Exploring the causal effect of job loss on health and usage of healthcare services in the UK

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1. Non-technical executive summary

Previous research generally shows that moving out of work can cause someone's health to deteriorate, but it focuses mainly on the short-term effects and mainly on mental health. There is also limited recent research in a UK context. This study has filled evidence gaps by estimating the causal impacts of job loss on mental health, physical health, and healthcare usage in UK a setting; and by exploring how the dynamic effects change over multiple years after job loss. This can be used to inform the policy design and cost-benefit analysis of both DWP labour market policies and NHS healthcare services.

Key findings:

- I find **no clear evidence that job loss causes any change to GP visit rates or hospital outpatient visit rates**.
- I find strong evidence that **job loss causes a sharp deterioration in mental health in the short-term** (in the first year after job loss), but for the average person, this appears to return to pre-job loss levels within 1-2 years. There is indicative evidence suggesting there may be mental health anticipation effects, whereby if an employee knows their job is a risk, this could cause their mental health to start deteriorating before the job loss even happens.
- I find some evidence that, on average, job loss causes a **slight deterioration in physical health** that persists over time.
- People who do not return to work or move into other inactivity groups in the years following their job loss tend to experience a larger, more persistent decline in both mental and physical health. For this group, **physical health gradually worsens over time after the job loss**, at least for 3-4 years. However, it is not possible to definitively say whether this is because sustained worklessness causes a worse deterioration in health (compared to those who return to work), or because a worse deterioration in health causes sustained worklessness.

Contents

Acronyms:

Acronym	**Meaning**
FE	Fixed effects
GP	General practitioner
LM	Labour market
MH	Mental health
GHQ-12l	General Health Questionnaire – 12 items

SF12-PCS	Short-Form survey – 12 items – Physical Component Summary
IV	Instrumental variable
DiD	Difference-in-differences
LATE	Local average treatment effect
ATT	Average Treatment Effect on the Treated
DWP	Department for Work and Pensions
SCBA	Social Cost Benefit Analysis
HMRC	His Majesty's Revenue and Customers
DHSC	Department for Health and Social Care
NHS	National Health Service
ONS	Office for National Statistics
UKHLS	United Kingdon Household Longitudinal Study

2. Introduction

In Oct-Dec 2023, 1.3m working-age people in the UK were unemployed (ONS, 2024a) and 3.0m were economically inactive due to being either 'Temp sick' or 'Long-term sick' (ONS, 2024b). Internationally, research shows that work is, in general, good for health, and involuntary job loss or worklessness can cause health to deteriorate and usage of healthcare services to increase (Waddell & Burton, 2006). However, there are few recent high-quality studies in a UK setting, particularly for healthcare usage. In the DWP SCBA framework, the estimated effects of job loss/gain on healthcare usage are based on outdated studies with methodological limitations (DWP, 2013). There is also limited evidence even in the international literature about how the dynamic health effects change over time following job loss. These evidence gaps restrict the Government's ability to assess the full costs and benefits of LM interventions (both those aiming to prevent job loss and those aiming to support returns to work). In addition, understanding the importance of determinants of health and healthcare demand is necessary to develop effective policies to improve population health and reduce NSH demand, which is particularly pertinent today with record NHS waiting lists (FT, 2024).

This study therefore aims to estimate the causal impact of job loss on healthcare usage and health in the UK, and explore how this changes dynamically in the years following job loss. I use UKHLS panel data to identify a wide group of individuals who have moved from work into involuntary worklessness (either unemployment or inactivity due to long-term sickness). To draw conclusions about causality, I consider job losses caused by redundancies and argue this is likely to be mostly exogenous of deteriorating health. I use DiD with staggered treatment timing, which offers significant advantages over methodologies used in previous studies as it enables exploration of how the dynamic effects change over time after the job loss. I measure the effect on visits to GPs, hospital outpatient visits, MH, and physical health. This evidence can fill a number of important evidence gaps around the casual effects of job loss on health, which can enable fuller estimation of the costs and benefits of Government LM interventions, and support development of policies aiming to improve health and reduce NHS demand.

3. Literature review

3a. Summary of empirical findings

Many international and UK empirical studies have found that involuntary job loss and worklessness have a negative causal effect on health among people of working-age. This includes measures of MH (Waddell & Burton, 2006; Flint, et al., 2013; Gathergood, 2013; Montgomery, et al., 1999; Iversen & Sabroe, 1988; Burgard, et al., 2005; Schröder, 2013) and more general measures of health, illnesses, or conditions (Waddell & Burton, 2006; Cooper, et al., 2015; Burgard, et al., 2005; Mangalore, 2006; Ferrie, et al., 1995; Schröder, 2013; Beale & Nethercott, 1987). However, few studies explore the impact on measures of physical health (Waddell & Burton, 2006; Schröder, 2013). Job loss or worklessness also appears to cause increased usage of healthcare services, measured by visits to a GP or hospital (Waddell & Burton, 2006; Beale & Nethercott, 1987; Mangalore, 2006; Carr-Hill, et al., 1996), although there are no recent UK studies on this. Where data distinguishes between different types of worklessness, there is evidence of this causal effect for both unemployment and economic inactivity due to long-term sickness, but not for other forms of economic inactivity[1] (Flint, et al., 2013; Gathergood, 2013; Carr-Hill, et al., 1996).

Many studies have explored the immediate effects of job loss or the average effects of being in a workless state, but few studies have explored how the health effects change dynamically over time, particularly over the long-term after job loss (Waddell & Burton, 2006). Some have found evidence of an anticipation effect, whereby knowing a job was at risk caused a worsening of health and increased healthcare usage even before the job loss happened (Beale & Nethercott, 1987; Iversen & Sabroe, 1988; Ferrie, et al., 1995; Flint, et al., 2013). Flint et al. (2013) found that job losses negatively impacted MH even when controlling for the contemporaneous effects of being workless, suggesting the immediate MH impacts may be larger than the longer-term effects. Schröder (2013) is a rare study that explicitly explored the long-term effects, and found that involuntary job losses caused a range of adverse health effects even after 25 years. Returning to work has been found to positively impact MH (Waddell & Burton, 2006), but this appears to be smaller than the negative impact of leaving work (Flint, et al., 2013; Carr-Hill, et al., 1996).

3b. Critical review of empirical methods

The complex two-way relationship between work and health creates challenges for researchers attempting to isolate the casual effect of work on health from the reverse effect. Using cross-sectional data to analyse differences in health between employed and workless people is likely to result in selection bias. Health selection bias (or reverse causality) can occur because ill-health creates barriers to work that causally reduce the probability of being employed, or increase the probability of job loss. For example, Bryan et al. (2022) and Lagomarsino and Spiganti (2020) show that worse MH has a negative causal impact on someone's chances of being employed. Social selection bias could exist because socio-economic or demographic factors (e.g. having a socio-economically disadvantaged upbringing) may cause people to be both less healthy and less likely to be in work. Selection bias can be caused by both time-constant differences between individuals (e.g. chronic health conditions, genes, or upbringing) and time-varying differences or idiosyncratic shocks

[1] E.g. being economically inactive due to caring responsibilities, being a student, or early retirement.

prior to the job loss (e.g. an accident leading to severe back pain, or a divorce affecting MH). In studies that do not account for these types of selection bias, estimates of the impact of work or LM transitions on health may be upward biased.

Some studies use regression models or multilevel modelling whilst controlling for social, economic, or demographic confounders (Mathers & Merton, 1994; Carr-Hill, et al., 1996; Mangalore, 2006). Adjusting for these confounders may remove some social selection bias, but it is unlikely to remove health selection bias.

Clark (2003), Flint et al. (2013) and Gathergood (2013) explored the short-term impacts (0-3 years) of LM transitions on MH using FE regression models with UKHLS panel data. FE models remove all selection bias arising from time-constant differences *between* individuals, by focusing on variation in outcomes *within* individuals (changes over time for each individual). Clark (2003), and Flint et al. (2013) additionally reduced some social selection bias due to time-varying factors by controlling for socio-economic confounders. However, FE models require within-unit variation in the treatment variable, meaning individuals who do not change LM status would be dropped from the analysis. As changing LM status is a rare event for many people, a substantial portion of the sample may be lost, as would the health effects of being constantly employed or constantly workless. In addition, neither Clark (2003) nor Flint et al. (2013) controlled for selection bias from idiosyncratic health shocks prior to the LM change that could have both influenced both the LM change and a change in health, meaning the final estimate is likely to be upwardly biased. Gathergood (2013) overcame this endogeneity issue by using unemployment rates for industry-age-year groups as an IV for job loss. However, IV studies only estimate the LATE, the effect on a specific subset of the population who are affected by the IV. This may be of limited use to policymakers who need to understand the effect of work or job loss on the wider population.

Many studies exploring the impact of work on health instead overcome selection bias by exploiting natural experiments. Job losses due to business closures or downsizing are argued to be exogenous since it is unlikely that a business would close or downsize due to the characteristics or circumstances of one employee. Shröder (2013) found some evidence of selection bias when using ‘lay-offs’ that were not due to business closure (e.g. due to downsizing only), but this was due to pre-existing characteristics amongst those who were laid off, such as childhood economic conditions, and so these would automatically be controlled for in any FE models or DiD studies since they are time-constant. An example of a business closure study is Iversen and Sabroe (1988) and Iversen et al. (1989), who monitored the change in outcomes of employees made redundant following a Danish Shipyard closure. The only UK-based example is Beale and Nethercott (1987), who observed the change in health and healthcare usage of 129 employees who were made redundant following the closure of a UK meat processing factory. However, the change in outcomes for a group of employees before vs after job loss may be insufficient as a causal estimate of job loss, as it could be biased due to time-fixed effects (wider macroeconomic trends that would have affected the employees even if they had not lost their job).

Therefore, later natural experiment studies have estimated effects using DiD relative to control groups. DiD involves measuring, for both the treatment group and a control group, the change in average outcomes from a time before the treatment to a time after the treatment, and then calculating the difference in average changes between the two groups (i.e. observing any divergence in trends after the treatment took place).

Measuring the difference in average changes over time captures only time-varying differences between individuals, so it automatically removes any selection bias due to time-constant differences (e.g. social background, genes, or pre-existing chronic health conditions) and any time fixed effects that affect everyone's trends over time equally (e.g. macroeconomic conditions). If one can assume that post-treatment outcomes would not be affected by any time-varying effects other than the treatment (i.e. outcomes of both groups would follow parallel trends in the absence of the treatment), then any difference in changes over time between the two groups can be used as an unbiased estimate of the causal effect of the treatment. For example, Ferrie et al. (1995) compared the difference in health changes between civil servants facing job insecurity due to privatisation with the health changes of civil servants in other departments not facing privatisation.[2] Salm (2009) compared the difference in health changes of people who were laid off due to business closure, with the health changes of people who did not lose their jobs.

However, both of these studies only compared changes in outcomes over 2 periods (before vs after job loss), so did not capture how the dynamic effects changed over multiple time periods. In addition, whilst the business closure studies may have greater internal validity than other methods, there may be a trade-off in terms of lower external validity because they tend to focus on a specific type of job loss. Studies which focus closure of specific businesses may have even lower external validity. For example, the effects of job loss on healthcare usage for 129 meat factory workers found in the latest UK business closure study (Beale & Nethercott, 1987) may not reflect the effects of job loss for people in other industries. This UK study is also now likely to be outdated.

The present study therefore aims to make the following contributions to existing literature:

- **Estimate the causal effect of job loss on MH, physical health, and healthcare usage, in a UK setting and across a wide range of industries.** There are few UK studies of the effects on physical health and healthcare usage, and those that do exist are outdated, have significant methodological limitations, or have limited external validity.
- **Explore how the dynamic health effects of a job loss change over time in the years after job loss.** Most studies estimate the causal effect of job loss on health or healthcare usage at a fixed point in time after the event. There is a dearth of evidence on how the effects change dynamically over multiple years following the job loss.

4. Data and research method

4a. Data

I use UKHLS panel data,[3] which comes from a large longitudinal survey of UK households interviewed every 12 months. Panel data is necessary for the DiD methodology used in this paper and also enables exploration of how effects change dynamically over time. The UKHLS is large and covers households from across the UK, so the results will likely be highly generalisable across the UK population. Importantly, it also provides data

[2] In Ferrie et al. (1995), the exogenous natural experiment was privatisation and the treatment was the ***threat*** of job losses due to privatisation, rather than business closure and job loss per se.
[3] This is also now called 'Understanding Society'.

on a range of social, economic, and demographic factors, including LM transitions, job losses reasons, and measures of health and healthcare usage.

The UKHLS data also has a number of limitations. It is not possible to identify the timing of changes to health or visits to healthcare services between annual waves. This makes it harder to use temporal sequencing to ensure treatment variables are exogenous (e.g. ensuring the job loss happened before health started deteriorating). It also means I cannot easily identify cases where someone left work and rejoined work between two waves, so not all LM transitions are picked up. The effects of job loss on health and healthcare usage are likely to be smaller for the people who return to work more quickly, so excluding these groups is likely to somewhat downwardly-bias any estimated improvements in health or reductions in healthcare usage due to the job loss, and upwardly-bias any estimated deteriorations in health or increases in healthcare usage. On the other hand, large numbers of individuals leave and re-join the UKHLS survey each wave (see
Table 2). People who leave are likely to have characteristics that are correlated with some of the independent and dependent variables. For example, people whose health is worst affected by job loss may be most likely to drop out of the survey, in which case the effects on these people would not be captured. If so, this would upwardly-bias any estimated improvements in health or reductions in healthcare usage due to the job loss, and downwardly-bias any estimated deteriorations in health or increases in healthcare usage. All of these limitations could potentially be overcome by using a dataset which merges population-level administrative work and health data. However, no dataset like this is currently publicly available in the UK. In the absence of this, UKHLS is the best choice is it the largest longitudinal survey in the UK, with the most granular data.

