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Acute health shocks and labour market outcomes: Evidence from the post crash era

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ABSTRACT

We investigate the labour supply response to an acute health shock for individuals of all working ages, in the post crash era, combining coarsened exact matching and entropy balancing to preprocess data prior to undertaking parametric regression. Identification exploits uncertainty in the timing of an acute health shock, defined by the incidence of cancer, stroke, or heart attack, based on data from Understanding Society. The main finding implies a substantial increase in the baseline probability of labour market exit along with reduced hours and earnings. Younger workers display a stronger labour market attachment than older counterparts, conditional on a health shock. Impacts are stronger for women, older workers, and those who experience more severe limitations and impairments. This is shown to be robust to a broad range of approaches to estimation. Sensitivity tests based on pre-treatment outcomes and using future health shocks as a placebo treatment support our identification strategy.

1. Introduction

The relevance of health for labour market outcomes is well established in the economic literature (Currie and Madrian, 1999; Bound and Burkhauser, 1999) with empirical evidence covering a variety of countries documenting the detrimental effect of poor health and health deterioration on labour market participation (for example, Bound et al., 1999, Disney et al., 2006; Jones et al., 2010; Zucchelli et al., 2010; Lenhart, 2019). There are a number of reasons to be concerned with the determinants of labour market participation. Most significant is the possible substantial and enduring financial consequences of early labour market exit (Angelini et al., 2009), and their spillover effects on other family members both in the short- (Smith, 2005; Garcia-Gomez et al., 2013) and long-run (Morrill and Morrill, 2013; Zwysen, 2015). Labour market attachment in itself brings wider benefits to individuals, by nurturing personal identity and self-esteem, and providing opportunities for social contacts. Beyond individuals' financial and non-financial wellbeing, prolonging working lives and fostering disabled individuals' inclusion in the labour market has become a policy priority in most developed countries (OECD, 2003). This concern, which is even more pertinent in the light of population ageing and the need to limit the fiscal burden of social security provision, has led several European countries to adopt benefit reforms aimed at maintaining employment at the core of support for disabled people of working age.

Understanding the labour supply decisions of individuals following a major health shock is fundamental to informing policy around maintaining employment opportunities and contributing to reducing the employment gap between individuals with and without long-term health conditions. To this end, the relationship between health and labour supply has attracted a great deal of attention. Early empirical evidence, grounded in the theory of human capital investment, identified important associations between heath and labour market participation and wages, but was hampered by a reliance on cross-sectional data (for example, Grossman and Benham, 1973; Luft, 1975; Bartel and Taubman, 1979). More recently, the availability of rich longitudinal survey data enabling more reliable evidence on behavioural responses to

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changes in health, as well as greater understanding of the potential underlying explanatory mechanisms, has fueled interest in this important relationship.

Estimating meaningful effects of the impact of health on labour supply is, however, complex: issues such as health and economic activity being jointly determined, unobserved preferences, justification bias in survey self-reports of health status, and health-related selection into employment are typically difficult to overcome. An additional challenge is that the design and operation of pension, social benefit and welfare systems, as well as the structure of the labour market and the organisation of health and social care services all contribute to shaping labour supply decisions in response to a significant change to health (Garcia-Gomez, 2011, Cai et al., 2014, Datta Gupta et al., 2011). This is particularly pertinent given the profound impact the recent recession has had on the structure of labour markets (Immervoll et al., 2011, Jenkins and Taylor, 2012, Elsby et al., 2011, 2016) and the fiscal policy response leading to significant changes in welfare provision. However, up-to-date evidence on the causal impact of deteriorations in health on labour supply decisions in the post-recession period is sparse.

Also, the majority of the literature on the interaction of the health and the labour market has been concerned with older workers approaching retirement, with little concern for younger workers. While older workers exhibit higher morbidity risks[1], they face wider labour market exit options (i.e. in terms of eligibility for early retirement, and private and occupational pension schemes) and lower incentives to retrain for less demanding jobs. The consequences of early labour market exit for younger workers are likely to be more severe. Although survival rates have been generally improving for all ages, younger individuals exhibit lower case-fatality and mortality rates than older counterparts and have a greater number of potential years of working life remaining, making the study of their labour market outcomes of particular interest. Upon exit, younger workers typically transit into inactivity, rather than early retirement[2], possibly leading to income poverty. Beyond the immediate income loss, wider effects include foregone earnings increases, limited savings and asset accumulation and a poorer lifetime history of contributions, resulting in lower future pension entitlements. Adverse spillover effects on household members are likely to fall mainly on children rather than other adults, which may dampen intra-generational mobility. The few studies that have considered younger workers (e.g. Garcia-Gomez et al., 2010, Garcia-Gomez, 2011; Moran et al., 2011; Halla and Zweimüller, 2013) found a non-negligible response to health deteriorations with only minor differences detected with respect to the response of older workers. A potential reason for the paucity of research covering younger workers is the lack of adequate sources of data, given the relatively low incidence of sharp health deteriorations among younger workers[3] .

This paper aims to address these important gaps in the literature by providing up-to-date evidence, across all adults of working age, of the causal effects of exogenous shocks to health along both the extensive and intensive margins of labour supply, together with evidence on labour market and employer attachment, earnings, and job security of individuals remaining active in the labour market following a shock to health. The country we consider, the UK, offers a uniform policy setting characterised by a publicly funded health care system free at the point of use, with a limited role for private health insurance, in stark contrast with the US context, to which the vast majority of existing studies refer.

The recent release of Understanding Society: the UK Household Longitudinal Study (UKHLS) allows analysis of the response to a health shock across the full distribution of workers' ages, i.e. 16–65. This is possible thanks to an unique combination of a large sample size, a longitudinal dimension and a broad range of coverage including rich data on labour market experience and dimensions of health. A particular feature of the data that we exploit is that while there are a limited number of individuals experiencing a health shock (treated individuals) the data include a very large pool of potential controls. This allows us to adopt matching methods that permit a close balance of confounding covariates across treated and control individuals. This is achieved by a combination of coarsened exact matching (CEM; see Iacus et al., 2012) and entropy balancing (EB; see Hainmueller, 2012; Hainmueller and Xu, 2013). These are used in the spirit of Ho et al. (2007) to preprocess the data prior to parametric modelling to derive estimates of average treatment effects on the treated (ATTs). This approach has the attractive property of being doubly robust to one of either misspecification in the parametric model but complete covariate balance via matching, or incomplete balance through matching but correct specification of the regression model. In this context, we view matching as a means to achieve covariate balance with the intention of reducing model dependence in the subsequent regression when deriving ATTs.

To tackle the potential endogeneity of health and labour supply, our identification strategy exploits uncertainty in both the occurrence and timing of acute health shocks, defined by the incidence of cancer, stroke or myocardial infarction, which are arguably less prone to reporting bias and justification bias than many other health measures. We observe labour market active individuals until they experience a health shock during the waves of the UKHLS, and compare their labour supply responses to that observed in a matched control group. Accordingly, the only restriction we place on age is through the minimum age at which we observe an acute health shock in the data. While such shocks exclude the very young, in our sample they occur from age 30 upwards[4] .

The panel dimension of the data allows us to condition on unobserved individual heterogeneity through lagged outcomes. We treat the occurrence of an acute health shock as exogenous, conditional on observable characteristics and lagged outcomes. While the main outcome of interest is labour market participation, we also consider hours worked, earnings, perceived job security and work-related expectations and aspirations. In addition, we explore heterogeneity in labour market responses by demographic characteristics (age, gender) and health shock severity (induced impairment).

The main estimates imply a substantial increase in the baseline probability of labour market exit along with reduced hours and earnings following a health shock. These are shown to be robust to a broad range of approaches to estimation. Placebo tests based on pre-treatment outcomes and using future health shocks as a placebo treatment support our identification strategy. Our sub-group analyses show that in general younger workers display a

[1] The incidence of acute health shocks increases sharply with age (Feigin et al., 2003; Nichols et al., 2013; International Agency for Research on Cancer, 2012); for example, in the UK, more than half of cancer diagnoses relate to individuals aged between 50 and 74 years. However, non-trivial incidence rates are observed among younger adults.

[2] Due to early retirement eligibility rules, see OECD (2017).

[3] In contrast, there are a number of rich panel surveys of older people collecting information on health, labour market activity, and other domains, for example The Health and Retirement Study in the US; The English Longitudinal Study of Ageing in England; and The Survey of Health, Ageing and Retirement in Europe, in Europe.

[4] While the full sample for analysis spans ages 16 to 65, the matched sample is restricted to the common support, which results in ages ranging from 30 to 65, because the earliest observed health shock occurs at age 30.

stronger labour market attachment than older counterparts, conditional on a health shock. Impacts are concentrated among those whose shocks are associated with severe limitations and impairments.

2. Acute health shocks and employment

Studying the effect of health on labour market behaviour requires dealing with the endogeneity of health with respect to labour supply (Haan and Myck, 2009; Cai, 2010). Previous studies have addressed this potential source of bias using a variety of approaches. Strategies have included modelling labour market outcomes by exploiting variation in self-assessed health (Au et al., 2005, Lenhart, 2019) or satisfaction with health (Riphahn, 1999); the onset of health conditions (Garcia-Gomez, 2011); acute hospitalization episodes (Garcia-Gomez et al., 2013); and car accidents (Dano, 2005; Halla and Zweimüller, 2013).

We follow previous studies (Smith, 1999, 2005, Coile, 2004, Datta Gupta et al., 2011; Trevisan and Zantomio, 2016) and exploit, as a source of exogenous variation, major health shocks measured by the incidence of a cancer, stroke or myocardial infarction. The focus on these particular health conditions is motivated by two reasons. First, they occur suddenly and largely unexpectedly - in the case of stroke and myocardial infarction due to the nature of the condition; in the case of cancer, due to its often asymptomatic nature it typically becomes known upon diagnosis. Indeed, these conditions can be regarded as unanticipated shocks with respect to the timing of onset, as risk factors that might inform an individual about their health risk are largely uninformative with respect to the timing of the event. Second, given their nature as major health conditions, they are arguably less exposed to the chance of misreporting and justification bias than milder conditions (Baker et al., 2004; Bound, 1989, 1991; Benitez-Silva et al., 2004).

