The London School of Economics and Political Science

The role of increasing job strain in deteriorating fitness-for-work and rising incapacity benefit receipt

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Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

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Abstract

Over the past three decades, the number of incapacity benefit claimants in the UK has trebled. Conventional wisdom argues that this rise cannot reflect ‘real’ incapacity; Britons are perceived to have got healthier and jobs to have become less physically demanding. Yet self-reported work-limiting disability (WLD) grew over the 1990s. Moreover, some working conditions deteriorated, with ‘job strain’ (the combination of high job demands and low job control) rising sharply.

In this thesis, I investigate the possibility that rising job strain partly explains the rise in WLD and incapacity benefit receipt through four pieces of empirical research.

First, different surveys appear to conflict on whether job strain has risen. Given that trends in job strain are of paramount importance, I systematically review the available trend data across 44 individual datasets.

Second, I look at whether self-reported demands and control predict WLD and health-related job loss. Using the Whitehall II cohort, I look longitudinally at whether baseline job strain predicts WLD/health-related job loss at the following wave. I also look at the extent to which WLD mediates any relationship between job strain and health-related job loss.

Third, the Whitehall II analysis is limited to civil servants and is based on self-reports. I therefore complement this analysis by looking at average job strain in particular occupations and imputing this into the nationally representative BHPS. I then relate job strain to later WLD and incapacity benefit receipt in parallel fashion to the Whitehall II analyses.

Finally, the quantitative analyses leave unanswered questions about the meaning of ‘fitness-for-work’, the processes through which working conditions affect incapacity benefit receipt, and how these impact differently on different people. These are explored in a qualitative analysis of 32 interviews with people with health problems, culminating in a conceptual model of job strain, WLD, and incapacity benefit receipt.

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Abbreviations

The full list of acronyms for the surveys reviewed in Chapter 2 are given in Web Appendix 2a. All other acronyms are listed below.

AME	Average Marginal Effect
BHPS	British Household Panel Survey
CHD	Coronary Heart Disease
DDA	1995 Disability Discrimination Act (superseded by the 2010 Equalities Act)
DWP	Department of Work and Pensions
EB	Empirical Bayes estimates
ESA	Employment and Support Allowance
GHS	General Household Survey
GHQ	General Health Questionnaire (a measure of minor psychiatric morbidity)
IB	Incapacity Benefit (the specific post-1995 benefit rather than 'incapacity benefits' in general; see footnote 4)
LFS	Labour Force Survey
LLSI	Limiting Long-Standing Illness (activity-limiting disability)
IRT	Item Response Theory
SES	Socio-economic Status
WAI	Work Ability Index
WII	Whitehall II Cohort Survey
WLD	Work-limiting disability

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[1] This includes the following surveys: the 1997, 2001 and 2006 Skills Surveys; the Employment in Britain and Working in Britain surveys; SCELI; Social Class in Modern Britain; HSE 1993-1994; WERS 1998 and 2004; the OPCS Omnibus Surveys April/May/July 1996; the Psychosocial Working Conditions Surveys 2004-2008; the Fair Treatment at Work survey; the Psychiatric Morbidity Survey 2006-7; the European Working Conditions Surveys 1991-2005; the European Social Survey 2002-2008; BSA 1985-2005; the World Values Survey 1981 and 1990 (see Web Appendix 2a for details); plus BHPS, LFS 1984-2008; GHS 1974-2006; FRS 1994/5-2005/6; the Family and Working Lives survey, and the two HALS surveys.

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INTRODUCTION

The policy issue at the heart of this thesis is that 2.5 million people are claiming (and 1.8 million are receiving)[3] incapacity benefits[4] in the UK. This is not only a significant number of people – there were three times as many incapacity benefit claimants as unemployment benefit claimants before the recession[5] – but it also represents an enormous rise since the early 1980s, as shown in Figure 1. This rise seems to have been partly caused by factors that are unique to the UK: there were rises in many other high-income countries, but the UK rise was considerably greater than in any other OECD country.[6]

The consensus among the main political parties is that this rise is not caused by a rise in genuine incapacity; instead, an increasing number of people on incapacity benefits are not really unfit for work (Black 2007; Social Justice Policy Group 2007:201; Bambra 2008). As then-Prime Minister John Major put it, *"it beggars belief that so many people have suddenly become invalids, especially at a time when the health of the population has improved"* (quoted in Anyadike-Danes and McVicar 2008:17). More recently, the then Labour adviser – and now Tory Minister for Welfare Reform – Lord Freud publicly stated in 2008 that fewer than a third of IB claimants were 'legitimate claimants'.[7] Partly as a result, incapacity benefits are now being time-limited for all except the poorest (HM Treasury 2010), while a more strict medical test is gradually raising the bar for claims (Citizens Advice Bureau 2010).

The academic consensus is similar, tracing this 'hidden unemployment' back to the destruction of manual jobs in the 1980s (Beatty and Fothergill 2005, and reviewed in depth in Chapter 1). It is seen as out-of-the-question that there has been a genuine rise in ill-health on this scale (Berthoud 1998:10; Beatty and Fothergill 2005:839; Kemp et al 2006:2; OECD 2008:237), for good reason as I show in Chapter 1.

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[3] The figures for 'incapacity benefits' include 'credits only' claimants who pass the medical assessment for incapacity benefits but have insufficient contributions to receive the benefit. They therefore receive National Insurance credits, and 85% of credits-only claimants in 2004 received Income Support, some of whom received the disability premium (a sharp rise from 69% of credits-only claimants in 1997; data from Maria Eagle's answer to the Parliamentary Question by Paul Goodman 1/11/2004). The actual numbers of incapacity benefit *beneficiaries* as a share of all *claimants* dropped from 72% in 1999 to 62% in 2007, a point the government has noted (Peasgood et al 2006:5).

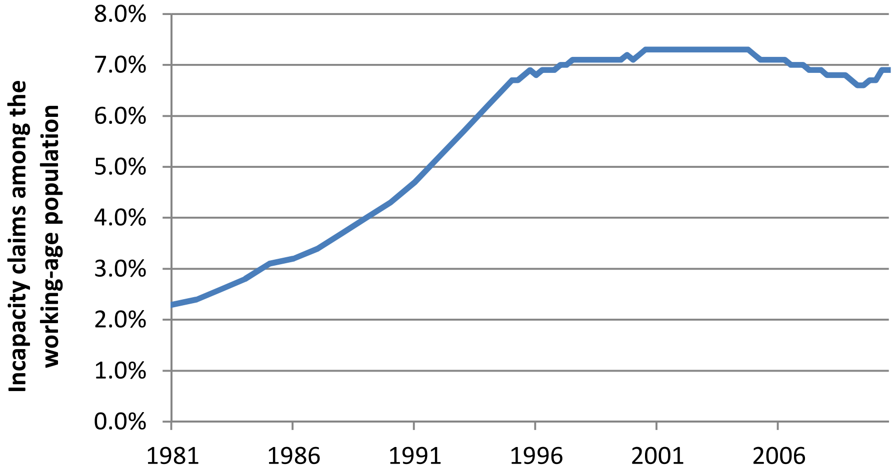
[4] By 'incapacity benefits', I am referring to all earnings replacement benefits for people with health problems or disabilities, including Invalidity Benefit (IVB; pre-1995), Incapacity Benefits (IB; post-1995), Severe Disablement Allowance (SDA), and the current Employment and Support Allowance (ESA), but not statutory sick pay paid by the employer.

[5] This figure – and all other figures on benefits receipt, unless otherwise specified – are calculated using the DWP Statistical tabulation tool at http://83.244.183.180/100pc/tabtool.html, accessed 17/3/2010.

[6] 8 of the 16 OECD countries with trend data on incapacity benefits saw rises of 45% or more 1980-1999, but the UK rise was substantially greater than the rise in any other country – a rise of 161% according to the OECD figures, compared to the next-highest rise of 88% in Australia (OECD 2003:61) (see also Kemp et al 2006:8).

[7] 'Two million wrongly get benefit', BBC News 2/2/08. http://news.bbc.co.uk/1/hi/uk_politics/7223687.stm accessed 20/3/08.

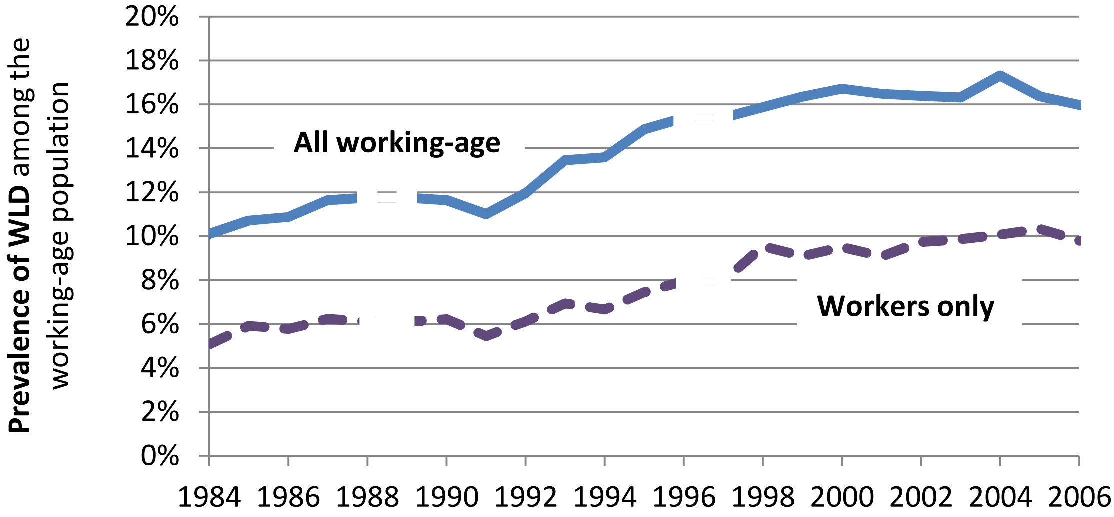
Figure 1: Trends in incapacity benefit claims in Britain



Working-age population only; includes all claimants (including those who do not actually receive incapacity benefits – see footnote 3), but excludes short-term claims. ***Sources****: (i) Benefit receipt 1981-1994: DWP 'IB-SDA long time series' using 5% administrative sample; (ii) Benefit receipt 1995-: DWP complete caseload data (WPLS); (iii) Population 1981-2009 from ONS mid-year population estimates for age 15-59/64; (iv) Population 2010: the GAD 2008-based projection for ages 15-59/64.*

However, there is another possibility that has received almost no attention from academics or policymakers: that part of the rise in incapacity benefits was due to a *genuine rise in incapacity to work*. Using self-reports of 'work-limiting disability' (WLD) – that is, people saying that their health interferes with the type or amount of work they can do – Figure 2 shows that there has been a decline in fitness-for-work in Britain, particularly over the 1990s. And this is not simply because more people are claiming incapacity benefits; even among workers there has been a doubling of WLD.

Figure 2: Trends in self-reported work-limiting disability in Britain

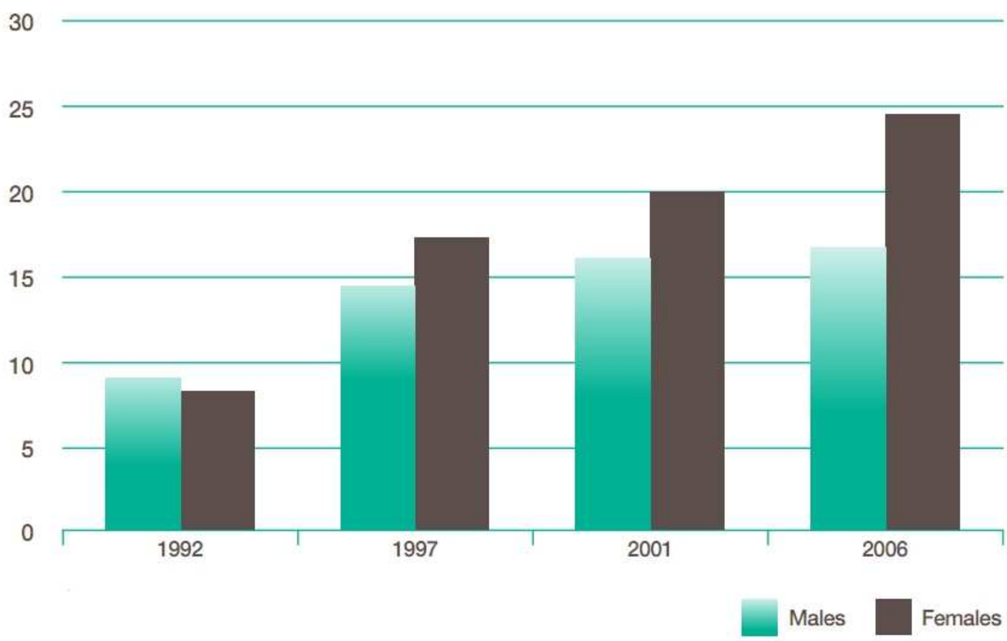


For details on sources and methods, see section 2.1.1.

One Bank of England study has even found that rising WLD explains half of the drop in male labour force participation 1984-2001 (Bell and Smith 2004). Of course, there are a great many possible explanations for this apparent trend that do not involve a genuine rise in work-limiting disability. Economists typically see WLD as a heavily-biased measure, and any of these biases could have increased over this period. For example, the 1990s saw the emergence of the 'work stress epidemic' (Wainwright and Calnan 2002), which may have made workers more likely to feel (or report being) limited at work.

But another explanation is possible – one that would have major consequences for our understanding of the trend in incapacity benefits. Reported work-limiting disability is not just a measure of health, but also depends critically on the nature of work. We might assume that the nature of work has improved with decline of heavy industry and rise of the 'knowledge economy'. But as Figure 3 shows, 'job strain' – a combination of high mandatory work demands with low job control – rose substantially over the 1990s, and continued to rise into the 2000s.

Figure 3: Trends in self-reported job strain in Britain



'Job strain' is a job with low control (below the median level of a control scale) and high demands ('strongly agree' that the job requires them to work very hard). Figure taken from Green (2008b; 2009b); see also section 3.1.3 and Chapter 2.

It is therefore possible that rising job strain contributed to rising incapacity benefits receipt, *not* by making people sicker, but rather by making it harder for sick people to work.

Yet there are many possible objections to this argument. Increases in self-reported job strain may reflect changes in how people respond to questions about their job, rather than actual changes in working conditions. The three trends presented above do not occur at exactly the same time, or show the same degree of change. And the path from job strain to incapacity benefits is unclear; any explanation of incapacity benefit receipt has to pay attention to *where* people claim and *who* ends up claiming it.

In this thesis I therefore ask, ***how far do trends in job strain explain rising levels of work-limiting disability and incapacity benefit receipt?*** In the following chapter, I review existing research that relates to some aspect of this question, and end the chapter by setting out this research question (and the more specific sub-questions) in more detail, and how I propose to answer it.

CHAPTER 1:
The research question

1. INTRODUCTION

While the introductory chapter explains the overall aim of the thesis, it is necessary to formulate this much more specifically, taking into account the wider literature. This is the purpose of the current chapter.

I begin in section 2 by providing a framework to understand fitness-for-work and incapacity benefits, and then revisiting the trends from the Introduction in this light. In section 0 I move onto the impacts of the nature of work and how this has changed over time. There are however several other explanations for the rise in incapacity benefit receipt, which I review in section 4. Finally, section 5 describes the research question (and sub-questions), and sets out the structure of the rest of the thesis.

In this chapter I draw on a wide variety of literatures, including occupational health, occupational epidemiology and occupational psychology; work sociology and industrial relations; labour economics and health economics; the sociology of health & illness and disability studies; and social policy, the 'home discipline' for this thesis. I am therefore not aiming to exhaustively review the overwhelming amount of potentially relevant work, but to link the (interdisciplinary) research question to ongoing debates in a number of fields.