I extract data on UKHLS waves covering fieldwork from 2015-16 to 2021-22 (henceforth named waves 1-7), because the healthcare usage information was only collected from 2015-16 onwards. This initial dataset includes 51,583 people over 241,108 person-waves (mean 5 waves per person). Excluding person-waves not of working age (16-65) removes 18% of people and 23% of person-waves. Excluding person-waves with missing data removes a further 10% of people and 13% of person-waves. The final sample for analysis includes 38,212 people over 161,593 person-waves (mean 4 waves per person). This is an unbalanced sample, with large numbers of respondents leaving and re-joining in each survey wave. Only 24% of the sample (9,088 people) are non-missing across all 7 waves (
Table 2).[4]

Table 1: Number of observations by wave

	Wave						
	1	**2**	**3**	**4**	**5**	**6**	**7**
Number of observations	28,202	26,631	24,019	22,887	21,305	19,823	18,726

Table 2: Number and % of non-missing waves across dataset per person

	Number of non-missing waves per person

[4] Restricting the data to a balanced panel would likely limit the sample to people with a very specific subset of characteristics and have limited generalisability to the wider population. It would also decrease the sample size significantly, reducing statistical power. I therefore allow an unbalanced panel in the DiD estimation, whereby if an individual has a missing wave, I use the previous non-missing wave instead. However, having larger gaps between waves (due to missing waves) could introduce bias as it reduces the certainty that the job loss occurred before health started deteriorating.

	1	2	3	4	5	6	7
Number of people	6,339	4,997	4,233	4,098	4,189	5,268	9,088
% of people	17%	13%	11%	11%	11%	14%	24%

Variables:

- **LM transitions** – I group LM statuses into: employed, involuntary worklessness (including both unemployment and inactivity due to long-term sick), and other inactive (excluding long-term sick).[5] I then convert these into transition variables combining, in each wave for each person, their LM statuses in their previous non-missing wave with their LM status in the current wave.[6]
- **Job loss wave** – For those who are employed in their first non-missing wave but later move into involuntary worklessness, I identify the wave in which this transition happens for the first time.
- **Job loss reason** – the reason given by the respondents for moving out of a job. Options included but were not limited to: 'Made redundant', 'Health reasons', 'Dismissed/sacked', and 'Took retirement'. For the person-waves involving a transition from employment into involuntary worklessness, I create a dummy equal to 1 if they reported that it was because they were made redundant.
- **GP visits** – The number of visits to a GP in the previous 12 months, categorised into 5 groups: 0 visits, 1-2 visits, 3-5 visits, 6-9 visits, and 10+ visits. This is used to create a binary GP visits dummy, equal to 1 if someone had 3+ visits and 0 if not.
- **Hospital outpatient visits** – The number of visits to hospital as an outpatient, categorised into the same 5 groups as GP visits. This is used to create a binary outpatient visits dummy, equal to 1 if someone had 1+ visits and 0 if not.
- **GHQ-12** – A measure of psychological distress, used as a proxy measure of MH. Answers to the 12 item General Health Questionnaire are converted into a Likert scale where 0 is the least distressed and 36 is the most distressed. This is a widely accepted measure of MH (Clark, 2003; Flint, et al., 2013; Gathergood, 2013).
- **SF12-PCS** – A measure of physical functioning, used as a proxy measure of physical health. Answers to the 12-item Short Form Survey are converted into a continuous scale where 0 is the lowest level of physical functioning and 100 is the highest.
- **Long-term impairment or condition** – Dummy equal to 1 if someone has an impairment, illness, or disability that has lasted or is expected to last over 12 months.
- **Sex**
- **Age** – Grouped into: 16-29, 30-49, and 50+.
- **Highest qualification** – Grouped into: Higher, A-level, GCSE, and 'Other or None'.
- **Marital status** – Grouped into: MarOrCo, NeverMarOrCo, and WidDivSep.

[5] I define involuntary worklessness to include both unemployment and economic inactivity due to long-term sick. Unemployment is by definition involuntary as it reflects workless people who are able and actively looking for work, whereas inactivity due to long-term sick is assumed to be involuntary because the given reason for not working (being long-term sick) is not an active choice. On the other hand, other forms of economic inactivity (being a student, early retirement, being a carer, or 'other') are excluded as they can involve at least some level of active choice in at least some cases.

[6] Where the t-1 wave is missing, I use the previous non-missing wave instead.

4b. Method

I use DiD with multiple time periods and staggered treatment timing, as set out Callaway and Sant'Anna (2021). As explained in section 3b, DiD involves calculating, for both a treatment group and a control group, the change in outcomes over time from a pre-treatment period to a post-treatment period. If one can assume the two groups would follow parallel trends in the absence of treatment, then any divergence in trends after the treatment does happen is estimated as the causal effect of the treatment. DiD offers a number of advantages over other empirical methods. It automatically removes all selection bias arising from time-constant differences between individuals. It does not rely on within-unit variation in LM statuses, like FE models do. It estimates the Average Treatment Effect on the Treated (ATT), rather than the LATE like IV studies do. The ATT is a more useful policy parameter because it has a higher degree of external validity than the LATE.

However, in the standard DiD set-up (Ferrie, et al., 1995; Salm, 2009), there is one treatment group who are all treated at the same time, and effects are estimated only in a single post-treatment period. The approach set out in Callaway and Sant'Anna (2021) extends the traditional DiD set-up for use when there are more than two time periods and variation in treatment timing. I use this method because it enables me to explore the dynamic effects over multiple years after job loss, and to use a larger treatment group by combining people whose job losses occurred in different waves. Observing trends across multiple pre-treatment periods can also be useful because if they are parallel, this provides indicative evidence in support of the parallel trends assumption. Another advantage of the Callaway and Sant'Anna (2021) extension is that it makes the parallel trends assumption more plausible by allowing it hold after conditioning on pre-treatment covariates (the 'conditional parallel trends assumption').

In this application, the treatment is a movement out of work into involuntary worklessness, though the specific treatment varies across different model specifications. Within each specification, multiple treatment groups (g) are created based on the timing of the first job loss (G). The control group in each wave for each treatment group is the 'not-yet-treated' (D_{it}=0), which includes those who are employed throughout, plus those who have been employed up to the current wave but have a job loss in a future wave.[7] The baseline for calculating post-treatment trends is the last pre-treatment period (g-1), and for pre-treatment periods it is the previous wave (t-1). The ATT is estimated as the average effect of the treatment in each time period for each group (the 'group-time average treatment effects') and is estimated using doubly-robust estimators. Models 1, 3, and 5 assume unconditional parallel trends, whereas models 2 and 4 assume conditional parallel trends by adjusting trends for a set of covariates in baseline periods. In all specifications, I assume no anticipation effects and allow an unbalanced panel.

[7] The alternative to using the 'not-yet-treated' control group is to use the 'never-treated' control group, who stay employed in all non-missing waves. The benefit of using the 'not-yet-treated' is that they are likely to better reflect the characteristics of the treatment group as it includes people who later become part of the treatment group. As a result, the parallel trends assumption is more likely to hold. The downside is that the composition of the control group changes over time, which means pre-treatment trends are less informative about likely post-treatment trends. Either way, this is unlikely to make a significant difference because the treatment groups are very small in comparison to the continuously employed (see figure 1).

Group-Time ATT for all t≥g, with parallel trends assumption based on not-yet-treated units:

$$ATT(g,t) = E[Y_t - Y_{g-1}|G = g] - E[Y_t - Y_{g-1}|D_t = 0, G \neq g].$$

Group-Time ATT for all t≥g, with conditional parallel trends assumption based on not-yet-treated units:

$$ATT(g,t) = E[Y_t - Y_{g-1}|X, G = g] - E[Y_t - Y_{g-1}|X, D_t = 0, G \neq g].$$

Where:

- Y_t is group g's average observed outcome in period t
- Y_{g-1} is group g's average observed outcome in the last period before being treated
- G_i is the wave when group g becomes treated
- D_t is a dummy equal to 1 if group g has been treated by period t, 0 if not
- X is a vector of pre-treatment covariates

These results are then aggregated into the average effect by length of exposure to treatment (the 'dynamic effect'), and the weighted average of all post-treatment effects across all groups and lengths of exposure (the 'simple effect'). The former is presented as an events study type plot.

There is a risk that some of the results could be biased due to anticipation effect, as found in previous research (Beale & Nethercott, 1987; Iversen & Sabroe, 1988; Ferrie, et al., 1995; Flint, et al., 2013). If employees' health starts deteriorating by t-1 because they know their job is at risk, then using t-1 as the baseline could mean any estimated improvements in health or reductions in healthcare usage due to the job loss may be upward-biased, and any estimated deteriorations in health or increases in healthcare usage may be downward-biased. In models 2 and 4, adjusting post-treatment trends based on t-1 covariates could amplify this bias. Therefore, as a sensitivity analysis, for any models and outcomes which show a positive pre-treatment effect in t-1, I account for possible anticipation effects by using t-2 as the baseline for estimating effects, instead of t-1. See annex C for these results.

Another risk is that the external validity of the results could be reduced since the data covers the COVID-19 pandemic and lockdowns. In the UK, the first COVID-19 lockdown began in late March 2020. This is likely to have had a significant negative impact on visit to healthcare service (both GPs and hospitals) and MH across waves 6 and 7 (covering GP/hospital visits in 2020-21 and 2021-22, respectively). If so, in some waves, this may have had a downward effect on the estimated effects of job loss on healthcare usage, which may not reflect the impact on healthcare usage in years where there are not pandemic lockdowns.[8]

4c. Model specifications

All treatment groups are employed in all waves until they experience at least one transition into involuntary worklessness. They vary based on the reason for job loss (either *any reason* or *redundancy*), whether they remain out of work in subsequent non-missing waves, and whether trends are adjusted for covariates.

[8] This is particularly likely to affect models 4-5, because a larger proportion of these treatment groups experienced the job loss in waves 6-7 (see Table 4). However, this would only affect the short-term dynamic estimated effects (as well as the aggregated simple effects), whereas the longer-term dynamic effects are of more interest in these models.

Models 1-3 consider the average effects of transitions into involuntary worklessness regardless of whether they remained workless in subsequent non-missing waves. Model 1 considers all job loss reasons and does not include controls, model 2 considers all job loss reasons and includes controls, and model 3 considers only job losses due to redundancies and does not include controls. Models 4 and 5 use narrower treatment groups who after transitioning into involuntary worklessness, remain involuntarily workless in all of their subsequent non-missing waves. Model 4 considers all job loss reasons and includes controls, whereas model 5 considers only job losses due to redundancies and does not include controls.

Model 1 explores the effect on the probability of having 3+ GP visits only, whereas models 2-5 explore the effects on probability of having 3+ GP visits, probability of having 1+ hospital outpatient visits, GHQ score, and SF12-PCS score. The controls used in models 2 and 4 include sex, age, highest qualification, marital status, GHQ score (except for when this is the dependent variable), SF12-PCS score (except for when this is the dependent variable), and a dummy equal to 1 if someone has a long-term health condition or impairment and 0 if not.

Table 3: model specifications

Model no.	**Treatment group – based on:**		**Adjusted for covariates**	**Outcome**
	Reason left work	**Post-job loss LM journey**		
Impact on GP visits				
1	Any	Involuntary worklessness	No	GP visits
2	Any	Involuntary worklessness	Yes	GP visits
3	Made redundant	Involuntary worklessness	No	GP visits
4	Any	Sustained involuntary worklessness	Yes	GP visits
5	Made redundant	Sustained involuntary worklessness	No	GP visits
Impact on hospital outpatient visits				
2	Any	Involuntary worklessness	Yes	Outpatient visits
3	Made redundant	Involuntary worklessness	No	Outpatient visits
4	Any	Sustained involuntary worklessness	Yes	Outpatient visits
5	Made redundant	Sustained involuntary worklessness	No	Outpatient visits
Impact on MH (GHQ-12l score)				
2	Any	Involuntary worklessness	Yes (exc. GHQ-12l)	MH
3	Made redundant	Involuntary worklessness	No	MH
4	Any	Sustained involuntary worklessness	Yes (exc. GHQ-12l)	MH
5	Made redundant	Sustained involuntary worklessness	No	MH
Impact on physical functioning (SF12-PCS score)				
2	Any	Involuntary worklessness	Yes (exc. SF12-PCS)	Physical functioning
3	Made redundant	Involuntary worklessness	No	Physical functioning
4	Any	Sustained involuntary worklessness	Yes (exc. SF12-PCS)	Physical functioning
5	Made redundant	Sustained involuntary worklessness	No	Physical functioning

Model 1

Model 1 serves two purposes. Firstly, I compare the estimated effects in model 1 to both the raw cross-sectional differences in outcomes between the employed and involuntarily workless in wave 1, and to the differences in outcomes between the constantly-employed and each treatment group in their first non-missing wave. I do these comparisons to assess the level of bias arising that could arise in cross-sectional analysis from time-constant differences. Secondly, I compare the results of model 2 to model 1, in order to assess the level of bias from time-varying factors that is removed by controlling for potential confounders in model 2.