Other studies that exploit acute health shocks often find a reduction in labour supply following the occurrence of a health event. The estimates of Smith (2005) and Coile (2004) are based on parametric modelling of the US Health and Retirement Study (HRS) data. Smith estimates a 15 percentage points immediate decline in labour market participation for older workers, following the onset of cancer, heart attack, stroke or lung diseases. Coile (2004) finds men to be 35 percentage points and women to be 23 percentage points more likely to exit the labour market after experiencing a major health shock (stroke, cancer or heart attack). Datta Gupta et al. (2011) adopt similar methods to compare older workers in the US and Denmark, and relate the stronger retraction in participation found for US workers (a counter-intuitive result when the institutional differences between the two countries are considered) to differential mortality and baseline health differences. Trevisan and Zantomio (2016) use propensity score matching and combine data from the Survey of Health, Ageing and Retirement in Europe (SHARE) and the English Longitudinal Study of Ageing (ELSA) to investigate the case of older workers in sixteen European countries. They find a significant reduction in labour market participation, amounting to 12 percentage points on average, with the strongest effects found for highly educated women, and in countries providing more generous disability benefits.

The studies above have considered the labour supply responses of older workers only. The few studies that have considered younger workers (for example, Garcia-Gomez et al., 2010, Garcia-Gomez, 2011; Moran et al., 2011; Halla and Zweimüller, 2013) found a non-negligible response to health deteriorations with only minor differences detected in comparison to the response of older workers. A related strand of research, covering younger as well as older workers, has been evolving with respect to cancer (mostly breast cancer) survivors, generally using US data (Bradley et al., 2002, 2005, 2013; Farley Short et al., 2008; Moran et al., 2011, Heinesen and Kolodziejczyk, 2013). These studies have largely relied on administrative register data and have applied a number of approaches, including matching techniques, to select appropriate controls for cancer survivors observed within population surveys[5]. Focusing on breast cancer survivors in the US and using a number of alternative data sources, Bradley et al. (2002, 2005, 2013) find a negative impact on employment, but also a greater number of hours supplied and higher wages for survivors who remained in the labour market. These results point to a need for more detailed consideration of the selection mechanisms and heterogeneity in labour market responses to health shocks. Conditioning on a single specific health condition, such as breast cancer, might ensure stronger internal validity given the greater knowledge about condition-specific health effects and treatments. However, this may come at the cost of sacrificing generalizability.

3. Data

The analysis is based on seven waves of Understanding Society: the UK Household Longitudinal Study (UKHLS, University of Essex, 2015) that builds on the British Household Panel Study (BHPS). The BHPS has been widely used in the study of health and labour (e.g. Disney et al., 2006; Jones et al., 2010; Garcia-Gomez et al., 2010; Robone et al., 2011; Bender and Theodossiou, 2014, Dawson et al., 2015, Lenhart, 2019).

The large sample size of UKHLS (circa 100,000 individuals) offers the opportunity to study sub-groups of the population previously regarded as too small for analysis using population based surveys (Buck and Mc Fall, 2012), capturing for example, heterogeneity in labour market responses to health shocks at different points in the lifecycle. Our UKHLS sample includes seven waves of annual data spanning 2009–2016, thus including the recession employment dip visible in Fig. 1.

The fieldwork for each wave is undertaken over two calendar years, with CAPI interviews for each household held in each wave. Together with a household questionnaire, all adults aged 16 or older are given an individual questionnaire. These questionnaires cover a wide range of topics including demographic characteristics, educational background, health, disability, labour market activity, job characteristics, and incomes and their sources.

The first time individuals are interviewed they are asked about past diagnoses of specific health conditions, including cancer, heart attack or myocardial infarction, and stroke[6]. This allows us to identify individuals who have already experienced the onset of a health shock. In subsequent waves individuals are asked whether, since the previous interview, they have been newly diagnosed as having any of the same list of conditions so that a full annual history of the onset of acute health shocks is observed. In addition information about health risk factors, such as diagnoses of coronary heart disease, angina, diabetes and high blood pressure, mostly relevant for CVD, is also collected[7].

Further information concerning health risk includes parents' longevity (individuals are asked whether the mother and the father were alive when the respondent was aged 14), indicative of genetic

[5] Health and Retirement Survey, Current Population Survey or the Panel Study of Income Dynamics.

[6] The full list includes: Asthma; Arthritis; Congestive heart failure; Coronary heart disease; Angina; Heart attack or myocardial infarction; Stroke; Emphysema; Hyperthyroidism or an over-active thyroid; Hypothyroidism or an under-active thyroid; Chronic bronchitis; Any kind of liver condition; Cancer or malignancy; Diabetes; Epilepsy; High blood pressure; Clinical depression.

[7] Congestive heart failure represents more of a consequence, than a risk factor, for infarction, but for this same reason it might capture unobserved factors correlated with CVD risk.

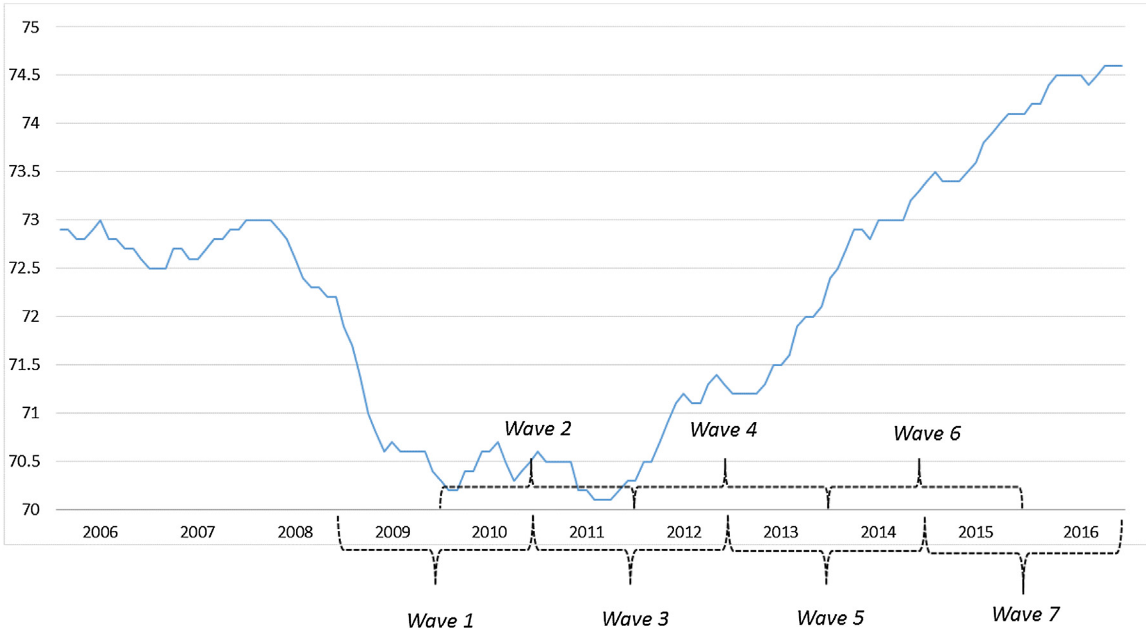

Fig. 1. UKHLS fieldwork and employment rate (ages 16–64) seasonally adjusted (ONS).

factors; a battery of standard health indicators, covering poor self-assessed health, the presence of a long-standing illness or disability, eleven types of limitations in activities of daily living (ADLs); and information about health habits and behavioural risk factors, via past and current[8] smoking participation and intensity, that are also indicative of time preferences.

We make use of demographic information including age, gender, race, marital status, number of children, and household size, together with socioeconomic characteristics including highest educational qualification, individual and household income from various sources, and housing tenure. With respect to labour market activity, at each wave respondents are asked about employment status (including self-employment), type of occupation, the number of hours worked (including overtime hours, both paid and unpaid), earnings, job satisfaction and other job and employer characteristics. At alternate waves an additional set of employment related questions are asked to employees about job conditions, covering their aspirations, expectations and perceived job security[9] .

4. Empirical strategy

The sample for analysis is restricted to individuals who are observed for at least two points in time, labelled *t-1* and *t*. These can be any consecutive waves across the seven waves for which we have observations. In addition, the sample is restricted to individuals who are labour market active, either as employees or self-employed, as of *t-1*, and who would be aged less than statutory retirement age as of time *t*.

Our empirical approach exploits acute health shocks, occurring between *t-1* and *t*, to identify the short run labour supply response, observed at times *t, t + 1, t + 2* and *t* + 3. We compare outcomes for individuals who experience an acute health shock (treated) with outcomes for observationally identical (as of *t-1*) individuals, who do not experience an acute health shock (control individuals). Pre-shock observational equivalence is defined by a wide set of potential confounders, including demographic and socioeconomic characteristics, underlying health risk factors, previous acute health shock history, as well as variables informative about labour market activity and labour market attachment.

Our identification strategy relies on the assumption that conditional on the set of confounding variables and lagged outcomes, the occurrence of a health shock can be treated as exogenous. In principle, outcomes could be regressed on treatment conditional on the set of confounding variables to recover the treatment effect. This approach, however, requires a number of potentially restrictive assumptions about model specification,[10] which in practice often amounts to an assumption that we know the correct model - an assumption that is difficult to verify. Attempting to derive causal effects from such an approach is therefore highly model dependent where alterations to the specification may produce different causal inferences. To ameliorate such problems and reduce model dependency we follow the approach set out in Ho et al. (2007). The essence of the approach is to use information in the set of control variables to preprocess the data prior to parametric modelling.

The aim of preprocessing is to reduce model dependence by using matching methods to create balance in covariates across treated and control individuals. Successful matching renders the treatment variable closer to being independent of control variables. Subsequent parametric regression modelling of the preprocessed data is therefore less dependent on specification assumptions and hence more likely to identify causal effects. Ho et al. (2007) set out three advantages of preprocessing data prior to parametric inference. First, the approach is straightforward to implement and only requires including a preprocessing step prior to running the parametric analysis a researcher would usually

[8] More precisely, as of Wave 2 or 5.