2. FITNESS FOR WORK AND INCAPACITY BENEFITS

2.1. What is fitness-for-work?

The best place to begin is with a clear idea of what 'fitness-for-work' actually means, setting it within a general framework of health and disability. The classic model of disability is that of Nagi (1965), who separated out three related concepts:[8]

- Pathology: the medical determination that the body is failing to work as 'normal';
- Functional limitations: the inability to perform specific tasks;
- Disability: the inability to perform a social role.

According to the traditional 'medical model', disability is the inevitable consequence of pathologies and functional limitations. Yet when we see disability in terms of a *social*

[8] This is similar to the influential model of Verbrugge & Jette (1994) and the WHO's (2001) *International Classification of Functioning, Disability and Health*.

role, it becomes clear that the way that role is designed is equally important. A person can therefore be disabled for one social role (e.g. 'worker') while non-disabled for another (e.g. 'parent').

What matters for understanding work is therefore whether functional limitations conflict with the demands of the work role. Any other functional limitations are not relevant, even if they conflict with other social roles (Lindeboom 2006:4). This is borne out by empirical research that shows that people with a longstanding illness *but no work-limiting disability* (WLD) were as likely to work as those without any illnesses at all (Jones 2006; Hills et al 2010).

2.1.1. Trends in WLD

The Introduction has already shown how WLD has increased; in this section I supply the technical details behind this.

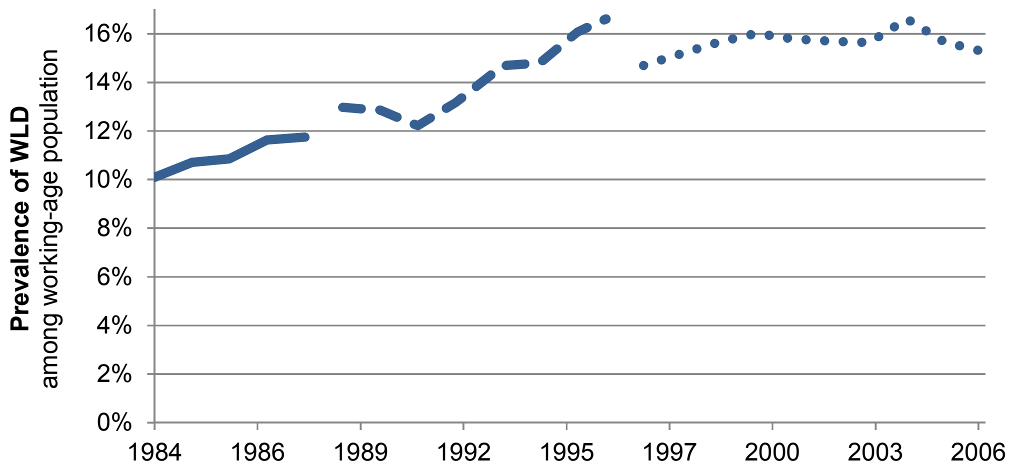
The main data source is the Labour Force Survey (LFS), which has asked about WLD in various forms since 1984. For all analyses I have excluded telephone interviews,[9] proxy interviews, and people outside of the working-age population; and used the weights from the 2003 re-weighting exercise (Appendix 1A). These steps improve comparability but have not always been done when looking at WLD trends in the LFS (Bell and Smith 2004; Faggio and Nickell 2005).

The original WLD question in the 1984 LFS first asks if respondents have any health problems or disabilities from a list of 10 conditions (including 'other health problems or disabilities'), and then asks *"Does this (do any of these) health problem or disabilities limit the kind of paid work that you can do?"* However, one problem in looking at trends in WLD is that the question structure and filtering were changed in 1988/9 and 1996/7, introducing two discontinuities into the data (Appendix 1A).

As can be seen in Figure 4, after each discontinuity the new series shows a different prevalence of WLD than before, which visually makes it difficult to see the increase in WLD over the entire period. In most figures I have therefore converted this into a single 'chained' series, with the post-1989 trends tied onto the actual value in 1987. This assumes that (i) there was no change in WLD 1988-89 or 1996-97; and (ii) the trends in the three measures of WLD have been consistent.

[9] From 1992 the LFS added follow-up waves using telephone interviews, which produce different levels of WLD.

Figure 4: Trends in WLD in the LFS, emphasising discontinuities



LFS data on the working age (16-59/64) population. See text and Appendix 1A for further details of data preparation.

Unfortunately there are relatively few other sources to validate the LFS trend against:

- The 1991 and 2001 Censuses asked people if they had a long-term illness that 'limits your daily activities or the work you can do' – effectively a combination of WLD and more conventional activity-based measures of disability (Bajekal et al 2004:47). This question shows sharp rises from 7.1% in 1991 to 12.5% in 2001 among women, and from 9.4% to 14.3% among men.[10] Given that there are few signs of an increase in activity-limiting disability over the 1990s (see section 4.1), this seems to support the LFS trend.

- Since it was set up in 1994/5, the Family Resources Survey (FRS) has asked people to choose between three responses: (1) I am unable to work at the moment; (2) I am restricted in the amount or type of work I can do; or (3) I am not restricted in the type of work I can do. Unlike the LFS and Census questions, this is not restricted to *long-term* health conditions. Combining responses of 1 and 2, this has tended to fluctuate between 13% and 14% (other than slight peaks of 14.6% and 15.0% in 1998 and 2000) – showing no rise, in contrast to the LFS.

- While not showing any trends, the level of WLD in the 1984 Health and Lifestyle Survey (HALS) was 10.0%, which matches the level of WLD in a similar question on 'work or social life' in the 1984 LFS.[11]

[10] Author's analysis of the working-age population, excluding those in communal establishments (see also Bell and Smith 2004:16).

[11] Author's analysis.

It is not uncommon for apparently similar questions in different surveys to show divergent trends. For example, US trends in self-rated health show either declines or rises depending on which of the major surveys is being used (Salomon et al 2009) (see also section 4.1 and Chapter 2). The WLD differences may also be due to the greater labour market focus of the LFS compared to the FRS (Burkhauser et al 2002:547; Bajekal et al 2004:52) or the slight changes in question wording in the 1995 and 1996 LFS (Appendix 1A) – but this does inevitably add an extra dimension of uncertainty to our understanding of trends. Nevertheless, the agreement between most of the surveys is relatively good, and the most likely picture is that there was a rise in WLD over the 1980s and 1990s in Britain.

2.1.2. The validity of WLD

But is it really possible that WLD has risen over the past 25 years? Many economists are sceptical about analysing WLD, because people's self-reports seem to be heavily affected by their labour market situation:

- First, people have strong incentives to report that they are too ill to work instead of simply unemployed: incapacity benefits are more generous than unemployment benefits and are less demanding of claimants (Alcock et al 2003:197,203-4), although this has varied over time (see section 4.3).

- Second, the sick are seen as more deserving of help than the unemployed (van Oorschot 2000), in a climate where there is a strong cultural expectation that people – and particularly working-age men – should work (Bellaby 2006:3), giving additional *non-financial* incentives to report a WLD. This will be somewhat counterbalanced by a reverse bias, where people are reluctant to admit they are sick in general (Collett 2003; Salway et al 2007) or too sick to work (Alcock et al 2003:133; Salway et al 2007:59; Barnes et al 2008:661) because of the challenge this poses to their identity and the possible negative reactions of others (see Chapter 8).

- Finally, people are less likely to see themselves as fit-for-work if their employers asked them to leave for health reasons (Alcock et al 2003:199; Guinea and Betts 2003:516) or they have struggled to find a new job (Magnussen et al 2007:193).

Teasing apart these processes is difficult – particularly given that work *per se* may genuinely affect people's health (Waddell and Burton 2006) – but there are empirical suggestions that work status does influence WLD reporting. Vignette methods offer a vivid demonstration; researchers describe hypothetical people's health in detail and then ask respondents to decide whether these people have a WLD (Salomon et al

2004). These show that non-working Americans are more likely to describe these hypothetical people as having a WLD than working Americans, although the same was not true of Europeans (Kapteyn et al 2009:30).

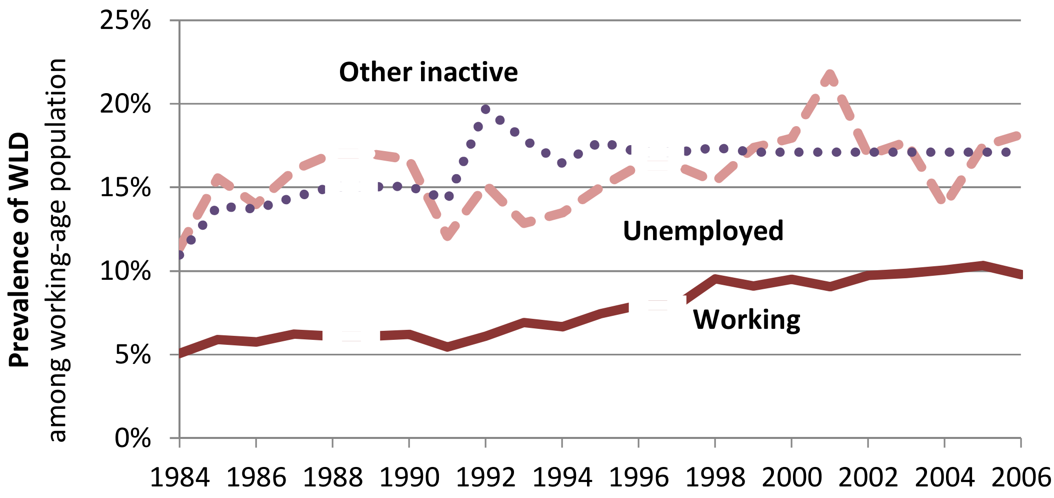
There are further reasons to be careful in interpreting work-limiting disability. WLD is a varied phenomenon, incorporating multiple different types of limitations (see Chapter 8) of different levels of severity (Berthoud 2006:9,65). Vignette methods have shown that people are more willing to describe hypothetical people as having a WLD if they are wealthier (Banks and Tetlow 2008:52), if they themselves are Dutch rather than American (Banks et al 2006; Kapteyn et al 2007), or if they know other people claiming incapacity benefits (see section 4.4.1). While some variation between groups is expected due to genuine differences in their working conditions, this seems unlikely to explain all of these differences.

Respondents may interpret questions in different ways, sometimes thinking about their current job and sometimes about their work in general (NatCen ELSA research team et al 2003; Banks et al 2006). Given that some people even with severe impairments do not report that their health limits the type or amount of work they can do (Burkhauser et al 2002:545), it seems likely that reported WLD is tightly bound up with the jobs that people either do or would consider doing (Chapter 8).

For all these reasons, many have argued that WLD is an undesirable measure of disability (see Burkhauser et al 2002:541-2). Yet I would argue that we still need to pay attention to trends in WLD, for two reasons. First, WLD is *the only measure available* if we are interested in trends in fitness-for-work rather than health. More specific and objective measures of health are valuable (see section 4.1) but provide evidence on what is fundamentally a different concept. It is partly for this reason that there is an extensive literature on WLD among health researchers and economists, both in the US (Feldman 1983; Bound and Waidmann 2002; Burkhauser et al 2002) and internationally (Borsch-Supan 2007; Kapteyn et al 2007; Oguzoglu 2010).

Second, although self-reported WLD is a biased measure of fitness-for-work, this does not necessarily mean that the trend in WLD is spurious. Contra the expectation that this simply reflects the incentives to report a WLD among increasing numbers of incapacity benefit claimants, Figure 5 shows that there was a near-doubling of WLD among *workers* alongside more inconsistent rises among the other inactive (in the 1980s) and unemployed (in the 1990s). This is unlike the situation in the US, where changes in WLD were entirely explained by changes in the numbers claiming incapacity benefits (Bound and Waidmann 2002:243-4).

Figure 5: Trends in WLD by economic status, 1984-2006



'Other inactive' includes all inactive groups bar the long-term sick. Trend shows a 'chained series' (see Figure 4) due to discontinuities 1988-9 and 1996-7. LFS data on the working age (16-59/64) population. See text and Appendix 1A for further details of data preparation.

To be clear: it is not my intention to argue here that this rise in WLD *definitely* represents a deterioration in fitness-for-work. There are several other possible explanations that I will consider in later sections, such as a decline in job satisfaction (section 3.2.3), the rise of the 'work stress epidemic' (Chapter 2), and labour demand (section 4.2). Fundamentally it is the task of the empirical chapters of this thesis to test the plausibility of whether fitness-for-work has genuinely deteriorated. Instead, my argument here is simply that there is an *apparent* rise in WLD which is surprising and requires explanation.

2.2. Incapacity benefits

While my focus here is on fitness-for-work, clearly there are many other factors that influence incapacity benefit receipt. Much has been written about 'presenteeism', and how this is more likely among those worried about job security, not wanting to let colleagues down, and with concerns about the cost of staying off work (Virtanen et al 2005; Farrell et al 2006:20,56; Barnes et al 2008:660). After taking an initial absence there are likewise a number of factors that influence the transition from sickness absence to incapacity benefits, and then whether people continue to claim incapacity benefits. I consider several factors in this chapter, and integrate them within a wider model in Chapter 8.

If we accept that incapacity benefits are influenced by non-health factors, then does this mean we should abandon our efforts to explain incapacity benefits through fitness-for-work? This is certainly implicit in much current debate (see Introduction), including academic contributions based on the biopsychosocial approach that emphasise the role of the individual's beliefs about their health condition (Wade and Halligan 2004). For example, the former DWP chief physician Mansel Aylward has argued that *"[common] health conditions do not explain long-term incapacity"* (Aylward and Sawney 2007:75), and even that *"psychosocial issues not health condition[s] predominate"* (Aylward 2008:3), with less than 25% of incapacity benefit claims seen as being primarily a matter of health. Some health researchers have even said outright that 'malingering and illness deception' is a likely cause of rising incapacity benefits receipt (Halligan et al 2003:20).

Yet there is equally incontrovertible evidence that health has a strong relationship with incapacity. People on incapacity benefits say that their health is a barrier to work, that if they go back to work it is usually because their health changed, that when in work their health is often a barrier to staying there, and that if they leave work again then their health is often a reason for their work ending (e.g. Bailey et al 2007:129-138). Even without relying on people's own explanations of their situation, there have been various convincing demonstrations that health predicts labour market withdrawal in general and incapacity benefits in particular.[12] And official reports have consistently found that levels of outright fraud in incapacity benefits are low (Halligan et al 2003:215; DWP 2011a).

None of this is very surprising: incapacity benefit receipt is fundamentally *both* a health phenomenon and a non-health phenomenon (Peasgood et al 2006), and therefore evidence on the role of non-health factors is *not* evidence that people claiming benefits have no health problems (Bound and Waidmann 2002:247). Or to put it another way, it is insufficient to explain incapacity benefits solely in terms of fitness-for-work, but equally insufficient to exclude fitness-for-work from our understanding entirely. The question in this thesis, then, is *how far* does changing fitness-for-work explain changing incapacity benefit receipt in the midst of these many other influences?

2.2.1. Trends in incapacity benefits

In the Introduction I showed the rise in incapacity benefits receipt, from under 2.5% of the working-age population to around 7.0% - a time when sickness absence was either

[12] This includes studies based on self-reported health (Molho 1989; 1991); an impairment-based disability severity scale (Berthoud 2006:11); instrumented health variables (Disney et al 2006:633; Jones 2007; Garcia-Gomez et al, in press:26); and biomarkers (Banks 2006:292). Sutton et al (2010) also show that incapacity claims for mental health are one of the strongest predictors of NHS mental health costs in local areas.

stable or declining.[13] Unfortunately, understanding the trends in the underlying transitions is more difficult. At first glance it appears that on-flows (people moving on to incapacity benefits) and off-flows (people leaving incapacity benefits) show different patterns:

- In the late 1980s and early 1990s there was a rise in on-flows (Berthoud 1998:30,34; Kemp and Thornton 2006:150; McVicar 2008:9); on-flows from employment rose from 0.9% to 1.3% of the working-age population (Endean 2005). At the same time, there was a much sharper decline in short-term (Endean 2005) or medium-term (Anyadike-Danes and McVicar 2008) off-flows, leading to a rise in the average duration of benefit claims (Kemp and Thornton 2006:151).

