not necessarily certain.

In the two-way fixed-effects (TWFE) specifications, individual fixed effects absorb time-invariant respondent characteristics and period fixed effects absorb shocks common to all respondents. The pooled pre/post specifications do not include individual fixed effects and are therefore more exposed to persistent differences between the treatment and control groups.

4.2 Matching and sample construction

To improve covariate overlap and reduce model dependence, we apply Coarsened Exact Matching (CEM) before estimation (Iacus et al. 2012). We match on age, sex, education, ethnicity, baseline health and baseline labour-market status and, where feasible, on the baseline value of the relevant outcome. CEM places covariates into substantively defined bins and removes observations outside common support. The resulting CEM weights are multiplied by the UKHLS longitudinal weights. Matching improves comparability on the included observables but althought it does not address unobserved time-varying differences or substitute for the parallel-trends assumptions.

4.3 Model 1: Clinical-vulnerability difference-in-differences

Model 1 uses the UKHLS COVID-19 Study classification of clinical vulnerability. Respondents at *medium or high risk* form the treatment group, and respondents at *low risk* form the control group. The classification is based on health characteristics that largely pre-date the post-pandemic outcomes, but it is not randomly assigned. The causal interpretation therefore depends on the identifying assumptions rather than on the classification being exogenous in a strict sense. Prior to matching, we have 11,233 observations, of which 2,941 are high/medium risk (and hence matched with one of the low risk observations).

For the main period, ending in 2024, we estimate the following TWFE specification:

$$Y_{it} = \alpha_i + \lambda_t + \delta(V_i \times Post_t) + \varepsilon_{it}, \tag{1}$$

where V_i equals one for respondents at medium or high clinical risk, $Post_t$ indicates the post-2020 period, α_i denotes individual fixed effects and λ_t denotes year fixed effects. The coefficient δ measures the differential post-pandemic change among clinically vulnerable respondents relative to low-risk respondents. It combines any direct health effects with indirect channels such as shielding, healthcare disruption and risk-avoidant labour-supply behaviour; these mechanisms cannot be separately identified.

We also estimate a pooled pre/post specification:

$$Y_{it} = \alpha + b_1 V_i + b_2 Post_t + b_3 (V_i \times Post_t) + \varepsilon_{it}, \tag{2}$$

where b_3 is the coefficient of interest. Dynamic versions replace the single interaction with interactions for individual years. The plotted group trajectories allow us to assess whether there is clear evidence of differential movement before 2020 and to examine the timing of any subsequent divergence; they are not conventional event-study coefficient plots relative to an omitted base year.

4.4 Model 2: Aligned-period difference-in-differences

Model 2 provides an alternative comparison based on two seven-year calendar windows: 2010-2016 and 2017-2023. Each window is re-indexed from period 0 to period 6. The first four periods form the pre-period and the final three periods form the post-period. Thus the post-period is 2014-2016 in the earlier window and 2021-2023 in the later window. The earlier window is not a group of people who were permanently unexposed to COVID-19; it is an earlier calendar-period comparison intended to capture the outcome change that occurred at the corresponding point of a seven-year window in the absence of the pandemic.

Let D_i indicate membership of the later aligned-period cohort and let $Post_s$ indicate the final three re-indexed periods. The TWFE specification is:

$$Y_{is} = \alpha_i + \lambda_s + \beta(D_i \times Post_s) + \varepsilon_{is}, \tag{3}$$

where λ_s are aligned-period fixed effects. The coefficient β measures whether the change between the first four and final three periods was larger in the 2017–2023 window than in the 2010–2016 window. A dynamic version replaces the post interaction with a set of interactions by aligned period.

The corresponding pooled pre/post model is:

$$Y_{is} = \alpha + b_1 D_i + b_2 Post_s + b_3 (D_i \times Post_s) + \varepsilon_{is}, \tag{4}$$

where b_3 is the coefficient of interest. Identification requires that, absent the pandemic, the within-window change would have been comparable across the two calendar windows after conditioning on the included controls and weights.

4.5 Model 3: Triple-differences specification

Model 3 combines the aligned-period and clinical-vulnerability comparisons. It differences across aligned time (the first four versus final three periods), calendar window (2017-2023 versus 2010-2016), and clinical vulnerability (medium/high versus low risk). The pooled DDD specification is:

$$\begin{aligned} Y_{is} = \alpha &+ b_1 D_i + b_2 Post_s + b_3 V_i + b_4 (D_i \times Post_s) + b_5 (D_i \times V_i) \\ &+ b_6 (V_i \times Post_s) + b_7 (D_i \times V_i \times Post_s) + \varepsilon_{is}. \end{aligned} \tag{5}$$

The TWFE version includes the lower-order time-varying interactions:

$$\begin{aligned} Y_{is} = \alpha_i + \lambda_s &+ \beta_1 (D_i \times Post_s) + \beta_2 (V_i \times Post_s) \\ &+ \delta (D_i \times V_i \times Post_s) + \varepsilon_{is}. \end{aligned} \tag{6}$$

The coefficient δ asks whether the medium/high-risk versus low-risk gap widened more in the later calendar window than at the corresponding point of the earlier window. This design does not relax the parallel-trends requirement; it replaces it with the stronger assumption that, without the pandemic, the evolution of the risk-group gap would have been comparable across the two aligned windows. Because clinical vulnerability is measured in the COVID-19 Study and then applied retrospectively to much earlier observations, this assumption may be particularly demanding. We therefore regard Model 3 as informative but exploratory.

