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ABSTRACT

The Dynamics of Disability and Benefit Receipt in Britain*

This paper exploits rarely-used longitudinal data to examine the impacts of disability onset on benefit receipt in Britain over the period 2004–2012. Differences in the timing of onset are exploited for identification in a framework that combines propensity score matching with difference-in-differences estimation. Disability onset increases receipt of disability insurance, a wider measure of sickness and disability benefits, and receipt of non-sickness benefits by six, eight and six percentage points respectively in the first year. These effects do not vary significantly by individual characteristics, but are larger for more severe disability onset, for those who did not previously report a long-term health condition, and for those who experienced disability onset under the less restrictive pre-2009 disability benefit regime. Contrary to the perception of disability benefits being an absorbing state, disability exit has an almost symmetrical impact on receipt of disability insurance and on wider sickness benefits in the first year.

JEL Classification: H51, H53, I38, J14

Keywords: disability, disability onset, disability exit, welfare benefits, disability insurance, propensity score matching

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NON-TECHNICAL SUMMARY

Disability benefits provide an essential safety net for many people of working age whose health means that they are unable to engage in paid work. But disability benefits may themselves contribute to low participation and employment rates among people with disability. There is also a perception that few people flow off disability benefits until either death or state pension age is reached. Further, high and/or growing disability benefit recipiency rates in many countries have led to concerns about their fiscal sustainability.

This paper examines the relationship between disability and benefit receipt. Specifically, it examines the effects of disability onset (becoming disabled, or first reporting a disability) and disability exit (no longer reporting a disability) on disability benefit receipt and on receipt of other welfare benefits in the UK over the period 2004–2012. The paper shows that disability onset increases receipt of disability insurance, a wider measure of sickness and disability benefits, and receipt of non-sickness benefits by six, eight and six percentage points respectively in the first year. These effects do not vary significantly by individual characteristics, but are larger for more severe disability onset, for those who did not previously report a long-term health condition, and for those who experienced disability onset under the less restrictive pre-2009 disability benefit regime. Disability exit has an almost symmetrical impact on receipt of disability insurance and on wider sickness benefits in the first year, leading to substantial decreases in recipiency rates.

The paper draws out several potential policy implications. First, interventions to prevent or delay impairments becoming work-limiting can reduce benefit receipt and may help to slow or reverse the growth in disability benefit rolls experienced by many OECD countries. Second, if there is to be targeting of interventions aimed at providing people experiencing disability onset an alternative to claiming disability benefits, it may be more efficient to target by the type of onset and not the type of individual. Third, the 2008 reforms to the main income-replacement disability benefit in Britain – the replacement of IB with ESA for new claimants – had a dramatic impact on reducing or at least delaying inflows, with only limited spillover effects into other benefit categories, although there remains a question as to where in the labour market or benefits system, if anywhere, the diverted claimants have ended up. Fourth, countries with disability insurance regimes that appear to act as absorbing states could learn from the almost equal responsiveness of disability insurance in Britain to disability onsets and disability exits.

1. Introduction

Disability benefits provide an essential safety net for many people of working age whose health means that they are unable to engage in paid work. But disability benefits may themselves contribute to low participation and employment rates among people with disability (Parsons, 1980; Haveman and Wolfe, 1984; Bound, 1989; Autor and Duggan, 2003; Maestas et al., 2013). There is also a perception that few people flow off disability benefits until either death or state pension age is reached, i.e. that being in receipt of disability benefits is an absorbing state (e.g. Karlsröm et al., 2008; Liebmann, 2015). Further, high and/or growing disability benefit recipiency rates in many countries have led to concerns about their fiscal sustainability (Autor and Duggan, 2006; Burkhauser et al., 2014).

This paper examines the relationship between disability and benefit receipt. Specifically, building on the strand of the literature that examines the dynamic relationships between disability and labour market outcomes (e.g. Charles, 2003; Jenkins and Rigg, 2004; Mok et al., 2008; Garcia-Gomez, 2011; Meyer and Mok, 2013; Singleton, 2014; Polidano and Vu, 2015), this paper uses little-exploited British longitudinal data to examine the dynamic relationships between disability and benefit receipt. Like this earlier literature, which primarily emphasised employment, the focus on dynamics is motivated by the dynamic nature of both disability (people flow into (experience onset of) and out of (experience exit from) disability) and benefit receipt (people flow on and off benefits). The overriding question is how does benefit receipt respond to changes in disability status?

The paper examines the impact of disability onset on four benefit receipt outcomes ranging from the main income-replacement disability benefit – the UK version of Disability Insurance (DI) known as Employment and Support Allowance (ESA) – to receipt of any non-universal welfare payment. This builds in particular on three of the studies cited above that consider benefit receipt among their outcomes. The first of these, also using British data, is Jenkins and Rigg (2004) which finds a positive impact of disability onset on income from own disability benefits and from other welfare benefits at the household level in the onset year and the year after onset. The later period studied here (2004-2012) follows (spans) major reforms to disability benefits in 1995 (2008) and to unemployment benefits in 1996 which all impacted on disability benefit rolls (see McVicar, 2008; Banks et al., 2015). Second, for the US, Singleton (2014) finds a positive impact of disability onset on DI receipt in the onset year and the seven subsequent years. Finally, for Australia, Polidano and Vu (2015) also find a positive

impact of disability onset on receipt of any income replacement welfare payment in the onset year and the four subsequent years. The current study looks at but also beyond DI receipt and catchall measures of benefit receipt given that many working age people with disability are not in receipt of disability benefits but may be in receipt of other benefits[1], and because flows onto and off disability benefits are not only from and to employment but also from and to other benefit payments including unemployment insurance (for the UK see Sissons et al., 2011; Beatty and Fothergill, 2015). Taken together this suggests even the sign of disability onset effects on receipt of welfare payments other than sickness and disability benefits is uncertain ex ante. The paper's first contribution comes from examining this broader set of benefit receipt outcomes, for a more recent period, than has been the case to date.

A second contribution is to exploit the sample size offered by the British data – with more disability onsets than is typical in this literature – to examine heterogeneous impacts of disability onset on benefit outcomes across several dimensions. To date what we know in this regard is limited to differences in DI application and receipt by severity of disability onset from Singleton (2014) (those experiencing onset of more severe disability are more likely to apply for and receive DI than those experiencing onset of less severe disability) and differences in receipt of any income replacement welfare payment by broad education level from Polidano and Vu (2015) (those with no qualifications are more likely to receive benefits than those with vocational or higher-level qualifications). There is more existing evidence on heterogeneous impacts of disability onset on employment and other labour market outcomes. Polidano and Vu (2015) finds variation by pre-onset employment status (impacts on employment are due more to reduced inflows than to increased outflows from work) and by education level (larger impacts for lower educated individuals). Jones et al. (2016) finds stronger employment effects of disability onset for men, older individuals, and those with more severe disability, but little difference by education level. The current paper examines differences in disability onset effects on benefit receipt along all these dimensions.

A critical question for social policy purposes is whether the impacts of disability onset vary according to the nature of the benefit regime in place at the time of onset. Garcia-Gomez (2011) suggests there are bigger employment impacts of disability onset and other negative health shocks in countries where disability (and other) benefits are more generous, where they are

[1]For the US Meyer and Mok (2013) present descriptive data on recipiency rates for various welfare benefit payments, including DI, between six and ten years after disability onset, although they stop short of presenting estimates of the impact of disability onset on these outcomes.

conditioned on not working, and where employers do not have to meet disability employment quotas. The current paper makes a third contribution by examining variation in outcomes within Britain either side of a major disability benefit reform introduced in 2008 to complement the cross-country approach of Garcia-Gomez (2011).

