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The dynamics of disability and benefit receipt in Britain

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Abstract

This article provides a comprehensive analysis of the dynamic relationship between disability and welfare benefit receipt in Britain. Exploiting rarely used longitudinal data, it examines the impact of disability onset and disability exit on receipt of a range of beneficial outcomes, utilizing differences in the timing of onset/exit for identification. Disability onset increases receipt of disability insurance (DI), sickness benefits, and non-sickness benefits by 6, 8, and 9 percentage points in the onset year, although almost 70% of those experiencing onset remain independent of the welfare system in the short-run. DI reforms that toughened screening, reduced generosity, and increased conditionality appear to have substantially reduced DI inflows following onset, but without affecting the overall probability of welfare receipt. Disability *exit*, although neglected in the literature, is common during working-age and leads to a decrease in DI (and total welfare receipt), suggesting DI is not an absorbing state.

JEL classifications: H53, I38, J14

1. Introduction

Disability benefits provide an essential safety net for many people of working-age who are unable to engage in paid work. But disability benefits may themselves contribute to low participation rates among disabled people (Maestas *et al.*, 2013). There is also a perception that few people leave disability benefits until either death or state pension age, i.e. that being in receipt of disability benefits is an absorbing state (Karlström *et al.*, 2008; Liebman, 2015). High and/or growing disability benefit recipiency rates in many countries have also led to concerns about their fiscal sustainability (Autor and Duggan, 2006; Burkhauser *et al.*, 2014). Furthermore, policy interventions which have constrained disability benefit growth in the past have often seemed to work, at least in part, through displacing disabled people onto alternative benefit payments rather than returning them to, or retaining them in, employment (Karlström *et al.*, 2008; Borghans *et al.*, 2014).

If governments are to constrain and target disability benefits more successfully in the future, without simply displacing disabled people elsewhere in the welfare system, then we need a clear understanding of who begins and who ceases to claim, what benefit, and under what circumstances. Although several studies examine *labour supply and employment* outcomes following disability onset (e.g. Charles, 2003; Mok *et al*., 2008; Garcia-Gomez, 2011; García-Gómez et al., 2013; Jones *et al*., 2018a, 2018b), few specifically study the impact on *benefit-claiming*. Notable exceptions are Jenkins and Rigg (2004) for Britain, Singleton (2014) for the USA, and Polidano and Vu (2015) for Australia, although only Singleton (2014) focusses primarily on benefit outcomes, with the other studies considering disability benefits as part of a broader range of outcomes. Furthermore, only Polidano and Vu (2015) look beyond disability benefits to a wider measure of income replacement benefits. Yet, as Meyer and Mok (2019) highlight, the implications of changes in labour market outcomes for the well-being of disabled people depend heavily on the availability and nature of welfare support. Consistent with this, we examine the relationship between disability onset and benefit receipt in Britain by exploring multiple measures of benefits, including those which extend beyond sickness and disability benefits to account for broader forms of social security and interrelationships between benefit types (Burkhauser *et al*., 2014).

Even more stark is the dearth of literature on the benefit-claiming impacts of disability *exit*, and for many people disability is not permanent (Burchardt, 2000; Meyer and Mok, 2019), suggesting disability benefits need not be an absorbing state. Jones *et al*. (2018a) are a partial exception to this, albeit one that considers the relationship between disability exit and labour supply rather than welfare benefits. Disney *et al*. (2006) examine the labour supply effects of positive health shocks, which are likely to be correlated but not synonymous with disability exit; however, they too do not examine benefit-claiming. The relative neglect of disability exit in the literature is all the more surprising since, if benefits (and behaviours) are sufficiently responsive, it potentially provides a route to reduce disability benefit recipiency and the associated costs to government. We seek to start to address this evidence gap.

This article aims to improve our understanding of the dynamic relationships between disability and benefit receipt. Specifically, we use underexplored British longitudinal data to examine the short-run associations between disability onset and benefit receipt, and disability exit and benefit receipt. The focus on Britain is motivated by its experience of rapid growth and subsequent persistence of disability benefit rolls, despite major (and on-going) reforms (see Burkhauser *et al*., 2014). We examine four (overlapping) measures of welfare benefit, ranging from receipt of the main income-replacement disability benefit at that time (the UK version of Disability Insurance (DI)), Employment and Support Allowance (ESA) (which replaced Incapacity Benefit (IB)), to receipt of any non-universal welfare payment.[1] We are therefore able to explicitly consider welfare support beyond disability benefits, where even the sign of disability onset and exit effects on receipt are uncertain *ex ante*.

Our novel methodological approach builds on two recent papers on the employment effects of spousal health shocks (Fadlon and Nielsen, 2021) and disability onset (Jones and McVicar, 2020), respectively, by constructing control groups for those experiencing disability onset/exit drawn from those experiencing onset/exit 1 year later, and comparing these estimates to those exploiting control groups who experience no change in disability status (the dominant approach in the literature). Like Fadlon and Nielsen (2021) and Jones and

1 ESA is no longer available to new claimants and has been replaced by Universal Credit.

McVicar (2020), we argue that these alternative control groups provide estimates that are closer to being interpretable as causal.[2] We also look beyond average effects to examine different types of disability onset; more sudden versus gradual onset, and onsets reflecting single versus multiple health conditions. Finally, we examine onset effects before and after a major UK disability benefit reform in 2008 which, by replacing IB with ESA, tightened eligibility and reduced the generosity of DI. In addition to providing new evidence from which to assess the impact of welfare reform within Britain, this adds to the international literature on the impact of changing disability benefit eligibility and conditionality (see, e.g. Karlström *et al.*, 2008 for Sweden; Borghans *et al.*, 2014 for the Netherlands; and Broadway and McVicar, 2021 for Australia), with important insights for countries such as the USA where caseloads have continued to grow, prompting renewed consideration of welfare reform.

Our results suggest disability onset increases the probability of receipt of IB/ESA, sickness and disability benefits more broadly, and other benefits not explicitly related to sickness or disability by 6, 8, and 9 percentage points, respectively. However, the type of disability onset matters; effects on IB/ESA and sickness benefit-claiming are driven almost entirely by those experiencing disability onset reflecting multiple health conditions (a proxy for severity), and (tentatively) by sudden rather than gradual disability onset. The nature of the benefit regime is also important; disability onset effects on IB/ESA are 8 percentage points larger pre-reform than post-reform, consistent with its aim to constrain inflows. Finally, we present tentative evidence that disability exit leads to a near-symmetrical percentage point reduction in IB/ESA receipt and a reduction in overall benefit receipt. As such, neither IB/ESA nor the wider benefit system acts as an absorbing state in Britain.

The remainder of this article is structured as follows. Section 2 provides a review of disability benefits and broader welfare support in Britain over the period of analysis. Section 3 outlines the construction of the longitudinal Local Labour Force Survey and more specific measures utilized in this analysis. The difference-in-difference propensity score-matching (DID-PSM) approach to estimation is outlined in Section 4. Section 5 presents the main findings with respect to disability onset and disability exit, and Section 6 briefly concludes.