Comparing models 2 and 4 (adjusting for covariates) with models 3 and 5 (redundancy job losses)

The control variables used in models 2 and 4 have been identified as factors which could cause selection bias because they could influence both health trends and the likelihood of job loss, thereby potentially breaking the parallel trends assumption. For example, adjusting for age, sex, highest qualification, and marital status could reduce social selection bias from possible time-varying differences such as different trends by sex,[9] having a divorce,[10] worsening health for older age groups, or varying trends by educational status. Adjusting for GHQ score, SF12-PCS, and a long-term condition/impairment dummy could reduce health selection bias. To produce unbiased estimates, these models only require the conditional parallel trends assumption to hold, which is more plausible that the unconditional parallel trends assumption used in model 1.

However, GHQ and SF12-PCS are each removed as controls when using models 2 or 4 to measure the impact on GHQ and on SF12-PCS, respectively, so some health selection bias could remain. In addition, even when adjusting for these covariates, bias could remain due to unobserved time-varying factors, or any time-varying factors/shocks which occur before the job loss but after t-1.[11] Also, if there are anticipation effects in t-1 (see section 4b), then adjusting for covariates in t-1 could amplify the bias caused by those anticipation effects.

Therefore, in models 3 and 5, I do not adjust for covariates, and instead attempt to identify exogenous job losses by focusing on those where the stated reason for job loss was being 'made redundant'. I argue that moving out of work due to redundancy is exogenous of deteriorating health, because employers generally choose to make redundancies due to business reasons and not due to the characteristics of individual employees. It is possible that given the decision has been made to make redundancies, employers may choose staff in poorer health if they perceive them to be less productive, or people with poorer health may more commonly work in industries with higher lay-off rates (Schröder, 2013). However, time-constant differences in health are already controlled for in the DiD approach, so selection bias can only be caused by cases where employers make employees redundant specifically because their health has been deteriorating over time. I argue this may happen only very rarely. In certain circumstances, employees can be dismissed on the basis of their health, but specifically making redundancies because of ill-health is illegal in the UK (Gov.uk, 2024). Therefore, I argue that any job losses happening specifically due to deteriorating health are more likely to be captured under 'Health reasons' or 'Dismissed/sacked' in the UKHLS survey. For these reasons, moving out of work due to being made redundant is assumed to be exogenous of deteriorating

[9] For example, the first year of the COVID19 pandemic had different LM and health effects on men and women (ONS, 2021). Women were more likely to be furloughed, and their MH was worse affected.

[10] Experiencing a divorce is associated with worse MH (Richards, et al., 1997).

[11] As the survey waves at 12 months apart, some changes in work or health that occur between waves may not be picked up in the data.

health. Also, if there were sufficient cases where health started deteriorating prior to t-1 and this led to redundancy, this would result in divergences in pre-treatment trends, so it will be partially tested.

Both pairs of models are included as they each offer advantages and drawbacks. Models 2 and 4 have larger sample sizes and therefore have higher statistical power to detect smaller effects. They are also more generalisable to job losses for a wider range of reasons. However, models 2 and 4 are likely to exhibit at least some selection bias and could amplify the bias caused by any anticipation effects, whereas the results of models 3 and 5 will arguably exhibit little/no selection bias.

Comparing models 2 and 3 (movements into involuntary worklessness) with models 4 and 5 (movements into sustained involuntary worklessness)

Models 2-3 reflect the average effects of job loss for a wider group of people than models 4-5, so are more generalisable. This also means they have larger sample sizes, enabling smaller confidence intervals and higher power to detect smaller effects. However, the estimated long-term effects in models 2-3 may include not only the effects of job loss and involuntarily worklessness, but also the effects of transitioning back out of involuntary worklessness (returning to work or moving into other inactivity groups). Whilst this is of policy interest, it is additionally useful to understand the long-term effects on people who remain workless. Therefore, models 4-5 enable exploration of how the dynamic effect of job loss changes over time for those who do not return to work or move into other inactivity groups. However, this is only indicative, as it will not be possible to definitively determine whether differences in health impacts between the two sets of models are a cause of sustained worklessness or an effect of it.

5. Descriptive statistics

Figure 1 shows the how treatment groups were developed, along with their respective sample sizes and proportions relative to higher level groups. The treatment groups are very small in comparison to those who are constantly employed in all non-missing waves, which makes up the majority of the control groups in each model. Table 4 shows the breakdown of each treatment group, by the first wave they moved from employment into involuntary worklessness. For the narrower groups who remained workless in subsequent waves (models 4-5), a larger proportion of job losses happened in waves 6-7, since they had fewer subsequent waves in which a further LM transition could occur after job loss.[12]

[12] This was particularly the case for those who were made redundant and remained workless (model 5), which could be related to the Coronavirus Job Retention Scheme (also known as the furlough scheme) ending on 30th September 2021.

Figure 1: Flow diagram showing development of treatment groups, with group size

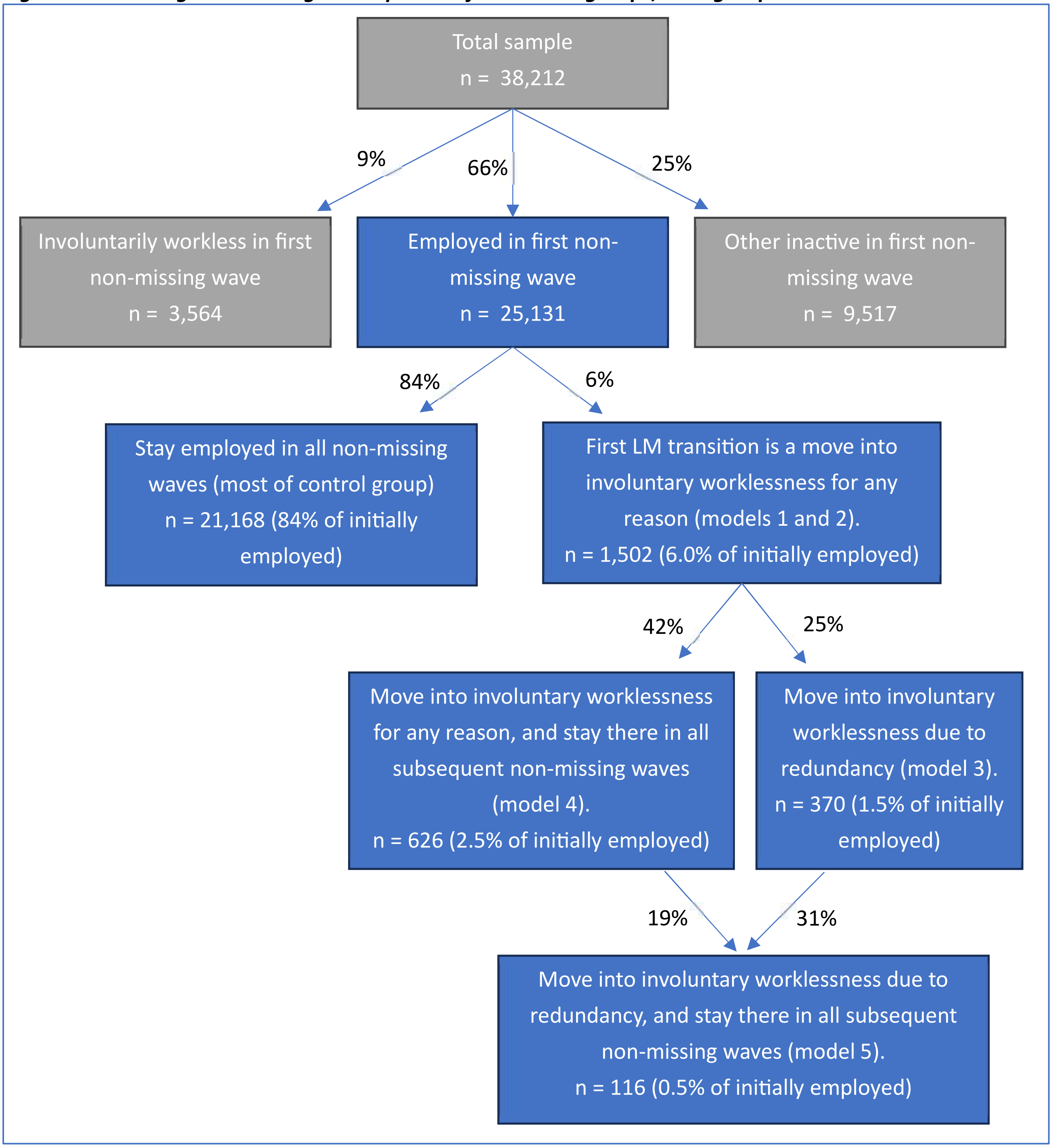


Table 4: Breakdown of each treatment group, by the wave of first job loss

Treatment group	**Wave of (first) job loss**					
	2	**3**	**4**	**5**	**6**	**7**
Models 1 and 2: any reason	21%	17%	17%	16%	17%	12%
Model 3: redundancy	22%	13%	14%	19%	20%	12%
Model 4: any reason and remain workless	14%	10%	15%	13%	19%	29%
Model 5: redundancy and remain workless	10%	9%	11%	11%	22%	37%

Next, I show how key aggregated outcomes vary over time in Table 5. HC usage declines significantly in waves 6 and 7 (2020-21 and 2021-22), which is likely due the COVID pandemic lockdowns which began in late March 2020. Psychological distress appears to worsen slightly too, although this worsening appears to begin in wave 5, before the COVID pandemic. Physical functioning does not change much across waves.

Table 5: Aggregated main outcome measures (for all sample), by wave

	Wave							**Total**
Outcome	**1**	**2**	**3**	**4**	**5**	**6**	**7**	
% with 3+ GP visits	32%	35%	34%	27%	33%	26%	20%	30%
% with 1+ OP visits	40%	42%	43%	40%	41%	37%	32%	40%
Mean GHQ-12L score	11.1	11.3	11.6	11.7	12	12.2	12.1	11.7
Mean SF12-PCS score	51.3	51.3	51.2	51.1	51.4	51.6	51.3	51.3

Table 6 provides cross-sectional summary statistics using wave 1 data. Compared to employed people, the involuntarily workless were almost twice as likely to have 3+ GP visits in the previous 12 months (+31%-points), more likely to have 1+ hospital outpatient visits (+19%-points), had worse MH (mean GHQ score was 4.9 units higher), and had worse levels of physical functioning (mean SF12-PCS score was 11.4 units lower). People aged 50+ were slightly more likely to be involuntarily workless than younger age groups, and had higher healthcare usage rates and worse health scores. Males and females were roughly equally likely to be involuntarily workless, but females had higher healthcare usage rates and slightly worse health scores. People with 'Other/None' qualifications were more likely to be involuntarily workless than those with better qualifications (four times more likely than those with higher educational qualifications), and generally had higher healthcare usage rates and lower health scores. People who had been widowed, divorced, or separated were more than twice as likely to be involuntarily workless than those who were married or co-habiting, had far higher healthcare usage rates and worse health scores.

Table 6: Cross-sectional (wave 1) differences in LM status and key outcomes between groups based on characteristics

Group in wave 1		**% employed**	**% involuntary worklessness**	**% inactive**	**% with 3+ GP visits**	**% with 1+ OP visits**	**Mean GHQ12 score**	**Mean SF12PCS score**
All		70%	21%	9%	32%	40%	11.1	51.3
LM status	Employed	100%	0%	0%	28%	37%	10.6	52.7
	Involuntary worklessness	0%	100%	0%	59%	56%	15.5	41.3
	Other inactive	0%	0%	100%	36%	41%	11.2	50.5
long-term illness or impairment	Yes	58%	20%	22%	58%	63%	13.5	43.5
	No	75%	4%	21%	23%	31%	10.2	54.2
Age bands	16-29	53%	9%	38%	28%	32%	10.9	54.3
	30-49	83%	8%	10%	31%	38%	11.3	52.1
	50+	67%	10%	23%	38%	47%	11.2	48.1
Sex	Female	66%	8%	25%	38%	43%	11.7	50.8

	Male	75%	9%	16%	25%	35%	10.5	51.8
Highest qualification	Higher	82%	5%	14%	29%	40%	10.8	52.8
	A-level	67%	7%	26%	31%	36%	11.1	52.1
	GCSE	63%	11%	25%	33%	40%	11.3	50.5
	Other/ None	50%	20%	30%	43%	43%	12	46.2
Marital status	Mar Or Co	78%	6%	16%	32%	40%	10.9	51.2
	Never Mar Or Co	54%	13%	34%	30%	35%	11.3	52.8
	Wid Div Sep	66%	16%	18%	43%	50%	12.6	46.8

Finally, in Table 7, I compare key outcomes in the first non-missing waves, between the constantly-employed and the treatment groups of each model specification.[13] People who remain employed in all waves generally have lower healthcare usage and better pre-existing health in their first non-missing wave (in which they were all employed) than people in each of the treatment groups. For example, there is a 13%-point difference in the percentage who had 3+ GP visits between the constantly employed and the widest treatment group (used in models 1 and 2). This strongly suggests there is health selection into each of the treatments at least due to pre-existing time-constant differences between individuals.