A fourth contribution is to explicitly examine the impact of disability *exit* – no longer reporting a disability – on benefit recipiency. This has been mostly overlooked by the dynamics of disability literature, despite the fact that for many people disability is temporary not permanent (see Burchardt, 2000; Meyer and Mok, 2013). One factor that may have contributed to this is the perception that disability benefits are essentially an absorbing state until either death or state pension age is reached, although this appears less the case in some countries than others (see OECD, 2010). Further, even temporary disability may have long-lasting effects on labour market outcomes through impacts on human capital accumulation and through state dependence (Charles, 2003; Mok et al., 2008; Oguzoglu, 2012a; Meyer and Mok, 2013). An exception to the dearth of studies on disability exit effects is Jones et al. (2016) which finds no impact of disability exit on employment. Another is Disney et al. (2006) which finds evidence of symmetric labour force participation impacts of negative and positive health shocks. Neither study examines benefit receipt impacts of disability exit.

A final contribution is methodological. The paper uses propensity score matching (PSM) combined with difference-in-differences (DID) methods to compare changes in outcomes for those experiencing disability onset or exit with matched controls. This in itself is not unique (for example see Garcia-Gomez et al., 2013; Polidano and Vu, 2015). In contrast to the existing dynamics of disability literature, however, the control groups here are drawn not from those who do not experience disability onset or exit, but from those who experience onset or exit one year later than the treatment groups, who are much more similar to the treatment groups in observable, and presumably unobservable, characteristics.

The remainder of this paper is set out as follows. The following section provides a brief overview of the British welfare system, and in particular disability benefits, pre and post 2008. Section 3 describes the data. Section 4 sets out the approach to estimation and discusses identification. Section 5 presents and discusses the results and Section 6 concludes. An appendix presents additional data details.

2. Disability and Other Benefits in Britain 2004-2012

The data cover the period 2004-2012, and this section briefly describes the working-age welfare system in Britain in place at that time, with particular emphasis on welfare payments – both earnings replacement and additional costs benefits – for people with disability. From 2004 until the 27th October 2008 the main earnings replacement disability benefit for those unable to work on grounds of disability – essentially disability insurance – was called Incapacity Benefit (IB). This was a contributory benefit, i.e. eligibility required a work history, and was (mostly) not subject to means-testing. Incapacity for work was determined by government doctors by means of a Personal Capability Assessment (PCA). IB was paid at one of three flat rates depending on the length of time the individual had been unable to work: a short term lower rate for the first 28 weeks, a short term higher rate for the next 24 weeks, and a higher long-term rate subsequently. Those who became sick or disabled while in work were generally ineligible for IB during the first 28 weeks of a spell out of work and instead could claim Statutory Sick Pay (SSP), for which employers were responsible.[2] Those unable to meet the contributions-based eligibility criteria for IB were potentially eligible for Severe Disablement Allowance (SDA) (although no new claims for SDA were granted after 2001) or to have their National Insurance credits – contributions 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 benefits claimants* – note the practical equivalence in British welfare-speak between *claiming* and *receiving* benefits, as opposed to *applying for* benefits – and made up the incapacity benefit roll, which stood at around 6.7% of the working age population in 2004, having hovered between 6% and 7% since the mid-1990s.

From 2003 a new set of work-first reforms called Pathways to Work (PtW), aimed at slowing the inflow to IB and boosting outflows for those having recently joined the roll, was gradually rolled-out. It made movement onto the IB program (including credits only) conditional on attendance at work-focused interviews, with the aim of steering at least some recipients into employment support services and ultimately back into the labour market. It also introduced a 'back to work' bonus payment, provided additional in-work condition-management health

[2] Note that unlike for DI in the US there is no mandatory waiting period for eligibility for IB other than for the period covered by SSP for those in work at the time of disability onset.

support for those returning to employment from IB, and brought PCAs forward so they took place around three months into the IB claim rather than around six months into the 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 6.2%. The unemployment rate hovered around 5% over this period.

In 2008, ESA replaced IB (and credits only IB) as the main earnings-replacement disability benefit for new applicants. This new program of insurance-based benefit for those with sufficient work history and means-tested social assistance benefit for those without sufficient work history included a new tougher Work Capability Assessment (WCA), with fewer exemptions, in place of the existing PCA. The requirement to attend work-focused interviews introduced under PtW was extended into a requirement to engage in work-related activity for all but the most severely disabled, linked explicitly to payments, with around one quarter of the existing benefit payment made conditional upon compliance. There was also no longer a higher rate of payment for longer-duration claims. Further, from April 2011 existing IB recipients started to be reassessed under the new ESA eligibility criteria, although this process was far from complete at the end of the window examined here. Some were judged ineligible as a result of medical re-screening under the stricter WCA. Disability recipiency rates continued to fall slowly over the years from 2008-2012, reaching 6% in 2012. Concurrently, the Great Recession led to a rapid increase in the unemployment rate (and in the claimant rate for unemployment benefits), rising to around 8% by the second quarter of 2009, where it remained through to 2012. The fact that disability recipiency rates did not increase during or in the years following the downturn, unlike in earlier downturns, suggests the reforms outlined above may have impacted on flows onto and off IB/ESA. For more details of this reform and early estimates of its impacts see Banks et al. (2015).

The main additional cost benefit throughout the period was called Disability Living Allowance (DLA), and this only began to be reformed – with its gradual replacement by Personal Independence Payments – subsequent to the period of interest here. The working-age recipiency rate for DLA rose slowly but steadily over the period, from around 4% in 2004 to around 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 (for those with low income to help with housing costs). For further details on these payments see Browne and Hood (2012).

3. Data

This paper exploits the Local Labour Force Survey (LLFS), which has a number of desirable properties. First, a 25% rotational panel structure means that individuals are retained in the sample for up to four years. Second, the LLFS is large sample with sufficient numbers of disability onsets (although fewer exits) to enable an examination of heterogeneous effects. Third, the data span a major disability benefit reform – the switch from IB to ESA from October 2008 for new applicants – which allows an examination of the impacts of disability onset on benefit receipt under different benefit regimes within the same country. The trade-offs are that four years is a relatively short longitudinal dimension (so longer term impacts of disability onset/exit cannot be examined), and that the longitudinal sample used is not fully representative of the wider working age population. The latter should be borne in mind when drawing conclusions from the analysis presented here.[3]

Analysis is restricted to respondents who provide valid information at four consecutive waves between 2004 and 2012, creating a balanced panel, and who are of working age throughout.[4] This leaves a maximum sample of 49,071 individuals (196,284 person-year observations). Note that the LFS is not primarily designed as a panel survey and it is the address rather than the individual that is traced across time. As a consequence observations in the LLFS panel are restricted to households that did not move address and individuals who remained resident within these households for four consecutive years. The sample therefore excludes individuals who experience disability onset/exit which is associated with selective residential mobility, e.g. for formal or informal care purposes (Norman *et al*., 2005). These are likely to be the most severe onsets or greatest recoveries. However, since migration more generally is dominated by young and healthy individuals, attrition increases the prevalence of disability in the LLFS panel relative to the unrestricted pooled LLFS by about two percentage points. Overall, compared to the full APS sample, the balanced LLFS panel has higher rates of disability and disability

[3] The LLFS is part of the Annual Population Survey (APS) which also contains observations from the main Quarterly Labour Force Survey (QLFS) and the APS boost. Special Licence LLFS data are first pooled from 2004 to 2012. Then, following Jones *et al.* (2016), system variables are used to undertake a matching process of individuals across time to construct a panel version of the LLFS. The LLFS covers Great Britain but since it was designed to boost the sample size of the main QLFS it is not geographically representative. This has a limited effect on the sample composition in terms of personal characteristics, but there are slightly higher proportions reporting disability and benefit receipt, and lower proportions reporting employment, than in the full APS sample pooled over the same period (see appendix Table A1).

[4] The questions used to identify disability changed in the LFS in 2013.

benefit claiming, lower rates of non-sickness benefit claiming, is slightly older, has fewer full-time students and singles, and more renters, in addition to the aforementioned differences in geographical coverage (see Table A1).