2. Disability and other benefits in Britain 2004–12

The data cover the period 2004–12, and this section briefly describes the working-age welfare system in Britain at that time, with particular emphasis on welfare payments, including earnings-replacement and additional costs and benefits for disabled people. From 2004 until October 2008, the main earnings-replacement disability benefit for those unable to work was IB.[3] This was a contributory benefit, i.e. eligibility required a work history, and

2 Deshpande and Li (2019) make a similar argument when exploring the impact of the closure of US social security offices on disability insurance claimants. They compare (treatment) areas with closures in time t to (control) areas with closures in future years since they argue the latter will be more similar than areas not experiencing closures.

3 We focus exclusively on government welfare support. Private disability insurance exists in the UK but tends to be tied to occupational pensions and mortgage and life insurance coverage. Our estimates of changes in government welfare support are those which exist *after* unobserved changes in private insurance, which might affect eligibility for means-tested forms of government support. Unfortunately, there is no information in our data from which to explore this further.

was subject to limited means-testing. Incapacity for work was determined by government doctors by means of a Personal Capability Assessment (PCA). Those who became disabled while in work were generally ineligible for IB during the first 28 weeks and instead could claim Statutory Sick Pay (SSP), for which employers were responsible.[4] Those unable to meet the contributions-based eligibility criteria for IB were potentially eligible for Severe Disablement Allowance (SDA) (although no new claims were granted after 2001) or to have their National Insurance credits, which contribute towards the state pension, paid. 'Credits only' claimants usually also received Income Support, a means-tested social-assistance payment, often with a 'disability premium'. Recipients of IB, SDA, and 'Credits Only' (but not SSP) were collectively referred to as *incapacity benefit claimants* and represented 6.9% of the working-age population in 2004 (see Supplementary Appendix Fig. A1), having hovered between 6% and 7% since the mid-1990s following a period of rapid growth.

From 2003 a new set of work-first reforms called Pathways to Work (PtW), aimed at slowing the inflow to IB and boosting outflows, was gradually rolled-out. It made movement onto the IB programme conditional on attendance at work-focused interviews, with the aim of steering recipients into employment support services and ultimately back into work. It also introduced a 'back to work' bonus payment, provided additional in-work condition management health support for those returning to employment, and brought PCAs earlier into the IB claim. Evaluation evidence on the impacts of PtW has been mixed (see Adam *et al.*, 2010; National Audit Office, 2010), although the IB claimant rate fell steadily between 2004 and 2008 to around 6.3%.

In October 2008, ESA replaced IB (and credits-only IB) as the main earnings-replacement disability benefit for new applicants. This insurance-based benefit (for those with sufficient recent work history) and means-tested social assistance benefit (for those without sufficient recent work history) included a new, tougher Work Capability Assessment, with fewer exemptions than the PCA. The requirement to attend work-focussed interviews introduced under PtW was extended to engagement in work-related activity for all but the most severely disabled, with payment made conditional upon compliance. There was also a reduction in payment levels for longer-duration claims which, in 2012, also became subject to increased means-testing after 1 year, for all but the most severely disabled.[5] Furthermore, from April 2011 existing IB recipients started to be reassessed under the ESA criteria, although this process was far from complete at the end of the window examined here. Some were judged ineligible as a result. IB/ESA recipiency rates continued to fall slowly from 2008 to 2012, reaching 6.0% in 2012. Concurrently, the Great Recession led to a rapid increase in the unemployment rate, rising to around 8% in 2009, where it remained through to 2012. The fact that IB/ESA recipiency rates did not increase during the downturn, unlike in earlier downturns, suggests these reforms may have affected flows onto and off IB/ESA. For early estimates of its impacts see Banks *et al.* (2015). The current system of Universal Credit, which brought several working-age benefits into a single payment, did not replace ESA until after the period of analysis.

The main additional cost benefit throughout the period was called Disability Living Allowance (DLA), and this began to be reformed, with its gradual replacement by Personal Independence Payments, subsequent to the period of interest here. The working-age

4 Unlike for DI in the US, there was no mandatory waiting period for eligibility for IB/ESA other than for the period covered by SSP for those in work at the time of disability onset.

5 These changes are summarized in Supplementary Appendix Table A1.

recipiency rate for DLA rose slowly but steadily over the period, from around 4.2% in 2004 to 4.5% in 2012. Other major working-age benefit types that are covered by the broadest measure of welfare recipiency used here (see Section 3) include Jobseeker's Allowance (JSA) (unemployment benefit), Income Support (social assistance), and Housing Benefit (means-tested support for housing costs).[6] For further details on these payments, see Browne and Hood (2012).

3. Data

We exploit the neglected 25% rotational panel structure of the British Local Labour Force Survey (LLFS) (part of the Annual Population Survey (APS)) (Office for National Statistics, 2020) where individuals are retained in the sample for up to 4 years (waves). Analysis is restricted to working-age respondents (16–64 for men, 16–59 for women) who provide information at four consecutive waves between 2004 and 2012,[7] creating a series of overlapping balanced panels covering six 4-year periods from 2004–7 to 2009–12, with maximum total sample of 48,947 individuals (195,788 person-year observations).[8] Detailed information on welfare benefits and personal characteristics is collected consistently over time on the basis of established LFS measures and, because of the large LLFS sample, there are sufficient disability transitions to enable estimation with reasonable precision. There are, however, two trade-offs. First, since the LLFS was designed to boost the APS in parts of Britain, it is not geographically representative.[9] Together with the balanced panel restriction which means the sample will contain individuals who remained resident at the same address during their 4-year panel, the result is a slightly older sample that has higher rates of disability than the full APS sample (see Supplementary Appendix Table A3).[10] Secondly, 4 years is a relatively short longitudinal dimension, limiting the extent to which we can examine longer-term impacts of disability onset/exit. Having said that, we examine benefit receipt both in the onset/exit year and the following year and, consistent with existing evidence on employment (see, e.g. Mok *et al.* 2008; Polidano and Vu, 2015; Meyer and Mok, 2019), present evidence of large effects in the year of onset/exit but only small additional effects in the following year.

6 Supplementary Appendix Table A2 provides a summary of welfare support in Britain over the period.

7 The questions used to identify disability changed in the LFS in 2013 restricting the period of analysis.

8 The state pension age for women gradually increased from age 60 in April 2010 but for consistency we impose the working-age definition which covered the majority of our sample period.

9 The LLFS sample excludes Northern Ireland and oversamples individuals in Wales and Scotland. Nevertheless, the composition of the sample in terms of other personal characteristics (including disability prevalence) is very similar to the full APS which is designed to be representative of the UK population (see Supplementary Appendix Table A3).

10 The differences in sample composition between the full and balanced LLFS samples in Supplementary Appendix Table A3 are consistent with younger, more mobile individuals, including students, leaving the sample due to attrition. Our sample will, however, also likely exclude individuals who experience disability onset/exit which is associated with forced residential mobility, e.g. for formal or informal care. If these individuals experience, the most severe onset/greatest recovery at exit then our estimates will be downward biased.