Table 7: Key outcomes for people in their first non-missing wave, differences between the constantly-employed and each treatment group

Group	**3+ GP visits**	**1+ OP visits**	**Mean GHQ**	**Mean SF12-PCS**
Constantly-employed	27%	36%	10.5	53.1
Models 1 and 2: any reason job loss	40%	43%	12.2	50.2
Model 3: redundancy job loss	34%	39%	11.5	51.7
Model 4: any reason job loss and remain workless	42%	45%	12	49.1
Model 5: redundancy job loss and remain workless	32%	42%	10.4	50.8

6. Findings

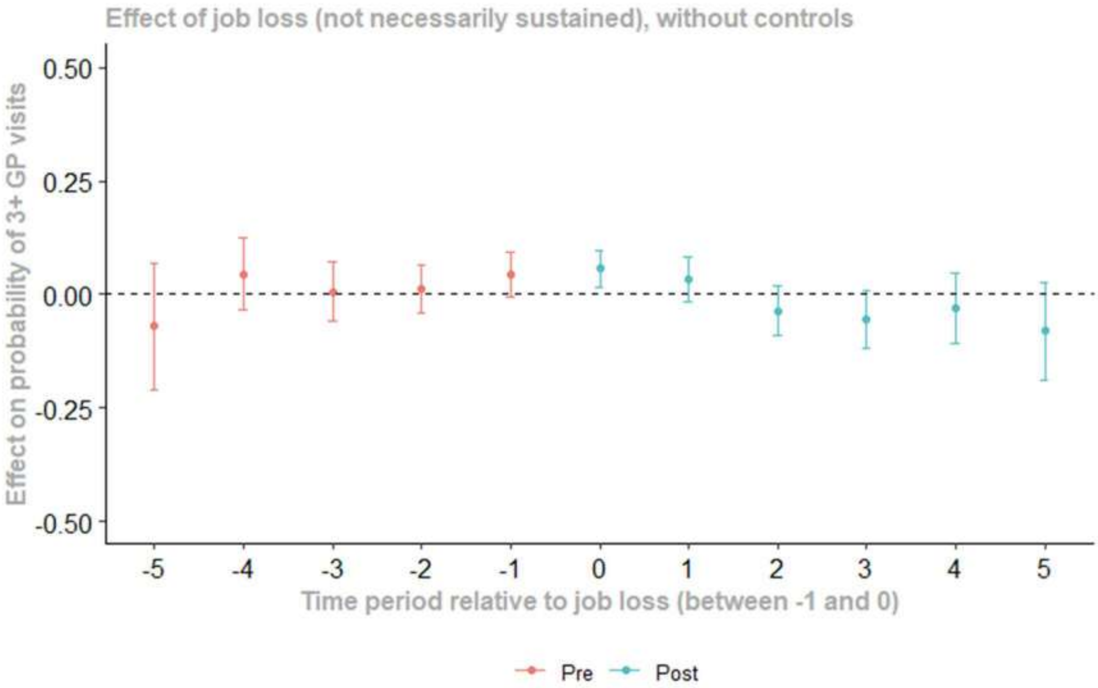
6a. Effect on probability of 3+ GP visits

In model 1 ('any reason' job losses and no adjustments for covariates), GP visit rates increased significantly by 6%-points in t0 (0-1 years after job loss), but there was no significant effect in any other period and the simple effect was not significant.

The unconditional pre-treatment trends provide no significant evidence against the parallel trends assumption. However, there is a small non-significant 4%-points increase in GP visit rates in the period immediately before job loss, which could indicate either a small amount of selection bias due to time-varying factors or anticipation effects.

[13] Note that some treatment groups are subsets of others, so there is overlap between these treatment groups.

Figure 2: Model 1, GP visits. Simple effect: -0.0187



In contrast to model 1, in model 2 (adjusting for covariates in baseline periods), the simple effect on GP visit rates is significant and negative, albeit small (-5%-points). There is no evidence of any short-term dynamic impact (in t or t+1), but the longer-term dynamic impacts (in t+2, t+3, and t+5) are significantly negative (between -6%-points and -11%-points).

As in model 1, model 2 shows a small non-significant positive effect on GP visit rates in t-1, which could indicate either a small amount of selection bias or anticipation effects. Adjusting for baseline covariates could somewhat reduce selection bias, but it could also amplify the bias caused by anticipation effects. If I account for possible anticipation effects by using t-2 as the baseline for post-treatment effects instead of t-1, the negative simple and dynamic effects on GP visits both disappear (see annex C).

For the subset of these job losses that were specifically due to redundancies (model 3), I find no significant dynamic or simple effects in either direction. For this group, pre-treatment trends appear to closely match the control group trends in the three periods before job loss, which provides supporting evidence that redundancy job losses are exogenous.

When looking at 'any reason' job losses specifically for those who remain workless afterwards (model 4), the effects appear similar to the wider group (negative simple effect, no dynamic effect in the short-term, negative dynamic effect over the longer term), but none are statistically significant.

In contrast, when looking at redundancy job losses for those who remain workless (model 5), the simple effect is instead positive, significant, and large (27%-points). The only significant dynamic effect was a positive effect in t+5, which appears to be an anomaly since it is much larger than the other periods and difficult to theoretically explain.[14]

[14] Note that the effects in model 5 have very wide confidence intervals due to low sample sizes, so impacts need to be very large to be statistically significant.

*Figure 3: Model 2, GP visits. Simple effect: -0.0533**

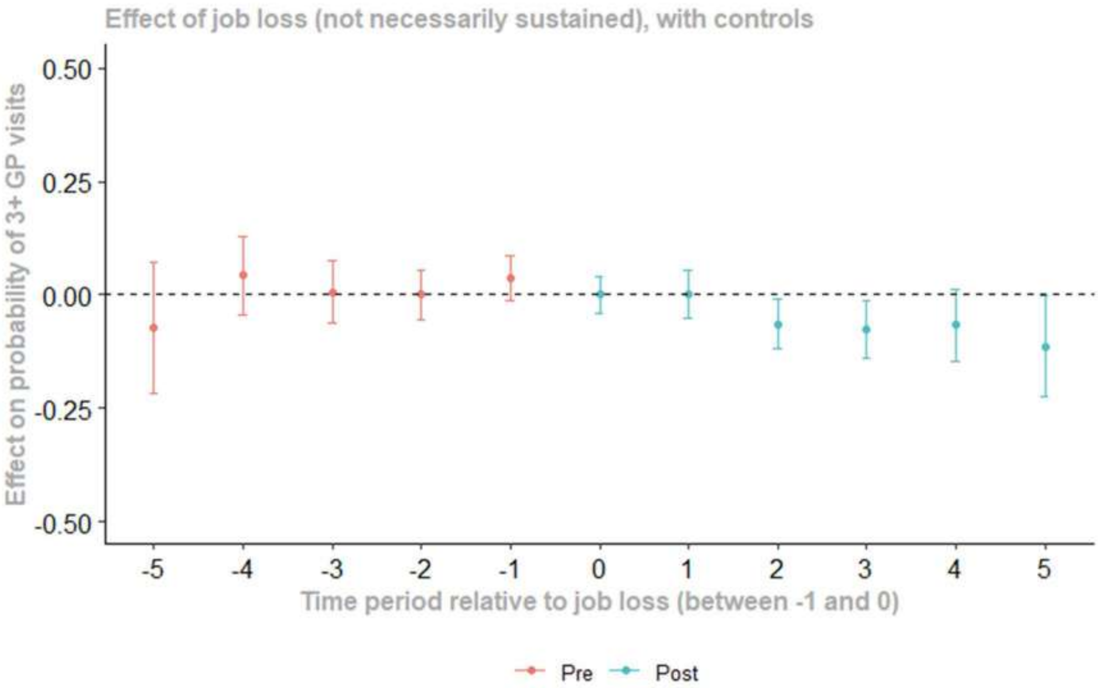


Figure 4: Model 3, GP visits. Simple effect: -0.0262

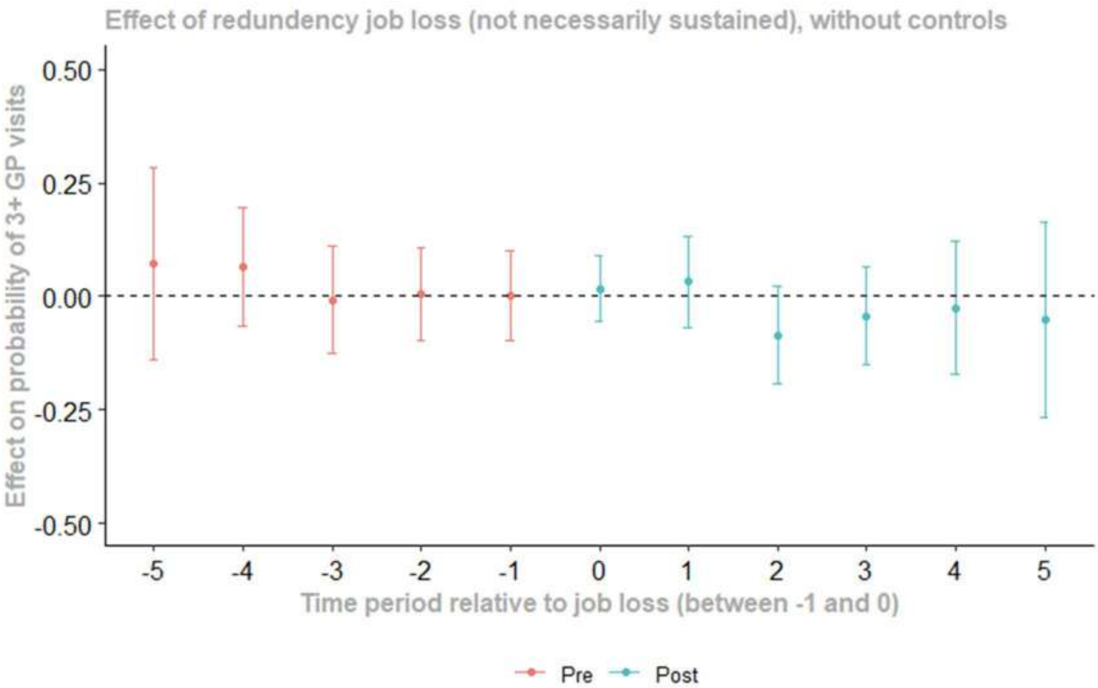


Figure 5: Model 4, GP visits. Simple effect: -0.0526

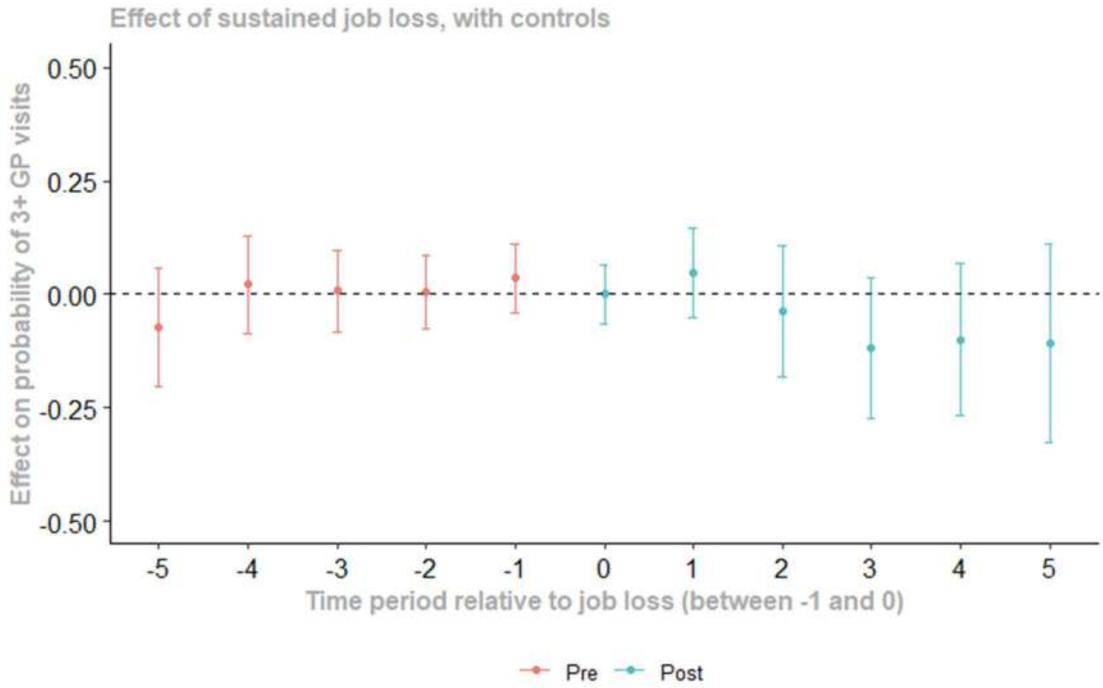
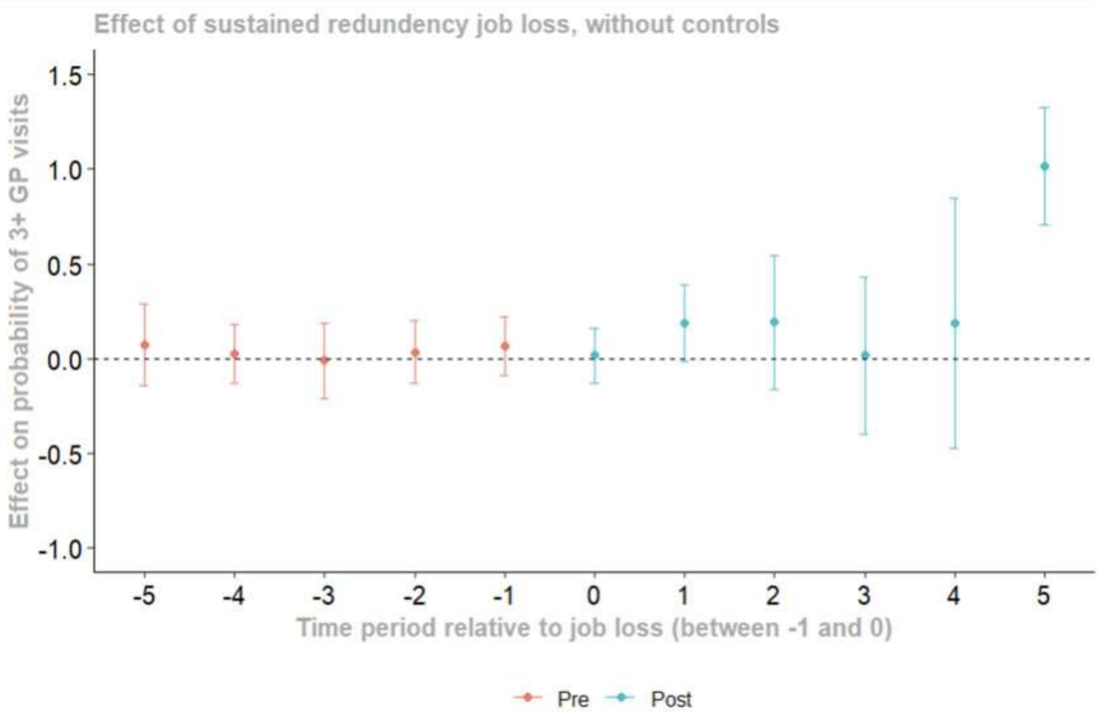


Figure 6: Model 5, GP visits. Simple effect: 0.2689 (NOTE DIFFERENT Y-AXIS SCALE)*



6b. Effect on probability of 1+ hospital outpatient visits

I find no evidence in models 2-5 that movements into involuntary worklessness have any effect (short-term, long-term, or simple) on the probability of having 1+ hospital outpatient visits. See annex B for these results.