Disability

Disability is defined here using answers from three survey questions. First, to be defined as having a disability requires a positive answer to an initial question on long-term health: *"Do you have any health problems or disabilities that you expect will last for more than a year?"* A positive answer is then required 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 the first question on long-term health, or those answering 'yes' to the first question but 'no' to both the follow-up questions, are classed here as non-disabled. In other words, in line with most studies in the dynamics of disability literature, and given the paper's focus is primarily on income-replacement benefits, the definition of disability adopted here is that of work-limiting (WL) disability. By this measure the prevalence of disability in the balanced panel sample is 17.48%.[5]

Relying on self-reported disability means measurement error may bias estimated onset and exit effects towards zero. By focussing on *changes* in self-reported disability status facilitated by longitudinal data, and more particularly on *consistent* patterns of disability reporting either side of a change, however, the scope for measurement error is reduced compared to cross-sectional approaches. Specifically, the paper follows Jenkins and Rigg (2004), Polidano and Vu (2015) and others by using two-period measures of disability onset and exit where the onset group are defined as those who experience two periods reporting no disability followed by two periods of reporting disability (0011), and the exit group are defined as those who report two periods of disability followed by two periods of no disability (1100). In place of the usual approach of drawing control groups from those who do not experience a change in their disability status, however, control groups are constructed here from those who do experience disability onset or exit but one year later (i.e. 0001 or 1110). Table 1 provides the sample sizes for the various groups. Note that using these definitions within the balanced panel means that onset or exit can

[5] The ONS (2014) has recently highlighted a discontinuity in the measures of disability in the LFS between 2009 and 2010. This relates to a minor change in the administration of the questionnaire where "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.

occur at any time between 2006 and 2011, and there is a broadly equal distribution of such events across this six-year window.

Table 1: Disability onset and exit treatment and control groups

		N	%
Onset	Control (0001)	1,346	2.75
	Treatment (0011)	596	1.22
Exit	Control (1110)	486	0.99
	Treatment (1100)	431	0.88

Notes: Balanced panel with a minimum of 4 waves within the LLFS. '0' denotes no reported disability and '1' denotes reported disability.

Another potential issue with self-reported disability is justification bias – for a given degree of disability, benefit recipients may be more likely than others to report themselves as disabled – which may impart biases to estimated onset and exit effects in the opposite direction. The extent to which this is economically important, however, is not clear. Benitez-Silva et al. (2004), for example, find that self-reported disability status is an unbiased predictor of DI eligibility decisions. Bound (1991) suggests that justification bias may even help to cancel out attenuation bias due to measurement error. Meyer and Mok (2013) argue that some alternative (more objective) measures may themselves be endogenous and may be too narrow, for example excluding conditions such as mental illness or pain which have no physical marker (also see Benitez-Silva et al., 2004). Like Meyer and Mok (2013), the current paper is constrained by the absence of objective measures of disability in the LLFS which might otherwise be used in place of, or to instrument, self-reported measures (e.g. see Disney et al., 2006; Garcia-Gomez et al., 2013).[6] Potential biases are discussed further in Section 4.

Benefit Receipt

All respondents are initially asked whether, in the reference week, they claimed any State Benefits or Tax Credits (including State Pension, Allowances, Child Benefits and National Insurance 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';

[6] Sensitivity analysis using an alternative self-reported definition – activity limiting disability – is available from the authors on request. Some have argued this is less susceptible to justification bias (e.g. Oguzoglu, 2012b) although it is also likely less relevant for income-replacement benefit outcomes. Estimated treatment effects take the same sign but tend to be smaller in magnitude than in the WL case.

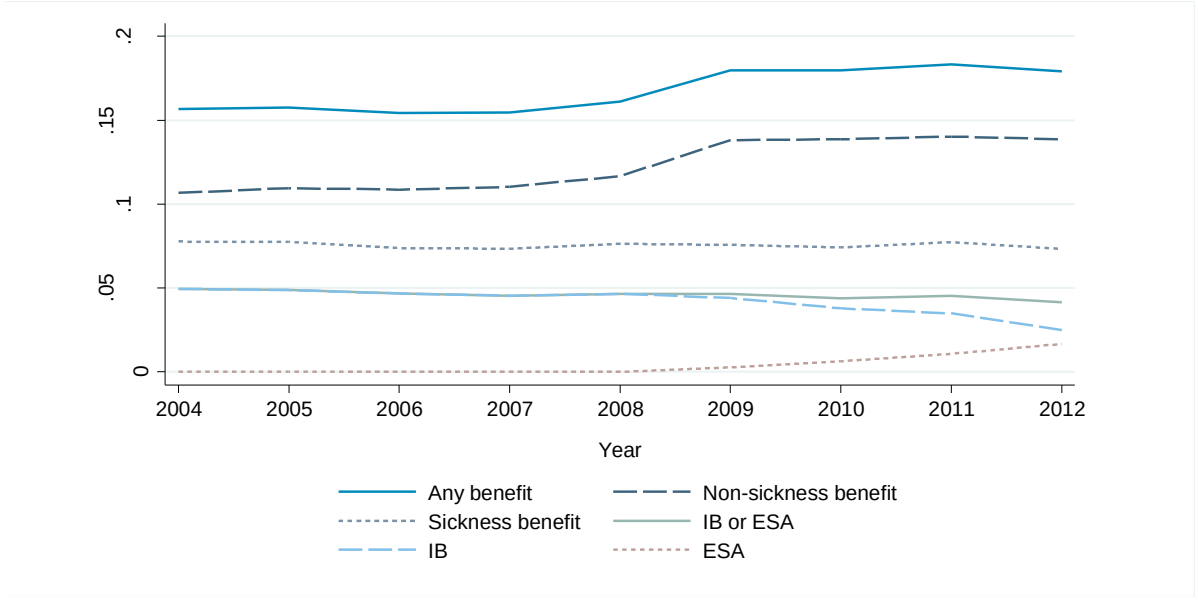
'Housing/Council Tax rebate'; 'Other'.[7] From this a binary variable is generated for claiming any of the benefits listed excluding those who report only universal benefits (Child Benefit, State Pension or both). This is the broadest 'any benefit' measure and is reported by 14.74% of the sample. Two narrower binary measures are then generated for 'sickness and disability benefit' (used interchangeably here with 'sickness benefit') and 'non-sickness benefit'. The latter is equal to one for those who report 'Unemployment related benefits', 'Income Support', 'Family related benefits (excluding Child Benefit)', 'Housing/Council Tax rebate', 'Other', and is zero otherwise.

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'. 'Invalid Care Allowance' (also reported) is excluded from the sickness benefit measure because this is claimed by a carer and not on the basis of own disability. In other words, the 'sickness benefit' measure covers both income-replacement and additional costs benefits (the latter not conditioned on being out of work). This additional information is also used to create a final narrower measure of receipt of IB or ESA (sometimes described as disability insurance in the text) which is equal to one for those who report receipt of either benefit but zero otherwise, and is reported by 4.92% of the sample. IB/ESA recipients are by definition also counted as recipients under the broader 'sickness and disability benefits' and 'any benefits' categories. It is also possible for individuals to be in receipt of benefits under both the sickness and non-sickness benefit categories simultaneously.

The percentage of the working age population claiming benefits in each of the four categories is traced from 2004 to 2012 in Figure 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 benefits, including through the years of and following the Great Recession. In contrast, note the increase in receipt of non-sickness benefits (driven primarily by increases in JSA receipt), and as a result in the any benefit measure, from 2008 to 2009.

[7] 'Tax Credits' is also listed among the full set of options but this is excluded from the measures available in the APS datasets.

Figure 1: Proportion reporting receipt of welfare benefits, 2004-2012



Notes: LLFS working-age population (2004-2012).