[9] UKHLS contains additional potentially relevant variables, for example mental health as measured by the GHQ instrument, biomarkers, and alcohol consumption. We do not, however, include these in the main analysis as they impose a reduction in sample size through a combination of being collected through the self-completion questionnaire (which registers significantly lower response rates); from a subset of respondents only or at a specific wave only (for example biomarkers).

[10] Such assumptions include correct specification of covariates, their interactions and non-linear terms, functional form for the regression and parametric distributional assumptions.

Table 1
Descriptive statistics: health risk variables.

	Health shocked		Potential controls		
	(n = 480)		(n = 81,162)		
	mean	s.d.	mean	s.d.	Pval (diff)
Age	50.28	9.51	42.11	11.54	0.000
Male	0.48	0.50	0.47	0.50	0.431
Father dead when respondent aged 14	0.06	0.25	0.03	0.17	0.000
Mother dead when respondent aged 14	0.01	0.11	0.01	0.11	0.779
Ever been a smoker	0.61	0.49	0.53	0.50	0.001
Whether currently a smoker	0.26	0.44	0.20	0.40	0.001
Has been a regular smoker in the past	0.26	0.44	0.21	0.40	0.003
Whether smoked heavily either currently or in the past	0.14	0.35	0.07	0.26	0.000
Self assessed poor health(t-1)	2.78	1.08	2.30	0.95	0.000
Number of limitations(t-1)[a]	0.46	1.13	0.20	0.70	0.000
Has long standing(t-1) illness/disability(t-1)	0.40	0.49	0.23	0.42	0.000
Ever diagnosed high blood pressure, until (t-1)	0.23	0.42	0.12	0.33	0.000
Ever diagnosed diabetes, until (t-1)	0.10	0.30	0.03	0.18	0.000
Ever diagnosed congestive heart_failure, until (t-1)	0.01	0.10	0.00	0.02	0.000
Ever diagnosed coronary_heart_disease, until (t-1)	0.04	0.20	0.00	0.05	0.000
Ever diagnosed angina, until (t-1)	0.04	0.19	0.00	0.07	0.000

Note: Variables in bold if *t*-test of equality of means between treated and controls rejected at the conventional 5% level.
[a]Counts limitations in activities of daily living, up to 12, including personal care, mobility, and cognitive tasks.Source: UKHLS, waves 1–7.

undertake. Second, by reducing the link between confounding variables and the treatment variable, preprocessing makes inference on subsequent parametric analysis less dependent on modelling choices and assumptions.[11] Finally, as preprocessing is undertaken by matching methods, the potential for bias is reduced when compared to parametric methods based on analysis of unmatched data. The idea of undertaking parametric modelling on preprocessed (balanced) data can be seen as an extension of commonly used matching approaches, which tend to rely on a simple comparison of means of the matched data.[12] Extending the approach to including a parametric regression of outcomes on the preprocessed data simply aids the identification of treatment effects where matching is not exact and covariate balance across treated and control individuals may not be perfect.[13] Parametric modelling following preprocessing in such circumstances will ameliorate any residual confounding caused by any remaining lack of balance in covariates.

Data preprocessing relies on methods for matching to create greater balance across control variables. We achieve this through a combination of coarsened exact matching (CEM) and entropy balancing (EB) to ensure common support and adequate covariate balance. Hainmueller (2012) suggests that coarsened exact matching can be run first to discard extreme observations and then followed up with entropy balancing on the reweighted data to better balance the covariates. Parametric regression analysis on the balanced data is subsequently undertaken to estimate the impact of health shocks on labour supply outcomes. Ho et al. (2007) describe this two-step approach as being doubly robust. That is, if matching is correct, but the subsequent regression is misspecified, or if matching is incomplete, but the specifications of the regression model is correct, treatment effect estimates will be consistent.

While all individuals start as untreated in the first wave, an individual is assigned only once[14] to the treatment group when their first observed health shock within the UKHLS sampling period occurs; treated individuals never act as potential controls at any other point in time. Potential control individuals are those who are never shocked while they are observed in the UKHLS survey.

Observability of all potential confounders, that is variables potentially affecting both labour market behaviour and the risk of experiencing an acute health shock, is crucial to the success of the empirical strategy. The approach, as with standard regression based modelling approaches, relies on an ignorability (conditional independence) assumption that there exists no omitted variables conditional on the treatment and control variables. This assumption is common in much applied research attempting to identify causal effects in observational data. Accordingly, the set of controls needs to be sufficiently comprehensive such that, conditional on these, variation in the occurrence or otherwise of an acute health shock can be regarded as ignorable. As illustrated in Section 3, the broad topic coverage of the UKHLS questionnaire is appealing in this respect. All of the time-varying potential confounders are measured as of *t-1*; the longitudinal dimension of the data allows us to control for time invariant unobservables through conditioning on some of the lagged outcomes to capture variation associated with unobserved covariates that are correlated with the lagged outcomes (O'Neill et al., 2016).[15]

A further requirement to ensure the success of our matching strategy is achieving common support and the availability of an adequate number of potential control individuals to achieve this. Despite the large samples available in UKHLS, the number of individuals observed to experience one of the major acute health shocks is limited to 480, which while small is not out of line with that of similar studies. The study does, however, offer a large pool of potential controls (81,162 individuals). Table 1 reports definitions and descriptive statistics for the set of health risk related conditioning covariates in the treated and potential control group. Striking differences in pre-shock health risks, including age,

[11] Where data are sufficiently numerous and of sufficient quality to allow exact matching across all confounding variables between control and treated individuals, subsequent estimates of treatment effects should not vary across different model specifications.

[12] In this context, matching is not a method of estimation and can be seen merely as a means to create balance in covariates. Ultimately, matching needs to be combined with some form of estimation to recover effects of interest.

[13] In the absence of exact matching on all treated units, a degree on imbalance across some or all of the covariates will remain. This is the situation often faced in practice and one where parametric regression following matching is well suited.

[14] Any additional health shock onset for the same individual is ignored.

[15] As explained in O'Neill et al. (2016) this represents an alternative to using a Difference in Differences approach for conditioning on time invariant unobservables.

Table 2
Descriptive statistics: other variables.

	Health shocked (n = 480)		Potential controls (n = 81,162)		
	mean	sd	mean	sd	Pval (diff)
Cohabiting with spouse/partner(t-1)	0.74	0.44	0.71	0.45	0.24
Household size (t-1)	2.90	1.30	3.11	1.37	0.00
Number of children (t-1)	1.92	1.35	1.45	1.28	0.00
Highest educational qualification: degree	0.28	0.45	0.34	0.47	0.01
Highest educational qualification: other_higher	0.14	0.34	0.14	0.35	0.76
Highest educational qualification: A levels	0.19	0.39	0.22	0.41	0.10
Highest educational qualification: GCSE	0.22	0.41	0.19	0.40	0.22
Highest educational qualification: other	0.11	0.31	0.07	0.25	0.00
No educational qualification	0.07	0.26	0.04	0.20	0.00
White	0.89	0.31	0.84	0.37	0.00
Equivalent household monthly income (t-1)[b]	2332	1664	2366	1572	0.63
Social renter (t-1)	0.14	0.35	0.11	0.32	0.03
Home owner (t-1)	0.77	0.42	0.75	0.44	0.21
Usual hours worked per week, including overtime(t-1)	36.83	14.49	36.02	13.94	0.20
Job satisfaction (t-1)[c]	5.28	1.49	5.29	1.43	0.90
Whether job is non-temporary (t-1)[d]	0.94	0.23	0.92	0.27	0.07
Type of occupation: management & professional (t-1)[e]	0.44	0.50	0.43	0.49	0.50
Type of occupation intermediate (t-1)[e]	0.23	0.42	0.23	0.42	0.74
Type of occupation routine (t-1)[e]	0.32	0.47	0.34	0.47	0.33
Employee (versus self-employed) (t-1)	0.87	0.33	0.88	0.33	0.77
Net monthly labour earnings (employees) (t-1)[f]	1519	1293	1479	1007	0.36
Year of interview (t)	2013	1.8	2012.8	1.8	0.14
Wave	4.16	1.68	4.27	1.71	0.17
Elapsed months since previous interview	13.34	4.93	12.64	3.34	0.00

[b]gross household income in month before interview, equivalised using the so-called 'modified OECD scale'; [c] measured on an increasing 7 points scale ranging from 'completely dissatisfied' to 'completely satisfied'; [d] as reported by respondent; [e] Corresponding to the National Statistics Socio-economic Classification (NS-SEC); [f] usual net pay per month in current employee job (nominal).*Source: UKHLS, waves 1–7.* Notes: Variables in bold if *t*-test of equality of means between treated and controls rejected at the conventional 5% level.

father's longevity, smoking status, general health and past diagnosed conditions are clearly evident.

Definitions and descriptive statistics for the set of other potential conditioning covariates are reported in Table 2. Again there are significant differences across the two groups with respect to household composition, education, race, and social renting. These point to a less advantaged socioeconomic situation for those who are likely to experience the onset of a health shock. These individuals also exhibit a greater lapse of time between the two observational points, *t-1* and *t*. This may reflect the occurrence of the health shock leading to postponement of the interview.

It is notable and encouraging that no statistically significant differences emerge, however, with respect to pre-treatment labour market variables. This provides an indication that systematic selection bias according to labour market outcomes may not be problematic. Nevertheless, the next section describes the selection of appropriate controls for each treated individual from the large pool of potential controls.

4.1. Implementation

The goal of matching is to improve balance in the covariate distribution of treated and control individuals while minimizing data loses due to a lack of suitable matches for treated individuals. Accordingly, covariate balance is an important measure by which different matching algorithms can be compared (Imai et al., 2008). In principle the many available matching routines could be applied to our data and evaluated on the basis of achieved balance. Our choice of method is informed both by data considerations and a desire to match as precisely as possible a subset of covariates thought, *a prior*, to be particularly strong confounders.