3.1 Disability

Consistent with earlier studies (e.g. Charles, 2003; Jones *et al.*, 2018a; Meyer and Mok, 2019), we adopt a work-limiting definition of disability. This requires a positive answer to an initial survey question on long-term health (LTH): 'Do you have any health problems or disabilities that you expect will last for more than a year?' and to either of the follow-up questions: 'Does this health problem affect the kind of paid work you might do? Does this health problem affect the amount of paid work you might do?'. Individuals answering 'no' to LTH, or those answering 'yes', but then 'no' to both the follow-up questions, are classed as non-disabled.[11] The prevalence of disability in the balanced panel sample, measured at wave 1, is 17.70%.[12]

The subjective nature of disability raises established concerns about potential measurement error (which may bias estimated effects, likely towards zero) and justification bias (which may inflate estimated effects, and might be reflected in diverging prior trends). Although Benítez-Silva *et al.* (2004) present evidence that questions the economic significance of the latter, Section 4 discusses how this potential bias is addressed through the approach adopted here.

To reduce measurement error and eliminate the most transitory changes in disability status, we adopt a two-period measure of disability onset/exit to identify the treatment groups, similar to Jenkins and Rigg (2004) in the case of onset. Denoting reported disability and no reported disability in wave t as '1' and '0', respectively, the onset treatment group consists of individuals who do not report disability for two waves, followed by two waves reporting disability (0011), and the exit treatment group consists of those individuals who report disability for two waves, followed by two waves not reporting disability (1100).[13] We specify two alternative control groups in each case. Following the approach in previous studies (e.g. Polidano and Vu, 2015), the first control group for onset is drawn from those continuously non-disabled (0000), i.e. those at risk of, but who do not experience onset. We do the same for exit, i.e. draw a control group from those continuously disabled (1111). We label these 'standard' control groups. Our alternative control group for disability onset, following the recent papers by Fadlon and Nielsen (2021) and Jones and McVicar (2020), is drawn from those who experience disability onset in the subsequent wave (i.e. 0001). Similarly, for disability exit, we construct an alternative control group from those who exit disability one wave later (i.e. 1110).[14] Table 1 provides the sample sizes for each group.

11 Those with a LTH condition that is not work-limiting have previously been found to exhibit similar employment rates to those without a LTH problem (Jones, 2006). This motivates their inclusion in the non-disabled group. We nevertheless explore the sensitivity of our estimates to the exclusion of individuals with LTH problems which are not disabling from the standard (non-disabled) control. The estimates are similar (available upon request).

12 Office for National Statistics (2014) highlights a discontinuity in the measurement of disability in the LFS between 2009 and 2010 where the following introduction 'I should now like to ask you a few questions about your health. These questions will help us estimate the number of people in the country who have health problems' was added to the survey. It is thought to have increased reported disability and, consistent with this, the prevalence of disability is about 0.8 percentage points higher in the LLFS in 2010 relative to 2009. While there is no evidence of a change in the type of main health problem reported among those disabled there is evidence of an increase in the prevalence of multiple health problems, often used as a measure of severity.

13 Since we only observe individuals for four waves we can say nothing about the permanency of disability onset/exit.

14 Because we are limited to four waves per individual, and because we need to retain two waves before reporting disability onset/exit for the treatment groups to explore pre-onset/exit trends, we

Table 1. Disability onset and exit treatment and control groups

		N	%
Onset	Treatment (0011)	585	1.21
	Control (0000)	35,219	72.61
	Control (0001)	1,311	2.70
Exit	Treatment (1100)	421	0.87
	Control (1111)	4,685	9.66
	Control (1110)	468	0.96

Source: Author's calculations using four-wave LLFS balanced panel.
Notes: Values in parenthesis relate to disability status in waves 1–4. '0' denotes no reported disability and '1' denotes reported disability. Figures are calculated as a percentage of respondents with valid information on disability in all four waves.

Note that disability onsets outnumber disability exits in our sample, but not by much. The dearth of literature on the economic impact of disability exit is particularly surprising in this light.

3.2 Benefit receipt

All respondents are initially asked whether, in the reference week, they claimed any State Benefits or Tax Credits. Those who respond positively are then asked 'Which of the following type of benefit or Tax Credits were you claiming?' and are given a long list of options, including 'Sickness or disability benefits'; 'Unemployment-related benefits'; 'Income Support'; 'State Pension'; 'Family-related benefits (excluding Child Benefit)'; 'Child Benefit'; 'Housing/Council Tax benefit'; and 'Other'.[15] A binary variable is generated for claiming any of the benefits listed excluding those who only report universal benefits (Child Benefit, State Pension, or both). This broad 'any benefit' measure is designed to improve our understanding of the total welfare impact of changes in disability status, including on the wider benefit system, and is reported by 14.72% of individuals at wave 1.

Two narrower binary measures are then generated for 'sickness benefit' and 'non-sickness benefit'. The former includes both income replacement and additional costs benefits linked to sickness or disability (the latter not conditioned on being out of work). Again, if we are to gain a comprehensive understanding of the impact of changes in disability status on welfare support, we cannot overlook additional costs benefits, which is often the case given the focus on DI. 'Non-sickness benefit' refers to claiming 'Unemployment-related benefits', 'Income Support', 'Family-related benefits (excluding Child Benefit)', 'Housing/Council Tax benefit', or 'Other'. This measure includes income-replacement benefits that might be claimed, for example, by those who are not disabled or who are not deemed eligible for ESA/IB, and therefore allows us to examine the impact of onset on welfare benefits not explicitly linked to sickness or disability, including possible benefit displacement at disability onset/exit. Several of these benefits depend on the household context, including

cannot restrict the control group of later-onsetters (later exiters) to those with two consecutive years of disability (non-disability). If this control group experiences shorter or less severe disability spells (shorter or less substantially disability-free spells) on average then this might impart upwards bias to the estimates.

15 'Tax Credits' is also listed among the full set of options, but this is not included in the APS datasets.

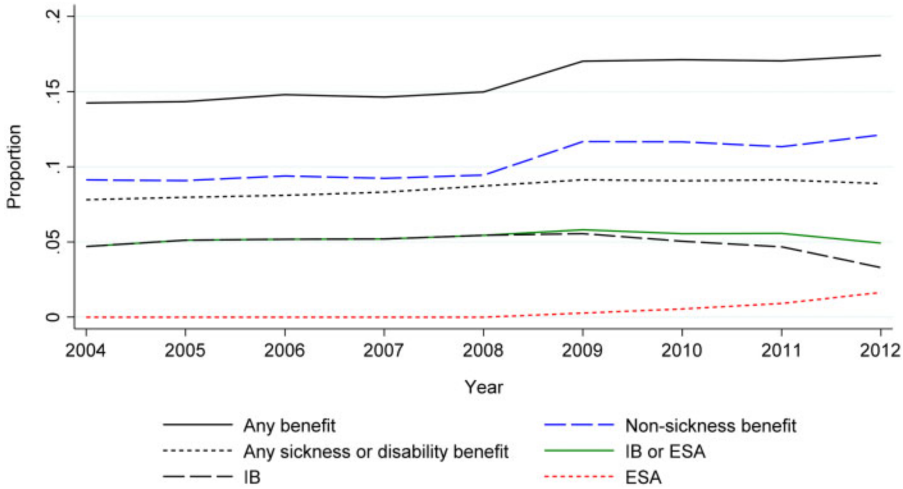

Fig. 1. Proportion reporting receipt of welfare benefits, 2004–12.
Source: Author's calculations using four-wave LLFS balanced panel.
Notes: See text for definitions of welfare benefit types.