Table 2 reports sample proportions in receipt of benefits according to each of the four measures by wave, split into disability onset/exit treatment and control groups as defined above. There are discrete jumps in these sample proportions at the time of onset / exit for both the treatment (wave 3) and the control (wave 4) groups. In some cases there are also smaller increases in the sample proportions in receipt of benefits in advance of onset – less so exit – although these appear to be common to both the treatment and control groups. The dynamics of disability literature cited above tends to be somewhat agnostic about such pre-onset trends, which could reflect declining health in advance of reported disability, time-varying confounders, or both. Further, note that the uptake in IB/ESA for those reporting disability onset is relatively modest; most of those experiencing disability onset do not receive these benefits in the onset year or the year following onset. Singleton (2014) similarly shows a relatively modest uptake of DI in the US for those experiencing disability onset. Table 3 reports the proportion of the pooled 2004-2012 LLFS sample in receipt of benefits by employment status. Note that only half (one quarter) of those describing themselves as disabled receive any non-universal benefit (IB/ESA). For non-employed people with disability – just under two thirds of those reporting themselves as disabled – the corresponding proportions are 80% and 40% respectively. Disability benefit receipt is not simply the flipside of employment for people with disability.

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

Any Benefit		Wave 1	Wave 2	Wave 3	Wave 4
Onset	Control (0001)	0.100	0.131	0.141	0.188
	Treatment	0.137	0.196	0.308	0.318
Exit	Control (1110)	0.477	0.480	0.470	0.432
	Treatment	0.251	0.260	0.199	0.184
Non-sickness Benefit		Wave 1	Wave 2	Wave 3	Wave 4
Onset	Control (0001)	0.085	0.098	0.105	0.120
	Treatment	0.104	0.151	0.194	0.183
Exit	Control (1110)	0.285	0.274	0.273	0.271
	Treatment	0.161	0.175	0.156	0.126
Sickness Benefit		Wave 1	Wave 2	Wave 3	Wave 4
Onset	Control (0001)	0.014	0.030	0.040	0.090
	Treatment	0.030	0.055	0.142	0.182
Exit	Control (1110)	0.303	0.316	0.312	0.259
	Treatment	0.124	0.106	0.053	0.068
IB or ESA		Wave 1	Wave 2	Wave 3	Wave 4
Onset	Control (0001)	0.004	0.012	0.018	0.051
	Treatment	0.009	0.028	0.093	0.113
Exit	Control (1110)	0.180	0.237	0.202	0.147
	Treatment	0.078	0.070	0.034	0.034

Notes: Balanced panel with a minimum of 4 waves within the LLFS. '0' denotes no reported disability and '1' denotes reported disability.

Table 3: Benefit receipt by disability and employment status, %

	Disabled			Non-disabled		
	All	Employed	Non-employed	All	Employed	Non-employed
Any Benefits	57.0	15.4	80.5	9.0	3.0	32.4
Non-sickness Benefit	34.3	6.4	50.1	8.1	2.4	29.7
Sickness Benefit	41.8	10.4	59.6	1.1	0.5	3.2
IB/ESA	27.0	3.1	40.5	0.4	0.1	1.6
N	125,115	45,061	80,054	659,612	523,961	135,651

Notes: LLFS working-age population (2004-2012).

As in the disability case, the fact that these benefit receipt measures are self-reported means measurement error in the outcome variables cannot be ruled out. This is most likely in responses to questions about specific benefit types, so may affect the narrower measures more than the broader ones. Note that the LLFS data for the narrowest IB/ESA measure track the corresponding administrative data for the actual benefit roll very well over the 2004-2012

period, but at nearly two percentage points lower in all years (see Appendix Figure A1). One potential explanation is that IB/ESA Credits Only recipients don't tick the IB/ESA box because they don't consider themselves IB/ESA recipients, although they are administratively counted as such. Recipients may also under-report IB/ESA receipt because of concerns it may be stigmatising and disability onset and exit effects may be underestimated as a result. For the broader definitions of benefit receipt, it seems reasonable to assume that measurement error is random, in which case the magnitude of the estimated onset/exit impacts will be unaffected although they may lose precision. For a recent discussion of under-reporting of transfers in household surveys see Meyer et al. (2009).

Control Variables

The LLFS contains detailed information on personal and employment-related characteristics using established definitions, measured consistently over time. In what follows, estimation is conditioned on a wide set of explanatory variables measured in wave 1, that is, two years prior to onset (exit) for the treatment group. Following Polidano and Vu (2015) these variables include age (age squared), gender, highest educational qualification, region of residence, marital status, presence of dependent children under 16 in the household, employment status, full-time student status and housing tenure. Estimation is also conditioned on the local unemployment rate and, in order to further mitigate potential concerns over non-random selection into disability onset or exit status, on benefit status in wave 1. Finally, given individuals may experience onset in different years, estimation is conditioned on year first included in the survey to control for time period effects.

Heterogeneity in onset effects is explored by splitting the sample by gender, age (older and younger defined as above or below age 45), highest qualification (higher and lower qualifications defined as above or below GCSE grade C), disability type (main health problem is classified as physical or mental condition), disability severity (single or multiple health conditions reported), local economic conditions (proxied by unemployment rates above/below the median), reporting a long-term health condition in or prior to wave 1, and onset pre and post ESA. Note that the ESA option, although introduced on 27th October 2008, was only included in the questionnaire from 2009 onwards.

4. Approach to Estimation and Identification Issues

The aim is to estimate the impact of disability onset (exit) on a set of benefit receipt outcomes. A random experiment cannot be implemented for this purpose, so observational data and econometrics are used to separate out treatment effects from differences in outcomes due to selection and reverse causality. Jenkins and Rigg (2004) showed that individuals who experience disability onset tend to be more disadvantaged prior to onset than those at risk of but who do not experience disability onset. The opposite is likely to be the case for disability exit. A potential mechanism for reverse causality is that benefit receipt, or more specifically inactivity associated with benefit receipt, directly leads to deteriorating health (e.g. Lindeboom and Kerkhofs, 2009; Deb et al., 2011; Colman and Dave, 2014). Another is that individuals may 'justify' benefit receipt by reporting disability.

Following earlier papers including Garcia-Gomez (2011) and Polidano and Vu (2015), this paper starts by taking a propensity-score-matching (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 in wave 1 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 (also see Table 4). For each individual experiencing onset (exit) the individual with the most similar probability of experiencing onset (exit) between wave 2 and 3 given their characteristics but who did not do so is then identified. Calculating how the treated individuals' outcomes differ from their matched partners' outcomes, and averaging these differences over all treated individuals, yields initial estimates of the impact of disability onset (exit) on those who experience it. If the CIA holds this is interpretable as the average treatment effect on the treated, or ATT.[8]

A key point of departure from Garcia-Gomez (2011) and Polidano and Vu (2015), however, is that the control groups consist not of those who do not experience disability onset/exit – who we might expect to be very different from those who do experience disability onset/exit in both observable and unobservable characteristics – but of those who experience disability onset/exit one year later than the treatment groups. In other words identification of the treatment effects here is potentially strengthened over and above that offered by PSM on conventional treatment

[8] The analysis is performed over individuals within the region of common support, so it's not quite *all* treated individuals. Strictly speaking, therefore, the resulting estimate is a local average treatment effect.

and control groups by additional exploitation of the timing of disability onset/exit. Fadlon and Nielsen (2015) adopt a similar approach to study responses to severe health shocks within households. As they point out, the trade-off for potentially tighter identification is that this approach cannot be used to examine longer-term impacts of the treatment. Here, this means disability onset/exit impacts can only be estimated in the year of onset/exit and not in subsequent years. With a longer panel one might be willing to risk more selection bias to allow estimation of longer-term impacts, but equally one might not.