An important practical consideration is that we have a far greater pool of potential controls at our disposal than individuals experiencing a health shock (treated individuals). This has a number of advantages that we are able to exploit. First, it enables us to consider matching routines that lead to greater balance in covariates but which are data hungry. In principle, exactly matching controls to treated individuals on all confounding variables produces perfect balance across the distribution of covariates. This approach is clearly data intensive where there are numerous confounding variables to consider and in practice is often not tenable due to treated individuals being discarded because no matches are available. This can lead to a more restricted definition of the estimated ATT applicable to the subset of treated individuals for whom controls can be found (see Rosenbaum and Rubin, 1985).

Coarsened Exact Matching (CEM) which locates exact matches within pre-defined strata for continuous confounders and mimics exact matching for discrete variables, offers a useful extension to exact matching. Given the large proportion of potential controls to treated individuals we are able to implement this approach in combination with other matching methods. Secondly, the large pool of potential controls allows for multiple matches per treated individual. This is preferable to one-to-one matching as it can reduce variance without necessarily compromising on bias. Thirdly, the large set of potential controls combined with the use of entropy balancing (EB) with CEM enables us to consider many confounding variables. All variables thought to affect both the treatment assignment (into a health shock) and, controlling for the treatment, the outcome of interest should be included in the matching exercise.[16] A conservative approach often adopted by researchers is to include many potential confounders as even variables weakly associated with treatment assignment have been shown to usually reduce bias more than increase variance (Rubin and Thomas, 1996; Heckman et al., 1998). Again, however, this is only possible in practice where the set of potential controls is considerably larger than

[16] Variables thought to be affected by treatment should not be included in the set of matching variables, to avoid introducing post-treatment bias.

the set of treated individuals (in our case, an average of 150 potential controls for each treated individual). To exploit these advantages which our data affords, we use a combination of CEM and EB. The properties of CEM are highlighted below.

While traditional matching methods typically imply a trade-off in the balance achieved across different conditioning variables, the CEM approach (Iacus et al., 2011, 2012) allows us to reduce the imbalance in any chosen confounder with no detrimental effect on the balancing of others. This monotonic imbalance bounding property is achieved by coarsening selected variables into meaningful groups and performing exact matching on the coarsened data, so that balance is achieved in the full joint distribution of coarsened variables, accounting for interactions and nonlinearities. Clearly, as the number of confounders increases, CEM may result in a progressively reduced sample size as exact matches with the set of potential controls become more difficult to locate.

In our setting CEM is employed to ensure that adequate balance is achieved with respect to confounders deemed most relevant, a priori, based on epidemiological and medical evidence, for capturing endogenous selection into experiencing an acute health shock. Firstly, these include age and gender which are known to shape the incidence and prevalence patterns of myocardial infarction (Smolina et al., 2012), stroke (Appelros et al., 2009; Feigin et al., 2003) and cancer (Curado et al., 2007; ACS, 2017). But also the other risk factors observed in the survey and known to significantly increase the incidence of these conditions (WHO, 2002). One behavioural risk factor known since the 1970s to affect all three conditions is tobacco use (Peto et al., 2003; Secretan et al., 2009). Also, acute shocks for these conditions lead to an increased risk for people who have experienced a past health event for the same condition (Rheingold et al., 2003; Castellino et al., 2002; Burn et al., 1994). Risk factors that are specific to CVD shocks i.e. infarction and stoke include high blood pressure (Lewington et al., 2002) and diabetes (Yusuf et al., 2004). Past diagnosis of angina or coronary heart disease, sharing similar underlying causes as infarction, also signal a possibly higher risk of these two CVDs (Braunwald et al., 2015).

As a first preprocessing step we perform CEM on year (to avoid matching individuals from different points in time), age (coarsened into 5 age groups, with thresholds set at 25, 35, 45 and 55), gender, being (or having been) a heavy smoker, lagged self-assessed health (coarsened into 3 groups), past experience of an acute health shock, and diagnosis of at least one of the following: high blood pressure, diabetes, congestive heart failure, coronary heart disease, angina. In practice, for the dummy variables (the majority of those considered here) and year, CEM corresponds to exact matching. This first step leads to a stratification of the sample into 859 strata. For 237 of these strata we observe both treated individuals as well as potential controls. To ensure common support, the remaining 622 strata (for which only observations from the set of potential controls are observed) are omitted from further analysis. This comes at the trivial cost of excluding only a single treated individual from further analysis. Details on the number of treated and control units, and their distribution in the successfully matched strata are shown in Table 3 (on the left and right respectively).

This first preprocessing step invokes common support and balancing in the joint distribution of the basic set of confounders. While avoidable bias is generally reduced, it potentially remains with respect to other confounders, as illustrated in Table A.1 in the Appendix.[17] To ensure adequate balance across these other covariates we combine the initial CEM step with entropy balancing across all of the observed covariates.

Table 3
First CEM round.

	#treated	#controls	*By stratum:*	#treated	#controls
All	480	81,162	mean	2	227.9
Matched	479	54,021	median	1	92
Unmatched	1	27,141	min	1	1
			10th perc.	1	4
			25th	1	4
			75th	2	1,655
			90th	4	1,702
			max	12	2,052

Source: UKHLS, waves 1–7.

The method of entropy balancing (EB; see Hainmueller, 2012; Hainmueller and Xu, 2013) is based on a maximum entropy reweighting scheme. This selects a set of weights w_i for each observation i in the control group that minimize an entropy distance metric:

$$\min_{w_i} H(w) = \sum_{[i|T_i=0]} w_i log(w_i/q_i)$$

where T_i is a binary indicator taking value 1 if the individual belongs to the treatment group, and 0 if the individual belongs to the control group and $q_i = 1/n_0$ is a base weight. Minimization is subject to a set of R balance constraint imposed on the covariates moments as in

$$\sum_{[iT_i=0]} w_i * c_{r\ i}(X_i) = m_r \;\; r \in 1 \ldots .R$$

where $c_{r\ i}(X_i) = m_r$ indicates the contraints on covariate moments imposed on the reweighted control group: usually that the sample mean of each covariate should be equal for treatment and control group; this can be augmented to balance other moments such as the variance and skewness. Also, normalizing constraints ensure that the weights are non-negative and sum to 1.

$$\sum_{[iT_i=0]} w_i = 1, w_i \geq 0 \;\; \forall i|T_i = 0$$

Numerical implementation of the method is presented in Hainmueller (2012) and computation in Hainmueller and Xu (2013).

We note that the EB method focuses on the univariate marginal distributions of each separate covariate and can be used to generate weights that ensure that the sample means for each are balanced between the treated and controls. In contrast the CEM method is more general in that it balances on the multivariate histogram for the joint distribution of the covariates and ensures that all higher moments and co-moments/interactions between the covariates are balanced as well. These co-moments can be accommodated in the EB approach by including interaction terms in the balance constraints. In our application of the EB algorithm we include first order interactions between the key covariates that are used at the CEM stage of the algorithm. Weights from the CEM stage are used as base weights and the weights that are generated by the EB algorithm are saved for use in the reweighted parametric regressions. No treated observations are excluded at this stage and each receives a weight of 1. A summary of overall balancing achieved, for each confounder, in terms of difference in means and bias, measured as standardised percentage difference in means, is presented in Table 4[18] . As can be seen, by construction, entropy

[17] CEM on all confounding variables is not possible due to the dimensionality of the matching problem.

[18] See also Figures A1-A4 in the Supplementary Material for the empirical Quantile-Quantile plot, obtained pre- and post- preprocessing, for each continuous confounder.

Table 4
Overall balancing of covariates following CEM & EB.

	Mean difference		*Bias*	
	Unbalanced	Balanced	Unbalanced	Balanced
Age	8.164	0.00	77.2	0.00
Male	0.018	0.00	3.6	0.00
Father dead when respondent aged14	0.035	0.00	16.3	0.00
Mother dead when respondent aged14	0.001	0.00	1.2	0.00
Ever been a smoker	0.075	0.00	15.3	0.00
Whether currently a smoker	0.064	0.00	15.1	0.00
Has been a regular smoker in the past	0.055	0.00	13.0	0.00
Whether smoked heavily either currently or in the past	0.068	0.00	22.4	0.00
Self assessed poor health(t-1)	0.475	0.00	46.6	0.00
Number of limitations(t-1)	0.260	0.00	27.6	0.00
Has long standing(t-1) illness/disability(t-1)	0.169	0.00	36.9	0.00
Ever diagnosed high blood pressure, until (t-1)	0.111	0.00	29.3	0.00
Ever diagnosed diabetes, until (t-1)	0.066	0.00	27.1	0.00
Ever diagnosed congestive heart_failure, until (t-1)	0.010	0.00	13.4	0.00
Ever diagnosed coronary_heart_disease, until (t-1)	0.041	0.00	27.2	0.00
Ever diagnosed angina, until (t-1)	0.033	0.00	23.0	0.00
Cohabiting with spouse/partner(t-1)	0.024	0.00	5.4	0.00
Household size (t-1)	−0.203	0.00	−15.2	0.00
Number of children (t-1)	0.475	0.00	36.0	0.00
Highest educational qualification: degree	0.405	0.00	20.4	0.00
White	0.056	0.00	16.5	0.00
Equivalent household monthly income (t-1)	−34.800	0.00	−2.1	0.00
Social renter (t-1)	0.032	0.00	9.5	0.00
Home owner (t-1)	0.025	0.00	5.8	0.00
Usual hours worked per week, including overtime(t-1)	0.812	0.00	5.7	0.00
Job satisfaction (t-1)	−0.008	0.00	−0.5	0.00
Whether job is non-temporary (t-1)	0.023	0.00	9.0	0.00
Type of occupation: management & professional (t-1)	0.015	0.00	3.1	0.00
Type of occupation intermediate (t-1)	0.006	0.00	1.5	0.00
Type of occupation routine (t-1)	−0.021	0.00	−4.5	0.00
Year of interview (t)	−0.100	0.00	−6.8	0.00
Wave	−0.108	0.00	−6.4	0.00
Elapsed months since previous interview	0.699	0.00	16.6	0.00