(where relevant) income of the spouse. The impact on receipt at onset would therefore potentially be moderated where reallocation of resources within the household is possible, including through added-worker effects.[16] Our estimates measure the actual impact on government welfare, measured to include such effects. Non-sickness benefits are claimed by 9.58% of individuals at wave 1.

Finally, those in receipt of 'Sickness or disability benefits' are asked to list which type of benefit they claim and the responses include: 'Incapacity Benefit'; 'Severe Disablement Allowance'; 'Statutory Sick Pay'; 'Disability Living Allowance'; 'Attendance Allowance'; 'Industrial Injuries Disablement Benefit'; and (from 2009) 'Employment and Support Allowance'.[17] This additional information is used to create a narrower measure of receipt of IB or ESA, the UK equivalent to DI, which is reported by 4.93% of individuals at wave 1.[18]

The percentage of the sample claiming each of the four measures of benefit is traced from 2004 to 2012 in Fig. 1. Note the decline in IB and rise in ESA recipients following the 2008 reforms, but the overall stability of the series for IB or ESA and for sickness benefit, including through the Great Recession. In contrast, note the increase in receipt of non-

16 In the context of disability, this effect might be offset by the incentive to provide informal care. Although we are not aware of UK evidence, Fadlon and Nielsen (2021) find evidence of added-worker effects in response to income shocks arising during widowhood but not severe health shocks which are well-insured in Denmark. García-Gómez et al. (2013) find that on average there is a decline in the probability of spousal employment in response to acute hospital admissions in the Netherlands, but it is those with high income that are more likely to withdraw their labour.

17 'Invalid Care Allowance' (also reported) is excluded from the sickness benefit measure because this is claimed by a carer and not because of own disability.

18 IB/ESA recipients are also recipients under the broader 'sickness' and 'any benefit' categories. It is also possible for individuals to be in receipt of both 'sickness' and 'non-sickness' benefits simultaneously, as well as potentially being in receipt of multiple benefits within each category.

Table 2. Proportions receiving welfare benefit by wave and treatment status

Any benefit		Wave 1	Wave 2	Wave 3	Wave 4
Onset	Treatment	0.133	0.185	0.301	0.307
	Control (0000)	0.054	0.059	0.060	0.059
	Control (0001)	0.101	0.127	0.136	0.182
Exit	Treatment	0.254	0.260	0.201	0.181
	Control (1111)	0.754	0.758	0.768	0.765
	Control (1110)	0.460	0.463	0.455	0.407
Non-sickness benefit		**Wave 1**	**Wave 2**	**Wave 3**	**Wave 4**
Onset	Treatment	0.101	0.141	0.192	0.175
	Control (0000)	0.047	0.058	0.050	0.049
	Control (0001)	0.085	0.098	0.103	0.113
Exit	Treatment	0.168	0.181	0.162	0.124
	Control (1111)	0.379	0.403	0.412	0.414
	Control (1110)	0.280	0.270	0.272	0.258
Sickness benefit		**Wave 1**	**Wave 2**	**Wave 3**	**Wave 4**
Onset	Treatment	0.031	0.053	0.139	0.179
	Control (0000)	0.006	0.006	0.007	0.008
	Control (0001)	0.014	0.027	0.035	0.089
Exit	Treatment	0.119	0.103	0.054	0.068
	Control (1111)	0.635	0.648	0.665	0.669
	Control (1110)	0.280	0.306	0.301	0.246
IB or ESA		**Wave 1**	**Wave 2**	**Wave 3**	**Wave 4**
Onset	Treatment	[a]	[a]	[a]	[a]
	Control (0000)	0.001	0.001	0.001	0.001
	Control (0001)	[a]	0.009	0.014	0.049
Exit	Treatment	0.070	0.062	0.032	0.030
	Control (1111)	0.421	0.443	0.449	0.454
	Control (1110)	0.164	0.236	0.200	0.145

Source: Author’s calculations using four-wave LLFS balanced panel.
Notes: ‘0’ denotes no reported disability and ‘1’ denotes reported disability. [a]Cells suppressed for reasons of disclosure control.

sickness benefit (partly driven by an increase in JSA receipt), and as a result any benefit, from 2008 to 2009.

Table 2 reports sample proportions in receipt of each benefit measure by wave, split into disability onset/exit treatment and control groups as defined above. As we might expect, benefit recipiency rates for the standard (non-disabled or disabled) control groups are flat across the four waves. But there are discrete jumps at the time of onset/exit for both the treatment (at wave 3) and the alternative control (at wave 4) groups, with much smaller changes for the treatment group in the following year (wave 4). In some cases, there are also smaller increases in benefit receipt *in advance* of onset which appear to be common to both the treatment and alternative control groups. Furthermore, note that the uptake of sickness benefit at disability onset is relatively modest; most (>80%) do not receive sickness benefit in the onset year or the year following onset. Singleton (2014) similarly shows a relatively modest short-run uptake of DI for those experiencing disability onset in the USA.

As in the disability case, the fact that these benefit receipt measures are self-reported means measurement error cannot be ruled out. Indeed, while the LLFS data for IB/ESA receipt track the corresponding administrative data well over the 2004–12 period, aggregate reported receipt is consistently lower (see Supplementary Appendix Fig. A1).[19] One potential explanation is that IB/ESA Credits Only recipients do not consider themselves IB/ESA recipients, although they are administratively counted as such. Recipients may also under-report IB/ESA because of concerns it may be stigmatizing. Either way, disability onset and exit effects may be underestimated as a result. For a discussion of under-reporting of transfers in household surveys, see Meyer *et al.* (2009).

3.3 Control variables

The LLFS contains detailed information on personal and employment-related characteristics using established definitions, measured consistently over time. In what follows, estimation for both disability onset and exit is conditioned on a wide set of variables measured in wave 1, i.e. 2 years before onset (exit) for the treatment group. Following Polidano and Vu (2015) these include age (age squared), gender, highest educational qualification, region of residence, marital status, dependent children under 16 years in the household, employment status, full-time student status, and housing tenure.[20] The inclusion of regional fixed effects captures time-invariant regional characteristics, but the estimation is also conditioned on the Nomenclature of Territorial Units for Statistics (NUTS) 2 area unemployment rate as a measure of labour demand. To further mitigate potential concerns over non-random selection into disability onset or exit, we also condition on benefit receipt in wave 1. The latter is not only likely to reflect awareness of the welfare benefit system but capture household characteristics such as low income, a determinant of benefit eligibility and receipt.[21] Finally, given the overlapping panel structure of the data whereby individuals may experience onset in different calendar years, estimation is conditioned on the first survey year to control for the economic cycle and other time period effects.