- After the reforms to incapacity benefits in 1995 (see section 4.3) until the late 2000s, on-flows fell steadily – but off-flows also fell (Kemp and Thornton 2006:150; Black 2008:34; Banks et al 2011).

On a descriptive level, rising incapacity benefits since the 1980s are therefore primarily due to consistently declining off-flows, rather than the rise and then fall in on-flows (Walker and Howard 2000:170; Endean 2005; Kemp and Thornton 2006:151; Anyadike-Danes and McVicar 2008). Yet interpreting this is difficult. It has become progressively harder to claim incapacity benefits since the mid-1990s as eligibility criteria have been tightened (see section 4.3). If we raise the bar for claiming benefits, then we would expect the people claiming the benefits to be sicker, and therefore less likely to leave benefits quickly. In this situation, declining on-flows and off-flows in the post-1995 period could plausibly be the result of (i) deteriorating fitness-for-work, plus (ii) tightened eligibility criteria for incapacity benefit receipt.[14] Moreover, by 1999 – when most countries had tightened their eligibility criteria for incapacity benefits (see section 4.3) – the UK had the highest onflow rate onto incapacity benefits in the OECD (OECD 2003:72-73). It is worth bearing in mind these figures on flows, but their actual implications for the hypothesis here are uncertain.

The picture is further complicated by the changing case mix of incapacity benefit claimants, in two ways. First, trends have been different in different age groups; at least since the mid-1990s when age data are easily available, there were rising claim rates among women 25-59 and men 25-49, but sharply declining rates among men aged 50-59 and 60-64.[15] Second, over the 1980s and early 1990s, incapacity benefits

[13] Sickness absence was stable 1984-1997 before declining more recently (Barmby et al 1999, cited by Barham and Leonard 2002:178-9; Black 2008:33; Leaker 2008), possibly due to rising pressures for presenteeism (Barnes et al 2008:660) and changes in sick pay (section 4.2).

[14] Declining off-flows and on-flows may also partly result from the introduction of the Disability Discrimination Act (section 4.4.2).

[15] Author's analysis of DWP data.

were less likely to be claimed for respiratory conditions and circulatory diseases, and more likely to be claimed for musculoskeletal disorders. From the late 1980s the absolute number of mental health claims rose (Moncrieff and Pomerleau 2000), but it was not until the mid-1990s that the *share* of mental health claims in the total stock began to rise. Since then the number of claims due to mental health has risen by 80%, while there have been slight falls in musculoskeletal (10%) and other (18%) claims, trends that can be seen across most high-income countries (OECD 2003:86; Kemp et al 2006:12). Rather than a single trend, then, it appears that the rise in incapacity benefits includes multiple possible trends – leading to rising musculoskeletal claims prior to the mid-1990s, and rising mental health claims since (see also section 4.2).

These are the facts that any potential explanation of rising incapacity benefit receipt must reflect. It is likely that there is no single explanation, but instead a range of factors operating in different ways at different times. In the rest of this chapter, I consider various possible explanations in turn.

3.1. Job demands and control

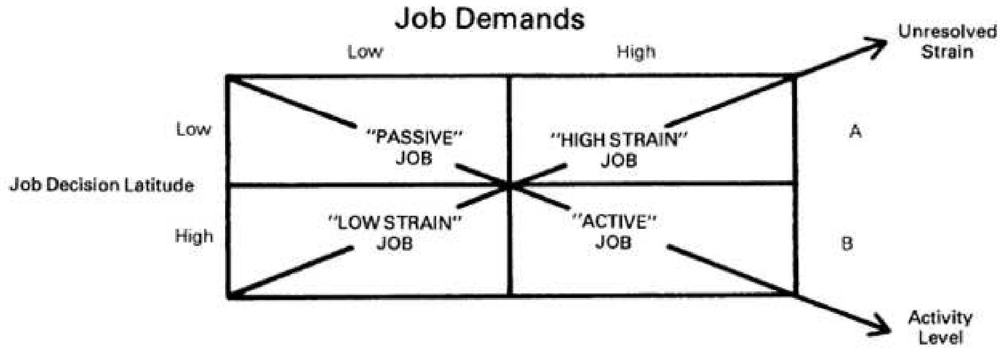
In linking working conditions to incapacity benefits I focus on demands and control, partly because these are two of the few relevant job characteristics for which good trend data are available (see below) – but also partly because the 'demands-control' model is perhaps the most widely-used model of work stress over the past three decades (Karasek et al 1998; Bambra et al 2007), and seems particularly likely to link to fitness-for-work.

As originally formulated by Karasek (1979), the demands-control model hypothesised that the combination of high job demands and low decision latitude – known as 'high job strain' – leads to mental strain. Job demands refers to mandatory work effort, measured by questions such as 'do you have to work very fast?' and 'do you have enough time to do everything?' (this is sometimes termed 'quantitative demands', to separate it from e.g. cognitive demands (Kristensen et al 2002)). Decision latitude refers to decision-making freedom, measured by questions such as 'I have a good deal of say in decisions about work' or 'I can decide when to take a break' (Karasek and Theorell 1992:337; Karasek et al 1998).[16] Karasek often combines decision latitude with 'skill discretion', which I describe further in section 3.1.2.

Different combinations of demands and decision latitude lead to four different types of job, illustrated in the widely-cited matrix in Figure 6.

[16] The standard questionnaire for demands-control is Karasek's 'Job Control Questionnaire', but this is copyrighted. Questions are instead taken from the Whitehall II questionnaire, which is loosely based on the JCQ.

Figure 6: Karasek's model of Job Strain



Reprinted from Karasek (1979)

A noteworthy feature of this model is that in addition to the distinction between high-strain and low-strain jobs (the 'unresolved strain' axis), Karasek also considers the difference between 'active' and 'passive' jobs (the 'activity level' axis). 'Active' jobs are those where high demands are matched by high decision latitude and therefore *"enable individuals to experience positive stimulation, success and self-efficacy"* (Siegrist 2006:S7), leading to high work motivation and learning opportunities (de Jonge and Kompier 1997; Karasek et al 1998). In contrast, high-strain work is hypothesised to lead to residual psychological strain, as low control reduces the possibility to both manage and cope with high demands (Karasek and Theorell 1992:33).

Since Karasek's seminal paper, an enormous literature has looked at the impact of demands, control and job-strain on different aspects of health; I restrict myself here to summarising some of the still considerable number of recent reviews and meta-analyses, based initially on Chandola's review-of-reviews (2010:54-59),[17] noting that these reviews include many of the same studies:

Heart disease

- Belkic et al (2004) found significant effects of job strain on <u>Coronary Heart Disease (CHD)</u> in 8 of 17 longitudinal studies and 6 of 9 case-control studies, which they interpret as 'strong and consistent evidence' of an effect (p107), particularly given 'biases to the null' from imputation methods (see below) and long follow-ups.

- Kuper et al (2002:297) found that psychosocial work characteristics in general (including effort-reward imbalance, see below) had significant effects on

[17] Excluding the review in Danish by Netterstrom & Kristensen (in Ugeskr Laeger), whose abstract indicates that it found that 11 of 19 studies found a clear association of job strain with ischaemic heart disease, together with partial associations in 3 further studies – although null results were found in all 5 US studies.

myocardial infarction in 5 of 13 studies (plus 5 studies that were moderately supportive or supportive in subgroups).

- Eller et al (2009:96) found that 6 of 16 studies (men) / 1 of 5 studies (women) showed a relationship between job demands and *ischaemic heart disease*, as did 3 of 13 studies (men) / 2 of 4 studies (women) on control. They interpreted this as showing moderate evidence for an effect of demands, and no evidence for an effect of control.

Other physical health outcomes

- Ariens et al (2001) found moderate evidence – albeit primarily from cross-sectional studies – that job demands (10 studies) and job control (6 studies) affect *neck pain*.

- da Costa & Vieira (2011) found 'reasonable evidence' for an effect of psychosocial factors (bundling in high distress and monotonous work with low control) on *shoulder disorders*, but found no studies of fibromyalgia or non-specified upper limb, elbow/foreman, and lower-limb disorders.

- Bongers et al (2002) found some evidence that high job demands predicted *shoulder/elbow/wrist/hand symptoms* (10 of 16 studies were significant), but less evidence for job control (6 of 16 studies were significant) – although again the studies were nearly all cross-sectional.

Mental health

- An update of Stansfeld and Candy (see below) by Netterstrøm et al (2008a) focusing on *depression* found inconsistent evidence across five studies, particularly for control, but found 'moderate evidence' (p129) for an effect of demands.

- Nieuwenhhuijsen et al (2010) found a significant impact of job demands (3 studies) and job control (2 studies) on *stress-related disorders*, measured by a scale of minor psychiatric morbidity (GHQ).

Other outcomes

- de Lange et al (2003) find that 6 of 12 longitudinal studies show a significant impact of job strain on *general health/wellbeing*.

One problem with this 'bean-counting' of significance levels is that it arbitrarily classifies effects as 'significant' or 'non-significant' without taking into account the precision of the estimate, the size of the effect, and the cumulative picture (Orlitzky et al 2003). Meta-analyses are therefore preferable, of which there are three:

- A conservative (random-effects) meta-analysis by Kivimaki et al (2006:436) found an effect of job strain on *CHD* when adjusting for age and gender, and including occupation-average studies (see below). However, this was substantially attenuated and non-significant when they adjusted for potential confounders and mediators, which they summarise as 'mixed' evidence in support of a causal effect (p438).

- Stansfeld and Candy (2006) found overall significant impacts of decision authority, job demands, and (most strongly) job strain on *common mental disorders*.

- Bonde (2008) found similar results to Stansfeld and Candy when focusing on the subset of studies concerned with *depression or depressive symptoms*.

In other words, there is reasonably strong evidence that job strain affects mental health disorders, but more inconsistent evidence as to its impact on CHD, and weak but suggestive evidence for musculoskeletal disorders. This conclusion must however be tempered slightly by evidence of both publication bias (Netterstrom et al 2008:126) and citation bias (Kuper et al 2002:309).

Far fewer studies test the hypothesised interaction effect of demands and control beyond their individual effects, however, and those that do usually find that the interaction is lacking (de Jonge and Kompier 1997; Taris 2006; Bonde 2008). While it is unclear if Karasek hypothesised an interaction (Warr 1990:285; Van der Doef and Maes 1999:89), there is only weak evidence for such an effect.

We may nevertheless expect high demands to be *beneficial* for some outcomes, given that workers feel rewarded if they are able to meet the challenges in their job (Karasek and Theorell 1992:64; Wainwright and Calnan 2002:114) as suggested by numerous psychological theories (Warr 1990:286; Karasek and Theorell 1992:90-1; Lepine et al 2005; Rydstedt et al 2006:7). The net result can be nonlinear effects of job demands, with both low and high (compared to moderate) demands having harmful effects (Stansfeld and Candy 2006:453; Green 2008b:4,17).[18] Nonlinearities are not always

[18] High *control* may also be experienced as stress in the form of stressful responsibility (Burger 1989, particularly for those with an external locus of control; Karasek and Theorell 1992:50; Schaubroeck and Merritt 1997; Wainwright and Calnan 2002:106; Meier et al 2008). In practice, though, the net effect of control is usually beneficial at all levels.

found, and where found are not always large in size (Rydstedt et al 2006), but here I must be open to the possibility.

3.1.1. A causal effect?

Even for the seemingly consistent effect of demands-control on mental health, there has been a long-running debate as to whether these results show causality. A plausible confounder is 'negative affectivity' – a disposition towards feeling negative emotional states. While some studies suggest that negative affectivity affects both work stress and health (Judge et al 2000), others argue that negative affectivity can be a *consequence* of poor working conditions (Karasek et al 1998:350; Spector et al 2000; Spector 2006:226).

A related issue arises with socioeconomic status (SES), which is tightly linked to control. Some studies suggest that the apparent effect of job demands-control is due to class, income or education (e.g. Huisman et al 2008) (and also implied by Macleod et al 2001) – but others argue that work-based measures of SES will attenuate the true effect of demands-control (North et al 1996:337) and therefore exclude work-based SES controls from their regression models (Kuper and Marmot 2003), although this is contentious (Karasek et al 1998:326; Rugulies et al 2007:307).

In the face of such factors that could plausibly be confounders, mediators or collinear variables, we can include extensive controls in regression models on the assumption that any residual association is very likely to be an underestimate of the true causal effect (see Chapters 3 and 4). In practice, the impact of controlling for negative affectivity is generally minimal, at least for mental health outcomes (Stansfeld et al 1999; Paterniti et al 2002; see also Waldenström et al 2003; Stansfeld and Candy 2006; Henderson 2010:77). Such results have been sufficient for Chandola and many other reviewers to conclude that there is a causal effect.

Still, it remains possible that the type of person who reports their job as high-strain is the same type of person that is likely to suffer (or report) ill-health, irrespective of their actual working environment. Researchers have found three ways of testing whether job strain has a genuine causal effect. The first way is to use ***aggregated self-reports*** within particular work units or occupations as a more objective measure of the work environment (or to use occupational scoring systems based on a mixture of self-reports and expert assessments such as the US O*NET system; see Cifuentes et al 2007). These overcome any biases from relying on individual self-reports of both the work environment and of outcomes ('common method bias').

The results when using these techniques are less consistent than for conventional studies – particularly for job demands, which only clusters relatively weakly within work units or occupations. Some reviews argue that these support the view of demands-control as having a causal effect, on the grounds that *some* of these studies still find an effect (Theorell and Hasselhorn 2005:518) – or even argue that the aggregated data are unhelpful in looking at the *individual feeling* of being subject to demands (Eller et al 2009:95). Others disagree. Earlier this year, a paper in the *American Journal of Epidemiology* (Kolstad et al 2011) compared workers' self-reports with the average job strain in their work units, finding that the significant effect of job strain on depression in the former was not replicated in the latter – interpreting this as evidence that the relationship on the individual level was spurious. I return to such studies in more detail in Chapters 5-7.

A second method is to use more ***objective ratings from managers or assessors***. A series of papers from the influential Whitehall II study finds some evidence for a causal effect: low control as assessed by personnel managers raises the risk of heart disease (Bosma et al 1997; Bosma et al 1998a; Bosma et al 1998b), long-term sickness absence (Stansfeld et al 2000) and (non-significantly) the biomarker fibrinogen (Brunner et al 1996). Yet no effect of control was found on mental health outcomes – indeed, there were slight signs that that high control was associated with *worse* outcomes in some cases (Stansfeld et al 2000). Moreover, high demands sometimes *reduced* the risk of poor mental health, poor social functioning (both for men only), long-term sickness absence (Stansfeld et al 1999; Stansfeld et al 2000) and possibly also heart disease (implicit in Bosma et al 1998a). Outside of this civil servant cohort, studies vary in whether they find null (Grebner et al 2005), partially significant (DeSanto Iennaco et al 2010), or significant (Waldenström et al 2008) harmful effects of externally-assessed demands on poor mental health/wellbeing.

Manager/assessor ratings have been criticised for being the still-subjective views of 'experts', that over-generalize from limited information, ignore rarer events and are limited to observable behaviour (Semmer and Zapf 1996:295; Spector et al 2000; Theorell and Hasselhorn 2005; Daniels 2006:270). They therefore have weak (Green 2001) – and sometimes non-existent (Green and James 2003:69) – relationships with workers' self-reports, although they correlate more strongly with *aggregated* self-reports of working conditions (Spector 1992:128; Green 2008a:11). It has been argued that it is individual perceptions rather than external assessments that cause mental strain (Stansfeld et al 1999) – but if the 'objective' environment does not influence perceptions, then it is unlikely that genuine changes in working conditions have led to increased WLD or incapacity claims.