Bias: standardized percentage difference in means between treated and controls.Source: UKHLS, waves 1–7.

balancing ensures equality of the samples means of all of the covariates between the treated and control samples.[19]

Finally, to estimate the ATT of an acute health shock we estimate parametric regression models (via probit or OLS depending on the binary or continuous nature of the outcome) on the preprocessed data using the weights obtained as an output from the combined CEM-EB algorithm and clustering by individual identifier. For binary outcomes, once the counterfactual outcome is predicted for each treated unit, based on the estimated non-linear model[20], the ATT is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals. Formally, the probit model can be written as:

$$Pr(Y_i = 1x_i) = \Phi(x_i\beta)$$

where Y denotes the binary outcome of interest and x the set of explanatory variables which includes both the binary treatment indicator T_i as actually observed in the data, and the full set of conditioning variables. The estimated $\hat{\beta}$ coefficients, estimated on the joint sample of treated and matched control observations, feed into the ATT computation as in:

$$ATT(Y) = \frac{1}{N_1}\sum_{i:\ T_i=1}\left[Y_i - \Phi\left(x_i^0\hat{\beta}\right)\right]$$

where N_1 denotes the number of treated individuals, and x_i^0 includes both the full set of conditioning variables and the treatment indicator set to $T_i = 0$, so that $\Phi\left(x_i^0\hat{\beta}\right)$ measures, for each individual who actually experienced the health shock, the predicted counterfactual outcome (i.e. under no health shock). In the case of continuous outcomes (such as hours of work or earnings measures) the ATT corresponds to the OLS coefficient estimated on the treatment indicator.

This approach, in contrast to a purely nonparametric comparison of weighted means in the preprocessed treated and control groups, allows us to condition further on the set of observable and time-invariant unobservable confounders, proxied by lagged outcomes, to account for any remaining imbalance. We follow Ho et al. (2007) and use standard methods to compute standard errors for inference on the ATTs derived from the regression models estimated on the preprocessed data (with appropriate weights as described above). Since preprocessing only affects the data by balancing on the confounders, the set of covariates can be considered fixed as can the preprocessing procedure.[21] This is akin to the usual assumptions in

[19] It is common for researchers to report tests of the null hypothesis of mean equivalence in the distribution of covariates between treated and matched controls. We follow Imai et al. (2008) (also see Ho et al., 2007) and do not report such statistics. As covariate balancing is a characteristic of a specific sample rather than a hypothetical population, hypothesis tests are misplaced (something Imai et al., 2008, term the balance test fallacy). In addition, in the absence of exact matching, balancing can always be improved for a given sample at least in principle and the closer the distribution of a covariate in the treatment group is to the corresponding distribution in the control group the better. Further permutations of matching may bring about better balance, irrespective of a test of mean difference following any particular matching attempt.

[20] Results from a sensitivity check, where OLS modelling has been used also for binary outcomes, are reported in Table A.2 in the Appendix (to be compared with Table 5).

[21] This views matching algorithms not as estimation techniques, but simply as methods to reduce covariate imbalance. The choice of matching approach is based on whichever procedure results in maximum balance. Accordingly, matching approaches that lead to less than maximum balance can be discarded and should not play a role in inference (see Ho et al., 2007, for a discussion).

Table 5
ATT after one year, overall sample.

	n (treated)	n (controls)	ATT	Std. Err.	P val	Relative effect
Labour market participation	479	54,013	**−0.03**	0.01	0.02	*−3.3*
Hours, unconditional on LMP	476	53,503	**−2.04**	0.66	0.00	*−6.0*
Hours, conditional on LMP	424	50,801	**−0.94**	0.48	0.05	*−2.6*
Limitations	478	53,999	**0.44**	0.06	0.00	*100.4*
Disability Benefit	476	53,875	**0.07**	0.01	0.00	*193.5*
Cond on LMP:						
Give up paid work (would like)	203	28,287	−0.01	0.03	0.65	*−3.5*
Give up paid work (expects)	201	28,110	**0.05**	0.02	0.01	*124.7*
Change employer and job (would like)	203	27,926	−0.04	0.03	0.13	*−15.1*
Change employer and job (expects)	196	27,128	0.00	0.02	0.95	*1.3*
Job satisfaction	424	51,186	0.00	0.07	0.97	*0.0*
Bad feelings about job	197	28,296	**−0.98**	0.29	0.00	*−8.9*
Cond on LMP, employees only:						
Perceived job security (1 to 4)	167	23,399	**−0.13**	0.06	0.03	*−4.0*
Earnings, unconditional on LMP	416	45,626	**−95.38**	33.67	0.01	*−6.8*
Earnings	373	43,359	**−64.46**	28.28	0.02	*−4.2*
Hourly earnings	372	43,041	−0.55	1.34	0.68	*−1.3*

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure is repeated for outcomes whose reference population is limited to employees only.
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

standard regression approaches where covariates are assumed fixed and exogenous. Standard errors and confidence intervals can then be computed in the usual way when applying parametric regression, but to the preprocessed data.

5. Results

5.1. Overall effects

Table 5 reports the main results for the various outcome measures we consider[22] . As a preliminary consideration, the onset of an acute health shock significantly and substantially increases the number of ADLs (approximately doubled, with respect to the baseline value), as well as disability benefit receipt (approximately tripled, with respect to the baseline value), confirming that the health conditions on which we focus do indeed capture non-trivial health deteriorations. On average, experiencing an acute health shock leads to a 0.03 reduction[23] in labour market participation (and consequent decrease in unconditional hours worked) and a reduction in the number of hours, for those who keep on working[24] . Our point estimate for labour market participation reduction is lower than found in several previous studies (which considered older workers only, and mostly before the onset of the recent economic crisis), although comparable to results obtained by Lenhart (2019) for UK workers. Indeed, the effect we estimate is by no means trivial: compared to the baseline labour market exit probability (7.47%), experiencing an acute health shock increases the risk of leaving the labour market by around 40 per cent.[25] Also, in contrast to Lenhart's (2019) results covering the pre-crisis years in the UK, we do find a small yet significant response also along the intensive margin of labour supply, i.e. a 3 percentage points reduction in hours worked by those who continue labour market activity after the health shock.

In addition to labour supply we estimate the impact of acute health shocks on job-related aspirations and expectations, job satisfaction and a measure of `feelings' about one's own job. As most of these indicators stem from questions administered at alternate waves only, the sample sizes available to estimate the ATTs are smaller than for labour supply. An increase in the expectation to give up paid work, despite not wishing to do so, is revealed. At the same time, health-shocked individuals are not more likely to wish a change in employer, or to expect doing so; neither is an effect on job satisfaction detected. Indeed, the ATT on the 'Bad feelings about job' indicator points to an increased post-shock employment and employer attachment, compared to individuals who do not experience an acute health shock. Overall, this evidence relates to literature showing how individuals who remain working with the same employer following a health shock, are more likely to receive appropriate workplace support and display longer employment spells than those who change employer (Hogelund and Holm, 2014). Further outcomes, measured for employees only (not the self-employed), include perceived job security (measured on a 1 to 4 scale) and earnings. After one year since the health shock occurred, no effect on hourly earnings is detected (as in Lenhart's (2019) shorter term analysis), but employees experiencing an acute health shock exhibit a significant reduction in perceived job security.

ATTs estimated for outcomes conditional on remaining in employment (i.e. hours, expectations, earnings etc.) might be biased by selection: the treatment might alter the composition of the employed treatment group in such a way that registered differences in outcomes may reflect such compositional change. In our setting, it is plausible to expect more resilient, and labour market attached, individuals to remain active despite the shock. For example, the apparently positive effect on labour market attachment could then simply reflect a compositional change.

[22] Raw mean differences for each labour market outcome pre- and post-matching are given in Appendix Table A.3.

[23] As labour market participation is 100% at the baseline by sample construction (it is a sample of workers), the ATT figure for LMP can be interpreted either as percentage points or percentages.

[24] When we calculate ATTs computed for heart attack, stroke and cancer separately we obtain results (in the Appendix, Table A.4) that are a little higher for the first two and lower for cancer. The reason for this distinction relates to the fact that cancer represents a condition which might have started before the individual becomes aware upon diagnosis, differently with respect to stroke and infarction, which are typically diagnosed upon occurrence at a particular point in time. This raises a concern that, in the case of cancer, health shock predictors measured in *t-1* might capture symptoms or manifestations, rather than causes, of the upcoming health shock. In this case, controlling for these preconditions may capture part of the treatment effect, since they were induced by the treatment itself as anticipation effect.

[25] Using the same methodology to study the effect of health shocks experienced by individuals not in employment on their entry probability, also reveals a significant effect. These results are reported in Appendix, Table A.5.

Tables 6 and 7 present ATTs computed separately for those who were working part- and full- time respectively before the occurrence of a health shock, a distinction that should proxy pre-shock labour market attachment. Hence evidence of a differential (higher) exit of part-time workers, with respect to those working full-time, might signal selection bias.

No significant difference in ATTs between full- and part-timers emerge, although the ATT size is slightly higher for part-timers. Also the labour supply response along the intensive margin is aligned across the two groups while, in terms of salary, full-time workers are subject to a reduction in hourly earnings. Overall the possibility of selection bias favouring more attached workers among those who remain active, although not clearly signaled in Table 7, cannot be excluded.

The multiple waves of UKHLS allow us to assess dynamic patterns in labour supply response over time. With respect to individuals who experience an acute health shock between *t-1* and *t*, ATTs for some of the outcomes can be estimated up to *t + 1*, *t + 2* and *t + 3*. Results, reported in Table 8, reveal that the reduction in labour market participation and hours worked is confirmed in *t + 2* and *t + 3*. A significant decrease in the number of hours worked by those who remain active emerges in *t + 1*, but loses statistical significance in *t + 2*, and *t + 3* as the sample size declines. Consistently with previous literature, the impact on overall earnings persists over the three waves.