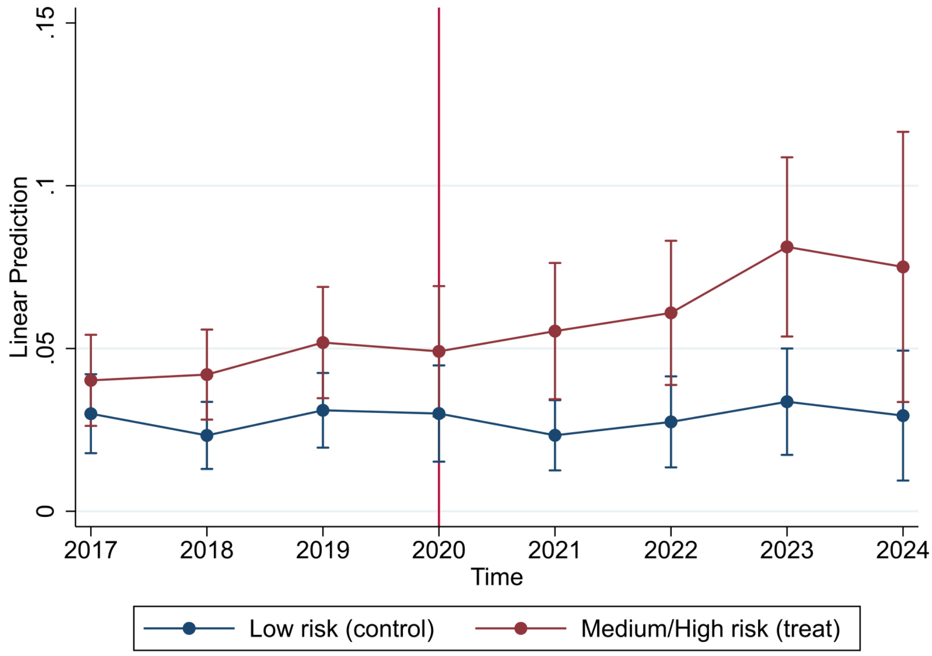
5 RESULTS

5.1 Inactivity due to long-term sickness

Inactivity due to long-term sickness rose after 2020, with a larger increase among clinically vulnerable respondents. In Model 1, the pooled pre/post estimate is 2.1 percentage points and the TWFE estimate is 1.3 percentage points; both are statistically significant at the 5 per cent level. Model 2 produces smaller positive estimates of 0.7 percentage points in the pooled specification and 0.5 percentage points in the TWFE specification, significant at the 10 and 5 per cent levels respectively. The dynamic group profiles show no clear pre-2020 divergence and a widening gap thereafter, although the confidence intervals are sufficiently wide that the parallel-trends assumption should be regarded as supported rather than confirmed.

These estimates are modest in absolute terms but material relative to the pre-pandemic prevalence of health-related inactivity. Against a baseline of around 5–6 per cent, the Model 1 estimates of 1.3–2.1 percentage points correspond to a relative increase of roughly 20–40 per cent among the clinically vulnerable group. This magnitude is quantitatively significant relative to the aggregate rise in UK inactivity, but the paper does not provide a population-level decomposition and therefore cannot directly quantify the share of the aggregate increase or of the UK–G7 divergence explained by this mechanism.

Figure 4
Model 1: inactivity due to long-term sickness.



Notes: Treatment group: respondents at medium or high clinical risk. Control group: respondents at low clinical risk.

Table 1
Long-term sickness.

	Model 1 (DiD)		Model 2 (DiD)	
	Pooled	TWFE	Pooled	TWFE
	(1)	(2)	(3)	(4)
Estimate	0.021**	0.013**	0.007*	0.005**
	(0.009)	(0.006)	(0.004)	(0.002)
R^2	0.005	0.782	0.001	0.812
Observations	19,987	13,792	76,276	53,815

Notes: Linear probability models. Standard errors clustered at the individual level. Longitudinal and CEM weights are used. In Model 1, treatment is medium/high clinical risk and control is low clinical risk. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

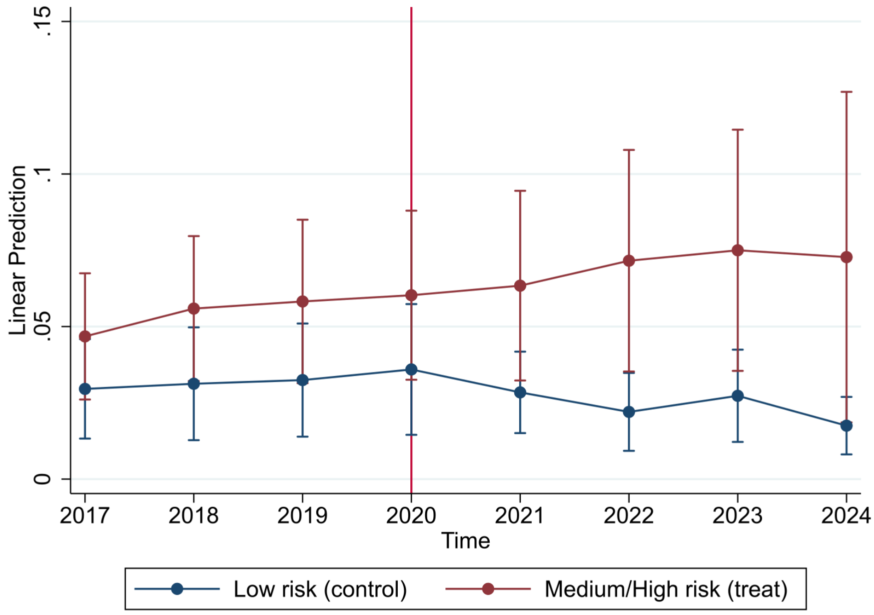
5.2 Disability benefits

For disability benefits (PIP/DLA), we find no statistically robust evidence of a disproportionate increase among clinically vulnerable respondents. The largest estimate is the

2.2 percentage-point coefficient in the pooled Model 1 specification, but its confidence interval includes zero ($p = 0.11$). The remaining point estimates are positive but smaller and are also statistically insignificant. The group trajectories are broadly flat before 2020 and show only tentative later divergence.