The final strategy is to look at ***exogenous influences*** on demands-control, and see if these also influence health – but this strong design is rare. There is some evidence that downsizing/privatization leads to ill-health (Egan et al 2007), but these studies are often small-scale and/or low-quality (Gustafsson and Saksvik 2005; Petterson et al

2005). There a greater number of studies that look at interventions that aim to change the working environment (Chapter 9), but it is rare to test how far these interventions affect health *specifically through changes in demands-control*; even many of the better studies will often only mention that demands-control deteriorated at the same time as health decreased (Petterson et al 2005; Bambra et al 2007). Mediation analyses do exist, such as the small oft-cited study by Bond & Bunce (2001) that shows that job control mediated the positive effects of a work reorganization intervention. However, these intervention studies do not overcome the need to look at self-reported demands-control as we need to check that the intervention actually had the intended effect (Daniels 2006:272) – particularly as *other* working conditions may also be affected (Kivimaki et al 2000). Moreover, such mediation analyses rely on additional assumptions that lose much of the benefit of looking at an exogenous influence on demands-control (Chapter 4), and *prima facie*, publication bias seems likely.

In this context one set of studies in particular stands out. Virtanen et al (2008) looked at high bed occupancy rates in Finnish hospital wards as a proxy for nurses' job demands. They found a dose-response relationship between overcrowding and antidepressant use, but no reverse causality whereby antidepressant use led to overcrowding. This was developed statistically in a later study by using bed occupancy as an instrument for job demands, which showed that that using self-reports led to *under*-estimates of the causal effect of demands (Kivimaki et al 2010b). At least in this case, it appears that the observed effect of job demands on mental health represents a genuine rather than spurious effect – although even some of the authors involved note that *"more and better evidence is needed before any firm conclusions can be drawn and alternative explanations be excluded"* (Kivimaki et al 2010a:87).

In conclusion, narrative reviews generally argue that there is strong evidence for a causal effect of job strain on a variety of health outcomes (Baxter et al 2009:55; Siegrist et al 2009:27; Chandola 2010; Leka and Jain 2010:89). However, the evidence reviewed here seems uncertain. The most systematic reviews and meta-analyses consistently find that self-reported demands-control influence mental health disorders, but results are inconsistent or weak for heart disease and musculoskeletal disorders. Moreover, methods that rely on more objective methods of assessing the work environment tend to find negative effects of low control but null or even beneficial effects of high demands. The most convincing instrumental variables study shows that high demands predicts poor mental health among nurses – but this is only a single study. A strong causal effect is therefore plausible but not firmly established, and there is a need for research that tries to use (and reconcile) different methods – an approach that is taken for the rest of this thesis and which feeds into the research recommendations in Chapter 9.

3.1.2. Demands-control and fitness-for-work

The Karasek theory of demands-control is fundamentally a theory of strain; in other words, a theory about how objective features of working conditions lead to a particular form of stress that can ultimately lead to heart disease and poor mental health. However, my interests here are broader than this; I am not just interested in ill-health but in *fitness-for-work.* I hypothesise that at a given level of physical or mental health, workers in high-strain jobs have a lower fitness-for-work than those in other jobs *simply because it is harder for people with health problems to continue doing a high-strain job.* In this thesis I will divide (i) the health pathway through which demands-control affects fitness-for-work, from (ii) this non-health pathway, which I will refer to as the 'job requirements pathway'.

Later in the thesis I discuss the temporal lag between cause and effect (Chapter 3), and specify the precise mechanisms through which this could occur, building on the existing body of qualitative research on health and work (Chapter 8). Nevertheless, there are strong *a priori* grounds for suspecting that demands and control affect fitness-for-work. A health problem that limits productivity is more likely to be a problem where demands are higher and the gap to the desired productivity level is larger (as hypothesised in Bloch and Prins 2001:43-45). And where job control is higher, workers will be more able to fit their health problems around their work without limiting productivity or taking absences, as in the model of 'adjustment latitude' (Johansson and Lundberg 2004).

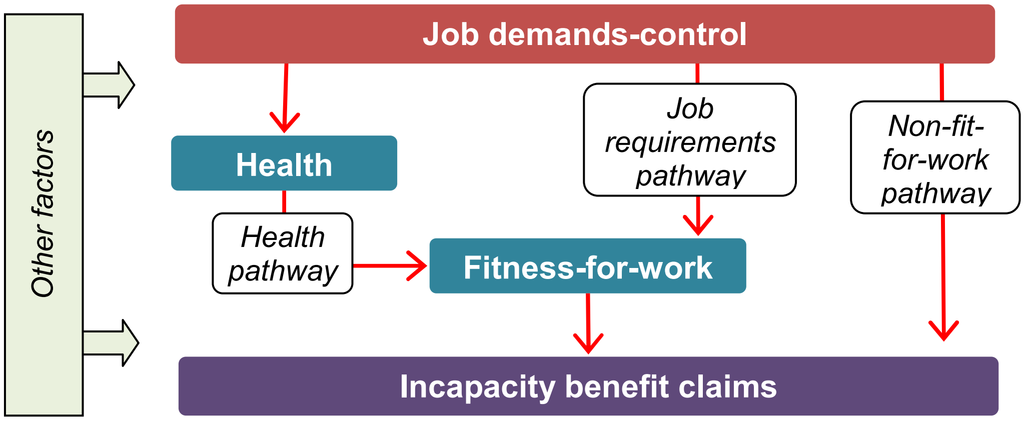
It is at this point necessary to clarify one aspect of the Karasek model from which I will be departing. Karasek's concept of 'decision latitude' is in fact made up of two related but independent constructs: 'skill discretion'[19] (the breadth of skills used by workers on the job) as well as 'decision authority' (which I refer to as 'control'). These are combined as *"a high level of skill gives the worker control over which specific skills to use to accomplish the task"* (Karasek and Theorell 1992:58). However, it is slightly confusing to describe skill discretion as 'job control' – questions such as 'my job demands creativity' also capture broader elements of the job (de Jonge and Kompier 1997:242). While skill discretion may well be related to strain, I do not see how it would be related to fitness-for-work, and as such I focus entirely on decision authority (although I return to this in Chapter 8).

Aside from their impacts on fitness-for-work, I also expect demands-control to affect labour market decisions through other mechanisms (Bound et al 1995:S251; Volanen et al 2010), shown in a simple schematic in Figure 7. The first two pathways are the health and job requirements pathways that I have already described, but to these I add

[19] Skill discretion is measured by questions such as 'do you have the possibility for learning new things through your work?' (taken from the Whitehall II survey).

a *non-fit-for-work pathway* that includes e.g. the effect of job strain on job satisfaction and the pressure of job demands on presenteeism (see section 2.2 and Chapter 8).

Figure 7: Causal pathways between demands-control and incapacity benefits



Perhaps surprisingly given the voluminous literature on demands-control, there are very few studies that investigate the job requirements pathway from job strain to fitness-for-work and then to labour market outcomes. Some studies have found that WLD influences work status independently of measures of health (Banks and Tetlow 2008:54; Kapteyn et al 2008:504), but job strain is not considered. The few studies that provide direct evidence on the pathways are those that looked at the residual correlations between demands-control and performance at work (Karlsson et al 2010; van den Heuvel et al 2010), sickness absence (Vaananen et al 2003; D'Souza et al 2006), retirement intentions (Heponiemi et al 2008) or leaving work (Blekesaune et al 2008) after controlling for health. Yet even in these rare cases, mediation analyses are not always conducted, the measures of health are variable, the methodological quality is often low, and there are likely to be other factors than fitness-for-work operating (section 2.2).

Many more studies have linked demands-control to the Work Ability Index (WAI), an instrument now translated into 20+ languages (Ilmarinen and Tuomi 2004; Ilmarinen 2007). The WAI has been shown to be a good predictor of workplace performance (Van den Berg et al, forthcoming) and incapacity benefits (Nygard et al 2005; Alavinia et al 2009a). There are also numerous other measures of workplace limitations/productivity (Prasad et al 2004; Escorpizo et al 2009), such as the Work Limitations Questionnaire (Lerner et al 2001; Munir 2008). Emerging findings suggest that job control is particularly important for performance at work for those with low work ability (Van den Berg et al, forthcoming).

Yet interpreting these results is difficult, for two reasons. First, the WAI is *itself* a fusion of health measures and fitness-for-work measures (as shown in the factor analysis of Martus et al 2010). As such, the Work Ability Index cannot be interpreted as a measure of fitness-for-work (rather than health) unless it is separated into its two components. Second, even when using only self-reported work ability, it is rare that the health and non-health pathways from job strain to work ability are separated (Reiso et al 2003; Heponiemi et al 2008), and when this is done it is even rarer for the pathways to be interpreted in this way (Solem 2008).

Recent studies by Alavinia and colleagues probably come closest to my questions here: they looked at the effect of job autonomy on productivity/absence after controlling for WAI (Alavinia et al 2009b; Alavinia et al 2009c), or the effect of demands-control on WAI after controlling for health (Alavinia et al 2007). These suggested that the effect of demands-control on WAI is only minimally mediated through health (Alavinia et al 2007), but that WAI mediates the effect of control on incapacity benefits (Alavinia et al 2009a).[20] This fits with my hypothesis, but explicit mediation pathways are never tested, and the measures of health included are relatively few.

Irrespective of whether through the health or job requirements pathways, we need to know whether demands-control influences fitness-for-work and the various related outcomes, and there is a moderately large existing literature addressing this:

- *Fitness-for-work*: a number of cross-sectional studies show a relationship between demands-control and the WAI (Tuomi et al 2001; Alavinia et al 2009a; Bethge et al 2009), WLD (Gould et al 2008:104) or self-reported work ability (Gould et al 2008; Prumper et al 2011), as well as related concepts such as poor performance at work (van den Heuvel et al 2010; Van den Berg et al, forthcoming). However, such findings are not universal (change in WAI in Tuomi et al 2001; Solem 2008:61; demands in Alavinia et al 2009a). A recent review of both cross-sectional and longitudinal studies found evidence that both job demands (5 of 7 studies were significant) and job control (3 of 4 studies) influence the WAI, both cross-sectionally and over time (van den Berg et al 2009). There is less evidence for other measures of work functioning (Lagerveld et al 2010:281).

- *Expectations and intentions*: a number of studies show that people in high-strain jobs expect to retire at an earlier age than those in other jobs (e.g. Sutinen et al 2005; Volanen et al 2010). More specifically on fitness-for-work,

[20] The study only implies this rather than testing this directly – they show a cross-sectional relationship of job autonomy with the WAI, a longitudinal effect of the WAI on disability pension within the next two years, and no direct of job autonomy on disability pension controlling for the WAI. However, they neither explicitly test the indirect pathway (see chapter 3), nor do they examine the influence of job autonomy without adjusting for WAI score.

Europeans in high-strain jobs (compared to those in other jobs) are much less likely to say they expect to be *able* to carry out their job at age 60 (Villosio et al 2008:47).

- *Sickness absence:* while there are more studies of sickness absence than any of the other fitness-for-work-related outcomes, there are still relatively few compared to e.g. heart disease, with particularly few longitudinal studies eligible for inclusion in reviews (Vahtera et al 2000:59; Rugulies et al 2007:294; Chandola 2010:59).

  Nevertheless, high job strain does appear to increase the risk of sickness absence (the review of 3 studies in Chandola 2010:59). Looking separately at demands and control, several reviews (of a small number of individual studies) suggest that control does reduce the risk of sickness absence (Allebeck and Mastekaasa 2004:57; Lund et al 2005; Duijts et al 2007; Rugulies et al 2007:294,301), including in the form of 'adjustment latitude' (Johansson and Lundberg 2004; Hultin et al 2010) or flexitime (Baltes et al 1999). This conclusion is strengthened by a study using work-unit averages (Virtanen et al 2007) and the results of intervention studies affecting control, such as one general review (Bambra et al 2007) or a study of flexible working interventions (Dalton and Mesch 1992).

  The findings for job demands are more mixed (Rugulies et al 2007:294,301). It appears that demands primarily increases the risk of long-term absence and has null (or even beneficial) effects on short-term absence (Allebeck and Mastekaasa 2004:57; Kaye 2004a:58; D'Souza et al 2006; Duijts et al 2007; Roelen et al 2007; Virtanen et al 2007), although not all studies agree (Lund et al 2005). This suggests that high demands are an additional pressure for presenteeism (see section 2.2) while simultaneously causing ill-health, reduced fitness-for-work and greater long-term absence. As before, there are only weak signs of the hypothesised interaction between demands and control (Allebeck and Mastekaasa 2004:57).

- *Return-to-work after sickness absence (RTW)*: of the few studies that exist, some suggest that neither demands nor control influence return-to-work among those who already absent due to illness (Reiso et al 2001; Janssen et al 2003; Schultz et al 2004 cited by Kuijer et al 2006:456; Lund et al 2006). In contrast, others find an effect of control (Johansson et al 2006; Fukuoka et al 2009) or job strain (in countries where most people return-to-work with their old employer; Bloch and Prins 2001:92,277).[21]

Interpreting studies on return-to-work is difficult for the reasons given in section 2.2.1; that is, demands and control may influence the type of person who is absent in the first place, leading to selection effects. There are however some signs that accommodations at work that *change* demands and control may help people return-to-work (Chapter 8), and that accommodations in general delay the transition from WLD to incapacity benefits (Burkhauser et al 1999).

- *Health-related job loss*: Haahr et al (2007) look at job losses that respondents felt were partly due to health in Denmark. They found no significant effect of low control after controlling for health/SES/work factors, although the survey was relatively low powered (n=135 job losses).

- *Incapacity benefits* (aka 'disability pension' or 'disability retirement'): there are a relatively small number of high-quality studies that link demands-control to incapacity benefits. Given that these have to my knowledge not previously been systematically reviewed, I here present descriptions of each study in turn (grouped by country) before summarising the overall picture:

    1. Borsch-Supan and Roth (2010:6) found that psychological demands of work do not predict incapacity benefit receipt among 50-64 year old Europeans in SHARELIFE.

    2. Alavinia et al (2009a) found no significant effect of demands or control on disability pension among male Dutch construction workers – although only univariate analyses were performed in a form that can be reviewed here, and the power of the analyses was low (40 cases of disability pension).

    3. Stattin and Jarvholm (2005) found that both demands and control have strong impacts on disability pension in male Swedish construction workers, although the study has very few controls beyond physical workload.

    4. Christensen et al (2008) found a significant effect of job control on disability pension in Denmark, although this study again has few controls beyond physical/ergonomic working conditions.

[21] It also seems in Bloch et al that the impact of job strain disappears after controlling for self-reported work ability (and other covariates) implying that job strain affects return-to-work through WLD (p94, 236). This is however not explicitly tested, and a separate analysis based on the same data (Anema et al 2009) seems to show independent effects of job strain and work ability.

5. Lund et al (2001) found no significant effect of demands and control on disability retirement or long-term (>2 month) sickness in a cohort of Danish municipal workers in physically demanding jobs (instead finding an effect of skill discretion) – but this analysis is extremely low-powered (only 67 cases).

6. Friis et al (2008) in a high-powered study (nearly 700 cases) found that high control highly significantly reduced the risk of disability pension in in Danish nurses, but found no effect of job demands.

7. Krause et al (1997) found a bivariate impact of high job demands on disability retirement in Finland (p=0.051), but this became non-significant after adjusting for SES, and the survey is again low-powered (67 cases).

8. Laine et al (2009) found that high demands and particularly low control increase the risk of disability retirement in prime-age (19-50) Finnish public sector workers when using both self-reports and work-unit averages. However, when adjusted for health and health risk behaviour these results became non-significant, reflecting the low-powered nature of the analyses (93 cases) – although people in high-strain (above-average demands, below-average control) work units were significantly more likely to claim a disability pension that those in low-strain work units, even in the fully-adjusted models.