5.2. Sensitivity checks and placebo tests

Our preprocessing method combines coarsened exact matching and entropy balancing along with a parametric modelling stage and is intended to condition on the observed covariates in a flexible way that is robust to misspecification of either the matching process or the parametric model. To gauge the sensitivity and robustness of our results to alternative approaches to estimation, ATTs for labour market participation are computed using a range of other conditioning procedures.

First, two of the most commonly used matching estimators are compared. These are nearest neighbour propensity score matching (NNPSM) and Mahalanobis distance matching (NNMDM). Both of these approaches are applied using standard default settings: with one-to-one matching to the nearest neighbor with replacement and without calipers. The propensity score is estimated by a probit model using the full list of covariates. Notably the balancing of specific covariates worsens when these standard matching approaches are used, resulting in higher mean and median absolute bias in all cases (see Table 9). In addition, we apply simple parametric estimators (both non-linear binary choice and OLS models) which are not preceded by any preprocessing adjustment or matching procedure. Finally, a simpler EB approach is used without combining it with an initial CEM step.

With the exception of Mahalanobis distance matching the size of ATTs, reported in Table 10, are comparable across the different methods. This reinforces the observation made about Table 2 above which shows that no statistically significant differences emerge between treated and controls with respect to pre-treatment labour market variables. In this application systematic selection bias according to labour market outcomes may not be especially problematic and the estimated treatment effects appear to be robust to a range of different ways of conditioning on the controls ranging from the doubly robust preprocessing approach through semiparametric matching methods to simple parametric models.

Our identification strategy relies on the assumption of conditional independence of treatment given our set of observed confounders, which include some lagged outcomes. To test for possible bias arising from additional unobserved confounders, we

Table 6
ATT, full-timers.

	n (treat)	n (contr)	ATT	Std. Err.	P val	95%	CI	Relative effect
Labour market participation	322	31,562	**−0.03**	0.01	0.048	−0.059	0.000	*−3.2*
Hours, unconditional on LMP	320	31,292	**−2.43**	0.86	0.005	−4.103	−0.749	*−6.1*
Hours, conditional on LMP	289	30,111	**−1.21**	0.60	0.045	−2.382	−0.028	*−2.8*
Cond on LMP, employees only:								
Perceived job security (1 to 4)	107	14,017	−0.14	0.07	0.057	−0.289	0.004	*−4.2*
Earnings, unconditional on LMP	278	26,840	**−126.15**	47.49	0.008	−219.229	−33.071	*−7.4*
Earnings, conditional on LMP	250	25,819	**−81.41**	40.12	0.042	−160.049	−2.781	*−4.4*
Hourly earnings, conditional on LMP	249	25,635	1.16	1.71	0.498	−4.519	2.197	*2.5*

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure has been repeated each time the reference population varied.
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

Table 7
ATT, part-timers.

	n (treat)	n (contr)	ATT	Std. Err.	P val	95%	CI	Relative effect
Labour market participation	154	13,145	−0.02	0.02	0.393	−0.067	0.026	*−2.3*
Hours, unconditional on LMP	153	12,995	−1.06	0.80	0.184	−2.622	0.50481	*−5.0*
Hours, conditional on LMP	133	12,100	−0.62	0.65	0.338	−1.904	0.65405	*−2.6*
Cond on LMP, employees only:								
Perceived job security (1 to 4)	60	5,336	−0.16	0.10	0.094	−0.349	0.0276	*−4.7*
Earnings, unconditional on LMP	135	10,166	−5.34	33.64	0.874	−71.287	60.6134	*−0.7*
Earnings, conditional on LMP	121	9,497	−0.72	30.29	0.981	−60.107	58.6685	*−0.1*
Hourly earnings, conditional on LMP	121	9,408	0.16	2.05	0.939	−3.862	4.176	*0.4*

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure has been repeated each time the reference population varied.
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

Table 8
ATT after two ($t+1$), three ($t+2$) and four ($t+3$) years.

	t + 1					
	n (treat)	n (contr)	ATT	Std. Err.	P val	*Rel. Eff.*
Labour market participation	365	43,792	**−0.06**	0.02	0.001	*−7.2*
Hours, unconditional on LMP	360	43,307	**−3.82**	0.85	0.000	*−11.8*
Hours, conditional on LMP	294	40,112	**−1.67**	0.63	0.008	*−4.6*
Cond on LMP, employees only:						
Earnings, unconditional on LMP	318	36,710	**−153.04**	42.44	0.000	*−11.2*
Earnings, conditional on LMP	260	33,963	**−74.26**	35.26	0.035	*−4.8*
Hourly earnings, conditional on LMP	256	33,670	−0.06	1.60	0.972	*−0.1*
			t + 2			
	n (treat)	n (contr)	ATT	Std. Err.	P val	*Rel. Eff.*
Labour market participation	289	33,435	**−0.09**	0.02	0.000	*−10.0*
Hours, unconditional on LMP	284	33,042	**−3.27**	0.95	0.001	*−10.5*
Hours, conditional on LMP	216	30,005	−0.75	0.68	0.269	*−2.1*
Cond on LMP, employees only:						
Earnings, unconditional on LMP	250	27,849	**−104.19**	50.60	0.040	*−7.9*
Earnings, conditional on LMP	191	25,223	0.66	37.11	0.986	*0.0*
Hourly earnings, conditional on LMP	187	24,997	−1.27	1.64	0.439	*−2.8*
	t + 3					
	n (treat)	n (contr)	ATT	Std. Err.	P val	*Rel. Eff.*
Labour market participation	208	23,561	**−0.08**	0.03	0.002	*−10.0*
Hours, unconditional on LMP	204	23,149	**−3.86**	1.16	0.001	*−13.1*
Hours, conditional on LMP	149	20,528	−1.46	0.89	0.100	*−4.0*
Cond on LMP, employees only:						
Earnings, unconditional on LMP	180	19,498	**−143.45**	62.92	0.023	*−11.3*
Earnings, conditional on LMP	131	17,231	−61.34	52.69	0.244	*−3.8*
Hourly earnings, conditional on LMP	127	16,954	−3.23	1.65	0.051	*−7.0*

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure is repeated for outcomes whose reference population is limited to employees only.
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

run two checks for robustness: one based on `placebo outcomes', the other on `placebo treatments'.

The first consists of applying our preprocessing algorithm to estimate ATTs on outcomes measured at *t-1* and *t-2*, that is, outcomes prior to the health shocks occurring. If our conditioning strategy had succeeded in removing all potential sources of bias, we would expect to detect no difference in the lagged outcomes of treated and matched controls. On the contrary, significant differences in lagged outcomes would likely signal that ATTs estimated in *t* or the following years could partly reflect pre-existing differences between treated and matched controls that our matching strategy failed to remove.

Results from this first placebo exercise are reported in the top panel of Table 11. Because of conditioning on being labour market active in *t-1*, the labour market participation outcome can only be assessed at *t-2*, while other outcomes can be assessed at both *t-1* and *t-2*. No statistically significant difference in the *t-1* and *t-2* outcomes of individuals who experience an acute health shock between *t-1* and *t* is revealed, suggesting that our matching strategy has succeeded in controlling for endogenous selection into experiencing the acute health shock.

In a similar vein, the second placebo exercise consists of assessing current outcomes for individuals who will go on to experience a future health shock, using the same preprocessing strategy. This corresponds to matching individuals who will and will not experience an acute health shock between *t-1* and *t*, with preprocessing based on their *t-2* time-varying characteristics, and outcomes assessed as of *t-1*. Results, reported in the bottom panel of Table 11, point at a similarity in outcome trajectories before the health shock between those who experience a shock and those who do not. This is reassuring with respect to the effectiveness of our preprocessing adjustments.

A common concern when using panel data is that non-random attrition might bias estimates of interest. In our setting, for example, individuals experiencing more severe health shocks might be more likely to be lost to follow-up or die. If substantial, such attrition will result in an underestimation of the impact of an acute health shock. The survey drop-out rates, measured before the sample for analysis is restricted to those observed for at least two waves, are reported in the top panel of Table 12.

In the light of such non ignorable drop-out rates, as a sensitivity exercise, we re-estimate ATTs applying attrition weights. We first estimate a binary model of attrition, conditional on the set of confounders controlled for in the main analysis, under the assumption of attrition being selective on observables. The attrition weights are then derived as the inverse of the estimated propensity of remaining in the sample, and are incorporated into our estimation procedure.

As apparent from a comparison of Table A.6 (in the Appendix) with the corresponding unweighted results in Table 5, attrition weighted results are substantially unchanged. As a further robustness check, we repeated the analysis using longitudinal survey weights provided with UKHLS which may control for the initial survey non-response and obtained substantially similar results (reported in Appendix Table A.7). The distributions of both estimated and survey provided attrition weights can be compared in Appendix Table A.8. Finally, ATTs have also been estimated using drop-out in waves $t+1$ and $t+2$ as the outcomes: the non-significant ATTs for these placebo tests reported in the bottom panel of Table 12, strengthen the case for there being non-selective attrition.

6. Heterogeneous effects

6.1. Demographics

We investigate heterogeneity in labour market adjustments by stratifying the sample according to individual's pre-shock

Table 9
Balancing of means – comparison with other matching methods.