This pattern contrasts with the large aggregate rise in PIP claims after 2020. It is consistent with several possible explanations: administrative processes and application surges may have affected both risk groups; PIP and DLA are available to people both in and out of work; and awards depend on assessed functional limitations rather than labour-market status. The estimates do not identify which of these mechanisms is responsible, but they show that aggregate caseload growth was not clearly concentrated among the medium/high clinical-risk group relative to the low-risk group.

Figure 5
Model 1: disability benefits.



Notes: Treatment group: respondents at medium or high clinical risk. Control group: respondents at low clinical risk. Where the embedded legend groups medium risk with the control group, the legend is incorrect and this note gives the classification used in the estimates.

Table 2
Disability benefits.

	Model 1 (DiD)		Model 2 (DiD)	
	(1)	(2)	(3)	(4)
Method	Pooled	TWFE	Pooled	TWFE
Estimate	0.022	0.014	0.016	0.004
	(0.014)	(0.009)	(0.013)	(0.009)
R^2	0.007	0.788	0.001	0.828
Observations	12,881	9,328	20,484	13,817

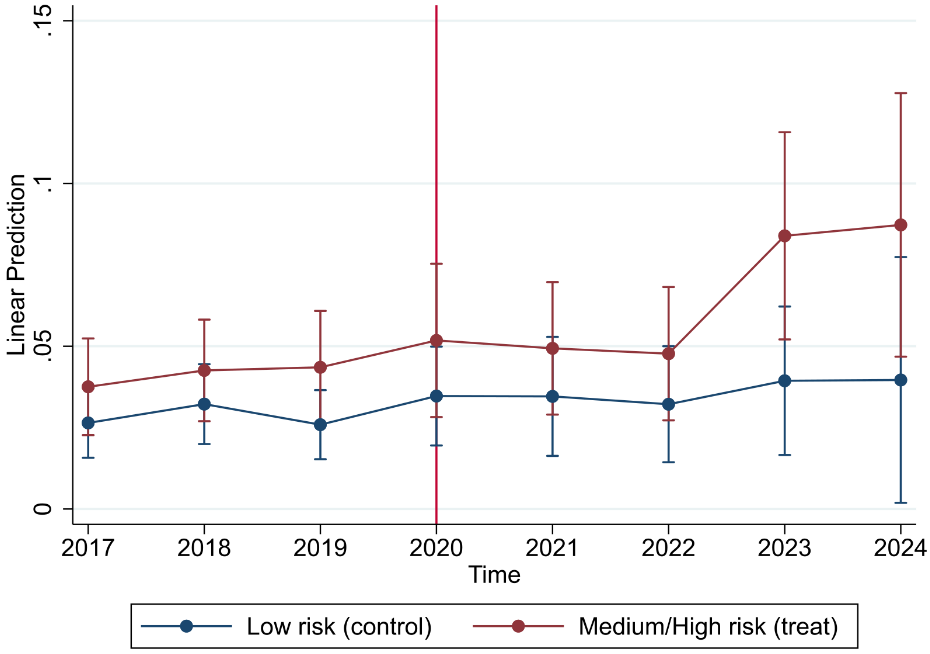
Notes: Linear probability models. Standard errors clustered at the individual level. Longitudinal and CEM weights are used. In Model 1, treatment is medium/high clinical risk and control is low clinical risk. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.3 Incapacity benefits

The incapacity-benefit estimates, covering ESA and the UC health-related measure, are also positive but statistically insignificant in all four principal specifications. The results therefore do not show that the aggregate increase in incapacity claims was disproportionately concentrated among clinically vulnerable respondents. Caseload growth may instead reflect a broader combination of health deterioration, labour-market conditions, financial incentives, assessment outcomes, policy changes and application behaviour.

The plotted trajectories suggest a possible divergence in 2023 and 2024, but the confidence intervals are wide and the formal estimates remain insignificant. Any delayed response would be institutionally plausible because claims can involve application, medical assessment and, in some cases, reconsideration or appeal. Given the imperfect coding of the UC health element and the absence of statistically robust estimates, this pattern should be treated as suggestive rather than as evidence of a lagged causal effect.

Figure 6
Model 1: incapacity benefits.



Notes: Treatment group: respondents at medium or high clinical risk. Control group: respondents at low clinical risk.

Table 3
Incapacity benefits.

	Model 1 (DiD)		Model 2 (DiD)	
	(1)	(2)	(3)	(4)
Method	Pooled	TWFE	Pooled	TWFE
Estimate	0.013	0.005	0.015	0.006
	(0.010)	(0.007)	(0.011)	(0.008)
R^2	0.003	0.751	0.003	0.726
Observations	20,419	13,968	19,224	12,969

Notes: Linear probability models. Standard errors clustered at the individual level. Longitudinal and CEM weights are used. In Model 1, treatment is medium/high clinical risk and control is low clinical risk. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.4 Triple-differences estimates

The Model 3 estimates, reported in Table A.1 in the Online Appendix, are positive for long-term sickness but small and statistically insignificant: 0.4 percentage points in the pooled DDD and 0.2 percentage points in the TWFE DDD. The estimates for disability and incapacity benefits are also statistically insignificant. These results do not contradict the main DiD estimates, but they provide less support for a precise causal interpretation. The DDD sample and specification are demanding, and the retrospective use of a COVID-era clinical-risk classification for the earlier aligned period may weaken the comparability required by the design.