Table 4: Sample means for observable characteristics, by treatment and control group pre and post-matching

	Onset			Exit		
	Treatment	Control (0001) (pre-matching)	Control (0001) (post-matching)	Treatment	Control (1110) (pre-matching)	Control (1110) (post-matching)
Age	45.205	45.354	46.183	44.218	45.500	44.958
Gender						
Male	0.501	0.473	0.497	0.549	0.512	0.546
Highest qualification						
Degree	0.147	0.158	0.165	0.143	0.117	0.120
Other Higher Education	0.093	0.115	0.099	0.098	0.090	0.137
A level	0.242	0.208	0.217	0.232	0.204	0.283
O level	0.229	0.231	0.217	0.199	0.157	0.157
Other	0.109	0.116	0.092	0.134	0.147	0.132
None	0.181	0.172	0.179	0.193	0.286**	0.171
Students						
Full-time student	0.052	0.046	0.038	0.036	0.032	0.022
Marital Status						
Single	0.268	0.232	0.243	0.249	0.318*	0.235
Married	0.588	0.617	0.591	0.599	0.525*	0.633
Widowed/divorced	0.145	0.151	0.165	0.151	0.157	0.132
Children						
Dependent child in household	0.311	0.332	0.264	0.325	0.216**	0.342
Housing Tenure						
Owned outright	0.265	0.243	0.262	0.261	0.271	0.252
Mortgaged	0.487	0.547*	0.503	0.493	0.391**	0.549
Rented	0.249	0.210	0.235	0.247	0.338**	0.199
Region						
Tyne and Wear	0.024	0.029	0.026	0.025	0.042	0.028
Rest of North East	0.060	0.046	0.068	0.064	0.050	0.070
Greater Manchester	0.076	0.074	0.062	0.050	0.057	0.081
Merseyside	0.040	0.028	0.032	0.034	0.037	0.050
Rest of North West	0.042	0.044	0.046	0.028	0.045	0.025

South Yorkshire	0.020	0.013	0.012	0.028	0.027	0.025
West Yorkshire	0.024	0.014	0.014	0.014	0.017	0.014
Rest of Yorkshire & Humberside	0.020	0.023	0.026	0.045	0.039	0.028
East Midlands	0.016	0.013	0.012	0.025	0.012	0.014
West Midlands Metropolitan county	0.044	0.042	0.030	0.036	0.044	0.022
Rest of West Midlands	0.026	0.028	0.032	0.028	0.025	0.014
East of England	0.034	0.028	0.032	0.022	0.020	0.042
Inner London	0.016	0.020	0.006	0.017	0.015	0.020
Outer London	0.040	0.035	0.024	0.039	0.042	0.059
South East	0.082	0.082	0.087	0.010	0.082	0.112
South West	0.048	0.070	0.052	0.048	0.057	0.056
Wales	0.195	0.184	0.233	0.171	0.201	0.129
Strathclyde	0.090	0.090	0.091	0.101	0.080	0.092
Rest of Scotland	0.100	0.135	0.099	0.123	0.104	0.118
Economic Conditions						
Employed	0.739	0.772	0.744	0.647	0.458**	0.689
Local area unemployment rate	0.066	0.063*	0.065	0.065	0.066	0.066
Year of observation						
2004	0.157	0.144	0.157	0.190	0.164	0.190
2005	0.171	0.138	0.171	0.176	0.132	0.176
2006	0.171	0.181	0.171	0.179	0.152	0.179
2007	0.159	0.213*	0.159	0.149	0.139	0.148
2008	0.175	0.168	0.175	0.157	0.211	0.157
2009	0.169	0.156	0.167	0.149	0.202	0.148
Any benefit	0.139	0.102*	0.139	0.241	0.473**	0.241
N	497	1,128	365	357	402	194

Notes: Balanced panel with a minimum of 4 waves within the LLFS. '0' denotes no reported disability and '1' denotes reported disability. All characteristics are measured at Wave 1. Matching is undertaken using a NN(1) matching algorithm. * and ** denote statistically significant differences between the treatment and control group at the 95% and 99% level respectively.

Given the particular nature of the control groups, there are few differences in observables prior to matching, and no statistically significant differences remain after matching (see Table 4). This lends strong support to the CIA, although in practice unobserved confounding factors may still remain. To the extent that any such unobserved confounders are time-invariant (e.g. preferences, history) the ATT can be recovered either by exact matching on each of the wave 1 outcomes (following Garcia-Gomez, 2011) or by combining PSM with DID (following Garcia-Gomez et al., 2013; Polidano and Vu, 2015).[9] Specifically, differences in outcomes

[9] This approach was originally proposed by Heckman et al. (1997).

between the treatment and control groups are differenced between waves 1 and 3 (two years prior to and the year of onset/exit). The DID-PSM estimate of the ATT between wave 1 and the year of onset (exit) is therefore the difference-in-differences between wave 1 and wave 3 outcomes for the matched sample.

Formally, the DID-PSM estimator can be expressed as:

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

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 $(Y_{lt} - Y_{lt'})$ 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.[10] Standard errors are calculated following Abadie and Imbens (2006) and take into account that the propensity scores are estimated.

The DID-PSM approach outlined above, although it partially relaxes the CIA, requires an assumption of parallel trends between treatment and control groups. Standard practice is to examine trends prior to treatment to provide some indication of its reasonableness. Given the definition of onset (exit) and the four-wave length of the panel used here, pre-treatment information is limited to waves 1 and 2 in each case. The DID-PSM estimate for the difference-in-difference in outcomes between waves 1 and 2 (defined in a corresponding manner to waves 1 and 3 above) is therefore interpretable as a test of diverging prior trends. As shown in Tables 5 and 7, diverging prior trends are rejected for all outcome variables in the case of disability onset and for all but one in the case of disability exit.

Parallel prior trends would be unlikely here were justification bias driving differences in outcomes between treatment and control groups. Nevertheless, reverse causality within onset/exit year, which could be driven by the justification mechanism, remains a potential threat to identification. The annual wave structure of the LLFS means that onset/exit takes place between wave 2 plus one day and wave 3, and similarly for benefit inflow/outflow. If there is reverse causality and if benefit receipt inflow (outflow) precedes disability onset (exit)

[10] Estimates based on nearest neighbour (NN(1)) matching with replacement are reported in the text. The estimates are qualitatively robust to alternative matching algorithms and these estimates are available from the authors on request.

within year, estimated treatment effects may be biased upwards in magnitude. To address this concern Garcia-Gomez (2011) exact matches on onset year outcomes and only examines outcomes in the year after onset. The trade-off is that one learns nothing of onset-year effects, and because those with onset-year effects are dropped the results may end up being less generalizable. The former is particularly important in this case because it means the Garcia-Gomez (2011) approach cannot be used with the combination of treatment and control groups specified here, which only permit estimation of treatment effects in the onset/exit year. The paper therefore proceeds under the assumption that any such within-year reverse causality is negligible, although results with exact matching on outcomes in the onset/exit year, with control groups replaced by no-onset and no-exit alternatives in each case, are also presented in Table A2 as a sensitivity analysis. A further sensitivity analysis repeats this exact matching exercise with the original control groups (wave 4 onsets/exits) but with alternative treatment groups where onset/exit occurs in wave 2.

Finally, in order to examine whether and how the impacts of disability onset vary across different groups of individuals and in different labour market and policy contexts, the sample is split along the different dimensions prior to the matching procedure, with control groups, constructed in the same way as in the main estimates, also drawn from these split samples. For conciseness only the DID-PSM estimate of the treatment effect is presented in each case (see Table 6), separate balancing tests are not presented, and there is no separate discussion of identifying assumptions for the estimates of heterogeneous effects.

5. The Benefit Recipiency Impacts of Disability Onset

Post-matching estimates of disability onset effects are reported in Table 5. The first three rows of each block report post-matching levels (columns 1 and 2) and PSM estimates for the difference between treatment and control group recipiency rates (column 3) in each of waves 1-3. Under the standard CIA, and assuming negligible reverse causality within year, the wave 3 estimates give the ATTs in the year of onset. For IB/ESA and the more general sickness benefits measure, the treatment group have wave 3 benefit recipiency rates that are respectively six and nine percentage points higher than the control group, or recipiency rates that are roughly three times larger than those for the control group at the same point in time or for the treatment group in the previous year. For the any benefit measure those experiencing disability onset have a recipiency rate not quite double that for the controls, with a gap of 12 percentage points. All three differences are statistically significant at the 99% level. For non-sickness benefits

there is a six percentage point difference that is statistically significant at 95%, although the ratio of treatment group to control group (or prior treatment group) recipiency rates is smaller than for the other outcomes. It may be that some of those experiencing disability onset are switching from non-sickness benefits to sickness and disability benefits here, but the positive sign shows that there are greater numbers (net) flowing onto non-sickness benefits in response to disability onset than there are flowing off.