4. Approach to estimation

Following earlier papers including Garcia-Gomez (2011) and Polidano and Vu (2015), we start by taking a PSM approach to identify disability onset (exit) impacts separately from compositional differences between those in the treatment and control groups, under a standard conditional independence assumption (CIA) (see Rosenbaum and Rubin, 1983). Specifically, the treatment and control groups are matched exactly on receipt of any benefit

19 It is more difficult to compare rates for our broader measures of welfare benefit since they include multiple benefit types which are not all recorded in the available administrative data.

20 Unfortunately, the LLFS does not contain information on household income. This motivated the inclusion of housing tenure as well as exact matching on welfare benefit receipt (see below). Our results are robust to replacing measures of individual with household welfare and, to the inclusion of information relating to the presence of a spouse in the household and its interaction with spousal employment.

21 Our findings are also robust to additionally controlling for prevalence of welfare benefit receipt at the NUTS 2 area level as a proxy for local area prosperity (available upon request).

and by year in wave 1 before estimating a probit model for treatment status (disability onset/exit) regressed on an extensive set of wave 1 observables as set out in the previous section. For each individual experiencing onset (exit) between waves 2 and 3, the individual with the most similar probability of experiencing onset (exit) given their characteristics, but who did not do so, is then identified (their nearest neighbour (NN)). Calculating how the treated individuals' outcomes differ from their matched partners' outcomes, and averaging these differences over all treated individuals within the region of common support[22] yields initial estimates of the impact of disability onset (exit) on those who experience it.[23] If the CIA holds this is interpretable as the average treatment effect on the treated (ATT).

Interpreting differences in benefit receipt between those who do and do not experience disability onset/exit as causal, however, even when conditioning on observable differences between individuals, is complicated by potential biases due to simultaneity (benefit receipt may impact on disability or on *reported* disability through justification bias), unobserved time-invariant, and unobserved time-varying confounders. In other words, the CIA may not hold. Augmenting PSM methods by differencing (e.g. Polidano and Vu, 2015) or exact matching on pre-onset outcomes (e.g. Garcia-Gomez, 2011) will reduce or eliminate some of the resulting biases. Here we do both, exact matching on receipt of any benefit in wave 1 and, following Polidano and Vu (2015), differencing all post-onset/exit outcomes relative to wave 1.[24]

The DID-PSM estimate of the ATT can be expressed as:

$$\mathrm{DiD} = \frac{1}{n}\sum_{l=1}\left[\left(Y_{lt+} - Y_{lt-}\right) - \sum_{j=1} W(l,j)\left(Y_{jt+} - Y_{jt-}\right)\right] \tag{1}$$

where n denotes the number of individuals within the treatment group each denoted l, with those in the control group denoted j. The time period pre- and post-treatment is denoted here as $t-$ and $t+$, respectively, and $(Y_{jt+} - Y_{jt-})$ measures the change in benefit receipt between wave 1 and subsequent waves for treated individual l. The change in outcomes in the matched control group is generated by weighting the difference in outcomes across individuals j in the region of common support.[25] Standard errors are calculated following Abadie and Imbens (2006) to take into account that the propensity scores are estimated.[26]

22 In our main models, the proportion of the sample in the region of common support exceeds 98%.

23 This is implemented in Stata using PSMATCH2 (Leuven and Sianesi, 2003).

24 Because we cannot entirely rule out reverse causality *within* onset/exit year in sensitivity analysis we follow Garcia-Gomez (2011) by additionally exact matching on onset/exit year outcomes and examining outcomes in the following year.

25 Estimates based on nearest neighbour (NN(1)) matching with replacement are reported in the text but these are robust to alternative matching algorithms, including NN(5), local linear regression and kernel density matching. The results are also similar to alternatively using weighted regression models where treatment status is regressed on differenced outcomes, and the control group is reweighted via entropy balancing, such that the first moment covariate distributions (for the same covariates applied in PSM) match the treatment group (see Hainmueller and Yu, 2013). See Supplementary Appendix Tables A10 and A11.

26 To examine whether the impacts of disability onset varies by personal and disability-related characteristics, the sample is split along the different dimensions before matching, with control

Of course biases driven by unobserved time-varying confounders may remain. Following Fadlon and Nielsen (2021) and Jones and McVicar (2020), we argue that such biases are likely to be smaller, although not necessarily entirely absent, when comparing outcomes for those experiencing disability onset/exit at time t with those experiencing disability onset/exit at time $t+1$ (the alternative control), since they are likely to be more similar in terms of both time-invariant and time-varying unobservables than is the case when compared with those who do not experience disability onset/exit. Supplementary Appendix Tables A4 (onset) and A5 (exit) report balancing tests for both control groups in each case, and confirm there are no significant differences in observable characteristics between the disability onset treatment and, matched standard and alternative control groups in either case, and only minor differences between the exit treatment, and matched standard and alternative control groups.

To further assess the relative merits of the standard and alternative control groups, consider the nature of disability onset/exit. Disability onsets encompass both sudden events (e.g. as a result of a traffic accident) and gradual deteriorations in health that result in individuals crossing a subjective threshold at which they begin to report disability. Consistent with the literature, we focus on disability onset as crossing the reporting threshold but, by using the alternative control group, are able to net out at least some of the impact of pre-onset deterioration in health because it is also experienced by the alternative control group.[27] In this respect, by more explicitly focussing on the threshold, the alternative control provides a relatively conservative measure of the impact of disability onset and, therefore, estimates from the standard and alternative control are likely to provide a range of magnitudes reflecting their differing ability to capture the influence of pre-onset changes. Similar arguments apply for disability exit.

5. The benefit recipiency impacts of disability

5.1 Disability onset

DID-PSM estimates of disability onset effects, using the standard (non-disabled) control group, are reported in Table 3 which contains four panels, each of which relate to a specific measure of benefit introduced above. Within each panel, the three rows give the DID-PSM estimates for benefit outcomes in waves 2, 3, and 4, relative to wave 1, which, because they wash out time-invariant unobserved heterogeneity, allow the CIA to be partially relaxed.[28] In each case, the difference in benefit receipt between the relevant waves is provided for the treatment and matched control group, as well as the difference between them which forms our DID-PSM estimate. So, by way of illustration, the DID-PSM which relates to the wave 4–1 difference for any benefit, indicates that there has been an increase in benefit receipt for the disability onset treatment group that is 18.5 percentage points larger than for the matched standard control group.

groups, constructed in the same way as in the main estimates, also drawn from these split samples. In each case the proportion of the sample in the region of common support exceeds 95%.

27 For sudden disability onsets, where there is no pre-onset deterioration in health, the alternative control group is likely to better account for other time-varying unobservables than the standard control, as per the arguments of Fadlon and Nielsen (2021).

28 The corresponding post-matching levels and PSM estimates for the difference between treatment and control groups in each wave are provided in Supplementary Appendix Tables A6 and A7 for the standard and alternative control groups, respectively.