5.5 Education as an alternative proxy for vulnerability

As a robustness exercise, we examine whether respondents with lower educational attainment experienced larger post-pandemic changes in the outcomes. Individuals aged 25 and over with qualifications below GCSE level form the *treatment group*; respondents with higher qualifications form the *control group*. The pooled estimates show a qualitatively similar increase in inactivity due to long-term sickness, but the TWFE estimates are smaller and generally insignificant. The disability- and incapacity-benefit results are also mixed. The exercise therefore provides some corroborative evidence but does not independently establish the clinical-vulnerability mechanism.

Education captures several overlapping dimensions of disadvantage. Respondents with lower qualifications tend to have poorer baseline health, less secure employment and weaker occupational mobility, and may face higher replacement rates when earnings are low. They are also more likely to experience long COVID (Blanchflower and Bryson 2023). The education results should therefore be interpreted as evidence about the interaction of health and socioeconomic disadvantage, rather than as a clean alternative measure of clinical risk.

6 DISCUSSION

The principal finding is that the post-2020 increase in inactivity due to long-term sickness was larger among respondents classified as clinically vulnerable than among comparable low-risk respondents. The main DiD estimates are between 1.3 and 2.1 percentage points, a substantial increase relative to the pre-pandemic rate for this group. By contrast, we find no statistically robust relative increase in disability or incapacity benefit receipt. The more demanding DDD estimates are positive for long-term sickness but small and imprecisely estimated.

These estimates measure a relative widening of the gap between clinical-risk groups, not the total effect of the pandemic. Low-risk respondents were also exposed to infection, healthcare disruption and the wider economic shock. This may have reduced the observed difference between the groups, but the direction and scale of any resulting bias cannot be established from the design. The evidence is therefore consistent with persistent labour-supply effects among clinically vulnerable individuals, but it does not identify the separate contributions of long COVID, deterioration in pre-existing conditions, delayed healthcare, mental-health problems or behavioural responses to risk.

The UK context may help explain why these health effects translated into sustained inactivity. The country entered the pandemic with pressure on NHS capacity and relatively limited occupational-health provision, and subsequently experienced long waits for elective and mental-health care. Population health was already deteriorating in some groups, while pre-existing conditions also increase the risk of long COVID (Blanchflower and Bryson (2023); Calvo Ramos et al. (2024); Heald et al. (2025)). Labour demand recovered strongly, but shortages were concentrated in sectors such as health and social care, accommodation and hospitality, where work may be particularly difficult for people with limiting health conditions.[3] These mechanisms are plausible interpretations of the results, rather than channels directly tested by our models.

The absence of robust relative effects on benefit receipt is also informative. PIP and DLA are awarded on the basis of functional limitations and do not require claimants to be out of work. Incapacity-related support depends on the Work Capability Assessment and interaction with Universal Credit. Application decisions, administrative delays, as-

[3] Barrero et al. (2023) find that US labour-force participation remained lower in jobs involving more face-to-face contact.

sessment outcomes, reconsideration and appeals can all weaken or delay the mapping from deteriorating health to benefit receipt. The aggregate rise in claims also appears to have been broadly distributed across risk groups. Our findings are therefore consistent with the disability-insurance literature's emphasis on institutions in shaping caseload dynamics (Autor and Duggan (2003); Maestas et al. (2013); Kostøl and Mogstad (2014)), but they do not estimate the effect of any particular programme rule.

Several limitations should be emphasised. First, labour-market status and benefit receipt are self-reported, and the available UC variables identify health-related entitlement imperfectly. Second, participation in the COVID-19 Study and continued observation through 2024 may be related to health and employment in ways that survey weights do not fully correct. Third, the clinical-risk indicator was collected during the pandemic and is applied as a fixed classification to earlier observations; this is particularly restrictive in the aligned-period DDD design. Fourth, the study cannot distinguish among the possible health and institutional mechanisms. Finally, the benefit samples are smaller and the estimates less precise, limiting our ability to detect modest effects or analyse detailed subgroups.

The results suggest three broad policy implications, while not directly evaluating any of them. First, reducing delays in healthcare and rehabilitation may help prevent health limitations from becoming permanent labour-market exits. Second, better integration of occupational health, employment support and the benefit system could make it easier for people with fluctuating or partial work capacity to remain in or return to employment. Third, benefit design must balance access to insurance for people genuinely unable to work against potential labour-supply disincentives (Low and Pistaferri 2020). These implications follow from the combination of our findings and the wider literature rather than from a direct policy experiment in this paper.

Future research should link survey data to administrative health and benefit records, examine regional variation in NHS and GP capacity, and follow vulnerable respondents over a longer period. Such work would help distinguish health deterioration from administrative and labour-market mechanisms and establish how far the UK experience reflects distinctive institutions rather than a common but differently measured post-pandemic health shock.

Overall, the pandemic left a persistent imprint on the UK labour market. Clinically vulnerable respondents experienced a larger rise in long-term-sickness inactivity, but this relative deterioration was not matched by a clearly disproportionate increase in disability or incapacity benefit receipt. The results point to a consequential interaction between health shocks and institutions, while leaving the precise mechanisms open.

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ONLINE APPENDIX

Results for the triple-differences specification (Model 3) are shown below for each outcome.

Table A.1
Model 3 Results

	LT Sick		Incapacity		Disability	
	Pooled	TWFE	Pooled	TWFE	Pooled	TWFE
Estimate	0.004	0.002	0.004	-0.005	0.010	-0.018
	(0.011)	(0.007)	(0.028)	(0.018)	(0.028)	(0.023)
R^2	0.009	0.735	0.008	0.738	0.002	0.795
Observations	44,268	27,287	11,907	7,439	12,842	8,135

Notes: Standard errors clustered at the individual level. Longitudinal and CEM weights are used. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The long-term-sickness estimates are positive—0.4 percentage points in the pooled specification and 0.2 percentage points in the TWFE specification—but statistically insignificant. The incapacity- and disability-benefit estimates are also statistically insignificant. A central limitation of this design is that the clinical-risk classification was measured in the COVID-19 Study and then applied retrospectively to observations in the earlier aligned period. Changes in health status over the intervening years may therefore make the required comparison of risk-group trajectories less stable. For this reason, Model 3 is interpreted as an exploratory robustness exercise rather than as the preferred specification.