	Bias (std. % diff. in means)				
	Unbalanced	*CEM&EB*	*NNPSM*	*NNMDM*	*Simple EB*
Age	77.2	0.00	−2.7	22.1	0.1
Male	3.6	0.00	−2.9	−2.5	0.0
Father dead when respondent aged14	16.3	0.00	2	2	0.0
Mother dead when respondent aged14	1.2	0.00	3.9	1.9	0.0
Ever been a smoker	15.3	0.00	−0.4	4.6	0.0
Whether currently a smoker	15.1	0.00	−1	3.5	0.0
Has been a regular smoker in the past	13.0	0.00	−5.9	1.5	0.0
Whether smoked heavily either currently or in the past	22.4	0.00	−4.8	0.7	0.0
Self assessed poor health(t-1)	46.6	0.00	2.7	0.4	0.0
Number of limitations(t-1)	27.6	0.00	−0.7	4.6	0.0
Has long standing(t-1) illness/disability(t-1)	36.9	0.00	0	5.5	0.0
Ever diagnosed high blood pressure, until (t-1)	29.3	0.00	−4.4	2.8	0.0
Ever diagnosed diabetes, until (t-1)	27.1	0.00	−4.3	2.6	0.0
Ever diagnosed congestive heart_failure, until (t-1)	13.4	0.00	5.7	0	0.0
Ever diagnosed coronary_heart_disease, until (t-1)	27.2	0.00	2.8	0	0.0
Ever diagnosed angina, until (t-1)	23.0	0.00	−2.9	0	0.0
Cohabiting with spouse/partner(t-1)	5.4	0.00	−1.9	−13.5	0.0
Household size (t-1)	−15.2	0.00	−2.2	−7.2	0.0
Number of children (t-1)	36.0	0.00	−3.5	12	0.1
Highest educational qualification: degree	20.4	0.00	0.4	−1.4	0.0
White	16.5	0.00	3.7	−4.3	0.0
Equivalent household monthly income (t-1)	−2.1	0.00	−5.9	−0.5	0.0
Social renter (t-1)	9.5	0.00	−6.9	1.9	0.0
Home owner (t-1)	5.8	0.00	5.4	−7.8	0.0
Usual hours worked per week, including overtime(t-1)	5.7	0.00	−7.9	0.3	0.0
Job satisfaction (t-1)	−0.5	0.00	0.3	−1.4	0.0
Whether job is non-temporary (t-1)	9.0	0.00	−2.5	−3.3	0.0
Type of occupation: management & professional (t-1)	3.1	0.00	5.5	−5.5	0.0
Type of occupation intermediate (t-1)	1.5	0.00	0.5	5.9	0.0
Type of occupation routine (t-1)	−4.5	0.00	−5.3	0.4	0.0
Year of interview (t)	−6.8	0.00	−6.9	3.5	0.0
Wave	−6.4	0.00	−4.2	2	0.0
Elapsed months since previous interview	16.6	0.00	2.5	9.3	0.0
Mean absolute bias	26.3	0.0	3.4	4.4	0.0
Median absolute bias	22.4	0.0	2.9	2.5	0.0

Notes: NNPSM – nearest neighbor propensity score matching.
NNMDM – nearest neighbor Mahalanobis distance matching.Source: UKHLS, waves 1–7.

Table 10
Estimated ATT for LMP – comparison with other methods.

Method	n (treat)	n (contr)	ATT	Std. Err.	P val	Rel.Eff
CEM + EB	479	54,013	−**0.03**	0.01	0.022	*−3.3*
NNPSM, no caliper	480	81,146	−0.03	0.02	0.191	*−2.7*
NNMDM, no caliper	480	81,162	−**0.06**	0.02	0.001	*−5.9*
Simple parametric (binary)	480	81,146	−**0.04**	0.01	0.003	*−4.4*
Simple parametric (OLS)	480	81,146	−**0.04**	0.01	0.003	*−4.5*
Simple EB	480	81,146	−**0.03**	0.01	0.016	*−3.4*

Notes: NNPSM – nearest neighbor propensity score matching. NNMDM – nearest neighbor Mahalanobis distance matching. ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. Relative effect computed as (ATT/Conterfactual outcome for reweighted or matched control group)*100.Source: UKHLS, waves 1–7.

demographic characteristics[26] . First we consider age. A priori, acute health shocks might be expected to stimulate different labour market responses at different points in the lifecycle. At the time when the health shock occurs, younger workers have acquired less health-specific human capital, i.e. human capital which is only useful if the person is healthy (Charles, 2003), than older workers, and in this respect leaving a current job might be less costly. Also, younger workers face a longer time horizon for earned labour income, which strengthens their incentive to invest in re-training towards more physically suited jobs or tasks. On the demand side, this would be reinforced, in tight labour markets, by the more favourable prospects of re-employment younger workers face (e.g. higher employer job offer arrival rates), with respect to older workers, although this is less likely to be the case in times of adverse economic conditions, such as the period we are considering. In times of restrictions on job opportunities, the availability of replacement incomes is likely to play a major role in shaping workers' response to health shocks, as evidenced by the increase in disability benefits rolls typically registered during recessions (Pasini and Zantomio, 2013). The wider options that older workers face in this respect would appear predictive of a higher exit from employment.

[26] The analysis on heterogeneous subgroups is inevitably conducted on reduced and possibly less balanced samples, increasing the role for the parametric regression adjustment.

Table 11
Placebo tests.

	t-1					t-2				
	n (treat)	n (contr)	ATT	Std. Err.	P val	n (treat)	n (contr)	ATT	Std. Err.	P val
LMP	–	–	–	–	–	381	39,092	0.011	0.009	0.227
Hours	479	54,021	−0.025	0.641	0.968	378	38,911	0.012	0.720	0.986
Limitations	479	54,021	−0.004	0.044	0.925	380	39,084	0.074	0.053	0.166
Disab. Benefit	478	53,888	0.010	0.008	0.186	379	39,001	0.001	0.007	0.830
Job Satisfaction	479	54,021	−0.001	0.067	0.988	365	37,000	0.088	0.073	0.227
Earnings	418	46,254	24.121	46.963	0.608	315	31,358	5.010	49.079	0.919
Current outcomes on later shocks										
	n (treat)	n (contr)	ATT	Std. Err.	P val					
LMP	394	41,566	−0.005	0.011	0.651					
Hours	391	41,189	0.275	0.637	0.666					
Limitations	393	41,557	0.051	0.044	0.244					
Disab Benefit	393	41,469	0.012	0.009	0.175					
Job Satisfaction	367	39,606	−0.023	0.072	0.747					
Earnings	334	34,641	47.639	46.402	0.305					

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure is repeated for outcomes whose reference population is limited to employees only.Source: UKHLS, waves 1–7.

Table 12
Drop out rates and ATT on drop out.

Drop out rate					
wave 1	19.04	wave 4	9.2		
wave 2	13.99	wave 5	13.08		
wave 3	10.73	wave 6	16.84		
	n (treat)	n (contr)	ATT	Std. Err.	P val
drop-out (t + 1)	318	36,732	−0.005	0.015	0.727
drop-out (t + 2)	223	25,808	−0.028	0.015	0.063
drop-out (t + 3)	150	16,786	0.006	0.021	0.788

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator.Source: UKHLS, waves 1–7.

Indeed, we do observe a substantial difference between younger and older workers, contrary to previous studies (based on pre-economic 2008 crisis data), which found small or negligible differences between the two. Estimates of ATTs computed separately for younger and older workers, with the threshold set at the median age of 51 years, are reported in Table 13. No reduction in labour market participation is observed for younger aged workers, despite the significant increase in ADLs experienced following an acute health shock. Conversely, the 0.05 reduction in participation observed for older workers, which is broadly comparable to the figure reported by Trevisan and Zantomio (2016) for older workers in England, represents a major decrease in labour market participation, with respect to the baseline 8.1% exit rate[27] .

We further observe a substantial difference in age-related disability benefit uptake across the two age-groups with the probability of uptake in the older group almost twice the rate observed in the younger group[28] . Taken as a whole, these results indicate a strong gradient in the labour supply response to health shocks by age. The more limited re-employment prospects experienced by younger individuals, and in particular the lower educated, during the economic crisis, coupled with lower access to replacement incomes, may have induced individuals to retain existing employment.

Table 14 reports estimated ATTs by gender. Previous literature has generally found either no major difference in the way men and women respond to health shocks, or a stronger response for women than men. This stronger response is confirmed in our analysis. The 0.037 reduction in women labour market participation is substantial relative to their 6.2% baseline exit probability, while no comparable effect is evident for men. This gender difference does not appear to be driven by shock-induced impairments, as women generally appear to experience no more disabling shocks, compared to men. Rather, it might be traced back to different preferences for leisure and households' division of market and domestic work (Killingsworth and Heckman, 1986).

6.2. Educational gradients

Previous studies that have investigated educational gradients in labour supply adjustments following a health shock report contrasting results. For example, Heinesen and Kolodziejczyk (2013) and Taskila-Brandt et al. (2004) found less educated workers in Denmark and Finland respectively more likely to exit the labour market, presumably due to experiencing more disabling health shocks while being employed in more physically demanding jobs compared to their more educated counterparts. A stronger impact of acute health shocks on the earnings of lower, as opposed to higher, educated workers is reported by Lundborg et al. (2015) for Sweden. Across different institutional settings, possibly characterised by less generous replacement incomes, the opposite gradient has also emerged. For example, Trevisan and Zantomio (2016) found higher exit rates for more educated older women in Europe; evidence that points at the explanatory role of financial constraints to labour market exit. When differentiated by educational status our results (Table 15) suggest a significant reduction in labour supply at both margins (participation and hours worked) only for less educated workers, who appear to experience more severe disabilities compared to more educated individuals. Presumably these responses might also reflect lower opportunities for securing alternative or less physically demanding jobs.

6.3. The role of impairment

Consistent with findings from Coile (2004), the level of shock-induced impairment plays a crucial role in explaining observed

[27] The strong age gradient in employment response is confirmed when part- and full- time workers are considered separately.

[28] Disability benefit in the UK can be accessed by passing (beside a disability assessment) a mild contributory condition, or a means-test, and consists in a flat payment. Therefore there is no scope for exploiting variation in eligibility and benefit amount as drivers of labour market exit.

Table 13
: ATT by age group.