Table 5: PSM and PSM-DID estimates of disability onset treatment effects on proportions receiving benefits

Any Benefit	Treatment Group (0011)	Control Group (0001)	Difference	T stat for Difference
Wave 1	0.139	0.139	0.000	0.00
Wave 2	0.195	0.163	0.032	1.49
Wave 3	0.312	0.191	0.121**	4.48
Difference (2-1)	0.056	0.024	0.022	1.49
Difference (3-1)	0.173	0.052	0.121**	4.48
Non-sickness Benefit	Treatment Group (0011)	Control Group (0001)	Difference	T stat for Difference
Wave 1	0.104	0.113	-0.008	-0.74
Wave 2	0.151	0.135	0.016	0.76
Wave 3	0.197	0.141	0.056*	2.41
Difference (2-1)	0.046	0.022	0.024	0.92
Difference (3-1)	0.093	0.028	0.064*	2.55
Sickness Benefit	Treatment Group (0011)	Control Group (0001)	Difference	T stat for Difference
Wave 1	0.032	0.012	0.012	1.01
Wave 2	0.054	0.024	0.024	1.56
Wave 3	0.145	0.052	0.093**	4.30
Difference (2-1)	0.022	0.010	0.012	0.91
Difference (3-1)	0.113	0.032	0.081**	4.05
IB or ESA	Treatment Group (0011)	Control Group (0001)	Difference	T stat for Difference
Wave 1	0.010	0.006	0.004	0.65
Wave 2	0.028	0.010	0.018*	1.99
Wave 3	0.095	0.034	0.060**	3.69
Difference (2-1)	0.018	0.004	0.014	1.75
Difference (3-1)	0.085	0.028	0.056**	3.57

Notes: Balanced panel with a minimum of 4 waves within the LLFS. '0' denotes no reported disability and '1' denotes reported disability. Estimates are based on NN(1) matching and are estimated over the region of common support. T statistics are based on Abadie and Imbens (2006) standard errors. * and ** denote statistical significance at the 95% and 99% level respectively. Difference (2-1) tests for diverging prior trends. Difference (3-1) gives the PSM-DID estimate of the ATT.

Because they allow the CIA to be relaxed, the preferred ATT estimates are the PSM-DID estimates given in the third column of the fifth row in each block. It turns out that these are

very close in magnitude and statistical significance to the straight PSM estimates because wave 1 differences in all four outcomes are negligible. Further, where there are outcome measures in common with earlier papers – in particular the catchall measure of benefit receipt – the estimates presented here are qualitatively consistent with those of Jenkins and Rigg (2004), Singleton (2014) and Polidano and Vu (2015), with effects of the same order of magnitude. Note that diverging prior trends are rejected in all cases (third column and row four of each block).

Finally, estimated treatment effects are again qualitatively robust and similar in magnitude when the treatment and control groups are matched exactly on onset-year outcomes, following the approach of Garcia-Gomez (2011), thereby ensuring that disability onset precedes any change in benefit status (see Table A2). Reverse causality does not drive the estimated onset effects.

5.1. Heterogeneous impacts of disability onset

Table 6 presents the PSM-DID estimate of the treatment effect on each of the four outcomes for samples split by gender, age and so on. Few differences between subsamples in the estimated onset effects are statistically significant and few are large in magnitude. For example, there is little difference in the estimated impact of disability onset on benefit receipt by gender. Differences by qualification level, age, local unemployment rate and wave 1 employment status are also typically small and all are statistically insignificant. Taken together, the apparent near-uniformity of disability onset effects along these dimensions, at least in the year of onset itself, 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 in low unemployment areas) in driving benefit outcomes than is sometimes suggested in the wider disability benefits literature (e.g. Black et al., 2002; Autor and Duggan, 2003; McVicar, 2006). Note that inasmuch as they are comparable these results contrast with Polidano and Vu (2015) who report differences in onset effects on a catchall benefits measure by qualification level for Australia, and also with heterogeneous impacts on employment reported by qualification level (Polidano and Vu, 2015), gender and age (Jones et al., 2016).

Instead Table 6 shows large magnitude and statistically significant differences in onset effects by severity of disability (single or multiple health conditions reported) and by whether the individual reported a long-term health condition in wave 1. Specifically, more severe disability

onsets and disability onsets for those *not* reporting a pre-existing long-term health condition lead to considerably larger inflows to both disability benefit categories but no larger inflows to non-sickness benefits. The former result is consistent, again inasmuch as outcomes and proxies for severity are comparable, with Singleton's (2014) finding that those experiencing onset of more severe disability are more likely to apply for and receive DI than those experiencing onset of less severe disability, and with the Jones et al. (2016) finding that employment effects of disability onset are larger for those experiencing more severe disability onset. The latter result appears new and likely distinguishes, at least in part, between the effects of deterioration in a pre-existing chronic condition and a negative health shock for those previously in good health. In other words Table 6 suggests it is the nature of the disability onset and not the 'type' of individual or their labour market context that matters for benefit outcomes, at least in the year of onset itself.

Table 6: Heterogeneity in the PSM-DID estimates of disability onset treatment effects on proportions receiving benefits

	Any Benefit	*Non-sickness Benefit*	*Sickness Benefit*	*IB/ESA*
DID-PSM	3-1	3-1	3-1	3-1
Male	**0.138**	**0.079**	**0.087**	**0.083**
Female	**0.132**	**0.072**	**0.096**	**0.068**
Low Qual	**0.173**	**0.119**	**0.092**	**0.065**
High Qual	**0.119**	0.062	**0.078**	**0.074**
Older	**0.142**	0.059	**0.092**	**0.053**
Younger	**0.114**	0.069	**0.109**	**0.099**
Mental	0.135	0.115	0.115	0.096
Physical	**0.138**	**0.075**	**0.073**	**0.068**
Single	**0.116**	**0.102**	0.014	0.028
Multiple	**0.153**	**0.080**	**0.136***	**0.105***
Pre-2009	**0.149**	**0.072**	**0.124**	**0.116**
Post-2009	**0.156**	**0.096**	**0.064**	**0.032***
High unemployment	**0.193**	**0.137**	**0.096**	**0.064**
Low unemployment	**0.153**	**0.067**	**0.094**	**0.082**
Employed	**0.146**	**0.067**	**0.105**	**0.078**
Not employed	**0.133**	0.086	0.070	0.055
Long-term health	**0.100**	0.063	0.018	0.018
No long-term health	**0.156**	0.069	**0.134***	**0.105***
Past health	**0.222**	0.156	**0.111**	0.089
No past health	**0.135**	**0.068**	**0.087**	**0.071**

Notes: Balanced panel with a minimum of 4 waves within the LLFS, with individual samples defined on the basis of information in wave 1 or at onset as appropriate. Estimates are PSM-DID estimates for wave 3-1 based on NN(1) matching 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 * denotes a statistically significant difference from the relevant comparison group at the 95% level.