Figures for each of the outcomes under Model 2 are shown below.

Figure A.1
Model 2 - long-term sickness.

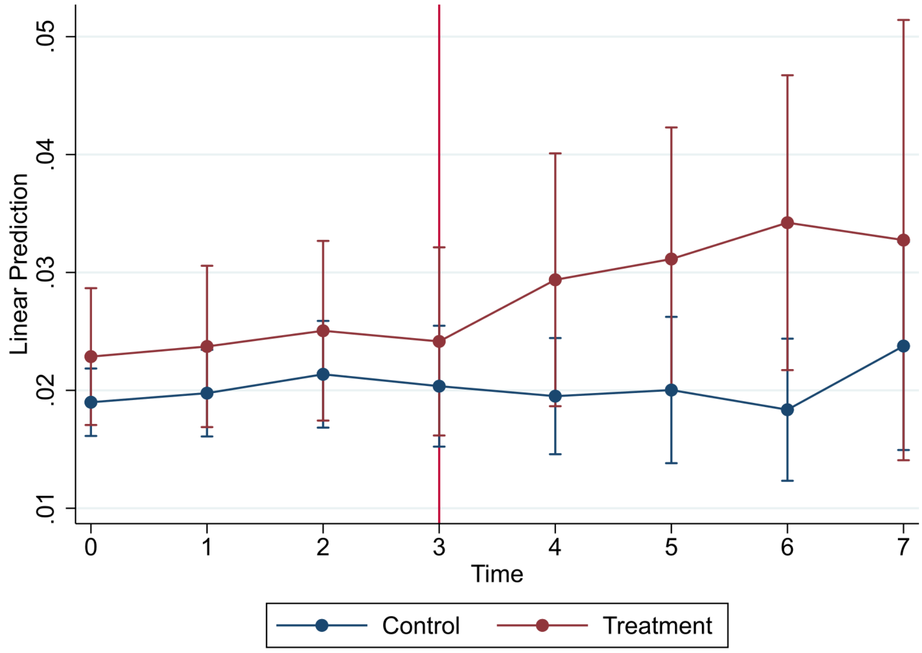


Figure A.2
Model 2: incapacity benefits.

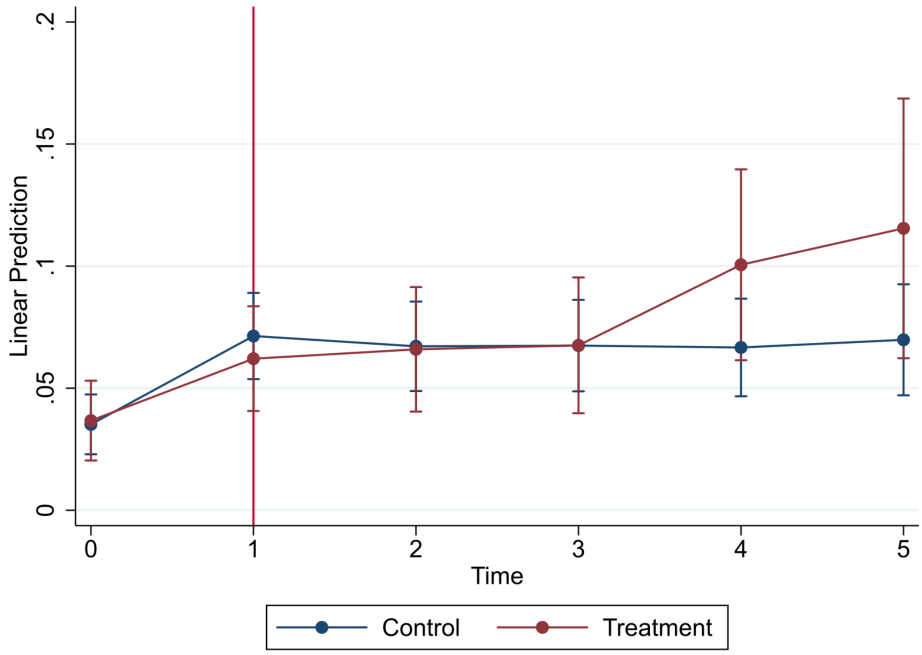
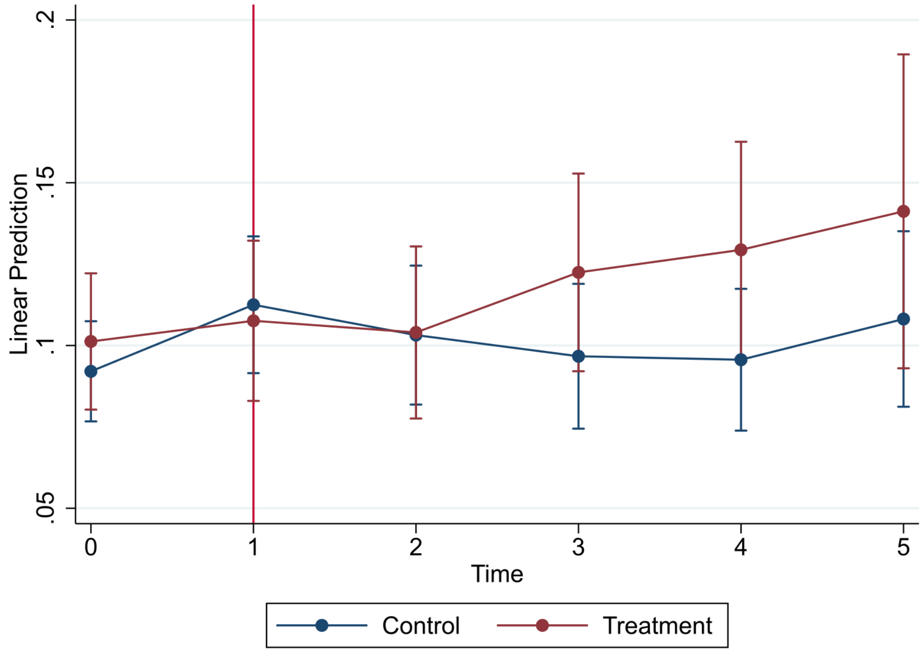


Figure A.3
Model 2: disability benefits.