Table 3. DID-PSM estimates of disability onset treatment effects on proportions receiving benefits, standard control group

Any benefit	Treatment	Control	Difference	*T*-stat for difference
Difference (2 – 1)	0.051	−0.021	0.072**	4.05
Difference (3 – 1)	0.168	−0.017	0.185**	8.68
Difference (4 – 1)	0.173	−0.011	0.185**	8.36
Non-sickness benefit				
Difference (2 – 1)	0.040	−0.015	0.055**	3.30
Difference (3 – 1)	0.091	−0.011	0.103**	5.43
Difference (4 – 1)	0.074	−0.008	0.082**	4.33
Sickness benefit				
Difference (2 – 1)	0.023	−0.002	0.025*	2.41
Difference (3 – 1)	0.109	0.000	0.109**	6.84
Difference (4 – 1)	0.149	−0.002	0.151**	8.37
IB or ESA				
Difference (2 – 1)	0.019	−0.002	0.021**	2.99
Difference (3-1)	0.080	0.000	0.080**	6.56
Difference (4 – 1)	0.103	0.000	0.103**	7.42

Source: Author's calculations using four-wave LLFS balanced panel.
Notes: Standard control group (0000) where '0' denotes no reported disability. Estimates are based on NN(1) matching with replacement and are estimated over the region of common support. *T*-statistics are based on Abadie and Imbens (2006) standard errors. * and ** denote statistical significance of the difference at the 95% and 99% level, respectively.

A key problem with interpreting the estimates in Table 3 as causal is immediately apparent: while we are restricted to two waves pre-onset, all four measures of benefit receipt begin to diverge between the treatment and control group during this time, suggesting either pre-onset divergence in health, other forms of time-varying unobserved heterogeneity, or both. The divergence is substantial in magnitude and statistically significant in each case. The implication is that the DID-PSM estimates likely overestimate the effects of disability onset on benefit receipt, and substantially so. Furthermore, because we do not know how much of the pre-onset increases in benefit receipt is due to deterioration in health, this problem remains even if we are willing to interpret disability onset as including health-related lead effects before disability onset.

Table 4 repeats this exercise for the alternative control group. Note that in this case estimates cannot be presented for wave 4 (the year following onset for the treatment group) because the control group themselves are treated by this time. Although we acknowledge this as a limitation, consistent with the broader literature on labour supply, Table 3 tentatively suggests that the effects of disability onset on benefit receipt are pronounced in the onset year but change more modestly in the subsequent year. In fact, Table 3 suggests that overall benefit receipt following disability onset does not increase between waves 3 and 4. This, combined with evidence of a fall in the probability of receipt of non-sickness benefit and a rise in the probability of receipt of sickness benefit between waves 3 and 4, appears to suggest an element of migration in benefit types in the year following disability onset.

Table 4. DID-PSM estimates of disability onset treatment effects on proportions receiving benefits, alternative control group

Any benefit	Treatment	Control	Difference	T stat for difference
Difference (2 – 1)	0.051	0.008	0.044*	2.01
Difference (3 – 1)	0.168	0.013	0.154**	6.06
Non-sickness benefit				
Difference (2 – 1)	0.040	0.004	0.036	1.78
Difference (3 – 1)	0.091	0.000	0.091**	3.93
Sickness benefit	Treatment	Control	Difference	T stat for difference
Difference (2 – 1)	0.023	0.008	0.015	1.12
Difference (3 – 1)	0.109	0.029	0.080**	4.25
IB or ESA				
Difference (2 – 1)	0.019	0.006	0.013	1.61
Difference (3 – 1)	0.080	0.019	0.061**	4.37

Source: Author's calculations using four-wave LLFS balanced panel.

Notes: Alternative control group (0001) where '0' denotes no reported disability and '1' denotes reported disability. Estimates are based on NN(1) matching with replacement and are estimated over the region of common support. *T*-statistics are based on Abadie and Imbens (2006) standard errors. * and ** denote statistical significance of the difference at the 95% and 99% level, respectively.

In contrast to Table 3, Table 4 shows much less evidence of divergence in outcomes before onset; in each case, the DID-PSM estimate for waves 2–1 is smaller in magnitude and, with the exception of any benefit, the differences are statistically insignificant, consistent with our conjecture that unobserved confounders are less likely than for the standard control group. As a result these estimates of disability onset, although perhaps conservative, are more plausibly interpreted as approaching causal than is the case when using the standard control group. For any benefit, the significance of the pre-treatment divergence, may nevertheless suggest the alternative control, albeit to a lesser extent, still overestimates the causal effect of disability onset.

For IB/ESA and the more general sickness benefit measure, the DID-PSM estimates suggest that disability onset leads to a 6 and 8 percentage point increase in benefit recipiency, respectively. These figures can be put into perspective using the additional information in Supplementary Appendix Table A7: sickness benefit recipiency rates in the year of onset are roughly three times larger than those for the control group at the same point in time and twice as large as those for the treatment group in the previous year. For any benefit measure, and bearing the above caution in mind, the corresponding DID-PSM estimated increase in benefit receipt is 15 percentage points, with receipt roughly double that for the control group or 70% higher for the treatment group in the wave before onset. For non-sickness benefits, there is a 9 percentage point difference, corresponding to a 35% increase since the wave before onset. *Ex ante*, there are two potential off-setting effects: increases in non-sickness benefit receipt as those experiencing onset begin to claim such benefits, including possibly in addition to sickness benefits, and reductions in pre-existing non-sickness benefit receipt as some of those experiencing disability onset migrate to sickness benefits. The estimated 9 percentage point increase shows that the former effect dominates. This would be missed by a narrower focus on ESA/IB or a more general measure of sickness benefits. All four differences are statistically significant at the 99% level.

Together these estimates confirm the importance of disability onset for all four measures of benefit receipt.[29] All of these estimates are smaller in magnitude than the corresponding estimates in Table 3, but they are still large effects in both absolute and proportional terms. The increase in any benefit receipt, for example, is larger than the corresponding 9 percentge point decrease in employment presented by Jones and McVicar (2020), suggesting increased benefit receipt even among those remaining in employment post-onset. It is also larger, as is the increase in ESA/IB receipt, than the 4 percentage point increase in receipt of (any) income-replacement benefit in Australia reported by Polidano and Vu (2015). Having said that only 30% of those experiencing disability onsets claim any benefit in the onset year and fewer than 15% claim sickness benefit. Contrary to popular perception, most working-age people experiencing work-limiting disability onset remain entirely independent of the UK benefit system, at least in the short-run, raising questions as to the role played by other forms of support, such as through private insurance or reallocation within the household.

5.2 Heterogeneous impacts of disability onset

Table 5 presents the corresponding waves 3–1 DID-PSM estimates by severity of disability onset (proxied by single or multiple conditions), whether the individual had an existing LTH condition or not (proxying sudden as opposed to gradual onset), and by whether onset occurred before or after the reform that replaced IB with ESA, using the alternative control group in each case.[30]

First, consider single versus multiple health conditions. There are much larger effects on sickness benefit receipt for onsets corresponding to multiple health conditions. In particular, for IB/ESA and sickness benefits more generally, only onsets corresponding to multiple conditions, interpretable as the most severe onsets, significantly increase benefit receipt in the short-run. Such benefits are explicitly targeted towards those with more severe disabilities, so the direction of these differences, if not quite the *absence* of single-condition disability onset effects, is as expected. It is also consistent with Singleton's (2014) finding that

29 Supplementary Appendix Table A12 presents PSM estimates with exact matching on outcomes up to and including the onset year. These estimates confirm the positive impact of disability onset on sickness benefits, IB/ESA, and any benefit. Reverse causality within-year does not therefore drive these effects. Here though, we find no significant impact on non-sickness benefit receipt which might suggest pre or immediate uptake of non-sickness benefits at disability onset.