The final sample split is for onset pre and post-2009, i.e. onset under the old IB regime in place up to the end of October 2008 compared to onset under the new ESA regime in place subsequently. 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. There are also several examples in the wider disability benefits literature where benefit reforms reducing payments, increasing the stringency of medical screening, and/or conditioning on work-related activity – all of which are aspects of the shift from IB to ESA – have impacted on program growth in the desired direction (e.g. Gruber, 2000; Adam et al., 2010; Staubli, 2011; de Jong et al., 2011), although there are counterexamples (e.g. Campolieti, 2004; Karlström et al., 2008). Here, the estimates presented in Table 6 show considerably larger disability onset impacts on IB/ESA recipiency under the pre-reform regime than under the post-reform regime, with the difference statistically significant at 95%. The much smaller and oppositely-signed gap in onset effects on non-sickness benefits suggests that few of those who might otherwise have ended up in receipt of IB/ESA are displaced onto other benefits. This is consistent with Banks et al. (2015) which similarly finds little evidence of displacement onto non-sickness benefits from the ESA reforms.[11]

5.2 The benefit recipiency impacts of disability exit

Post-matching estimates of disability exit treatment effects are reported in Table 7. As in the case of onset, the first three rows of each block report post-matching levels (columns 1 and 2) and PSM estimates for the difference between treatment and control group recipiency rates (column 3) in each of waves 1-3, and the fifth row of column 3 in each block reports the preferred PSM-DID estimate of the ATT. Again as for onset, the PSM and PSM-DID estimates are similar, so the discussion below focuses on the PSM-DID estimates. Note that diverging

[11] The introduction of ESA approximately coincided with the Great Recession – from 2007Q4 to 2009Q1 in Britain – and the post-recession labour market through to 2012 was slacker than the pre-recession labour market from 2004. If anything, however, this would lead to underestimates of the impact of the reform on the benefit receipt effects of disability onset because one would expect higher recipiency rates across all income-replacement and means-tested benefits in the 2009-2012 period than in the 2004-2008 period. On the other hand, the discontinuity in disability measurement between 2009 and 2010 may lead to overestimation of the difference between pre-2009 and post-2009 if the latter period includes those with less severe disabilities than was the case prior to the discontinuity.

prior trends are rejected in all but one case, with the exception being the wider measure of sickness and disability benefits.[12]

Table 7: PSM and PSM-DID estimates of disability exit treatment effects on proportions receiving benefits

Any Benefit	Treatment Group (1100)	Control Group (1110)	Difference	T stat for Difference
Wave 1	0.241	0.241	0.000	0.00
Wave 2	0.252	0.277	-0.025	-0.75
Wave 3	0.199	0.289	-0.090**	-2.69
Difference (2-1)	0.011	0.036	-0.025	-0.75
Difference (3-1)	-0.042	0.048	-0.090**	-2.69
Non-sickness Benefit	Treatment Group (1100)	Control Group (1110)	Difference	T stat for Difference
Wave 1	0.151	0.129	0.022	1.05
Wave 2	0.171	0.137	0.034	1.11
Wave 3	0.151	0.154	-0.003	-0.10
Difference (2-1)	0.020	0.008	0.011	0.36
Difference (3-1)	0.000	0.025	-0.025	-0.87
Sickness Benefit	Treatment Group (1100)	Control Group (1110)	Difference	T stat for Difference
Wave 1	0.118	0.137	-0.020	-0.83
Wave 2	0.106	0.188	-0.081**	-3.00
Wave 3	0.059	0.185	-0.126**	-4.65
Difference (2-1)	-0.011	0.050	-0.062*	-2.09
Difference (3-1)	-0.059	0.048	-0.106**	-3.52
IB or ESA	Treatment Group (1100)	Control Group (1110)	Difference	T stat for Difference
Wave 1	0.073	0.087	-0.014	-0.63
Wave 2	0.067	0.118	-0.050*	-2.08
Wave 3	0.036	0.120	-0.084**	-3.54
Difference (2-1)	-0.006	0.031	-0.036	-1.49
Difference (3-1)	-0.036	0.034	-0.070**	-2.67

Notes: Balanced panel with a minimum of 4 waves within the LLFS. '0' denotes no reported disability and '1' denotes reported disability. Estimates are based on NN(1) matching and are estimated over the region of common support. T statistics are based on Abadie and Imbens (2006) standard errors. * and ** denote statistical significance at the 95% and 99% level respectively. Difference (2-1) tests for diverging prior trends. Difference (3-1) gives the PSM-DID estimate of the ATT.

[12] For disability exit there is unlikely to be a close parallel with the 'negative health shock' interpretation of some disability onsets. Instead, disability exit is more likely to reflect a subjective threshold being reached following gradual recovery from an underlying health condition, or following an accident, in which case divergence in outcomes between waves 1 and 2 may be interpretable as part of a broader 'recovery' treatment effect over several waves, rather than a discrete single-wave exit effect. Note, however, that diverging prior trends are rejected for this outcome measure when using all three of the alternative matching algorithms in sensitivity analysis.

With the exception of the non-sickness benefits measure, all other PSM-DID estimates differencing waves 3 and 1 reported in Table 7 are negative and statistically significant at the 99% level. In other words, assuming negligible within-wave reverse causality, disability exit leads to decreases in sickness and disability benefit receipt and disability insurance in the year of exit. These estimated ATTs are large and of broadly similar magnitude to the onset effects. Most notably, this holds for the narrowest measure of disability benefits – the IB/ESA measure – contrary to what one would expect were such benefits acting as an absorbing state until either death or state pension age is reached. In Britain over this period, claiming behaviour and/or the administration of the main income-replacement disability benefit appears to have been equally responsive to changes in disability status in either direction. That is not to preclude the kinds of long-lasting effects of temporary disability on labour market outcomes suggested by Charles (2003), Mok et al. (2008), Oguzoglu (2012a) and Meyer and Mok (2013), but it does suggest such effects may be partly mitigated if disability benefit regimes, including in the US, can be made more responsive to disability exit. The absence of an impact of disability exit on non-sickness benefit claiming suggests either that disability exit does not substantially drive exits from non-sickness benefits in the exit year where such benefits are claimed in isolation or combined with sickness benefits, or that enough of those moving off disability benefits initially switch to non-sickness benefits to replace any such exits from non-sickness benefits.

Finally, as for onset, estimated treatment effects are again generally qualitatively robust and broadly similar in magnitude when the treatment and control groups are matched exactly on exit-year outcomes, following the approach of Garcia-Gomez (2011), thereby ensuring that disability exit precedes any change in benefit status (see Table A2). Reverse causality does not drive the estimated exit effects.

6. Conclusions

This paper shows that disability onset in Britain over the period 2004-2012 substantially increased receipt of disability insurance (IB/ESA), sickness and disability benefits more generally, and non-sickness benefits all in the year of onset. It is the first paper in the international dynamics of disability literature to examine multiple benefit receipt outcome measures in this way. Onset effects are larger the more severe the onset and for onsets that reflect negative health shocks rather than a gradual deterioration in pre-existing health conditions. In contrast, onset effects vary little by individual characteristics. In other words, for

benefit receipt, at least in the year of onset, it is the type of disability onset that matters and not the type of individual. There is further heterogeneity in the impact of disability onset depending on the nature of the disability benefit regime in place at the time of onset, with a much smaller onset effect in the 'tougher' disability insurance (ESA) regime post-2009 compared to that in place pre-2009 (IB). Finally, contrary to what one would expect were disability benefits essentially acting as an absorbing state, disability exit – which is observed in the data only slightly less frequently than disability onset – has an almost symmetrical impact on receipt of disability insurance and on receipt of wider sickness and disability benefits in the exit year.

Notwithstanding caveats about the extent to which these results might generalise to other contexts and other data sources, there are several potential policy implications. First, interventions to prevent or delay impairments becoming work-limiting can reduce benefit receipt and may help to slow or reverse the growth in disability benefit rolls experienced by many OECD countries. Second, if there is to be targeting of interventions aimed at providing people experiencing disability onset an alternative to claiming disability benefits, it may be more efficient to target by the type of onset and not the type of individual. Third, the 2008 reforms to the main income-replacement disability benefit in Britain – the replacement of IB with ESA for new claimants – had a dramatic impact on reducing or at least delaying inflows, with only limited spillover effects into other benefit categories. Aspects of this reform are plausibly replicable internationally, although there remains a question as to where in the labour market or benefits system, if anywhere, the diverted claimants have ended up. Fourth, countries with disability insurance regimes that appear to act as absorbing states – the US is one example – could learn from the almost equal responsiveness of disability insurance in Britain to disability onsets and disability exits. As the British experience over this period shows, there is nothing inevitable about the kind of ratchet effect on the DI roll in the US that a lack of outflows implies.