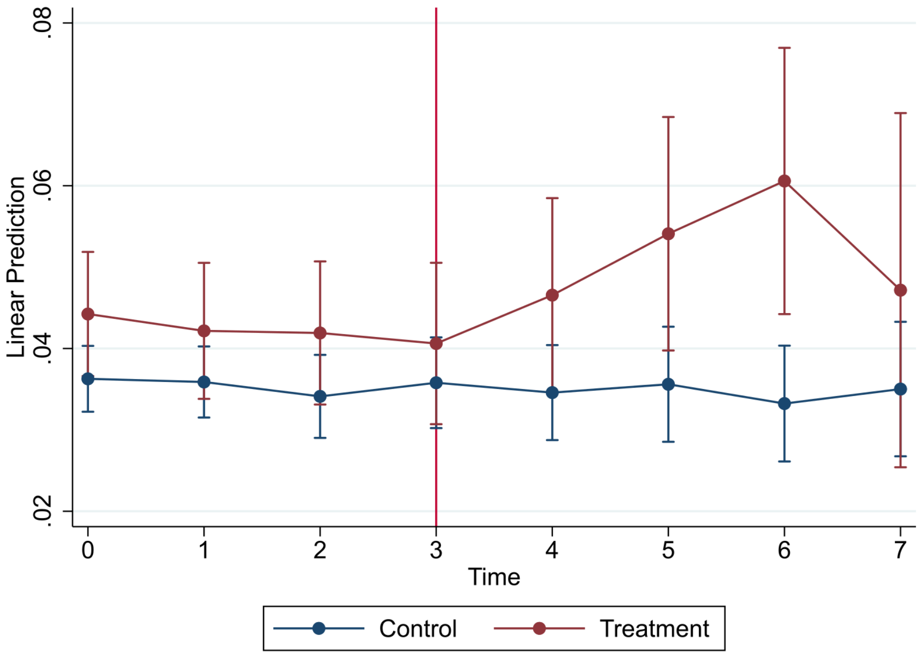
Education as an alternative classifier

As a robustness exercise, we use educational attainment as a proxy for vulnerability. While education is not a clinical risk measure, it captures socioeconomic disadvantage that is strongly associated with health and labour market outcomes. Adults with lower educational attainment typically have poorer baseline health, weaker occupational mobility, and less secure employment trajectories. They also face higher replacement rates, which can reduce financial incentives to remain employed when confronted with adverse health or economic shocks.

We classify individuals aged 25 and above with qualifications below GCSE level as the treatment group, and those with higher qualifications as the control group. Estimates closely mirror our main findings. Low-educated individuals experienced a disproportionate increase in inactivity due to long-term sickness after 2020, but there is little evidence of corresponding rises in disability or incapacity benefit receipt.

This pattern suggests that the education proxy captures both health-related vulnerability and structural disadvantage. However, results should be interpreted with caution, since education interacts with multiple mechanisms - underlying health, occupational opportunities, and financial incentives - that complicate causal attribution. Even so, the analysis reinforces the central conclusion: lower educational attainment was associated with a larger post-pandemic rise in sickness-related inactivity, while the benefit estimates were less consistent.

Figure A.4
Model 2 - Long-term sickness (education).



Notes: Treatment group: qualifications below GCSE level. Control group: higher qualifications. Any embedded legend indicating the reverse classification is incorrect.

Table A.2
Long-term sickness

	Model 1 (DD)		Model 2 (DD)		Model 3 (DDD)	
	Pooled	TWFE	Pooled	TWFE	Pooled	TWFE
	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.046*	0.007	0.011**	0.002	0.037	-0.004
	(0.027)	(0.016)	(0.005)	(0.003)	(0.032)	(0.018)
R^2	0.033	0.855	0.001	0.812	0.044	0.813
Observations	12,076	8,226	90,205	65,087	90,171	65,055

Notes: Standard errors clustered at the individual level. Longitudinal and CEM weights are used. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Treatment is below-GCSE education; control is higher education.

Table A.3
Disability benefits.

	Model 1 (DD)		Model 2 (DD)		Model 3 (DDD)	
	Pooled	TWFE	Pooled	TWFE	Pooled	TWFE
	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.047*	0.012	0.009	-0.005	-0.003	-0.002
	(0.027)	(0.012)	(0.013)	(0.008)	(0.044)	(0.026)
R^2	0.019	0.781	0.001	0.857	0.051	0.857
Observations	13,543	8,828	24,302	16,430	24,301	16,430