I find no evidence against the conditional parallel trends assumption in models 2 and 4, nor any evidence against the unconditional parallel trends assumption in models 3 and 5.

6c. Effect on GHQ-12l scores

GHQ scores represent levels of psychological distress, so a higher score suggests worse MH.

Models 2-4 all shows a significant negative effect on GHQ scores in t-3 (2-3 years before job loss) of between 9 and 13 units. This divergence in pre-treatment trends could suggest that the parallel trends assumption may not hold. However, this could be an anomaly, as it seems unlikely that people with improving MH in one year would be more likely to experience job loss specifically 2-3 years later. In addition, in t-1 and t-2 (the final 2 years before job loss), there is instead a small non-significant positive effect on GHQ scores. Again, these could suggest either there is selection or an anticipation effect.

All models (2-5) find that movements into involuntary worklessness lead to a significant increase in GHQ scores in t0 (0-1 years after job loss), of between 1.9 and 2.4 units. However, in models 2-4, this appears to be only temporary, with GHQ scores returning to around pre-treatment levels by t+1. Models 2-4 also find no significant simple effect.

Unexpectedly, for 'any reason' job losses (model 2), the longer-term dynamic effects on GHQ scores are mostly negative, although these are small (between -0.4 and -1.8 units), only significant in t+3 and t+5, and are not found in any other models. In addition, accounting for possible anticipation effects in t-1 by instead using t-2 as the baseline causes the negative long-term effects on GHQ scores to disappear (see annex C).

In contrast, the dynamic effects of redundancy job losses for people who remain workless in subsequent waves (model 5) remain significantly positive in t+1 (4.7 units) and t+2 (2.9 units). The effects also appear positive and even larger in t+3 and t+4, although these are not statistically significant.[15] Model 5 also finds a large significant positive simple effect on GHQ scores of 3.4 units.

[15] Note that the effects in model 5 have very wide confidence intervals due to low sample sizes, so impacts need to be very large to be statistically significant.

Figure 7: Model 2, GHQ-12l scores. Simple effect: -0.3521

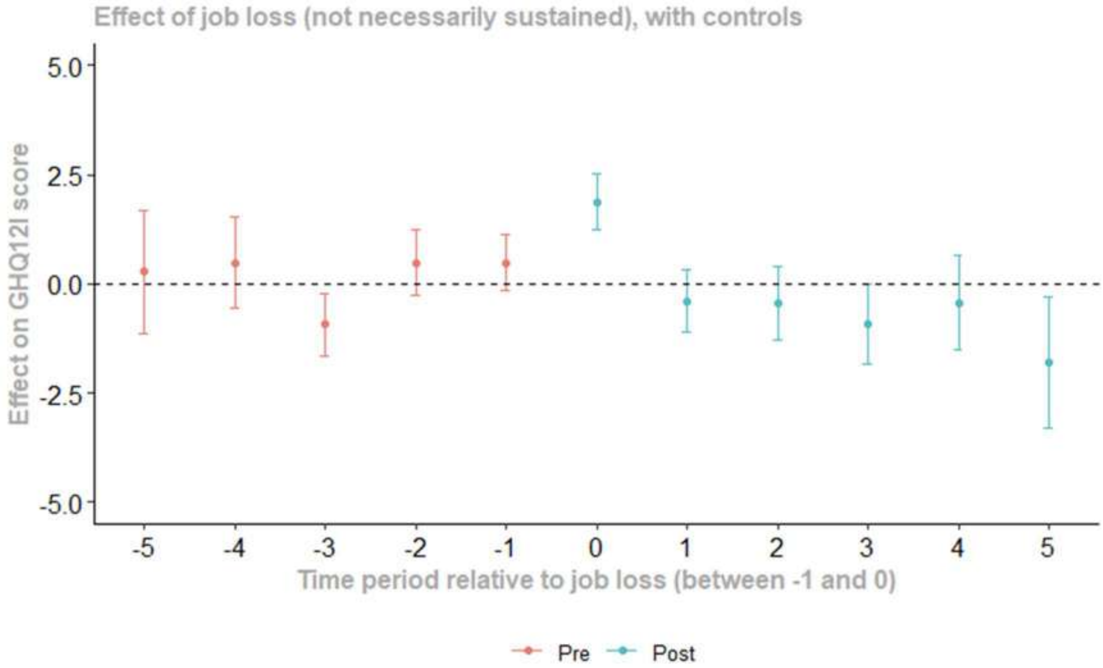


Figure 8: Model 3, GHQ-12l scores. Simple effect: 0.0006

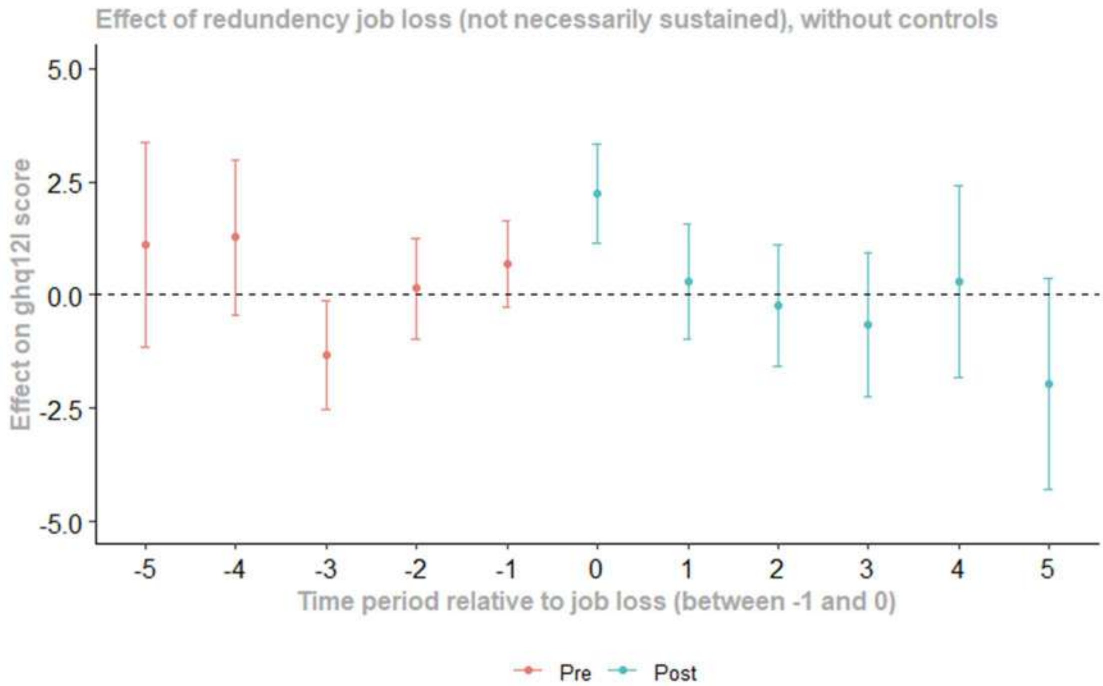


Figure 9: Model 4, GHQ-12l scores. Simple effect: 0.8118

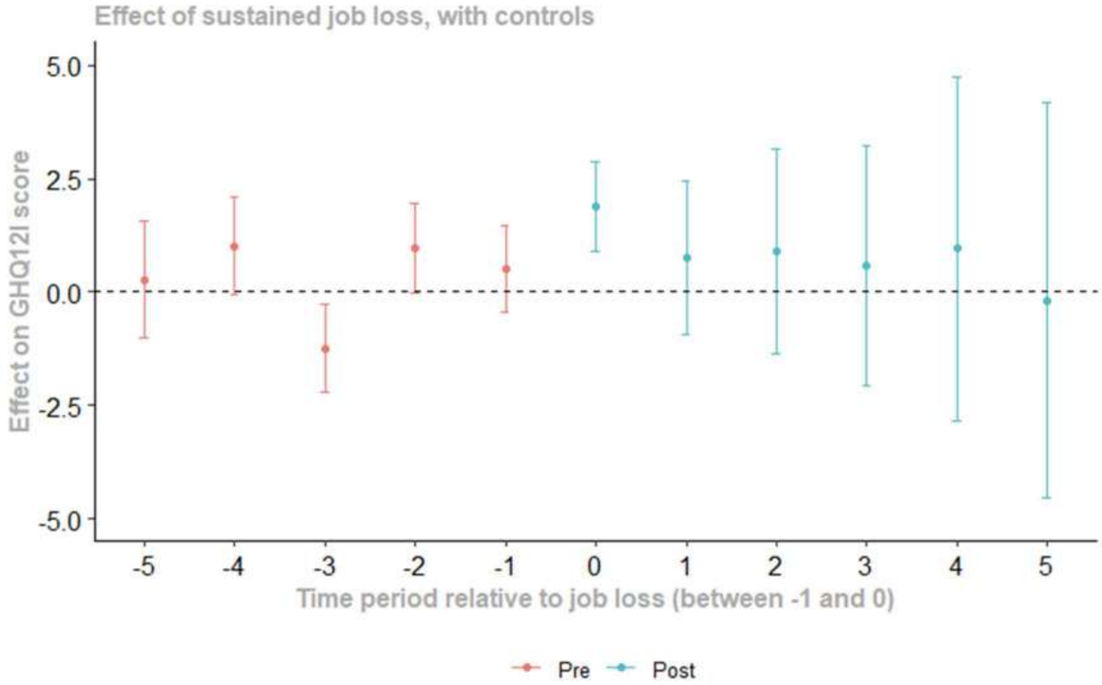
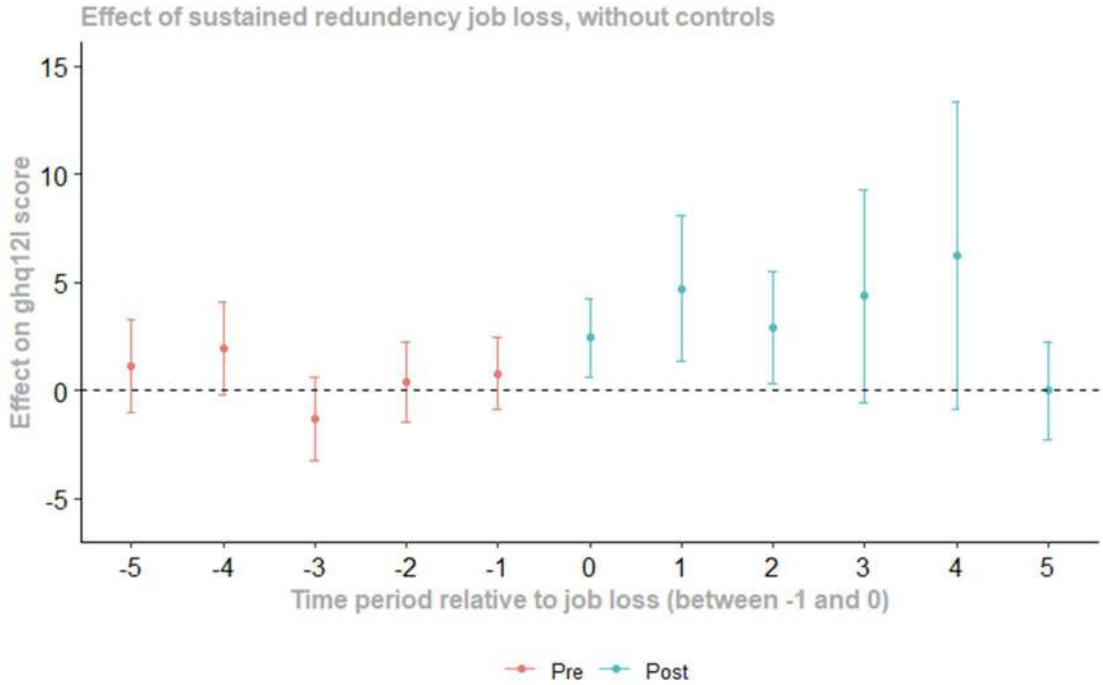


Figure 10: Model 5, GHQ-12l scores. Simple effect: 3.4332   (NOTE DIFFERENT Y-AXIS SCALE)*



6d. Effect on SF12-PCS

SF12-PCS scores represent levels of physical functioning, so a higher score suggests better physical health.