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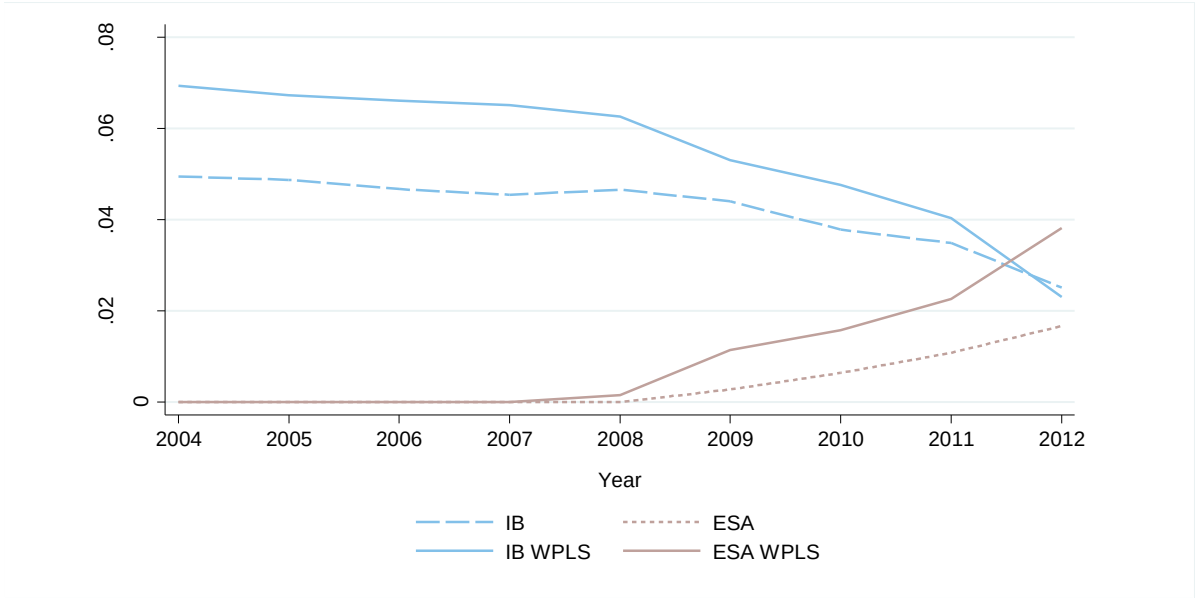
Appendix: Further Data Details

Figure A1: LLFS and WPLS Proportion reporting receipt of IB and ESA, 2004-2012



Notes: Data from the Work and Pensions Longitudinal Study (WPLS) is accessed via NOMIS. Claimant rates relate to Great Britain and are created using claimant numbers as of November each year and mid-year working-age population estimates.

Table A1: Representativeness of LLFS balanced sample

	APS (2004-2012)	LLFS (2004-2012)	
	All waves	All waves	4 waves
WL disabled	15.13	15.49	17.48
DDA disabled	15.57	16.12	18.11
Long-term health problem	28.05	28.15	32.70
Past long-term health problem	7.76	7.59	8.39
Employment	72.44	71.54	76.34
Any Benefit	15.49	16.76	14.74
Non-sickness Benefit	11.68	12.70	9.58
Sickness Benefit	6.53	7.03	8.00
IB or ESA	3.89	4.27	4.92
Gender			
Female	49.54	49.70	49.70
Male	50.46	50.30	50.30
Age	38.45	38.11	42.83
Highest qualification			
Degree	19.84	18.74	20.00
Other Higher Education	8.62	8.97	10.99
A level	22.69	22.93	22.76
O level	22.73	22.62	21.83
Other	11.60	11.72	9.82
None	14.51	15.02	14.60
Students			
Full-time student	8.70	8.74	4.83
Not full-time student	91.30	91.26	95.17
Marital Status			
Single	39.79	40.88	25.49
Married	47.58	46.16	61.53
Widowed/divorced	12.63	12.95	12.99
Children			
Dependent child in household	39.20	39.24	41.74
No dependent child in household	60.80	60.76	58.26
Housing Tenure			
Owned outright	17.29	16.38	21.57
Mortgaged	50.18	49.38	59.87
Rented	32.53	34.24	18.56
Region			
Tyne and Wear	2.66	3.67	3.19
Rest of North East	3.47	4.89	4.72
Greater Manchester	5.21	6.65	6.77
Merseyside	2.59	3.27	3.42
Rest of North West	4.36	3.54	3.43
South Yorkshire	2.16	1.97	1.91
West Yorkshire	2.95	1.42	1.36
Rest of Yorkshire & Humberside	2.99	3.31	3.23
East Midlands	5.46	2.62	1.93
West Midlands Metropolitan county	3.97	3.85	3.79
Rest of West Midlands	3.76	2.65	2.95
East of England	6.79	3.28	2.96
Inner London	4.28	4.24	2.08
Outer London	5.57	3.56	2.77

South East	11.69	9.18	7.96
South West	7.40	6.46	6.48
Wales	9.57	16.80	17.98
Strathclyde	5.47	8.15	9.83
Rest of Scotland	7.21	10.49	13.24
Northern Ireland	2.43	-	-
Local Area Unemployment Rate	0.063	0.066	0.064
Year of observation			
2004	19.72	22.45	20.07
2005	12.09	13.37	19.43
2006	10.18	9.17	16.80
2007	10.11	8.84	15.96
2008	9.86	8.79	14.03
2009	9.51	8.75	13.72
2010	9.47	9.07	-
2011	9.53	9.83	-
2012	9.52	9.72	-
Interview type			
Face-to-face	78.82	74.42	85.59
Telephone	21.18	25.58	14.41
Sample			
QLFS	59.89	-	-
LLFS	40.11	100	100
N	1,099,439	440,947	49,071

Notes: All characteristics are measured at Wave 1. The APS sample excludes the APS boost.

Table A2: PSM estimates of disability onset and exit treatment effects on proportions receiving benefits one year post onset/exit wave, exact matching on onset/exit wave outcomes

Onset	Treatment Group (0011)	Control Group (0000)	Difference	T stat for Difference
Any Benefit	0.313	0.211	0.102**	4.94
Non-sickness Benefit	0.181	0.137	0.044*	2.38
Sickness Benefit	0.179	0.062	0.116**	6.42
IB or ESA	0.108	0.031	0.077**	5.20
Onset	Treatment Group (0111)	Control Group (0001)	Difference	T stat for Difference
Any Benefit	0.419	0.312	0.107**	4.10
Non-sickness Benefit	0.213	0.193	0.019	0.87
Sickness Benefit	0.266	0.103	0.163**	5.17
IB or ESA	0.166	0.077	0.089**	3.93
Exit	Treatment Group (1100)	Control Group (1111)	Difference	T stat for Difference
Any Benefit	0.175	0.316	-0.141**	-5.85
Non-sickness Benefit	0.115	0.179	-0.064**	-3.20
Sickness Benefit	0.071	0.141	-0.071**	-3.61
IB or ESA	0.034	0.127	-0.093**	-4.26
Exit	Treatment Group (1000)	Control Group (1110)	Difference	T stat for Difference
Any Benefit	0.132	0.161	-0.029	-1.43
Non-sickness Benefit	0.116	0.105	0.011	0.80
Sickness Benefit	0.017	0.067	-0.050**	-3.19
IB or ESA	0.003	0.047	-0.044**	-3.21

Notes: Balanced panel with a minimum of 4 waves within the LLFS. '0' denotes no reported disability and '1' denotes reported disability. Treatment and control groups – note the alternative construction of both here – are exact matched on the specific benefit measure up to and including the onset/exit wave. Estimated ATTs – for wave 4 in the first and third panels and wave 3 in the second and fourth panels – are based on a NN(1) matching algorithm estimated on wave 1 characteristics over the region of common support in addition to the exact matching. T statistics are based on Abadie and Imbens (2006) standard errors. * and ** denote statistical significance at the 95% and 99% level respectively.