9. Using the same dataset as Laine et al (2009) but looking at all ages (1178 cases), Vahtera et al (2010) found that both self- and co-worker-assessed worktime control led to a lower risk of disability pension among public sector employees in Finland. When split by diagnosis this effect was only significant among musculoskeletal disorders, but this partly reflects smaller numbers of cases for other disease groups.

10. Krokstad et al (2002) found a strong effect of job control on disability pension in Norway after accounting for a number of potential confounders (though they include only two measures of health: general health and LLSI). They also found that a measure similar to quantitative demands (being worn out by high demands in concentration and attention) seemed to raise the risk of disability pension, but only in older workers.

11. Hagen et al (2002) found that high control and excessive demands (not the *lack* of demands) reduce the risk of disability pension from back

pain. Despite this being a large study (715 cases), these models only adjusted for age, gender and other work characteristics, and neither demands nor control were included in the final multiply-adjusted model.

12. Haukenes et al (2011) found a moderate effect of job control but little effect of job demands on disability pension in Norway (209 cases), controlling for physical demands and a large number of health controls.

13. Claussen & Dalgard (Claussen and Dalgard 2009) found that low control strongly increased the risk of disability pension among middle-aged Oslo inhabitants (498 cases) after controlling for income and a nine-category occupational grouping.

14. Blekesaune & Solem (2005) used occupation averages of control, ‘stress’ and physicality in a Norwegian cohort of 60-67 year old workers. Low control increased the risk of disability retirement, but only for men, and there were no health controls available. There was no effect of ‘stress’.

There are also several studies that are not available in English, nearly all from the Nordic countries – but as null results are not often mentioned in these summaries, it is difficult to integrate these results.

Looking across these studies, it seems that (i) job control is relatively consistently associated with lower risks of incapacity benefits; (ii) there is rarely an effect of job demands, and on occasion high demands seems to reduce the risk of incapacity claims; (iii) these results seem to be true whether researchers use self-reported or occupation-averaged demands-control; (iii) many of these studies have relatively few (or no) controls for SES and/or health; and (iv) most studies (12 of the 14) have been conducted in the Nordic countries, with no previous studies from the UK.

- *Work in general (including early retirement)*: various studies have found that demands and/or control predicts intention to leave work (Widerszal-Bazyl et al 2008) or to retire (e.g. Elovainio et al 2005; Harkonmäki et al 2006; Siegrist et al 2007; Vartia and Hirvonen 2011). Studies on actual retirement behaviour have less clear results; some found effects of control (Blekesaune and Solem 2005) while others did not (Quinn 1977; Lund and Villadsen 2005) or found mixed results (Blekesaune et al 2008), while the few studies of demands found no effect (workers under 60 in Quinn 1977; Henseke 2011). This includes studies that used occupational averages (Blekesaune and Solem 2005; Blekesaune et al 2008; Henseke 2011) or expert assessments of work characteristics (Quinn

1977), in the latter case showing moderate-sized but non-significant impacts of job strain among people with health limitations. Blekesaune et al (2008) is the only one of these studies to have been conducted in the UK.

The general picture from these studies it that job control affects fitness-for-work and labour market outcomes, but job demands only has an effect on fitness-for-work. These conclusions are again tempered by the concerns in section 3.1.1 around establishing causality, particularly for fitness-for-work where there are few high-quality studies, but there appears to be more agreement between methods that job control genuinely does have an effect. However, almost the only studies conducted in the UK have been conducted for sickness absence, with no UK studies looking at fitness-for-work or incapacity benefits.

3.1.3. Trends in job demands and control

I presented a figure in the introduction that showed a sharp rise in job strain 1992-2006. This was based on Green's analysis of specialist surveys of work (the Skills Surveys) that he conducted along with Felstead, Gallie and colleagues, which show sharp rises in demands and declines in control in the 1990s, particularly but not exclusively in the public sector (Green 2004:617; Green 2006; Felstead et al 2007). If we combine this with the evidence on the effect of demands-control in sections 3.1.1 and 3.1.2, we would be surprised if job strain had *not* contributed to rising WLD and incapacity benefits receipt.

However, this hypothesis relies on the finding that there has been a rise in job strain in Britain, and three other prominent surveys appear to contradict this：

- First, the largest surveys of work in Britain are the Workplace Employment Relations Surveys (WERS) in 1998 and 2004. These show a small but statistically significant *rise* in job control 1998-2004 (Brown et al 2007:950), although managers' evaluations of job discretion in the largest occupational group in their establishment shows a slight decline (Green 2008a:14).

- Second, the European Working Conditions Surveys (EWCS) show a rise in low-autonomy work 1995-2005 in Britain (OECD 2008:252) but a *decline* in demands from 1995 (Green and McIntosh 2001; Pena-Casas and Pochet 2009:27) – a trend that has recently been described as the 'de-intensification' of work (Burchell 2010).

- Third, the International Social Survey Programme (ISSP) surveys show no change in proxies for control ('independent working') or demands ('exhaustion after work') in Britain 1989-2005 (Olsen et al 2010:234).

These disagreements are generally not mentioned (e.g. Burchell 2006:9; Coats and Lekhi 2008:37; Chandola 2010), and when directly questioned, researchers have suggested privileging the Skills Surveys as the most robust trend.[22] Nevertheless, in the absence of further investigation this raises difficult questions for both the present research and for policy more generally,[23] as if there has been no rise in job strain then clearly strain played no part in deteriorating fitness-for-work. In Chapter 2, I therefore systematically review the available trend data on demands-control, and evaluate the methodological quality of each trend comparison.

A further issue is whether the rise in self-reported demands-control reflects a genuine change in working conditions or simply a change in how people respond to surveys, mirroring the issues with WLD. I also deal with this issue in the context of the systematic review of demands-control trend data in Chapter 2.

Even at this stage, it is necessary to put the argument into an international context in two ways. First, the rise in British job strain does not appear to be replicated internationally. While intensification is relatively commonplace (Green 2006:58; Pejtersen and Kristensen 2009), control has only declined in a few countries (Sweden and Finland) with other countries often seeing *rising* control. Incapacity benefits have risen substantially – and the disability employment rate among the low-skilled has fallen – in some but not all countries (OECD 2003:61; Whitehead et al 2009:32-35). Likewise, the rise in WLD is not consistent internationally. In the US, demands and control have both *improved* since the 1970s (Tausig et al 2005), trends in WLD have been inconsistent since the 1980s (Trupin and Yelin 2003; Kaye 2004b:4), and there have been both rises in the number of incapacity benefit claimants and falls in the employment of people with disabilities (Burkhauser and Stapleton 2003). The hypothesis under test here is thus for Britain, rather than all high-income countries.

Second, while Britain has seen a sharp rise in job strain compared to its previous level, and this rise was not seen in other countries, this does *not* mean that Britain has particularly high levels of job strain. Assuming that people in different countries interpret these survey questions in similar ways – a much greater assumption than within a single country over time – then the UK still has moderately low levels of both demands and control after the 1990s rise in job strain, although it is relatively close to the EU average on both counts (see also Gallie 2007:115; Chandola 2010:50). And in

[22] e.g. WERS researcher Andy Charlwood (personal communication in Web Appendix 1a).

[23] During a brief period in the Prime Minister's Strategy Unit in 2009, the conflict between different oft-cited datasets became a problem when formulating a policy 'narrative' around demands-control.

a broader context, not only were British working conditions much worse a century ago than they are today (Wainwright and Calnan, in press), but working conditions in many low-income countries today can be substantially more harmful to health than those in the UK (Moutsatos 2009).

3.2. Other work characteristics

Many aspects of the work environment influence either health or labour market outcomes (Murphy 2008). I here briefly review several of the most important work characteristics, and then consider how much they have changed since the early 1980s – and therefore whether these are competing explanations to demands-control for the rises in WLD and incapacity benefits.

3.2.1. Other models of work stress

The initial demands-control model was later supplemented by social support at work in the 'iso-strain' model of Johnson (de Jonge and Kompier 1997:241), where social support was argued to both buffer psychological strain (Karasek and Theorell 1992:70) and to measure more *collective* forms of control (Levi 2000:81). Social support appears to affect common mental health disorders (Stansfeld and Candy 2006; Bonde 2008), but the results for sickness absence, return-to-work and disability pension are inconsistent (Janssen et al 2003:230; Allebeck and Mastekaasa 2004:57,60; Stattin and Jarvholm 2005; Lund et al 2006; Duijts et al 2007).

Within occupational health psychology, this has been further revised to the 'job demands-resources model' (Demerouti et al 2001), where broader concepts of both demands (quantitative, emotional, physical) and resources (not just control but also e.g. feedback) are used. It is hypothesised that high demands with low resources lead to burnout (e.g. Kain and Jex 2010:250-1), but that high demands with high resources produce high motivation (Bakker et al 2010).

While the demands-control model has been the most important model of work stress in occupational epidemiology, another model has risen to almost equal billing: Siegrist's model of 'effort-reward imbalance' (ERI) (Siegrist 1996; Levi 2000:85). The ERI model hypothesises that that stress can result from a mismatch between high extrinsic effort and low extrinsic rewards, particularly for people who strive excessively and have strong desires for approval who are described as 'overcommitted' – with the combination of low rewards (rather than low control) with high effort causing the experience of stress (Siegrist et al 2009:11). Meta-analyses find a strong effect of ERI on health, particularly on mental health outcomes (Kivimäki et al 2006; Stansfeld and Candy 2006).

These are the main models of work stress, but other relevant work characteristics that have (inconsistently) been linked to health, fitness-for-work or labour market outcomes include: *work time control* (Siegrist et al 2009:26), *role conflict and role clarity* (Sprigg et al 2003:3; Lund et al 2005), *management style* (Barling and Carson 2008), *skill discretion and repetitive work* (as described above; see also Ariëns et al 2001; Lund et al 2001; Janssen et al 2003:230; Vaananen et al 2003; Lund et al 2006; Christensen et al 2008; Labriola et al 2009), *job insecurity* (Bartley 2005; Lund et al 2006; Stansfeld and Candy 2006), *shift work* (Griffiths et al 2009:17; Wang et al 2011) including the flexibility of shift scheduling (Joyce et al 2010), *teamwork* (Van den Berg et al 2008) and *workplace violence* (Murphy 2008). Indeed, the fact that so many different working characteristics have been related to health-related outcomes has caused at least one reviewer to argue that all of these results may be caused by omitted variables bias (Bonde 2008:441), although we might equally believe that a multitude of independent effects is plausible.

3.2.2. Job tasks: physical and emotional work

Aside from the health impact of heavy physical workloads (de Zwart et al 1997; Berntson and Marklund 2007), it is generally argued that *"a man has to be healthier to remain employed in a manual rather than in a managerial, professional or clerical occupation"* (Beatty et al 2000:620). This is supported by evidence showing that manual jobs lead to WLD independently of any effect on health (Banks and Tetlow 2008:46), as well as studies using the WAI (Aittomaki et al 2003). Whatever the pathway, physical demands usually influence work-related outcomes such as sickness absence (Allebeck and Mastekaasa 2004:57,60; Duijts et al 2007), return-to-work (Lund et al 2006:230), and disability pension (Krause et al 1997; Hagen et al 2002; Karpansalo et al 2002; Labriola et al 2009). Indeed, measures of quantitative job demands also seem to capture some physical demands, with people in manual jobs often interpreting 'working hard' as 'working hard *physically*' (Choi et al 2011).

Yet it may also be the case that some of the tasks that replace physical work can be detrimental to health, such as the 'emotional labour' of displaying certain feelings to customers (see Korczynski 2002, ch8), which can be broadened to the wider range of interpersonal demands (Handel 2008:31). The effect of emotional demands on mental health is mixed, seemingly because people derive substantial satisfaction from successful interactions with other people (Korczynski 2002; Zapf and Holz 2006; Stalker et al 2008). Nevertheless, overall levels of emotional demands have been linked to mental ill-health (Nieuwenhhuijsen et al 2010), sickness absence and return-to-work (Lund et al 2005; Lund et al 2006; Rugulies et al 2007:301).

3.2.3. The changing nature of work

The nature of work has radically changed in most OECD countries due to changes that are variously described as 'globalization', 'neoliberalism', or 'post-Fordism' (Sauter and Murphy 2003; Petterson et al 2005; Moutsatos 2009). Many aspects of these changes are positive: there has been a rise in wages and skill demands (Green 2006:29-35; Green 2011), a decline in the number of manual workers and a steady rise in the number of professionals and 'knowledge workers'. But for others it has been less positive, with a polarization between high-skilled and low-skilled jobs at the expense of those in between (Goos and Manning 2007). Some have even claimed (slightly implausibly) that the workplace is reverting to forms last seen 100 years earlier (Quinlan et al 2001a; b).

Amid the many claims and speculations, robust data are crucial. For example, it has been argued that job insecurity has increased in recent decades (Sennett 1998; Moutsatos 2009), but actual data on the likelihood of losing a job show no increase from the mid-1980s (Nickell et al 2002; McGovern et al 2007), levels of temporary employment are unchanged (Fitzner 2006; ippr 2010), and from the late 1990s, subjective insecurity declined in Britain (Burchell et al 1999; Green 2009c; Olsen et al 2010:234). Instead, we see relatively small declines in tenure among subgroups (particularly men, see Coats and Lekhi 2008:33), rises in agency work (which is rare) and a smaller rise in temporary work (which is more common) (Forde and Slater 2010), and rises in worries about losing *valued aspects* of jobs over the 1980s and 1990s (Burchell et al 1999).

Similarly, despite widespread perceptions that average work hours have increased, they have stayed flat or even declined since the 1980s; instead, it is the *dispersion* of work hours that has increased, with part-time work increasing and long hours rising and then falling more recently (Evandrou and Falkingham 2000:33; Green 2001; Green 2006:46; Siegrist et al 2009:18; Green 2011). There were also rises in part-time (though not full-time) self-employment 1992-2009 (Forde and Slater 2010), flexible working time arrangements 1998-2004 and 2003-6 (Kersley et al 2005:30; Green 2011) and working primarily from home 1981-2008 – although despite this, increasingly people want to spend less time working and more time with family/friends (Crompton and Lyonette 2007:62).

A variety of other trends exist for different aspects of work. The number of manufacturing jobs in the UK fell from over 5m in 1984 to around 3.5m today (Wilson et al 2006:52), but over the 1990s there are suggestions of stable or even *rising* physical demands (Felstead et al 2007:28,86; Green 2009a:26; Olsen et al 2010:233-234), probably due to rising demands more generally. Most other types of skill use rose 1997-2006, including literacy, numeracy, cognitive, and computer skills (Felstead et al 2007). Perceived management relations by the mid-2000s were often better than in the early-to-mid-1980s (Brown et al 2007; Bryson and Forth 2010). Workers now feel more able to help people in their jobs and more useful to society (Crompton and Lyonette 2007:60). And threats and assaults at work rose 1991-1995 before declining slightly by 1999, according to British Crime Survey data (Stansfeld et al 2004:45). *Prima facie,* none of these seem likely to have significantly contributed to deteriorating fitness-for-work.

Finally, in terms of more general assessments of work, job satisfaction declined 1992-2000 (Taylor 2002:10), at least among women (Rose 2005:463), although this itself seems to be largely explained by declining job control (Green and Tsitsianis 2005). Likewise, work commitment fell 1989-1997 (Crompton and Lyonette 2007), although for women this period of falling work commitment was balanced by rising commitment both before (Gallie et al 1998:188) and since (Crompton and Lyonette 2007). Commitment to the employing organization itself also declined 1992-2000, particularly among older workers (White 2009b), which seems to be a downstream reflection of the trends in demands-control described above (although there are reverse effects of commitment on discretion; see Green 2008a). More recently organizational commitment has risen among younger workers and declined among older workers (Green 2008a; White 2009b).