In models 2-5, pre-treatment trends in SF12-PCS scores provide no evidence against the parallel trends assumption.

The simple effects of job loss on SF12-PCS scores are negative in all four models, and significant in models 2, 4, and 5 (between -0.8 and -7.7). The simple effect is larger for those who remain involuntarily workless in subsequent waves (between -1.6 and -7.7) compared to the wider groups which include some people who make further LM transitions (between -0.8 and -1.0).

In models 2 and 3, the dynamic effects appear consistently small and negative across all post-treatment waves, but the only significant dynamic effect is in t0 in model 2.

For those who remain workless (models 4 and 5), the negative effect on SF12-PCS scores appears to increase over multiple years, at least until 2-4 years after job loss (t+2 and t+3). For the 'any reason' job losses, the effect size increases from t0 to t+2. For the redundancy job losses, there is no significant effect in t0 or t+1 but there are large significant effects in t+2 and t+3. In both models 4 and 5, the effects in t+4 and t+5 are unclear due to wide confidence intervals.

Figure 11: Model 2, physical functioning. Simple effect: -0.8074*

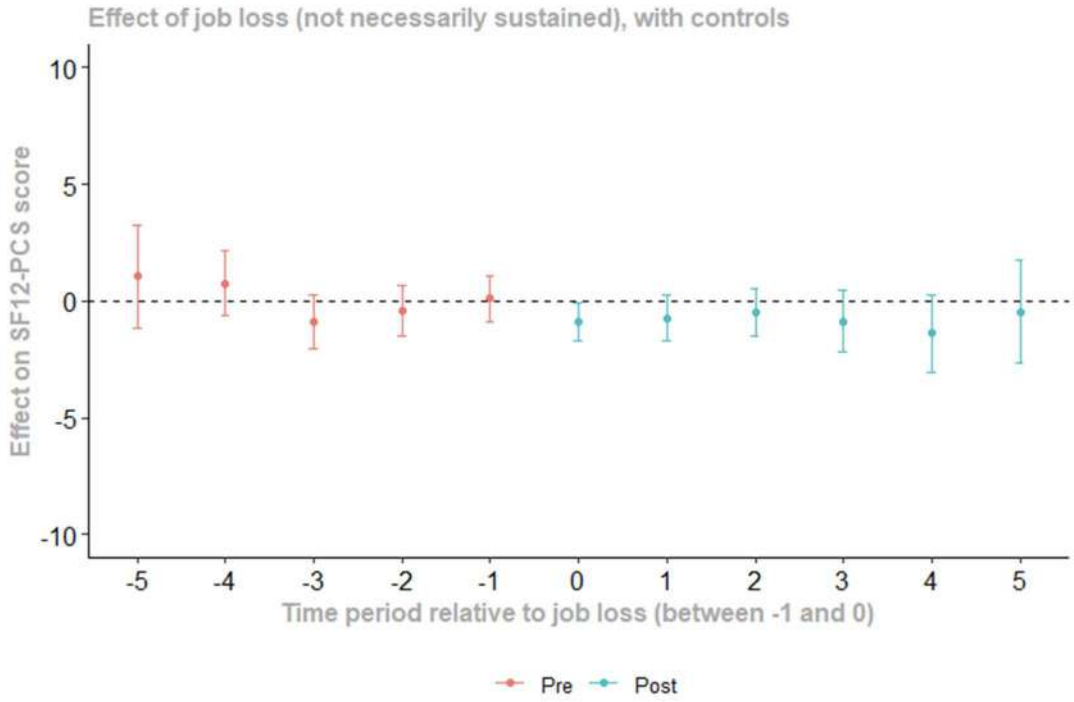


Figure 12: Model 3, physical functioning. Simple effect: -0.9530

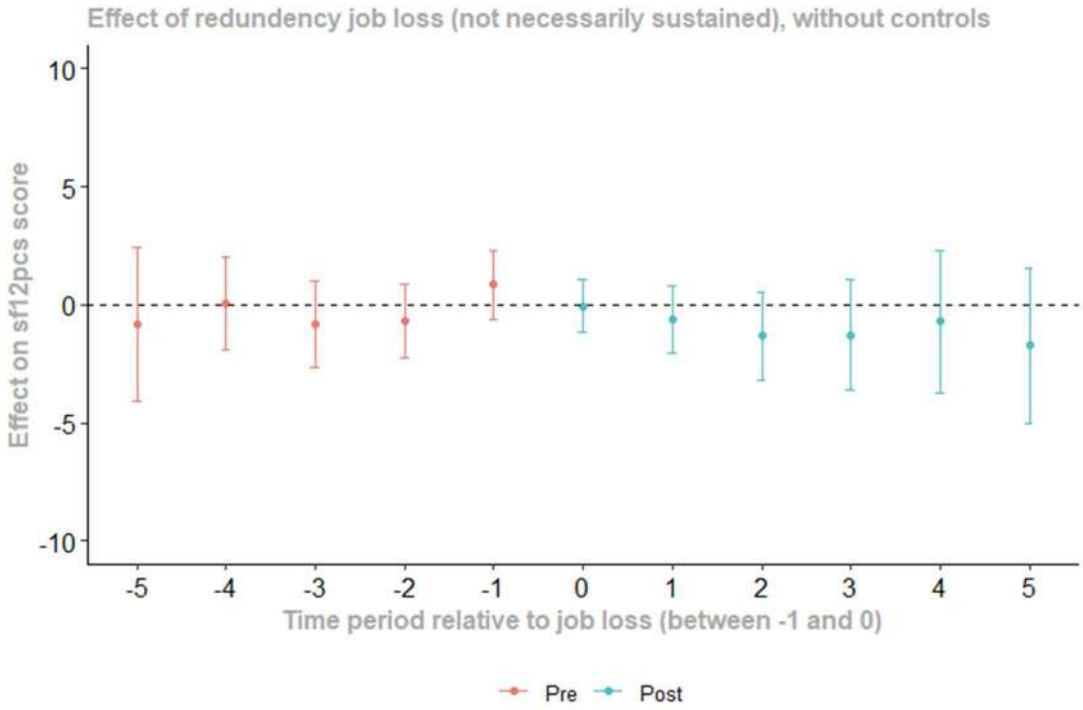


*Figure 13: Model 4, physical functioning. Simple effect: -1.6121**

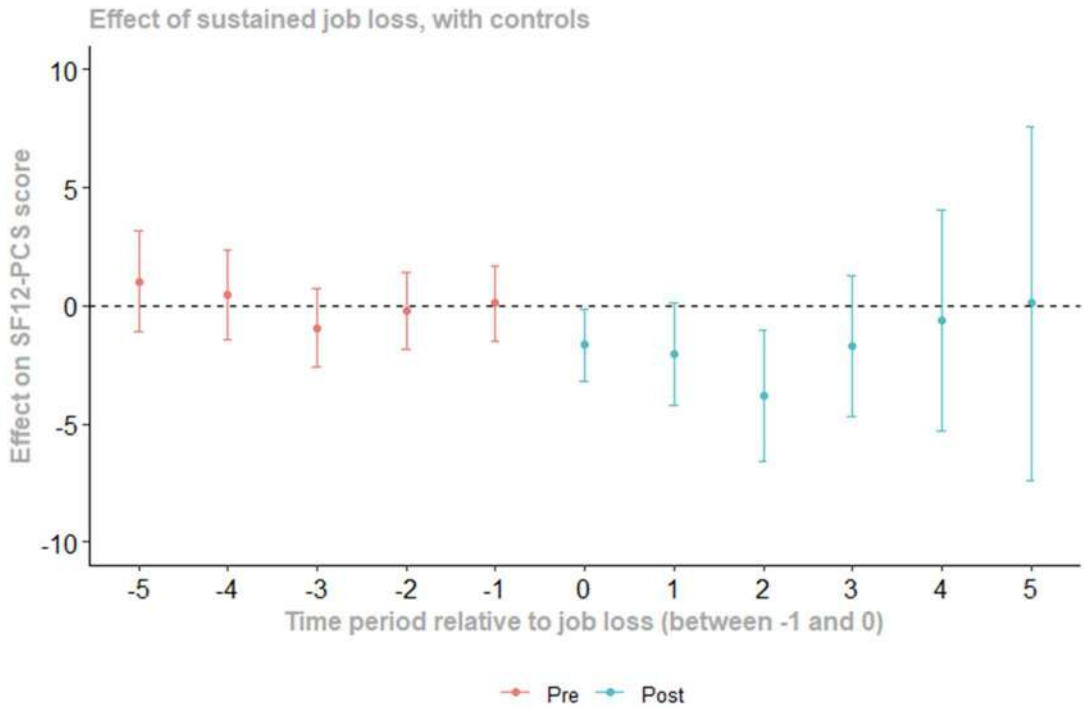
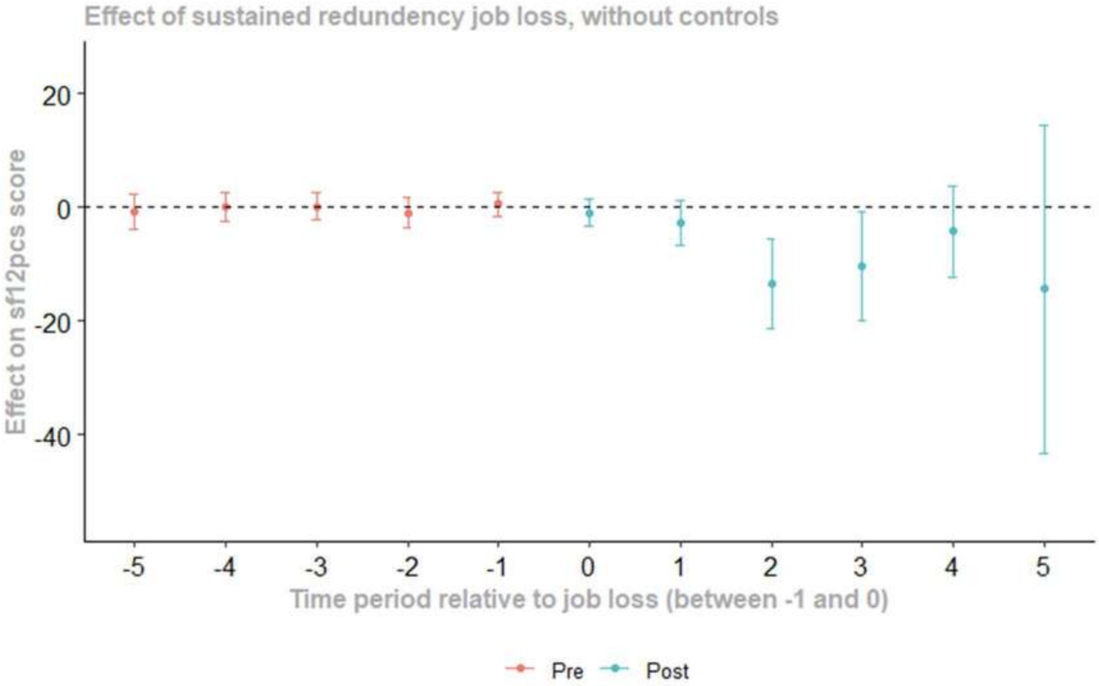


Figure 14: Model 5, physical functioning. Simple effect: -7.7003   (NOTE DIFFERENT Y-AXIS SCALE)*



6e. Discussion

The basic DiD model used in model 1 found no simple effect of job loss on the probability of having 3+ GP visits, and only a small positive dynamic effect of 6%-points 0-1 years after job loss. This is far smaller than both the 31%-points cross-sectional difference between employees and involuntarily workless in wave 1 (Table 5), and the 13%-point difference between the constantly-employed and treatment group in their first non-missing wave (before the job loss) (Table 7). Together, these results show that cross-sectional analysis could exhibit a considerable amount of selection bias due to pre-existing, time-constant differences between people.

Overall, pre-treatment trends provide little evidence against the parallel trends assumption. However, some models show a small, non-significant positive effect on GP visits and GHQ scores in t-1, and adjusting post-treatment trends for t-1 covariates (model 2) slightly reduces the size of the effects on GP visits comparted to model 1, with some even becoming slightly negative. This shows that GP visit rates and GHQ scores increase slightly in t-1 for the treatment groups relative to the control groups, and therefore probably increase slightly in the unobserved period between t-1 and job loss too. This could reflect either selection bias due to time-varying factors (e.g. idiosyncratic health shocks) and/or anticipation effects, both of which have also been found in the existing literature. The fact that when estimating the impact on MH, this also appears for redundancy job losses (models 3 and 5) which are assumed to be exogenous, could suggest it may reflect anticipation effects rather than selection. Models 2-5 all make efforts to remove selection bias, although some may remain in models 2 and 4. However, none of the main models account for anticipation effects in t-1. Since t-1 is used as the baseline for estimating effects, anticipation effects could downwardly-bias any estimated increases in healthcare usage or deterioration in health, or upwardly bias any estimated reductions in healthcare usage or improvements in health. In addition, adjusting for t-1 covariates in models 2 and 4 could amplify the bias caused by anticipation effects. Therefore, on models and outcomes with an uptick in t-1, sensitivity analysis was conducted to account for anticipation effects by instead using t-2 as the baseline for estimating effects (see annex C).