30 Estimates for further sample splits by personal characteristics are presented in Supplementary Appendix Table A13 but (with one exception) none of the differences in these estimated onset effects are statistically significant. Taken together, the apparent near-uniformity of disability onset effects along these dimensions tentatively suggests a more limited role for factors such as replacement rates (likely higher for lower qualified and young workers), and employment prospects (likely better for higher qualified workers and in low unemployment areas) in driving benefit outcomes than is sometimes suggested in the wider disability benefits literature (e.g. Autor and Duggan, 2003), at least in the short-run. It perhaps also hints at a modest role for household support mechanisms (likely for those who are married). The caveat, however, is that we are asking a lot from the data by splitting the sample in this way.

Table 5. Waves (3–1) DID-PSM disability onset effects on proportions receiving benefits, by nature and timing of onset, alternative control group

Nature/timing of onset	Any benefit	Non-sickness benefit	Sickness benefit	IB or ESA
Single condition ($n = 227$)	**0.084***	**0.070**	0.013*	0.022*
Multiple conditions ($n = 294$)	**0.180**	**0.075**	**0.150**	**0.109**
LTH condition wave 1 ($n = 234$)	0.077*	0.043	0.000*	0.013*
No LTH condition wave 1 ($n = 286$)	**0.196**	**0.115**	**0.115**	**0.105**
Pre-2009 onset ($n = 276$)	**0.123**	0.051	**0.112**	**0.112***
Post-2009 onset ($n = 243$)	**0.169**	**0.091**	**0.066**	**0.033**

Source: Author's calculations using four-wave LLFS balanced panel.
Notes: Individual samples defined by the nature or timing of onset. Estimates are DID-PSM estimates for waves 3–1 based on NN(1) matching with replacement and are estimated over the region of common support with Abadie and Imbens (2006) standard errors. Bold indicates statistically significant from zero at the 95% level and asterisks denote a statistically significant difference from the relevant comparison group at the 95% level. Figures in parentheses give the number of individuals in the treatment group in each case.

those experiencing more severe disability onset in the USA are more likely to apply for and receive DI than those experiencing less severe disability onset. In contrast, the differences in the onset effects on non-sickness benefit receipt, where eligibility for the latter is not (directly) related to disability severity, are smaller and statistically insignificant. However, in both cases, disability onsets corresponding to both multiple and single health conditions do substantially increase benefit receipt.

Disability onset effects for those without an existing LTH condition in wave 1 leads to large increases in IB/ESA (10.5 percentage points) and sickness benefit receipt (11.5 percentage points). There is no corresponding impact for those with a LTH condition in wave 1. Again there seems to be a dividing line whereby only those with certain types of disability onset enter the benefit system immediately via sickness benefits. There is an important caveat here, however. To the extent that this is interpretable as splitting the treatment group by more sudden versus gradual-deterioration onsets, our use of the alternative control group in the latter case takes us further away from Fadlon and Nielsen (2021), and because disability onset is interpreted as crossing the threshold, arguably provides a conservative measure of the impact of disability onset which nets out the effects of pre-onset deterioration in health.

The final sample split is for onset pre and post-2009, i.e. onset under the IB regime compared with the new ESA regime. This provides a within-country parallel to the cross-country study of Garcia-Gomez (2011) who finds bigger employment impacts of negative health shocks in countries where disability benefits are more generous and conditioned on not working. Here we find a large and statistically significant difference in disability onset effects on IB/ESA receipt, which is more than three times larger pre-reform than post-reform.[31] This, however, is not matched by a significant decrease in onset effects on 'any benefit' receipt suggesting the fall in IB/ESA is not reflected in the overall probability of benefit receipt. One potential explanation for this, in a parallel to Karlström *et al.* (2008), is that the tougher

31 Potential mechanisms through which this operates include increased stringency in medical assessments, and disincentives to claiming associated with increased conditionality and reduced generosity (see Supplementary Appendix Table A1).

disability benefit regime displaced many of those experiencing disability onset elsewhere in the benefit system. The post-2009 increase in impacts on non-sickness benefit, however, is too small (and statistically insignificant) to account for all of the change in ESA/IB effects, suggesting a reduction in receipt of *multiple benefits* at onset, that is, there has been a reduction in the effect of disability onset on ESA/IB primarily among those combining ESA/IB with receipt of other benefits at onset.[32] There are two caveats to these points, so they are only suggestive. First, the introduction of ESA approximately coincided with the Great Recession which formally lasted from 2008Q2 to 2009Q3 in Britain. The post-recession labour market through to 2012 was slacker than the pre-recession labour market from 2004. This could mean we underestimate the impact of the reform on the benefit receipt effects of disability onset if, as a result, disabled people experience disproportionately higher benefit recipiency rates in the 2009–12 period than in the 2004–8 period.[33] Secondly, and potentially acting in the opposite direction, the discontinuity between 2009 and 2010 in the wording of the questionnaire which increased disability prevalence might be expected to result in the post-ESA period capturing less severe disability, consistent with the broadening definition. Evidence on the basis of reporting multiple health problems, albeit itself affected by the discontinuity is, however, not consistent with this.

5.3 Disability exit

Table 6 follows a similar format to Table 3 but presents DID-PSM estimates for the impacts of disability exit on benefit receipt, using the standard (disabled) control group.[34] The interpretation, therefore, closely corresponds to Table 3, for example, the top panel shows that individuals experiencing disability exit are 24.9 percentage points less likely to be in receipt of any benefit in wave 4, compared with otherwise observably comparable individuals who remain disabled. As in the case of disability onset, the extent to which we can interpret the estimates in Table 6 as causal is questionable given the evidence of pre-exit divergence in all four outcomes, suggesting gradual improvements in health and/or confounding time-varying unobserved heterogeneity. As for onset, the implication is that DID-PSM estimates of disability exit on benefit receipt likely overestimate the true effects.

Table 7 repeats this exercise for the alternative control group. Here pre-exit divergence can be rejected for all four outcomes. With the exception of non-sickness benefit, all other DID-PSM estimates of disability exit effects are negative and statistically significant. Most notably, this holds for the narrowest measure of disability benefits (IB/ESA) contrary to what one would expect were such benefits acting as an absorbing state. In terms of absolute magnitude, this effect (at −5 percentage points) appears to be close to symmetrical with the effect of disability onset. In other words, in Britain over this period, claiming behaviour and/or the administration of the main income-replacement disability benefit appears, albeit

32 Banks et al. (2015) also suggest that the introduction of ESA led to a fall in IB/ESA receipt but with no clear shift into employment or alternative benefits (specifically JSA). Our examination of the wider benefit measures alongside sickness and non-sickness benefits helps to explain this apparent puzzle.

33 In addition to the direct effect on employment, this effect would be reinforced by any reduction in potential reallocation within the household.