In conclusion, the nature of work has changed substantially over the 1980s and 1990s; for example, emotional demands and other skill requirements substantially increased over the period in question here. But there are few signs of any deterioration in working conditions on the scale of the rise in job strain over the 1990s, and therefore few other workplace-based candidate explanations for rising WLD and incapacity benefit receipt. The only exceptions to this are job satisfaction, work commitment and organizational commitment which all fell in the 1990s – but this in itself may be due to declining control. Given the absence of any changes at work other than demands-control that can explain trends in fitness-for-work or incapacity benefits, I now turn to other possible explanations, beginning with trends in health.

4. Other Explanations

4.1. Health and disability

It is widely assumed that the health of Britons has improved in recent decades, given that life expectancy increased and mortality fell, and that health therefore cannot explain any of the rise in incapacity benefits receipt (Bell and Smith 2004:19; Office for National Statistics 2008:95; Beatty et al 2009:4). Yet there is no necessary connection between mortality and morbidity; not just because mortality and morbidity are often driven by different conditions (Murray and Chen 1992; Crimmins et al 1999; Grundy 2003), but also because mortality improvements partly reflect the increased survival of those with health problems (PMSU 2005:35; Wilson et al 2005).

If we examine the data on trends in self-reported general health, we actually see a similar (but less marked) rise in ill-health as we saw for WLD in both of the major survey series.[24] However, this may in fact represent *the same trend* that we saw before; that is, when answering questions about their general health, people partly base this on their fitness-for-work.[25] General health is also difficult to interpret in that it is a *relative* measure (Murray and Chen 1992); respondents compare their health to a norm – often those of their own age (Eriksson et al 2001; Mallinson 2002) – and if population health deteriorates then we expect this norm to change, downward-biasing any underlying trends.

One way of getting around this is to look at a slightly different measure, that of activity-limiting longstanding illness (LLSI), which qualitative research suggests is interpreted as referring to limitations in a wide variety of non-work activities (Manderbacka 1998:323). In practice this includes nearly all people with a WLD but also others with non-work-limiting health problems (Bajekal et al 2004:63) – 74% of those with a WLD also report an LLSI, but <0.5% of those without a WLD report an LLSI.[26]

LLSI has commonly been used to show trends in disability (Bartley and Owen 1996; Cousins et al 1998; Bartley et al 2004; Bell and Smith 2004; Faggio and Nickell 2005; Berthoud 2007:9-10; Berthoud 2011), although it is actually a measure of functional

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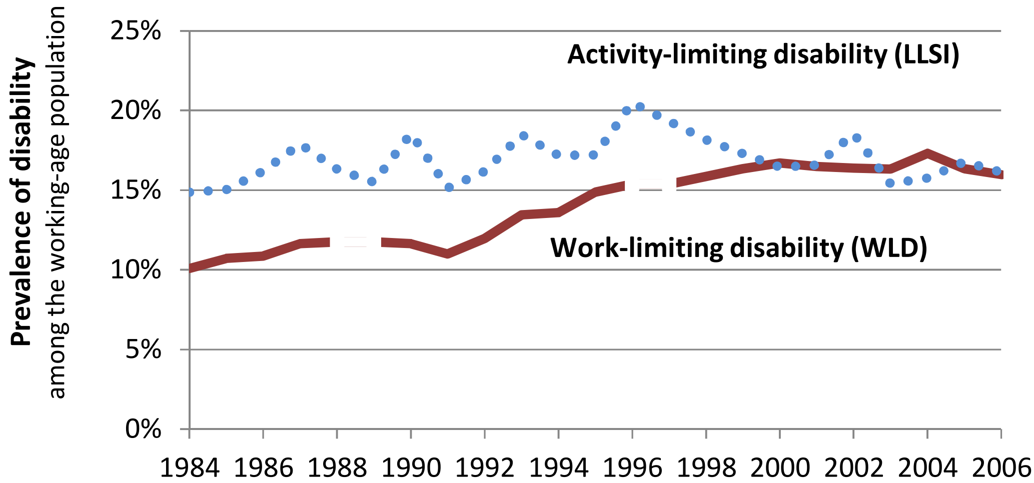
[24] In the Health Survey for England, bad/very bad health rose from 4.7% to 7.1% 1993-2001 before plateauing (unweighted data from http://www.ic.nhs.uk/statistics-and-data-collections/health-and-lifestyles-related-surveys/health-survey-for-england/health-survey-for-england-2006-latest-trends Table 11, accessed 25/3/2011). In the GHS, 'not good' health rose from 8.1% to 10.8% 1984-2001, and stayed at that level until falling to 9.3% in 2006, correlating very strongly with trends in WLD (r=0.91).

[25] 20% of respondents to one survey cited 'work/being busy' as the most important factor in deciding their own health (1984/5 Health and Lifestyles Survey, unweighted data).

[26] Author's analysis of weighted LFS data 1997-2006.

limitations rather than 'disability' in the formal definition outlined in section 2.1.[27] Over the period of interest here, LLSI has stayed roughly flat other than a temporary peak in 1996 (see Figure 8), in contrast to the continuously rising trend in WLD. As existing analyses point out however (e.g. Berthoud 2007), this conceals a sharp rise in LLSI among the declining numbers of those with no qualifications (a point to which I return in section 4.2), and some surveys show rises in LLSI since the late 1990s.[28]

Figure 8: Trends in activity-limiting and work-limiting disability 1984-2006



WLD trend shows a 'chained series' (see Figure 4) due to discontinuities 1988-9 and 1996-7. LFS (WLD) and GHS (activity-limiting disability, 'LLSI'; unweighted) data on the working age (16-59/64) population. See section 2.1.1 and Appendix 1A for further details of data preparation.

Despite static levels of functional limitations (as measured by LLSI), it would still be possible for rising WLD to simply reflect changes in health if there was a change in the mix of health conditions. This could happen if those conditions more associated with WLD – primarily mental health problems and pain (Banks et al 2006; Jones 2006; Kapteyn et al 2008) – rose, while those conditions less associated with WLD fell.

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[27] LLSI is here measured by asking people if they have a longstanding illness or disability, and then asking if this 'limits your normal day-to-day activities in any way'

[28] From the slight rise in LLSI among 45-64 year olds, Griffiths has argued that rising LLSI 'is surely one important factor in explaining the increase in claims for incapacity benefit' (2010:8).

Different surveys also show different trends. A similar definition of disability in the LFS seems to have increased continuously 1997-2005 (Berthoud 2006:85); in my own analyses of weighted LFS data, LLSI rises from 13.3% 1997 to 17.9% in 2004. Similar discrepancies between surveys can be seen for longstanding illness (irrespective of whether or not it is limiting; 'LSI'): it is roughly flat in the GHS, but shows rises in both the LFS (1984-1988 and 1997-2006) and Health Survey for England (1994-2004). Arguably LLSI rises in the LFS but not the GHS suggest that it is a perceived focus on the labour market that causes people to focus more on work limitations.

Finally, it is worth being aware that LLSI rose in the 1970s prior to the period of interest here (Berthoud 2011), with rising LLSI explaining 13% of the rise in incapacity benefit receipt 1975-1990 (Berthoud 1993, cited by Berthoud 1998:30).

Mental health is particularly important here given that it has accounted for an increasing number of incapacity benefit claims since the mid-1980s (Moncrieff and Pomerleau 2000). In recent decades diagnosed mental illness in the UK has risen substantially (Collishaw et al 2004; OECD 2008:221), but it is difficult to know if this reflects a genuine decline in mental health, or a greater willingness to report and diagnose mental health conditions (Borch-Jacobsen 2002; Stansfeld et al 2008b:29-30). Among adults, it appears there was no underlying decline in mental health over the 1990s in some sources (using the GHQ12 in HSE, see OECD 2008:214) (or in the BHPS, see Rose 2005:466), and only a slight decline 1993-2000 in the Psychiatric Morbidity Survey (McManus et al 2009).

Unsurprisingly the trends in the prevalence of different physical conditions in the general working-age population are not uniform. Ischaemic heart disease rose slightly in the mid-90s before declining (Craig and Mindell 2008), while comparisons of the national birth cohorts suggests that back pain doubled in the 1980s before declining in the 1990s (Wadsworth et al 2003). Asthma (Wadsworth et al 2003) and diabetes (Craig and Mindell 2008) showed more consistent increases. Yet the extent to which rises in reported conditions reflect genuine increases in ill-health is unclear; medical care seems to make people *more* likely to report medical conditions (Dow et al 2000), partly because it makes 'silent' conditions apparent (Waidmann et al 1995:277-278; Johnston et al 2007).

Overall, it does not seem likely that genuine health declines explain any of the rise in WLD and incapacity benefit receipt – although neither is there much evidence that health has improved, despite mortality reductions. Functional limitations (measured by LLSI) were effectively flat when looking across the population as a whole, trends in physical conditions were inconsistent, and mental health symptoms seem to be unchanged. Self-reported poor health did rise slightly, but given that respondents take their fitness-for-work into account when describing their health, this is likely to simply show an overlap with the previously seen increase in WLD. Nevertheless, one remaining possibility is that the increasing *medicalisation* of mental health problems contributed to rising WLD and incapacity benefit receipt – a possibility being addressed in ongoing investigations of mental health and incapacity benefits by both the DWP and OECD, and to which I return in section 4.4.1.

4.2. Labour demand

Labour demand is the dominant explanation of rising incapacity benefit receipt in Britain, mainly associated with the influential theory of 'hidden unemployment' by Beatty, Fothergill and colleagues (Beatty et al 2000). When labour demand is low, some of those becoming unemployed will have health problems and be eligible to claim incapacity benefits. But beyond this, Beatty et al introduced the metaphor of a 'queue for jobs'. When labour demand is low, people with health problems get pushed to the back of the queue as employers can afford to be more selective, and health becomes more important in determining whether someone gets a job. This is not to suggest that the health problems of incapacity benefit claimants are not 'real' – rather, these people are the 'hidden unemployed' because if labour demand was high, their health would not interfere with their ability to get a job (Alcock et al 2003:120,126).

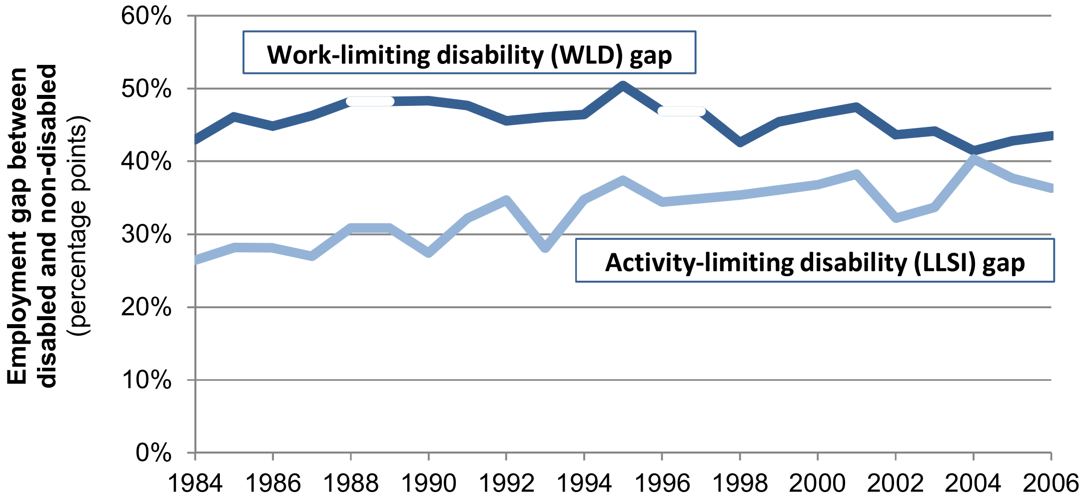
A very widely-cited piece of evidence in support of labour demand is the decline in the relative employment rates for disabled people (as measured by LLSI) and particularly low-skilled disabled men 1987-2000 (Bartley et al 2004; Bell and Smith 2004; Faggio and Nickell 2005; Kemp and Thornton 2006:160; Berthoud 2007; Berthoud 2011), a trend first noted by Bartley and Owen (1996). It is this group for whom labour demand has particularly declined at a time of rising employment among women. However, we have already seen (section 4.1) that the trends in WLD do not mirror trends in LLSI; whereas LLSI was stable, WLD rose over the course of the 1990s.

If we replicate the disability employment gap using WLD instead of LLSI, then *we do not see a decline in the employment rate among disabled people*, as shown in Figure 9 and Figure 10.[29] The differences between the two definitions can be explained by the facts that:

- People with WLD are less likely to be employed, while people with LLSI but no WLD have little (if any) employment disadvantage;
- There has been a rising proportion of those with a LLSI who also report a WLD.

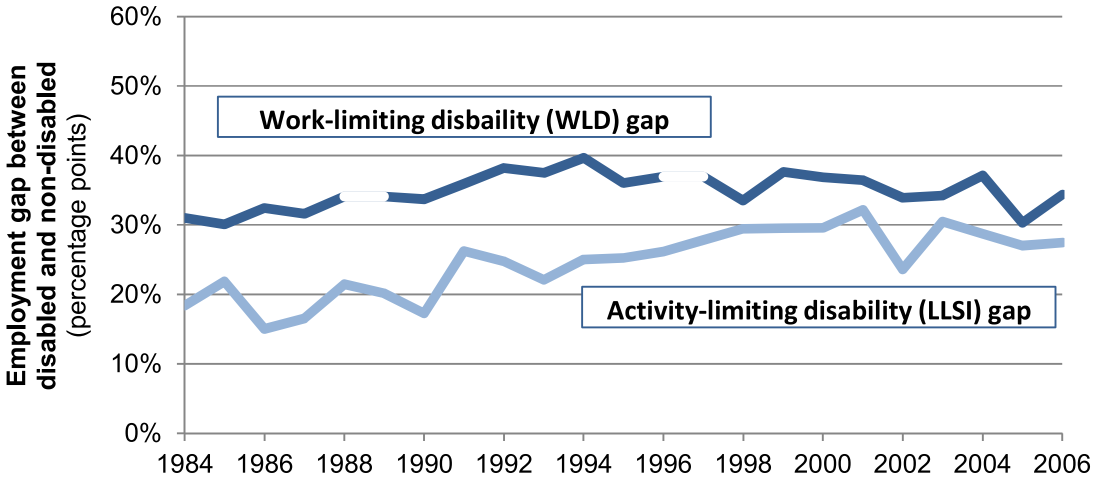
[29] Whitehead et al (2009:33) argue that the employment gap *using a WLD definition in the LFS* has risen in the UK, particularly for the low-skilled. While some details are provided (p77), it is unclear how the WLD data were made comparable across the LFS discontinuities, which may be why their results are different from those presented here. Their analysis also differs slightly in terms of age range (25-59).

Figure 9: Trends in the employment gap between disabled and non-disabled men



The employment gap is calculated as the employment rate among non-disabled men, minus the employment rate in disabled men. WLD trend shows a 'chained series' (see Figure 4) due to discontinuities 1988-9 and 1996-7. LFS (WLD) and GHS (activity-limiting disability, 'LLSI'; unweighted) data on the working age (16-64) population. See section 2.1.1 and Appendix 1A for further details of data preparation.

Figure 10: Trends in the employment gap between disabled and non-disabled women



The employment gap is calculated as the employment rate among non-disabled women, minus the employment rate in disabled women. WLD trend shows a 'chained series' (see Figure 4) due to discontinuities 1988-9 and 1996-7. LFS (WLD) and GHS (activity-limiting disability, 'LLSI'; unweighted) data on the working age (16-59) population. See section 2.1.1 and Appendix 1A for further details of data preparation.

This does not necessarily mean that labour demand is unimportant – it could be the case that a decline in labour market demand for particular groups somehow makes increasing number of health problems seem work-limiting (Faggio and Nickell 2005:3-4). Certainly using the LLSI definition, any rise in disability was almost exclusively concentrated among those with low skills (Faggio and Nickell 2005:17), as was the rising disability employment gap (Bartley and Owen 1996; Bell and Smith 2004:7,12`18; Faggio and Nickell 2005:4,16,18; Black 2008:32). If we focus on WLD, we find that the disability employment gap stayed roughly constant for both those with no qualifications and those with a degree, and that the rise in WLD can be seen almost as strongly for (the increasing numbers of) workers with a degree as for (the declining numbers of) those with no qualifications at all. While low-skilled men are particularly affected in some ways,[30] rising WLD is a phenomenon that can be found across educational levels.