None of the models find any impact on hospital outpatient visits.

Across all movements out of work into involuntary worklessness (model 2) and assuming no anticipation effects, job loss appears to on average have no short-term effect on GP visit rates (in 0-3 years) and over the long-term cause a small decrease in GP visit rates (3-6 years). I find evidence that it causes a sharp, temporary deterioration in MH in the first year but a small improvement in MH over the long-term, and a very small immediate reduction in physical functioning which possibly persists over multiple years. However, when accounting for possible anticipation effects, the estimated improvement in MH and reduction in GP visits disappear.

Across all job losses due to redundancies (model 3), I still find no short-term effect on GP visits and some negative effects over the long-term, but these are smaller than in model 1 and not significant.[16] As in model 1, model 2 finds a sharp, temporary deterioration in MH in the short-term, but unlike model 1, there does not appear to be any improvement in MH in the long-term. On physical functioning, the estimated positive simple effect is larger than in model 2 but still very small (<1 units on the SF12-PCS score) and not significant. Since redundancy job loss is assumed to be exogenous and this model does not potentially amplify anticipation effects by adjusting for t-1 covariates (like model 2 does), these results provide reassurance that job loss in general causes little/no difference to GP visit rates, a sharp temporary deterioration in MH, and little/no difference to physical functioning. However, any differences in results between model 1 and 2 could also be due to compositional differences in the treatment groups or heterogeneity by reason for job loss.

When focusing on the longer-term effects (after one year) of job loss for those who remain involuntarily workless in subsequent waves (models 4 and 5), some results change substantially. For 'any reason' job losses (model 4), the long-term effects on GP visits are similar to model 2 (i.e. negative) except for they are not significant, but the long-term MH effects change from an improvement to a deterioration (though not significant), and the negative effect on physical functioning increases in size and becomes significant. For redundancy job losses (model 5), some simple and dynamic effects on GP visits become positive and significant, the deterioration in MH persist for longer and the overall simple effect on MH becomes a significant deterioration, and the negative simple effect and some dynamic effects on physical functioning increase in size and become significant. Together, these results show that people who remain workless after job loss experience a worse deterioration in health than those who return to work or move into other inactivity groups, particularly in terms of physical health and particularly for people who were made redundant. The effect on physical health appears to build up gradually over time (possibly over 4 years) for those who remain involuntarily workless. This could imply three things. First, this could suggest that sustained worklessness *causes* a worse deterioration in health compared to returning to work. Second, it could suggest that having worse health deterioration makes it more difficult to return to work. Third, it could reflect compositional differences between the two treatment groups.

[16] However, it should be noted that the model 2 treatment group has a small sample size which means that impacts have to be very large to be statistically significant.

In terms of the average effects across all job losses, the short-term deterioration in MH and persistent impact on physical functioning are broadly as expected, but the long-term improvements in MH and reduction in GP visits are unexpected. Given that the long-term improvements in MH are reversed when focusing on the narrower group who remain workless, a possible explanation could be that some of the wider group returned to work or moved into other inactivity groups, and this caused a large positive improvement in MH for these people that exceeded the initial negative MH impact of job loss. However, this cannot explain the long-term average fall in GP visits, since this does not reverse when focusing on those who remain workless. An alternative possible explanation is that the improvement in MH and reduction in GP visits is upwardly-biased due to anticipation effects in t-1, as accounting for possible anticipation in sensitivity analysis results in these long-term effects on MH and GP visits disappearing. This conclusion is reinforced by the fact that the long-term reduction in GP visits and improvement in MH is not found when focusing on redundancy job losses, which is likely to have higher internal validity.

The finding that job loss has little/no impact on healthcare usage is a critical new finding in the UK literature, as previous UK studies on this topic suggested it caused an increase in healthcare usage, but these are outdated, have methodological limitations, and limited generalisability to the wider population (Beale & Nethercott, 1987). However, it is consistent with Iversen et al. (1989). The short-term MH impact is consistent which other studies of this topic (Flint, et al., 2013; Gathergood, 2013), but to the best of my knowledge, the finding that on average MH will return to pre-job loss levels within 1-2 years is novel as few studies explore these longer-term dynamics. In addition, I found little existing research focusing specifically on physical health effects or on how the dynamic health effects change over time, so the tentative finding that job loss may cause a gradual worsening of physical health for those who remain workless is also novel, to the best of my knowledge. However, this is consistent with Shroder (2013), who found that people who experienced involuntary job loss had worse physical health 25 years later.

It is important to consider these findings in context. The last two waves of data in this study (2020-21 and 2021-22) were affected by the COVID-19 pandemic and lockdowns, during which time face-to-face GP visits fell significantly. However, GP appointments levels have since increased again, meaning the estimated effects in this study could be an underestimate of the effects of job loss currently or in the future.

7. Conclusions and recommendations

Existing studies exploring the causal effect of job loss on health have focused mainly on the static short-term effects, leaving evidence gaps about the dynamic or longer-term effects. Recent studies have also mainly focused on MH effects, leaving gaps around the effects on physical health or usage of healthcare services, particularly in a UK setting.

This study has updated and built on existing literature by estimating the causal effects of job loss on MH, physical health, and healthcare usage in UK a setting; and by exploring how the dynamic effects change over multiple years after job loss. To do this, I used DiD with multiple periods and staggered treatment effects, as this offered important methodological advantages over existing studies. To increase external validity, I used a large panel dataset covering a range of individuals from across the UK. To increase internal validity, I examined the effects of job losses due to redundancies and argued this is likely to be exogenous of deteriorating health.

I have found no clear evidence that job losses, in general, cause any change in healthcare usage, strong evidence that they cause a temporary deterioration in MH, and some evidence that they cause a small deterioration in physical health that persists over time. The effects on all outcomes are worse for those who remain workless in subsequent years, particularly physical health. However, it's unclear whether this is because sustained worklessness causes a larger deterioration in health (compared to returning to work), or a larger deterioration in health causes sustained worklessness.

These findings improve our understanding of the true economic costs of people falling out of work, and relatedly the benefits of LM policies aiming to support workers to stay in work. This can be used to update and improve the health and healthcare impacts used in the DWP SCBA framework. These findings additionally improve our understanding of the determinants of health and healthcare usage, and suggest work outcomes should have a more prominent role in preventative NHS services.

However, there remain some important methodological limitations, which could be improved upon in future research. Despite using a large longitudinal survey, some models had small treatment groups and low statistical power. Having waves 12 months apart means I likely missed changes in work and health between waves, and could not definitively identify the temporal sequencing of work and health events. Repeating this study with a dataset that is larger and has more granular detail about changes in work and health could overcome these limitations. In particular, a population-level dataset merging HMRC/DWP administrative employment data with NHS administrative health data would provide more robust results.

These findings could also be further built upon in future research by exploring the causal dynamic impacts of other LM transitions or exploring the causal mechanisms such as how job loss affects loneliness or health behaviours. There is also likely to be heterogeneity in impacts, so further studies could explore how these results vary by job loss reason, health condition, socio-demographic characteristics, job factors, and local macroeconomic conditions.

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9. Annex

Annex A: Additional descriptive statistic tables

Table 8: % frequency of LM status, by wave.

	Wave						
LM status	**1**	**2**	**3**	**4**	**5**	**6**	**7**
Employ	70%	71%	72%	72%	73%	73%	74%
Other inactive	21%	20%	20%	19%	18%	18%	17%
Involuntary worklessness	9%	9%	8%	9%	9%	10%	9%

Table 9: % frequency of LM status transitions from previous non-missing wave to current wave, by current wave.

LM status transition	**Wave**							
	1	**2**	**3**	**4**	**5**	**6**	**7**	**Total**
Employ to Employ	0%	58%	63%	65%	66%	67%	67%	53%
Employ to Other inactive	0%	2%	2%	3%	3%	3%	3%	2%
Employ to Involuntary worklessness	0%	1%	1%	1%	2%	2%	1%	1%
Other inactive to Employ	0%	3%	3%	3%	3%	3%	3%	2%
Other inactive to Other inactive	0%	13%	13%	13%	13%	12%	12%	10%
Other inactive to Involuntary worklessness	0%	1%	1%	1%	1%	2%	1%	1%
NA to Employ	70%	9%	4%	3%	2%	2%	2%	16%
NA to Other inactive	21%	4%	3%	2%	2%	2%	2%	6%
NA to Involuntary worklessness	9%	2%	1%	1%	0%	1%	0%	2%
Involuntary worklessness to Employ	0%	1%	1%	2%	1%	2%	2%	1%
Involuntary worklessness to Other inactive	0%	1%	1%	1%	1%	1%	1%	1%
Involuntary worklessness to Involuntary worklessness	0%	5%	5%	5%	5%	5%	5%	4%
Total changes	0%	9%	11%	11%	11%	12%	12%	9%

Annex B: Detailed DiD results

Table 10: Model 1, GP visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.0187        0.0166    -0.0512      0.0138


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.0702      0.0478       -0.2044      0.0641
         -4   0.0451      0.0308       -0.0414      0.1316
         -3   0.0068      0.0246       -0.0624      0.0761
         -2   0.0111      0.0207       -0.0472      0.0693
```

```
         -1   0.0443     0.0177       -0.0053      0.0940
          0   0.0570     0.0154        0.0138      0.1001 *
          1   0.0321     0.0181       -0.0187      0.0829
          2  -0.0356     0.0200       -0.0917      0.0206
          3  -0.0551     0.0229       -0.1194      0.0092
          4  -0.0298     0.0281       -0.1089      0.0492
          5  -0.0810     0.0399       -0.1932      0.0311
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 11: Model 2, GP visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 -0.055        0.0171    -0.0885     -0.0216 *

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.0737     0.0502       -0.2158      0.0684
         -4   0.0418     0.0300       -0.0431      0.1268
         -3   0.0050     0.0239       -0.0625      0.0726
         -2  -0.0009     0.0200       -0.0573      0.0556
         -1   0.0361     0.0181       -0.0151      0.0873
          0  -0.0023     0.0151       -0.0451      0.0405
          1  -0.0012     0.0184       -0.0533      0.0508
          2  -0.0672     0.0192       -0.1215     -0.0129 *
          3  -0.0787     0.0221       -0.1411     -0.0162 *
          4  -0.0675     0.0264       -0.1423      0.0073
          5  -0.1131     0.0372       -0.2184     -0.0078 *
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 12: Model 3, GP visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.0262        0.0284    -0.0818      0.0294

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   0.0722     0.0813       -0.1576      0.3020
         -4   0.0661     0.0438       -0.0576      0.1898
         -3  -0.0075     0.0396       -0.1194      0.1043
         -2   0.0043     0.0383       -0.1040      0.1125
```

```
         -1   0.0016     0.0334       -0.0929      0.0961
          0   0.0165     0.0283       -0.0634      0.0964
          1   0.0322     0.0348       -0.0660      0.1305
          2  -0.0854     0.0372       -0.1907      0.0198
          3  -0.0426     0.0443       -0.1678      0.0827
          4  -0.0251     0.0526       -0.1738      0.1237
          5  -0.0529     0.0746       -0.2637      0.1578
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 13: Model 4, GP visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.0526        0.0351    -0.1213      0.0161


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.0718     0.0460       -0.2007      0.0571
         -4   0.0210     0.0367       -0.0818      0.1238
         -3   0.0075     0.0314       -0.0806      0.0957
         -2   0.0042     0.0288       -0.0766      0.0850
         -1   0.0352     0.0276       -0.0422      0.1125
          0  -0.0001     0.0231       -0.0649      0.0646
          1   0.0476     0.0347       -0.0498      0.1450
          2  -0.0375     0.0527       -0.1851      0.1102
          3  -0.1174     0.0526       -0.2648      0.0300
          4  -0.1002     0.0660       -0.2852      0.0849
          5  -0.1079     0.0777       -0.3258      0.1101
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 14: Model 5, GP visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 0.2689        0.0815     0.1093      0.4286 *


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   0.0721     0.0771       -0.1342      0.2784
         -4   0.0248     0.0576       -0.1295      0.1790
         -3  -0.0112     0.0738       -0.2087      0.1863
```

```
         -2   0.0354     0.0577       -0.1191      0.1899
         -1   0.0661     0.0547       -0.0803      0.2125
          0   0.0161     0.0582       -0.1397      0.1718
          1   0.1887     0.0778       -0.0196      0.3970
          2   0.1921     0.1319       -0.1611      0.5453
          3   0.0166     0.1634       -0.4208      0.4539
          4   0.1867     0.2648       -0.5222      0.8955
          5   1.0134     0.1146        0.7067      1.3201 *
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 15: Model 2, outpatient visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.0119        0.0167    -0.0447      0.0209

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.0540     0.0471       -0.1861      0.0782
         -4   0.0327     0.0344       -0.0639      0.1294
         -3  -0.0459     0.0258       -0.1184      0.0266
         -2   0.0403     0.0215       -0.0201      0.1007
         -1   0.0097     0.0182       -0.0414      0.0608
          0   0.0121     0.0154       -0.0311      0.0554
          1   0.0201     0.0196       -0.0348      0.0750
          2  -0.0232     0.0207       -0.0812      0.0348
          3  -0.0095     0.0256       -0.0812      0.0623
          4  -0.0168     0.0310       -0.1038      0.0702
          5  -0.0540     0.0365       -0.1564      0.0485
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 16: Model 3, outpatient visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 0.0013        0.0297    -0.0568      0.0594

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   0.0090     0.0831       -0.2229      0.2410
```