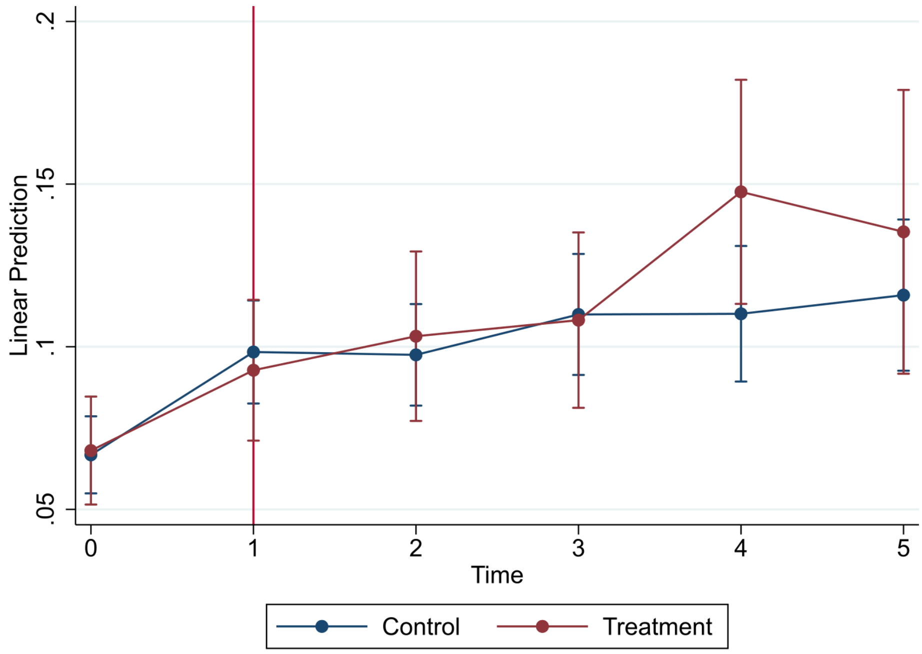
Notes: Standard errors clustered at the individual level. Longitudinal and CEM weights are used. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Treatment is below-GCSE education; control is higher education.

Table A.4
Incapacity benefits.

	Model 1 (DD)		Model 2 (DD)		Model 3 (DDD)	
	Pooled	TWFE	Pooled	TWFE	Pooled	TWFE
	(1)	(2)	(3)	(4)	(5)	(6)
Estimate	0.059*	0.029*	0.013	-0.002	0.005	-0.015
	(0.030)	(0.003)	(0.011)	(0.008)	(0.044)	(0.029)
R^2	0.046	0.781	0.003	0.754	0.055	0.755
Observations	15,042	9,555	27,237	18,368	27,236	18,368

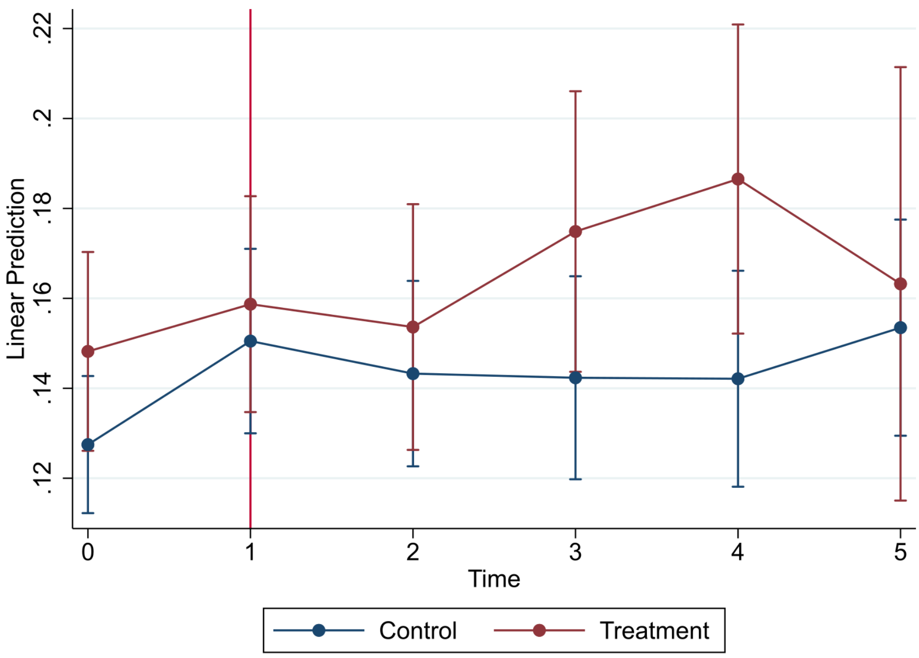
Notes: Standard errors clustered at the individual level. Longitudinal and CEM weights are used. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Treatment is below-GCSE education; control is higher education.

Figure A.5
Model 2 - Incapacity (education).



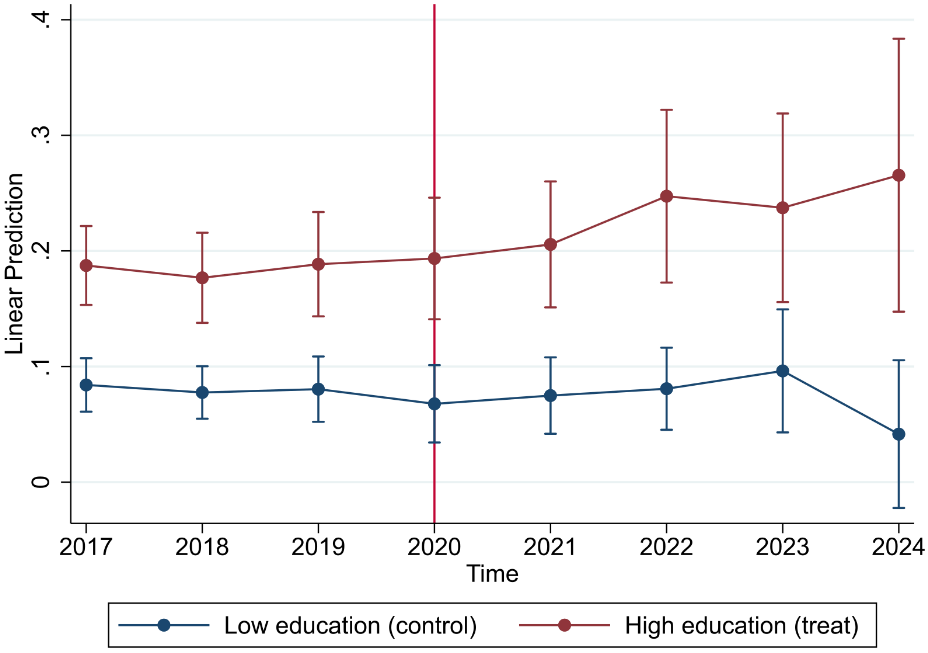
Notes: Treatment group: qualifications below GCSE level. Control group: higher qualifications. Any embedded legend indicating the reverse classification is incorrect.