However, the most persuasive evidence for a role of labour demand comes when we look at particular *places*. In areas where there were large job losses in heavy industrial work, such as the end of the Trident contract in Barrow-on-Furness (Beatty and Fothergill 2002), there was a sharp rise in incapacity benefit rates but *not* unemployment. Moreover, there has been a consistently strong geographical patterning of incapacity benefit receipt, with claimants concentrated in areas that saw declining industrial work and that continue to have low labour demand (Beatty and Fothergill 2005). Econometric analyses have similarly found that measures of local labour demand predict incapacity benefit rates and transitions (Disney and Webb 1991; Burchardt 2003b; McVicar 2007; Benitez-Silva et al 2010; McVicar and Anyadike-Danes 2010), and likewise predict the more recent improvement in incapacity rates in previously low-demand areas (McVicar 2009:16).

Beatty and Fothergill have developed their theory somewhat to reflect changes in the pattern of incapacity benefits. While the original 'hidden unemployed' were men made redundant from heavy industry, more recently they are both men and women who lost their jobs *specifically for health reasons* (Beatty and Fothergill 2007:17). Women not only have different paths onto incapacity benefits (Kemp and Davidson 2008b) that unlike men do not appear to be affected by local labour demand (Kemp and Davidson 2008a:Table 8, although see also the following paragraph), but also have seen

[30] The picture for those with some qualifications (below degree level or equivalent) tends to show that they were particularly affected – the rise in WLD among workers is greater (6.8% rather than 3.1-4.3%), and there was an overall increase in the disability employment gap in this group. These educational trends are also complicated by the changing educational composition of the British population.

Even when looking at WLD, it has been argued that the rise in WLD was much larger in those with low qualifications (Bell and Smith 2004:16; Faggio and Nickell 2005:17). However, this is likely to reflect the fact that non-workers with low qualifications are much more likely to claim incapacity benefits (and therefore report a WLD) than others. When we just look at WLD trends *in workers*, we find that the increase is similar across all educational levels; and *across the whole population* the disability employment gap is similar across educational levels.

continuously rising labour demand in recent decades (Simon and Whiting 2007). It is no longer accurate to picture incapacity benefit claimants as men with musculoskeletal problems who used to work in heavy industry and were made redundant (see also Kemp and Davidson 2008a).

Yet female claimants are still concentrated in the same areas as male claimants (Beatty and Fothergill 2005:849; Beatty et al 2009:964). There are several possible reasons for this – a greater acceptability of claiming benefits (see section 4.4.1), increased family breakdown, and increased incentives for the female partners of workless men to claim benefits (Beatty et al 2009:964-5). But Beatty et al argue that the most plausible explanation is a continuing lack of jobs: in these areas the supply of workers outstrips demand, the gender segmentation in the labour market declines with successive cohorts, and we therefore have competition between genders (Beatty et al 2009). Beatty and Fothergill have estimated that 40% of incapacity benefit claimants can be considered the 'hidden unemployed', based on the crude assumption that incapacity benefit levels, if there was full employment, would be the same as in the South-East in 1991 (Beatty and Fothergill 2005).

The evidence for explaining part of the rise in incapacity benefits by labour demand seems overwhelming, and fits the experiences of claimants who report wanting work but often struggle to find it (Rowlingson and Berthoud 1996, cited by Berthoud 1998:40). Nevertheless, substantial questions remain:

- Partly this is because changing labour demand over time (rather than place) does not seem to influence incapacity (Benitez-Silva et al 2010:501; Berthoud 2011); recessions tend to be times where previously economically inactive people start looking for a job and the unemployed are less likely to become inactive (Huddleston 2000:57; Gomes 2009:12-14). Likewise, incapacity is associated with unemployment but not the availability of jobs (DWP 2004 cited by Kemp and Thornton 2006:161). In both cases this suggests a problem of a mismatch between supply and demand rather than a shortage of labour demand per se.

- Partly this is because Beatty and Fothergill ignore the possibility of genuine spatial patterning in ill-health and fitness-for-work, possibly driven by selective migration (Benitez-Silva et al 2010:528). Spatial differences have been found for non-work-related disability benefits (Walker and Howard 2000:160), NHS mental health care costs (Sutton et al 2010), LLSI (Haynes et al 1997), mortality (McVicar 2009) sensitive measures of disability (Berthoud 2006:25-26) and *transitions* to disability (Burchardt 2003a:17). Moreover, these explain as much (McVicar 2009:9-13) or more (Berthoud 2006) variation in incapacity claims than labour demand itself.

- Partly it is because labour demand on its own does not appear to explain the entire rise in incapacity benefits – a Bank of England paper found that falling employment rates among those with a WLD explained only 1.7% of the 5.3% decline in participation among prime-age men (Bell and Smith 2004:18), and Beatty and Fothergill's crude estimates still suggest that the majority of claimants are *not* the hidden unemployed. As Kemp and Thornton put it in a recent review (Kemp and Thornton 2006:161), *"while disguised unemployment may be an important factor, it is unlikely that it is the only driver of the growth, and continued high level, of [incapacity] benefit claims."*

I do not consider labour demand in the analyses in Chapters 3-7 simply due to practicalities of time and space. Given its importance, though, I return to the role of labour demand in Chapter 8, linking it to the role of working conditions in a coherent model; and in Chapter 9 I recommend further research that extends the quantitative analyses here to look at both working conditions and labour demand.

4.3. The benefits system

There is overwhelming evidence that the incentives to claim benefits *per se* influence claimant rates, with the evidence coming from both the UK and abroad (Bell and Smith 2004:26; Faggio and Nickell 2005; McVicar 2006). Indeed, differing incentives are commonly suggested as the main reason explaining differing levels of incapacity benefit receipt internationally (Kemp et al 2006:15; Borsch-Supan and Roth 2010). If there are now greater incentives to claim incapacity benefits, then this could therefore explain part of the rise in incapacity benefits.

Yet the evolution of the benefits system since the early 1980s is complex.[31] In broad terms, earnings-replacement incapacity benefits separate from the general safety net began in 1971 when 'Invalidity Benefit' (IVB) was introduced, payable to those who had paid a certain level of national insurance contributions. This was replaced in 1995 with 'Incapacity Benefit' (IB), which in turn was superseded in 2008 by 'Employment and Support Allowance' (ESA).[32]

While tracking each part of this is difficult, the incentives to claim incapacity benefits have changed relatively little since the early 1980s (McVicar 2008) – and indeed, the

[31] Information collated primarily from Burchardt (1999), Berthoud (2011) and http://www.adviceguide.org.uk/index/life/benefits/benefits_for_people_who_are_sick_or_disabled.htm#severe_disablement_allowance [accessed 28/3/2011].

[32] For those without sufficient contributions (particularly housewives), a low 'Non-Contributory Invalidity Pension' was introduced in 1977, replaced by the slightly higher 'Severe Disablement Allowance' in 1984, and integrated within the ESA system since 2008; from 1987 to 2008 others claimed Income Support with a Disability Premium.

changes that have taken place primarily *reduce* the attractiveness of claiming. The 1995 reforms abolished the earnings-related top-up to incapacity benefits, which particularly reduced the incentive to claim incapacity benefits among older men (Bell and Smith 2004:8). Benefits in general were uprated with prices, falling behind wages that were rising more quickly (Anyadike-Danes and McVicar 2008:3), and exacerbated by more subtle means of changing the value of benefits (Atkinson and Micklewright 1988). Unlike the US (Autor and Duggan 2003), then, there has been no overall increase in the attractiveness of incapacity benefits compared to work.

More plausible are the increased incentives to claim incapacity benefits *relative to other benefits*. This is particularly the case for unemployment benefits, where activation policies of 'help and hassle' were tried earlier (Lødemel and Trickey 2000; Clasen et al 2006:141), and which have greater means-testing (Alcock et al 2003) that increased with the 1996 introduction of Jobseekers Allowance (Clasen et al 2006:141). Moreover, there are reports of a conscious effort by benefits office staff (and perhaps also medical professionals) to divert people away from unemployment in the late 1980s/early 1990s in order to keep the unemployment figures down (Clasen et al 2006:141).

The generosity of unemployment benefits therefore influences disability rolls (Borsch-Supan et al 2009:354), and it has been argued that changes in unemployment benefits in the UK are one of the main explanations of the growth of incapacity benefits (OECD 2003:62; Anyadike-Danes and McVicar 2008:4), particularly the 'Actively Seeking Work' test and the Restart interviews from the mid-1980s (Walker and Howard 2000:144). This diversion from unemployment benefits to incapacity may have particularly occurred in areas of low labour demand (Beatty and Fothergill 2005) with the tacit encouragement of GPs and benefits agency staff, although direct evidence is sketchy (Walker and Howard 2000:148).

Eligibility for benefits also plays a role – not just through the increased entitlement of women to contribution-based benefits (Berthoud 1993, cited by Berthoud 1998) and changing contribution conditions that affect the recently unemployed (as in the 1995 and 1999 reforms; Walker and Howard 2000:127, 141), but through the eligibility of people with different health conditions. However, there is little sign of a continued increase in the ease of claiming. The number of incapacity benefit claims referred to a medical examination fell from 23% in 1983 to 14% in 1994 (Bell and Smith 2004:21), but since then there have been numerous attempts to restrict eligibility. The stricter 'all-work test' was introduced alongside IB in 1995 (Berthoud 1998:35), replaced by the Personal Capability Assessment and now the Work Capability Assessment for ESA, which appears to be even more stringent in practice than it was intended to be after the pilots (Technical Working Group 2007). There were also attempts to reduce claims through the 'Benefits Integrity Project' under New Labour (Berthoud 1998:54-5;

Roulstone 2000). These will therefore have reduced on-flows to incapacity benefits after 1995 (Berthoud 1998:30,34), as discussed in section 2.2.1.

It is widely accepted that many people are languishing on incapacity benefits because they have been left there by the benefits system (see Introduction), in the British version of the widespread international use of disability/retirement benefits to facilitate economic restructuring (Kemp et al 2006:18-19). Yet despite considerable attempts to tighten eligibility and to encourage people on incapacity benefits to return-to-work, the numbers on incapacity benefits have changed little since 1995, and the initially-lauded Pathways programme has on reflection been considered a failure (National Audit Office 2010).

The main message from this section is that there has been no systematic increase in the attractiveness of incapacity benefits compared to work, but that they may well have become more attractive compared to unemployment benefits, particularly before 1995.

4.4. Other factors

Various other factors may also have played a role in rising WLD and incapacity benefit receipt, although the evidence base for these is less developed.

4.4.1. Cultural change

There is theoretically a possibility that people have become more willing to claim incapacity benefits holding everything else equal. This could be associated with the decline in manual work and its associated 'insensitivity and toughness' (Connell 1995, cited by Alcock et al 2003:191), the increasing openness and acceptance of mental health issues, the 'insidious medicalization' of subjective health complaints (Halligan cited by Aylward 2003:290), or a rise in the discourse of 'work stress' (Wainwright and Calnan 2002; Stansfeld et al 2008b:29). And even if the initial rise in incapacity benefits was due to other causes, there is likely to be a knock-on effect on the acceptability of claiming, including over benefits such as the well-publicised 1992 introduction of Disability Living Allowance (Walker and Howard 2000:138).

This is a difficult topic to provide empirical evidence on – 'culture' is often a residual explanation when all other explanations have failed, despite the long literature on 'stigma' and the 'sick role'. Two unusual studies find more direct evidence of cultural effects: van Soest et al (2006:22) found that knowing others who claim incapacity benefits makes you much more likely to see a health-problem as work-limiting, while Lindbeck et al (2007) found evidence that social interactions affect sickness absence using each of four different identification strategies – although the effects were sometimes small.

The question is, however, the extent to which such cultural factors have changed in recent decades. Contra the implication that incapacity benefit receipt has become more culturally desirable, people still report substantial stigma around it (Purdon et al 2006:24; Salway et al 2007), and there are even suggestions that this stigma may have increased. For example, the proportion agreeing that 'large numbers of people these days falsely claim benefits' rose from 72% in 1983 to 84% in 2008. More recently, there has been a fall in the numbers of people saying that more money should be spent on 'benefits for disabled people who cannot work', declining from 74% to 63% 1998-2008.[33]

Cultural change therefore remains a possible explanation (Walker and Howard 2000:135), but in the absence of convincing evidence it can be at best an explanation for those parts of the trend that other factors do not account for. I briefly return to the

[33] Weighted British Social Attitudes data obtained from www.britsocat.com, accessed 28/3/2011, for questions (X)FALSECLM and (X)SOCSPND2.

discourse around 'work stress' when discussing trends in the nature of work in Chapter 2, and more generally the influence of individual attitudes in Chapter 8.

4.4.2. Incentives to employ disabled people

Over time, employers have become ever-more liable for the costs of disability. Statutory Sick Pay was first introduced in 1983, but in 1991 employers became liable for 20% of the costs, and in 1994 they became liable for the full costs. The Disability Discrimination Act (DDA) 1995 created a duty for employers to make 'reasonable adjustments' for disabled people, including allowing time off, giving training, and adjusting equipment or premises (Howard and Cox 2007). These create additional costs that may make employers more reluctant to employ disabled people, although they also create counterbalancing incentives for retention and prevention, and empirical evidence on the effects of DDAs is equivocal (Jones 2006:33,36; Bell and Heitmueller 2009).

4.4.3. The double burden

A final possibility is that the increasing employment rate among women has left them with an increasing 'double burden' (Kemp et al 2006:15) of home/care and work demands, which is particularly difficult for those with health problems to cope with. The total hours worked among working *households* has therefore increased (Green 2001), despite the flat level of hours overall (see section 3.2.3). Yet as Kemp notes, there is only a little research on the link of this to incapacity benefit receipt to date and the evidence is mixed. Moreover, the trends in WLD and incapacity benefit receipt can be seen in *both* men and women; while men will be somewhat affected by the decreased non-working hours of their partners, we would still expect any effect here to be seen to a greater extent in women.

5. The Research Questions

5.1. Research questions

In this chapter I have shown rising WLD and incapacity benefit receipt in Britain – of which the latter has been extensively studied, although the former has been almost ignored. The causes of the benefits rise have been traced to declining labour demand in certain areas, together with changes in the benefits system that made claiming incapacity benefits more attractive. Both of these factors seem likely to play a role, yet few have claimed that these fully explain the extent of the rise in incapacity benefit receipt, and neither explanation convincingly accounts for the increase in WLD.

One factor that has not previously been explored is increasing job strain. There is an enormous literature on strain's health effects, with the most recent reviews suggesting a likely effect on mental health and possible effects on heart disease and musculoskeletal disorders, although causality is still uncertain. Beyond this, I hypothesise that job strain affects fitness-for-work independently of its effect on health; that is, it is harder for a person with a health problem to stay in a high-strain than a low-strain job.

This leads to three overarching research questions for this thesis:

1. **Do high job demands and low job control make people more likely to report a WLD?**
2. **Do high demands and low control make people more likely to suffer a health-related job loss and/or claim incapacity benefits?**

   I hypothesise a process that links these different outcomes: job strain reduces fitness-for-work, which increases the chances of health-related job loss, which ultimately leads to incapacity claims.
3. **If so, has rising job strain contributed to rising WLD and incapacity benefit receipt since the 1980s?**

The rest of this thesis is organised as follows. In Chapter 2, I investigate the evidence that job strain has risen, trying to reconcile the apparent differences between different surveys and discussing whether rising *reports* of job strain really mean that *objective* job strain has risen. In Chapters 3 and 4, I test whether job strain influences WLD (in Chapter 3) and health-related job loss (in Chapter 4) using the influential Whitehall II cohort of civil servants. The following three chapters conduct a similar analysis of the effects of job strain, replacing self-reported demands-control with imputed measures (described in Chapter 5) in a nationally representative dataset, and testing their effects on WLD (Chapter 6) and incapacity benefit receipt (Chapter 7). Following these six quantitative chapters, I present a qualitative analysis in Chapter 8 that considers the processes through which job strain may lead to benefit receipt via fitness-for-work limitations. Finally, I come to a conclusion in Chapter 9, before discussing the implications for research and policy.