```
        -4  -0.0027     0.0601       -0.1705      0.1651
        -3   0.0205     0.0488       -0.1156      0.1567
        -2   0.0444     0.0373       -0.0599      0.1486
        -1   0.0018     0.0362       -0.0992      0.1027
         0   0.0102     0.0271       -0.0654      0.0858
         1  -0.0069     0.0365       -0.1088      0.0949
         2  -0.0264     0.0393       -0.1363      0.0834
         3   0.0195     0.0448       -0.1056      0.1446
         4  -0.0327     0.0535       -0.1821      0.1168
         5   0.0440     0.0720       -0.1569      0.2449
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 17: Model 4, outpatient visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.0493        0.0292    -0.1065      0.0079

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
        -5  -0.0533     0.0490       -0.1901      0.0835
        -4   0.0456     0.0394       -0.0644      0.1556
        -3  -0.0589     0.0340       -0.1540      0.0362
        -2   0.0250     0.0325       -0.0659      0.1158
        -1   0.0051     0.0284       -0.0742      0.0845
         0   0.0337     0.0241       -0.0336      0.1011
         1   0.0573     0.0367       -0.0452      0.1599
         2   0.0042     0.0441       -0.1189      0.1273
         3  -0.0224     0.0501       -0.1622      0.1174
         4  -0.1786     0.0838       -0.4127      0.0556
         5  -0.1900     0.0815       -0.4176      0.0376
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 18: Model 5, outpatient visits

```
Overall summary of ATT's based on event-study/dynamic aggregation:
 ATT    Std. Error     [ 95%  Conf. Int.]
 0.1        0.1025    -0.1009      0.3009

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
```

```
         -5   0.0092     0.0803       -0.1969      0.2153
         -4  -0.0258     0.0774       -0.2244      0.1729
         -3   0.0746     0.0725       -0.1114      0.2606
         -2   0.0199     0.0600       -0.1341      0.1739
         -1   0.0558     0.0605       -0.0995      0.2111
          0   0.0140     0.0498       -0.1140      0.1420
          1   0.1147     0.0783       -0.0864      0.3157
          2   0.1026     0.1259       -0.2205      0.4257
          3  -0.0337     0.1238       -0.3516      0.2843
          4   0.3507     0.1757       -0.1004      0.8017
          5   0.0517     0.4289       -1.0494      1.1529
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 19: Model 2, MH

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.3521        0.2422    -0.8268      0.1226

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   0.2720     0.5086       -1.1311      1.6752
         -4   0.4724     0.3784       -0.5715      1.5163
         -3  -0.9373     0.2577       -1.6481     -0.2264 *
         -2   0.4764     0.2702       -0.2691      1.2220
         -1   0.4830     0.2361       -0.1684      1.1343
          0   1.8785     0.2271        1.2521      2.5049 *
          1  -0.3941     0.2579       -1.1057      0.3175
          2  -0.4376     0.3016       -1.2697      0.3945
          3  -0.9242     0.3304       -1.8357     -0.0127 *
          4  -0.4275     0.3949       -1.5168      0.6618
          5  -1.8078     0.5447       -3.3106     -0.3050 *
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 20: Model 3, MH

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 -6e-04        0.3981    -0.7809      0.7797
```

```
Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   1.1094     0.7835       -1.0456      3.2644
         -4   1.2688     0.6033       -0.3905      2.9282
         -3  -1.3380     0.4445       -2.5606     -0.1153 *
         -2   0.1392     0.4212       -1.0192      1.2977
         -1   0.6952     0.3550       -0.2811      1.6715
          0   2.2393     0.3983        1.1438      3.3349 *
          1   0.2975     0.4468       -0.9314      1.5265
          2  -0.2282     0.4782       -1.5436      1.0871
          3  -0.6514     0.5699       -2.2188      0.9159
          4   0.2974     0.7290       -1.7077      2.3024
          5  -1.9581     0.8320       -4.2466      0.3304
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 21: Model 4, MH

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 0.8118        0.6645    -0.4905      2.1141

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   0.2688     0.4866       -1.0990      1.6366
         -4   1.0164     0.4284       -0.1878      2.2207
         -3  -1.2409     0.3689       -2.2780     -0.2038 *
         -2   0.9803     0.3900       -0.1161      2.0767
         -1   0.5144     0.3645       -0.5103      1.5390
          0   1.8877     0.3516        0.8993      2.8762 *
          1   0.7520     0.6319       -1.0245      2.5284
          2   0.8877     0.8091       -1.3868      3.1621
          3   0.5730     0.9517       -2.1024      3.2484
          4   0.9574     1.3766       -2.9127      4.8274
          5  -0.1870     1.6448       -4.8108      4.4369
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 22: Model 5, MH

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
```

```
 3.4332        0.9123     1.6451      5.2213 *


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   1.1091     0.8318      -1.0500      3.2682
         -4   1.9574     0.7617      -0.0197      3.9346
         -3  -1.3261     0.6633      -3.0476      0.3955
         -2   0.3824     0.7393      -1.5364      2.3012
         -1   0.7899     0.5967      -0.7590      2.3387
          0   2.4322     0.7143       0.5782      4.2863 *
          1   4.6988     1.1888       1.6132      7.7844 *
          2   2.8816     1.0860       0.0629      5.7003 *
          3   4.3561     1.7886      -0.2863      8.9984
          4   6.2218     2.7253      -0.8520     13.2957
          5   0.0087     0.8233      -2.1281      2.1456
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 23: Model 2, physical functioning

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.8074         0.322    -1.4386     -0.1762 *


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   1.0549     0.7988      -1.1646      3.2744
         -4   0.7649     0.4953      -0.6114      2.1413
         -3  -0.8870     0.4186      -2.0501      0.2760
         -2  -0.4367     0.3948      -1.5337      0.6603
         -1   0.0928     0.3597      -0.9068      1.0923
          0  -0.8911     0.2990      -1.7219     -0.0603 *
          1  -0.7319     0.3615      -1.7365      0.2726
          2  -0.4950     0.3710      -1.5257      0.5357
          3  -0.8715     0.4739      -2.1883      0.4452
          4  -1.3973     0.6027      -3.0719      0.2773
          5  -0.4577     0.7899      -2.6523      1.7369
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 24: Model 3, physical functioning

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 -0.953        0.5734    -2.0768      0.1707


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.8197     1.1713       -4.0887      2.4493
         -4   0.0517     0.7085       -1.9255      2.0290
         -3  -0.8262     0.6500       -2.6402      0.9879
         -2  -0.6933     0.5526       -2.2355      0.8489
         -1   0.8435     0.5308       -0.6378      2.3249
          0  -0.0601     0.3960       -1.1652      1.0451
          1  -0.6303     0.5109       -2.0561      0.7954
          2  -1.3034     0.6687       -3.1697      0.5628
          3  -1.2739     0.8440       -3.6295      1.0816
          4  -0.7169     1.0768       -3.7219      2.2882
          5  -1.7335     1.1862       -5.0440      1.5769
---
Signif. codes: `*' confidence band does not cover 0


Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 25: Model 4, physical functioning

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -1.6121         0.813    -3.2055     -0.0186 *


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   1.0332     0.7697       -1.1188      3.1853
         -4   0.4953     0.6796       -1.4049      2.3954
         -3  -0.9454     0.5901       -2.5951      0.7043
         -2  -0.2284     0.5761       -1.8392      1.3823
         -1   0.0950     0.5644       -1.4831      1.6731
          0  -1.6463     0.5452       -3.1705     -0.1220 *
          1  -2.0181     0.7742       -4.1828      0.1465
          2  -3.8008     0.9979       -6.5909     -1.0107 *
          3  -1.6930     1.0707       -4.6866      1.3006
          4  -0.6205     1.6762       -5.3069      4.0660
          5   0.1062     2.6832       -7.3957      7.6081
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Table 26: Model 5, physical functioning

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -7.7003        2.3156   -12.2389     -3.1618 *


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.8252     1.1825       -3.9887      2.3383
         -4  -0.0548     0.9571       -2.6154      2.5057
         -3   0.2084     0.9045       -2.2113      2.6281
         -2  -0.9672     1.0646       -3.8153      1.8808
         -1   0.5233     0.8790       -1.8282      2.8748
          0  -1.0182     0.8644       -3.3306      1.2943
          1  -2.7290     1.4966       -6.7328      1.2747
          2 -13.4836     3.1180      -21.8249     -5.1423 *
          3 -10.3714     3.4689      -19.6514     -1.0913 *
          4  -4.2761     2.9174      -12.0809      3.5287
          5 -14.3238    11.2226      -44.3468     15.6992
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  0
Estimation Method:  Doubly Robust
```

Annex C: Sensitivity analysis – assuming anticipation effects

To account for anticipation effects in t-1, I use t-2 as the baseline for estimating effects instead of t-1.

Table 27: Model 2, GP visits, assuming anticipation effect in t-1

```
Overall summary of ATT's based on event-study/dynamic aggregation:
     ATT    Std. Error     [ 95%  Conf. Int.]
 -0.0142        0.0194    -0.0521      0.0238


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.0732     0.0475       -0.2037      0.0573
         -4   0.0423     0.0293       -0.0382      0.1229
         -3   0.0056     0.0252       -0.0636      0.0749
         -2  -0.0002     0.0199       -0.0550      0.0546
         -1   0.0361     0.0184       -0.0145      0.0867
          0   0.0290     0.0186       -0.0221      0.0801
          1   0.0285     0.0215       -0.0306      0.0877
          2  -0.0354     0.0255       -0.1054      0.0347
          3  -0.0299     0.0306       -0.1141      0.0542
          4  -0.0630     0.0386       -0.1692      0.0431
---
Signif. codes: `*' confidence band does not cover 0
```

```
Control Group:  Not Yet Treated,  Anticipation Periods:  1
Estimation Method:  Doubly Robust
```

Table 28: Model 4, GP visits, assuming anticipation effect in t-1

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 0.0191        0.0398    -0.0589       0.097

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5  -0.0714     0.0479       -0.2055      0.0627
         -4   0.0212     0.0353       -0.0778      0.1202
         -3   0.0079     0.0323       -0.0826      0.0984
         -2   0.0043     0.0299       -0.0794      0.0881
         -1   0.0352     0.0264       -0.0387      0.1090
          0   0.0246     0.0274       -0.0522      0.1015
          1   0.0952     0.0462       -0.0341      0.2246
          2  -0.0036     0.0708       -0.2019      0.1947
          3   0.0086     0.0700       -0.1874      0.2046
          4  -0.0295     0.0984       -0.3052      0.2462
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  1
Estimation Method:  Doubly Robust
```

Table 29: Model 2, MH, assuming anticipation effect in t-1

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 0.6671         0.246     0.1851      1.1492 *

Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   0.2758     0.4795       -1.0486      1.6001
         -4   0.4844     0.3653       -0.5246      1.4934
         -3  -0.9273     0.2901       -1.7286     -0.1260 *
         -2   0.4896     0.2395       -0.1719      1.1512
         -1   0.4830     0.2357       -0.1680      1.1339
          0   2.5660     0.2623        1.8415      3.2905 *
          1   0.1649     0.2605       -0.5546      0.8844
          2   0.1642     0.3314       -0.7512      1.0796
          3  -0.2029     0.4685       -1.4968      1.0911
          4   0.6434     0.5881       -0.9810      2.2679
---
Signif. codes: `*' confidence band does not cover 0
```

```
Control Group:  Not Yet Treated,  Anticipation Periods:  1
Estimation Method:  Doubly Robust
```

Table 30: Model 3, MH, assuming anticipation effect in t-1

```
Overall summary of ATT's based on event-study/dynamic aggregation:
   ATT    Std. Error     [ 95%  Conf. Int.]
 1.351        0.5094     0.3527      2.3494 *


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   1.1087     0.8437       -1.1384      3.3558
         -4   1.2705     0.5695       -0.2465      2.7875
         -3  -1.3353     0.4378       -2.5015     -0.1691 *
         -2   0.1417     0.4059       -0.9395      1.2228
         -1   0.6952     0.3526       -0.2438      1.6343
          0   3.2548     0.4869        1.9579      4.5517 *
          1   0.9009     0.4275       -0.2378      2.0397
          2   0.6232     0.5948       -0.9612      2.2076
          3   0.4634     0.8723       -1.8601      2.7869
          4   1.5129     1.2816       -1.9008      4.9265
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  1
Estimation Method:  Doubly Robust
```

Table 31: Model 4, MH, assuming anticipation effect in t-1

```
Overall summary of ATT's based on event-study/dynamic aggregation:
    ATT    Std. Error     [ 95%  Conf. Int.]
 1.6377        0.9852    -0.2932      3.5687


Dynamic Effects:
 Event time Estimate Std. Error [95% Simult.  Conf. Band]
         -5   0.2679     0.4727       -1.0410      1.5769
         -4   1.0156     0.4413       -0.2063      2.2375
         -3  -1.2374     0.3648       -2.2476     -0.2273 *
         -2   0.9899     0.3566        0.0025      1.9773 *
         -1   0.5144     0.3698       -0.5098      1.5385
          0   2.2806     0.3647        1.2705      3.2906 *
          1   1.3527     0.6317       -0.3967      3.1021
          2   1.8640     0.9296       -0.7103      4.4384
          3   0.2990     1.5055       -3.8701      4.4682
          4   2.3923     3.2325       -6.5591     11.3438
---
Signif. codes: `*' confidence band does not cover 0

Control Group:  Not Yet Treated,  Anticipation Periods:  1
Estimation Method:  Doubly Robust
```