Figure A.6
Model 2 - Disability (education).



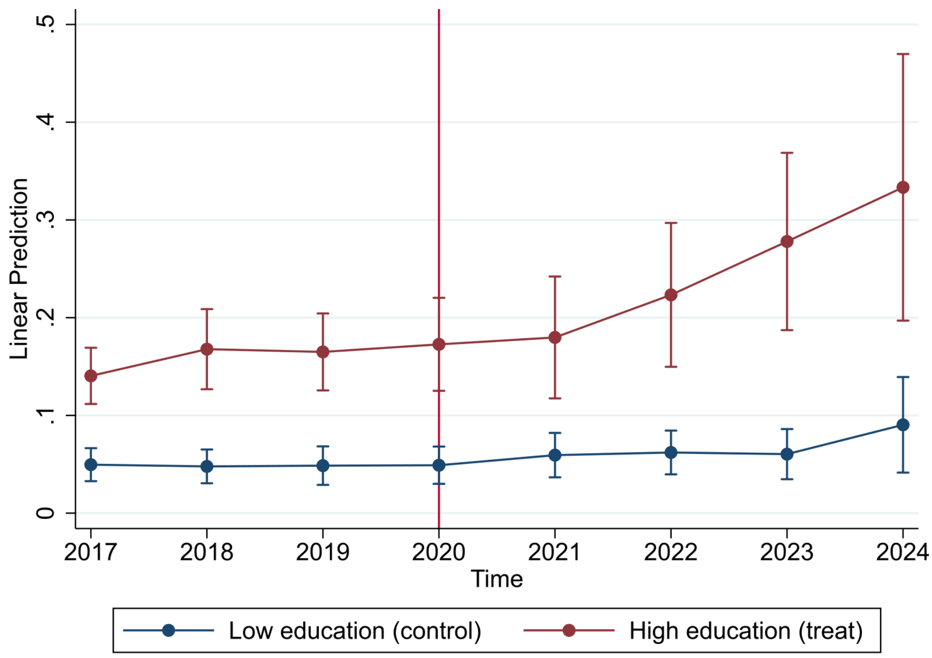
Notes: Treatment group: qualifications below GCSE level. Control group: higher qualifications. Any embedded legend indicating the reverse classification is incorrect.

Figure A.7
Model 1 - Long-term sickness (education).



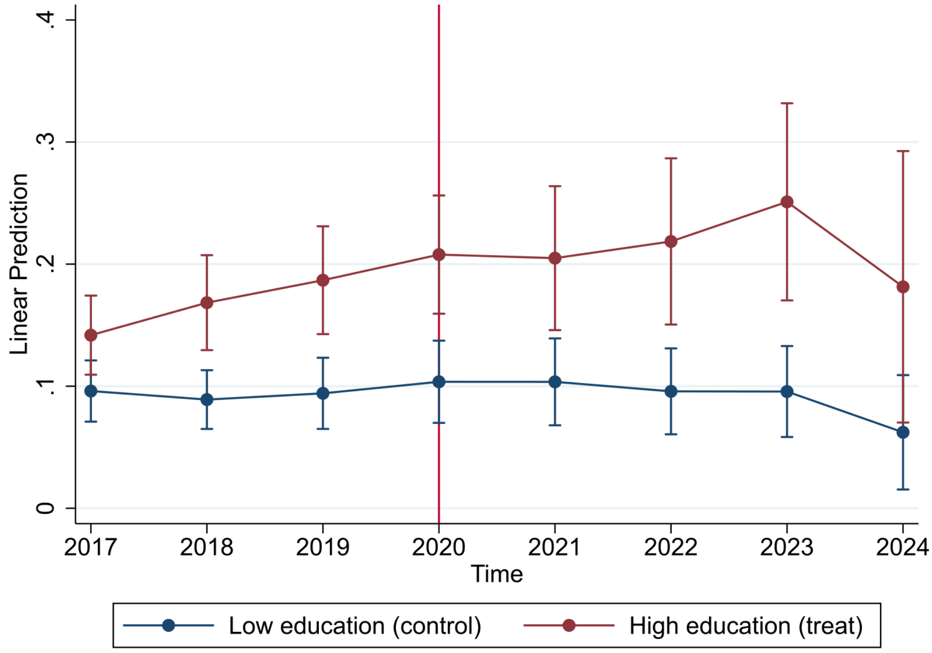
Notes: Treatment group: qualifications below GCSE level. Control group: higher qualifications. Any embedded legend indicating the reverse classification is incorrect.

Figure A.8
Model 1 - Incapacity (education).



Notes: Treatment group: qualifications below GCSE level. Control group: higher qualifications. Any embedded legend indicating the reverse classification is incorrect.

Figure A.9
Model 1 - Disability (education).



Notes: Treatment group: qualifications below GCSE level. Control group: higher qualifications. Any embedded legend indicating the reverse classification is incorrect.