34 The corresponding post-matching recipiency rates and PSM estimates for the difference between treatment and control group in each wave are provided in Supplementary Appendix Tables A8 and A9.

Table 6. DID-PSM estimates of disability exit treatment effects on proportions receiving benefits, standard control group

Any benefit	Treatment	Control	Difference	T stat for difference
Difference (2 – 1)	0.005	0.143	−0.138**	−4.53
Difference (3 – 1)	−0.049	0.181	−0.230**	−6.76
Difference (4 – 1)	−0.073	0.176	−0.249**	−7.67
Non-sickness benefit				
Difference (2 – 1)	0.014	0.087	−0.073**	−2.76
Difference (3 – 1)	−0.005	0.068	−0.073**	−2.84
Difference (4 – 1)	−0.043	0.060	−0.103**	−4.18
Sickness benefit				
Difference (2 – 1)	−0.016	0.100	−0.116**	−4.30
Difference (3 – 1)	−0.065	0.143	−0.208**	−6.86
Difference (4 – 1)	−0.514	0.160	−0.211**	−6.75
IB or ESA				
Difference (2 – 1)	−0.008	0.081	−0.089**	−4.02
Difference (3 – 1)	−0.038	0.087	−0.124**	−4.59
Difference (4 – 1)	−0.041	0.097	−0.138**	−5.60

Source: Author's calculations using four-wave LLFS balanced panel.
Notes: Standard control group (1111) where '1' denotes reported disability. Estimates are based on NN(1) matching with replacement and are estimated over the region of common support. *T*-statistics are based on Abadie and Imbens (2006) standard errors. * and ** denote statistical significance of the difference at the 95% and 99% level, respectively.

tentatively, to have been similarly responsive to changes in disability status in either direction.[35] While this does not preclude the long-lasting effects of disability onset on labour market outcomes suggested by Charles (2003), Mok *et al.* (2008), and Meyer and Mok (2019), the implication for disability benefit regimes that tend to act as absorbing states, including DI in the USA, is that there might be scope to make them more responsive to disability exit, including, for example, by exploring differences between the USA and UK system, such as in terms of eligibility, waiting times, permitted work, and/or benefit generosity. The absence of an impact of disability exit on non-sickness benefit receipt suggests that disability exit does not drive outflows from non-sickness benefits in the exit year, or that enough of those moving off disability benefits initially switch to non-sickness benefits to replace any such exits from non-sickness benefits. Nevertheless, the 7 percentage point reduction in any benefit suggests a net reduction in benefit recipients among those who exit disability, consistent with disability exit also being an important route out of welfare support entirely.[36]

35 Estimated exit effects are qualitatively robust to exact matching on outcomes up to and including the exit year, albeit for the alternative control group the impact on sickness benefit is smaller in magnitude than the corresponding estimates for onset (see Supplementary Appendix Table A12).

36 Given that the sample splits pre/post 2009 and with/without an existing LTH condition are not relevant for disability exit, and given there are fewer exits than onsets in the data, we do not present

Table 7. DID-PSM estimates of disability exit treatment effects on proportions receiving benefits, alternative control group

Any benefit	Treatment	Control	Difference	T stat for difference
Difference (2 – 1)	0.005	0.027	–0.022	–0.70
Difference (3 – 1)	–0.048	0.021	–0.070*	–2.21
Non-sickness benefit				
Difference (2 – 1)	0.014	–0.005	0.019	0.67
Difference (3 – 1)	–0.005	0.003	–0.008	–0.28
Sickness benefit				
Difference (2 – 1)	–0.016	0.008	–0.024	–0.80
Difference (3 – 1)	–0.065	0.032	–0.097**	–3.14
IB or ESA				
Difference (2 – 1)	–0.008	0.019	–0.027	–1.16
Difference (3 – 1)	–0.038	0.016	–0.054*	–1.96

Source: Author's calculations using four-wave LLFS balanced panel.
Notes: Alternative control group (1110) where '0' denotes no reported disability and '1' denotes reported disability. Estimates are based on NN(1) matching with replacement and are estimated over the region of common support. *T*-statistics are based on Abadie and Imbens (2006) standard errors. * and ** denote statistical significance of the difference at the 95% and 99% level, respectively.

6. Conclusions

Despite the international policy importance of disability benefits, this paper is one of very few in the literature to examine the impact of disability onset on benefit receipt, and the first to examine benefit receipt at disability exit. Using British data and a methodology that combines PSM with DID, our preferred but relatively conservative estimates, exploiting the control group of later onsetters, show that disability onset substantially increases receipt of IB/ESA, sickness benefits more generally and, despite *ex ante* ambiguity, non-sickness benefits in the onset year. Moreover, some of these effects are large; for example, sickness benefit recipiency rates more than double in the onset year. As such, proactive interventions to prevent or delay impairments becoming work-limiting would reduce benefit receipt and may help to slow or reverse the growth in claimant numbers. That sickness benefits particularly capture those with more severe onsets suggests that they are effectively targeted, but also that understanding the type of disability onset is critical in designing any such interventions. Furthermore, our evidence suggests that non-sickness benefits form an important part of welfare support post-onset, neglected in analysis restricted to DI, and potentially resulting in an underestimate of government support post-onset and total welfare spending related to disability. Contrary to popular perception, however, we find that more than two-thirds of those experiencing disability onset remain independent of the welfare system in the year of (and the year following) onset, which aligns to questions, often from disabled people themselves, as to whether the British benefit system adequately responds to the needs of those experiencing onset.

estimates of heterogeneous exit effects on benefit receipt here, although they are available from the authors upon request.

Consistent with evidence from international comparisons, our analysis of reform to DI in the UK in which IB was replaced by ESA and toughened screening, reduced generosity, and increased conditionality suggests that the nature of the benefit regime matters. Post-reform, disability onset was much less likely to result in receipt of such benefits but no less likely to result in receipt of benefits overall, suggesting changes in benefit mix (perhaps including reductions in multiple benefit receipt) but little effect on overall welfare recipiency rates in the short-run. Since ESA was initially introduced for new claimants, we can say nothing about the implications of the change for the impact of disability exit, but given this reform also targeted outflows, e.g. through PtW, this is an important question for future research.

Finally, we provide tentative evidence that disability exit leads to a (possibly symmetrical) decrease in ESA/IB and a decrease in overall welfare receipt. This, combined with the prevalence of disability exit in the UK, suggests it warrants further attention in the academic literature, not least to investigate who exits, and from policymakers since there are potentially considerable payoffs to better supporting those who can exit disability. In this respect, and notwithstanding caveats about the extent to which these results might generalize to other contexts, it would seem that countries with DI regimes that are experiencing on-going growth, and that appear to act as absorbing states, such as the US, could learn from the British experience. Moreover, given more recent (and on-going) reforms to ESA and other welfare payments in the UK, there is clearly a need for further examination, including that which can explore the longer-term implications of, and different patterns of, disability transitions.

Supplementary material

Supplementary material is available on the OUP website. It contains the Supplementary Appendix and replication files. The data used in this article are available from the UK Data Archive: https://beta.ukdataservice.ac.uk/datacatalogue/studies/study?id=6721

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