	16-51						52-65					
	n (treat)	n (contr)	ATT	95% CI		*Rel. effect*	n (treat)	n (contr)	ATT	95% CI		*Rel. effect*
Labour market participation	233	38,527	−0.004	−0.030	0.022	−0.4	244	15,481	**−0.050**	−0.089	−0.011	−5.5
Hours, unconditional on LMP	234	38,192	−1.323	−2.822	0.175	−3.8	242	15,311	**−2.538**	−4.463	−0.614	−7.7
Hours, conditional on LMP	220	36,630	−1.027	−2.184	0.131	−2.8	204	14,171	−0.995	−2.494	0.505	−2.7
Limitations	235	38,522	**0.337**	0.188	0.486	90.7	243	15,477	**0.529**	0.364	0.693	104.5
Disability Benefit	234	38,434	**0.045**	0.014	0.077	119.5	242	15,441	**0.086**	0.048	0.123	262.8
Cond on LMP, employees only:												
Perceived job security (1 to 4)	89	17,524	0.045	−0.179	0.090	1.3	78	5,875	**−0.2634**	−0.43	−0.10	−7.9
Earnings, unconditional on LMP	209	33,481	−46.454	−119.961	27.053	−3.2	207	12,145	**−109.68**	−199.32	−20.05	−8.0
Earnings, conditional on LMP	199	32,113	−39.534	−107.441	28.372	−2.6	174	11,246	**−73.166**	−142.63	−3.70	−4.8
Hourly earnings	199	31,896	0.642	−2.891	4.176	1.5	173	11,145	1.607	−5.231	2.018	3.6

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure has been repeated each time the reference population varied (younger workers, older workers, younger and older employees).
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

Table 14
: ATT by gender.

	Male						Female					
	n (treat)	n (contr)	ATT	95% CI		*Rel. effect*	n (treat)	n (contr)	ATT	95% CI		*Rel. effect*
Labour market participation	231	23,735	−0.018	−0.054	0.017	−2.0	248	30,278	**−0.037**	−0.072	−0.002	−3.9
Hours, unconditional on LMP	228	23,510	−1.891	−3.891	0.109	−5.0	248	29,993	−2.192	−3.774	−0.610	−7.1
Hours, conditional on LMP	201	22,356	−0.643	−1.979	0.693	−1.6	223	28,445	−1.146	−2.356	0.065	−3.5
Limitations	230	23,730	**0.463**	0.299	0.626	111.2	248	30,269	**0.449**	0.291	0.608	97.6
Disability Benefit	230	23,653	**0.081**	0.043	0.119	246.0	246	30,222	**0.055**	0.023	0.088	147.7
Cond on LMP, employees only:												
Perceived job security (1 to 4)	74	9,391	**−0.196**	−0.345	−0.048	−5.8	93	14,008	−0.078	−0.228	0.072	−2.3
Earnings, unconditional on LMP	190	18,497	−97.556	−203.035	7.923	−5.8	226	27,129	−74.035	−143.801	−4.269	−6.3
Earnings, conditional on LMP	170	17,605	−85.579	−174.651	3.493	−4.6	203	25,754	−39.026	−99.746	21.695	−3.1
Hourly earnings	169	17,463	2.836	−6.680	1.007	5.8	203	25,578	1.436	−2.146	5.017	3.6

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure has been repeated each time the reference population varied (male workers, female workers, male and female employees).
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

Table 15
: ATT by education.

	Low						High					
	n (treat)	n (contr)	ATT	95% CI		*Rel. Eff.*	n (treat)	n (contr)	ATT	95% CI		*Rel. Eff.*
Labour market participation	280	21,284	**−0.035**	−0.070	0.000	−3.8	196	18,381	−0.010	−0.045	0.025	−1.1
Hours, unconditional on LMP	278	21,111	**−2.573**	−4.311	−0.835	−7.9	195	18,165	−0.979	−2.880	0.922	−2.8
Hours, conditional on LMP	242	19,894	**−1.280**	−2.532	−0.027	−3.6	179	17,383	−0.240	−1.586	1.107	−0.6
Limitations	280	21,278	**0.554**	0.400	0.708	114.0	195	18,375	**0.317**	0.155	0.478	93.5
Disability Benefit	276	21,217	**0.076**	0.042	0.110	182.5	191	18,303	**0.055**	0.022	0.087	170.7
Cond on LMP, employees only:												
Perceived job security (1 to 4)	101	9,435	−0.141	−0.291	0.010	−4.2	65	7,620	−0.159	−0.336	0.018	−4.8
Earnings, unconditional on LMP	249	17,653	**101.84**	−167.61	−36.07	9.0	164	15,150	−44.38	−167.86	79.11	−2.5
Earnings, conditional on LMP	220	16,638	**−77.63**	−128.41	−26.84	−6.2	150	14,528	−11.38	−115.65	92.88	−0.6
Hourly earnings	219	16,540	−2.129	−5.935	1.678	−5.5	150	14,391	0.055	−3.744	3.633	0.1

Notes: ATT estimate in bold if significant at the conventional 5% level. The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator. The full matching procedure has been repeated each time the reference population varied (low educated workers, high educated workers, low and high educated employees).
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

labour supply adjustments. Table 16 reports ATTs estimated separately for individuals who experience a wider set of limitations following a health shock, compared to individuals who do not. The reduction in participation is significant for those who experience an increase in ADL limitations only. The severity of a health shock is also associated with a dramatically reduced perceived level of job security for individuals who remain in the labour market, and also with reduced earnings.

Our earlier finding of a stronger response for older workers might reflect the fact that they experience greater severity and impairment following a health shock than younger workers. To assess this possibility we estimate ATTs by age and impairment

Table 16
: ATT by impairment severity.

	No impariment						Induced impairment					
	n (treat)	n (contr)	ATT	95% CI		*Rel. Eff.*	n (treat)	n (contr)	ATT	95% CI		*Rel. Eff.*
Labour market participation	346	50,423	−0.006	−0.032	0.019	*−0.7*	133	3,590	−0.039	−0.091	0.013	*−4.5*
Hours, unconditional on LMP	344	49,945	−1.186	−2.615	0.244	*−3.5*	132	3,558	−2.887	−5.869	0.094	*−9.3*
Hours, conditional on LMP	319	47,565	0.742	−1.800	0.316	*2.0*	105	3,236	−1.611	−3.745	0.522	*−4.4*
Limitations	346	50,431	0.021	−0.020	0.063	*10.7*	132	3,568	**0.463**	0.213	0.713	*20.8*
Disability Benefit	343	50,294	**0.036**	0.014	0.058	*132.6*	123	3,498	**0.114**	0.051	0.177	*116.5*
Cond on LMP, employees only:												
Perceived job security (1 to 4)	129	21,924	−0.075	−0.196	0.047	*−2.2*	38	1,475	**−0.471**	−0.742	−0.200	*−14.5*
Earnings, unconditional on LMP	302	42,616	−68.063	−142.495	6.369	*−4.7*	114	3,010	−100.066	−228.093	27.962	*−8.4*
Earnings, conditional on LMP	280	40,602	−43.352	−102.145	15.441	*−2.8*	93	2,757	**−105.597**	−204.531	−6.663	*−7.5*
Hourly earnings	279	40,308	0.172	−2.967	3.311	*0.4*	93	2,733	−1.028	−4.706	2.651	*−2.6*

Notes: The ATT for binary outcomes is obtained averaging the difference between actual and predicted counterfactual outcomes over the distribution of treated individuals; for continuous outcomes, it corresponds to the estimated OLS coefficient on the treatment indicator.
The full matching procedure is repeated for outcomes whose reference population is limited to employees only.
Relative effect computed as (ATT/Conterfactual outcome for reweighted control group)*100.Source: UKHLS, waves 1–7.

(reported in Table A.9 in the Appendix). A strong disability gradient arises for older workers with the ATT in labour market participation for individuals with impairment being five times that estimated for individuals without impairment (-0.015 versus -0.073). In contrast younger workers are not responsive to the severity of the health shock. This suggests that shock induced disability is not the only explanation for the age gradient we observe.

7. Conclusions

The issue of labour market responses to acute health shocks, and of the mechanisms behind observed adjustments to these shocks, has remained relatively unexplored. The paucity of research covering the full age distribution of workers can largely be attributed to a lack of adequate sources of data, given the relatively low incidence rates of health shocks of sufficient magnitude to stimulate labour supply adjustments for a younger age group. However, given the potential impact on lifetime income and wealth accumulation together with the spillover effects on household members that the withdrawal of labour at younger ages implies, the inclusion of such individuals warrants consideration. Drawing on a recently available longitudinal survey of household in the UK (UKHLS), in this paper we combine coarsened exact matching and entropy balancing in a preprocessing algorithm to provide new evidence on the labour supply responses to acute health shocks experienced by workers of all ages. Inference is made with respect to workers observed after the onset of the 2008 financial crisis that profoundly changed European labour markets. While providing novel evidence, the focus on a later time frame with respect to previous studies, hampers comparability with results obtained by pre-recession literature.

Our approach identifies causal impacts of the incidence of acute health shocks on labour supply decisions. Acute health shocks are defined by the onset of a cancer, stroke or myocardial infarction, three conditions that can be regarded as unanticipated in the timing of onset, as well as being arguably less exposed to measurement bias compared to conditions that develop gradually over time. Despite the low incidence of acute health shocks, the combined matching algorithm yields ATT estimates that, while robust to alternative matching algorithms, are obtained from better balanced samples, reducing the scope for model dependence.

Results point to a significant reduction in labour market participation, with the average labour market exit risk increasing by around 40 per cent in response to an acute health shock. Among workers who remain active after the health deterioration an adjustment in hours and earnings is detected. We find evidence of heterogeneity in observed responses to health shocks. In particular, younger workers display stronger labour market attachment following a health shock than older workers and the impact of health shocks is concentrated on those who experience more severe limitations and impairment of daily activities.

Data constraints, stemming from a combination of a limited number of waves of data (currently seven), together with survey attrition, restrict our ability to observe the labour supply effects to a relatively short period of time following a health shock. It is worth noting, however, that previous literature indicates that the bulk of supply adjustments happen in the short run with limited adjustment thereafter (e.g. Halla and Zweimüller, 2013, Smith, 2005; Lenhart, 2019). As additional waves of data become available increasing the sample of individuals experiencing an acute health shock, the scope for investigating causal pathways, and the relative importance of disability, job characteristics, preferences for leisure and financial constraints, will become more fruitful.

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Declaration of Competing Interest

The authors declare that they have no conflict of interest.

Appendix A. Supplementary data

Supplementary material related to this article can be found, in the online version, at doi:https://doi.org/10.1016/j.ehb.2019.100811.

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