5.2. Scope and originality

Finally, it is worth clarifying the originality of this thesis. Given the extensive literature reviewed in this chapter, I am not claiming that any of the issues I examine *in themselves* are original. Previous research has noted the role of working conditions in 'work ability', investigated the causes of the rise in incapacity benefit receipt, linked job strain to ill-health and disability pensions, catalogued the increase in job strain, and even (to a small degree) outlined the rise in WLD. Instead it is in the particular *combination* of these factors that the originality of this thesis lies.

For example, many authors have drawn attention to the health implications of the changing nature of work, including outsourcing, privatisation, insecurity/precarious work and intensification in general (Aronsson 1999; Quinlan et al 2001a:362; Landsbergis 2003; Bartley et al 2004; Gordon and Schnall 2009:5; Siegrist et al 2009). These changes have been particularly strongly linked to stress (Esping-Andersen 2002:21; Burke and Cooper 2006; Cooper 2006; European Agency on Safety and Health at Work 2007; Murphy 2008:5; Stansfeld et al 2008b), with the term 'stress' itself becoming an 'epidemic' in the course of the 1990s (Wainwright and Calnan 2002).

However, it is exceptionally rare for the occupational health field to focus on the *non-health* impact of these changes on fitness-for-work – despite this being a widely accepted part of the 'work ability model'. I can find no prior studies that explicitly test the strength of the health vs. non-health pathways between job strain and health-related labour market outcomes (see the discussion in section 3.1.2). Virtanen et al's (2010:461) excellent recent study is typical here: they suggest that privatisation may explain work disability, but in making this link they focus only on the effects of privatisation on ill-health (rather than the non-health pathways).

Nor can I find many prior suggestions that rising job strain may have contributed to the rise in incapacity benefit receipt, not just through its impact on health but by making it more difficult for people with health problems to continue working. Indeed, I can find only two studies that explicitly look at this, neither of which is from the UK – van der Wel et al in Norway (2010) and Burkhauser et al in the US (2003) – both of which understandably find little role for job strain as strain did not rise in either country (section 3.1.3). These two studies apart, the role of the changing workplace in incapacity claims is (at best) relegated to a passing mention of health effects (Geurts et al 2000:97; OECD 2003:86; Kemp et al 2006:19,238; Davidson and Kemp 2008:225). Hence McVicar's (2008) invaluable review of the rise in incapacity benefits does not mention working conditions, nor does the team around Beatty and Fothergill (Alcock et al 2003), nor an influential but unpublished Government review (Huddleston 2000), and nor do recent Government statements on incapacity (Chapter 9).

Almost the only serious consideration of the changing nature of work is given in one book-length review and two very recent, more sociologically-informed discussions. Walker & Howard (2000:174-177) suggest that *"demands for higher productivity mean[s] that the employment threshold has risen, squeezing out the less demanding jobs that may once have been held by disabled people"* – yet provide no empirical evidence on changing working conditions. Whitehead et al (2009:8,36) noted the possibility of 'post-industrialisation' causing declining employment rates among low-skilled disabled people across several countries (see section 4.2), but provide no direct evidence on how working conditions have changed, nor of their link with worklessness. And Foster and Wass (submitted) present a fascinating discussion about how the organisation of work may have made it increasingly difficult for people with health problems to stay at work, although this paper is both unpublished and based primarily on two employment tribunal appeal transcripts.

Given that this question has not previously been asked directly, I necessarily miss out several aspects that ideally would be included – geographical variation in labour demand, emotional demands – although these are touched upon in Chapter 8 and the research recommendations in Chapter 9. This thesis aims to provide an initial understanding of the link between previously disparate agendas, but does not expect to provide a definitive answer.

Nevertheless, if I find that changing working conditions have significantly contributed to declining fitness-for-work and rising incapacity claims, then this has important implications for policy – giving additional reasons to improve working conditions, influencing the design of tests for incapacity, and (perhaps more importantly) changing the entire narrative about the reasons for rising incapacity claims. However, such implications depend on the empirical results in the following seven chapters, beginning with the trends in job strain in Chapter 2.

6. APPENDICES

This chapter has one Appendix (which appears at the end of the thesis) and one Web Appendix (available from www.benbaumberg.com/thesis.htm)::

- **Appendix 1A: WLD questions in the LFS** – details of changes in question wording and filtering.
- **Web Appendix 1a: Personal communication from Andy Charlwood** about comparing WERS trends to Skills Survey trends.

CHAPTER 2:
Trends in job control, demands and strain in Britain

7. INTRODUCTION

For job demands-control to have contributed to rising incapacity benefit receipt from the early 1990s, it is necessary for demands-control to have deteriorated over this period. As I showed in the previous chapter, the highest-quality survey series shows a sharp rise in job strain – but other surveys do not, and these divergent findings have not previously been reconciled or reviewed. The main goal of this chapter is therefore to establish whether reported job strain in Britain has truly deteriorated over the past 20 years.

To do this, I conducted a systematic review of all publicly available datasets that allow trend comparisons of demands or control in the UK. From the outset, I realised that the review's most likely conclusion was that the highest-quality data shows a deterioration in job strain, but other studies disagree – a conclusion I had already established in the previous chapter. Nevertheless, given the centrality of this trend to the thesis' hypothesis I felt it was necessary to undertake the review, which not only ensures that all the relevant data are incorporated, but also systematically reviews the methodological quality of each trend and provides a transparent basis for prioritising some data sources over others.

Aside from the systematic review of trends, this chapter asks three further questions about the trends in job strain. First, how plausible is it that job strain *genuinely* increased over this period, rather than reflecting an increased willingness to report poor working conditions? Second, is the rise in reported job strain in the Skills Surveys specific to certain occupations? And finally, is rising job strain more common among the low-skilled who make up the overwhelming majority of incapacity benefit recipients?

8. SYSTEMATIC REVIEW OF DATA

While data reviews are not new (e.g. Lambert et al 2007; Houdmont et al 2010), they are often not systematic in the sense of evaluating the methodological quality of different sources. Furthermore, this is the first such review specifically on trends in job strain, although there have been occasional discussions of two different series (on the European Working Conditions Surveys vs the Skills Surveys, see Green and McIntosh 2001). The current section describes how the present review was carried out, and summarises the results.

8.1. Methodology

To systematically search for publicly available data sources, I made use of the resources curated by the UK Data Archive. The Archive allows users to search by terms in the 'Humanities and Social Science Electronic Thesaurus' (HASSET), which was developed to provide a standardised hierarchy of keywords through which to access their 5,000 data collections.

For the purposes of this study, I hand-searched the keywords in Table 1, and created a list of surveys containing data on demands-control that were nationally representative of at least one country within the UK.

Table 1: Keywords used in the systematic review of demands-control data

Keyword	Number of studies with keyword
autonomy at work	21
job characteristics	44
job evaluation	50
job requirements	114
supervision	90
working conditions	616
working life	48
workloads	28

Keywords are from 'HASSET', the UK Data Archive's 'Humanities and Social Science Electronic Thesaurus'. Other keywords were also checked but found not to be relevant ('occupational psychology', 'occupational sociology', 'managerial characteristics', 'job description'). Search correct as of July 2010.

This search returned 68 individual survey waves that were relevant to this study. For each wave of each survey, I then created a database containing the *exact* question wording for each question on demands-control. 24 studies that did not contain at least one question on demands-control that was comparable to a survey at a different time point were then discarded.

For the 44 remaining studies, I noted contextual information that affected the comparability of each survey – namely sampling methodology, screening of respondents into the survey/work module, response rate, weighting, age range, survey month and survey mode – as well as details of questions on occupational and industrial classification, employment status, employment type, hours of work, sector, region, and educational level. These studies were combined into a pooled data file with a total of 185,509 observations, including both design weights and non-response weights if available. A summary of the key contextual information is presented in Web Appendix 2a.

In the review below, I look separately at each pair of years[34] for each individual question on demands and control. I test the statistical significance of each trend using dummies for survey year in simple bivariate regression models (logistic for binary outcomes, ordered logistic models for ordinal outcomes).[35] I also describe the size of any trend – for ease of interpretation, I use the size of percentage point changes rather than latent scales. For ordinal variables, I use the *cumulative* percentage (e.g. 'strongly agree' or 'agree') that changes the most over time.[36]

Finally, for each comparison I evaluate the comparability as 'low', 'medium' or 'high'. Usually surveys in different years are divided in clear cut fashion between the 'comparable' – often within a single survey series and explicitly designed for comparability – and the 'incomparable'. Yet sometimes a single series of surveys may actually have substantial differences in e.g. in sampling methodology that are glossed over (such as for the ISSP surveys in Olsen et al 2010:227), while questions from different surveys can be almost identical in terms of such technical details.

Irrespective of the intentions of the survey designers or whether the survey formally forms part of a 'series', I therefore evaluate the comparability based on the similarities in question wording, questionnaire routing, and sampling methodology (questions asked in different survey modes – such as self-completion vs. telephone vs. face-to-face – are treated as incomparable). Details of these categorisations and the steps taken to enhance comparability are provided in Appendix 2A. The categorisations should be interpreted as follows:

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[34] Where a single question is available in multiple comparable surveys, the tables only include the comparisons of successive surveys (as other comparisons can be obtained from these results). However, where non-successive comparisons have greater levels of comparability than successive ones, I additionally include these. For very frequent surveys (the annual PWCS and BSA series), I divide the series into periods of time during which the trend was homogeneous rather than presenting a long series of annual comparisons. Finally, for trend C16_1 I separate the three constituent series (BSA, Skills Surveys, Eurobarometers) as the number of series led to an implausible number (0.5bn) of pairwise combinations.

[35] In other words, for binary outcomes I use the model:

$$\Pr(y_i = 1) = \text{logit}(\beta_1 + \beta_2 \text{year}_i)$$

For ordinal outcomes I use the model:

$$\Pr(y_i = j) = \Pr(\alpha_{j-1} - \beta\text{year}_i < u_i \leq \alpha_j - \beta\text{year}_i)$$

...for each ordinal category j, where α_{j-1} is the cut-point for the category lower than j, and u_i is the residual component of the assumed underlying latent scale y_i^* after controlling for βyear_i. The ordinal models therefore make the proportional odds assumption; that is, that changes in demands/control over time affect all values of the underlying latent scale equally, rather than e.g. making people more likely to give extreme answers at both ends of the distribution.

[36] Where the greatest change in an individual response category is misleading as to the effect on the ordinal scale as a whole, this is noted in the tables and the size of effect is amended accordingly.

- 'Low comparability' means there are methodological changes between years that are likely to affect the trend results. These trends should be treated with extreme caution;
- 'Moderate comparability' means there are some changes in question wording/survey methodology but I would not expect these changes to affect the results substantially;
- 'High comparability' means that there is no reason to doubt the comparability of the results. It should nevertheless be remembered that there is always a possibility that unknown factors interfere with even the high-comparability trends (e.g. changes in the representativeness of samples over time, changing interpretation of question terms, changing questionnaire context effects).

8.2. Results

The final data review consists of 99 pairwise comparisons between surveys in different years. As the detailed results are difficult to present (like other systematic reviews), much of the body of the review is placed in the Appendices at the end of the chapter. Here in the main part of the chapter, I instead present the broad sweep of the findings.

To bring together these results, I divide the questions into six groups: two groups of job demands (working hard/fast; having enough time) and four groups of control (over pace/speed of work; over how work is done; over task choice/order; say in decisions at work).

8.2.1. Job demands

The summary trends in **working hard/fast** are shown in Table 2. In general the trends are reasonably constant across surveys and questions: there was a rise in working hard/fast since the early 1990s, which particularly took place during the early-to-mid-1990s and has been stable in the past few years.

Much of this picture is based on the Skills Survey series, which accounts for all of the high quality trends starting before 2004. Other series provide a little further data that is generally in agreement with the Skills Surveys: the HSE shows a slight non-significant rise in demands 1993-1994, the moderate-quality EWCS 2000-2005 agrees with the stability in *'working at very high speed'* in the Skills Surveys, and the low-quality longer-term EWCS trend shows a rise in demands 1991-2005. The 2000 WiB survey shows slight differences to the 2001 Skills Survey for reasons that are unclear (see Appendix 2A), but still shows a sharp rise in demands from EiB 1992.

There are nevertheless some slight differences between particular trends in the late 1990s and early 2000s. The most likely explanation is that there were different trends in different aspects of job demands: in the early 2000s the Skills Surveys showed rises in *'working very hard'* at the same time as stability in *'working at very high speed'*, a within-survey difference that cannot be due to differences in the sample or questionnaire context. It is plausible that the apparent difference between trends in the late 1990s is similarly due to differences between questions (there was little change in *'working very hard'* but a decline in *'working at very high speed'*). However, as these were asked in different surveys this is necessarily more speculative than for the early 2000s, and differences particularly in the late 1990s are not entirely explained.

Table 2: Trends in working hard/fast
(up arrows refer to *raised* demands, i.e. working harder/faster)

Period	Comparability High	 Moderate	 Low
Early to late 1990s		↑↑ (1992-7) ≈↑ (1993-4) ≈↑ (1993-4)	↑↑↑ (1991-1995)
Late 1990s to early 2000s		↓↓ (1995-2000) ≈↑ (1997-2000) ↓ (2000-2001)	
Early to mid 2000s	↑ (2001-6) ↑ (2001-6) ≈≈ (2001-6) ≈≈ (2001-6)	≈≈ (2000-5)	
Mid to late 2000s	≈↓ (2004-8) ≈≈ (2004-8)		
Longer periods			
Early 1990s to mid 2000s	↑↑↑ (1992-2006) ↑↑↑ (1992-2006) ↑↑↑ (1992-2006)		↑ (1991-2005)

***Key**: ↑↑↑/↓↓↓ Very large rise/fall in demands (>10 percentage points in at least one category); ↑↑ Large rise/fall in demands (5-10%); ↑ Moderate rise/fall in demands (2-5%); ≈↑ Small rise/fall in demands (1-2%); ≈≈ no change (<1%). Note that this table does not take account of the statistical significance of the changes, which are reported in Appendix 2B; greyed out trends are not statistically significant at conventional levels (p<0.05). Underlined trends are those from the Skills Survey series SCELI86/ EiB92/ SS97/ SS01/ SS06.*

The other available questions on job demands refer to ***having enough time to do the job*** and these trends are shown in Table 3. There are fewer data than for working hard/fast and in general the trends are less robust. The data that do exist suggest that there was a deterioration in this form of job demands in the early 1990s, but stability or slight improvements in the later 1990s / early 2000s, and a larger improvement 2004-2008.

An alternative interpretation is that the trend data contradict each other, with the EiB-WiB trend showing rising demands 1992-2000, but other overlapping trends showing declines (EWCS 1995-2000, WERS 1998-2004 and the low-quality Eurobarometer series 1996-2004). It is not possible to rule this out, but the slight rise in the short period 1993-1994 visible in the HSE trend offers a little ereassurance that there was a genuine decline in having enough time to do the job in the early 1990s that did not carry through into the later 1990s.

Table 3: Trends in having enough time at work
(up arrows refer to *raised* demands, i.e. *not* having enough time at work)

	Comparability		
Period	High	Moderate	Low
Early to late 1990s		↑[†] (1993-1994)	
Late 1990s to early 2000s		↓ (1995-2000)	↓↓↓ (1996-2001)
Early to mid 2000s			↑↑↑ (2001-2004)
Mid to late 2000s		↓↓ (2004-2008)	
Longer periods			
Early 1990s to early 2000s		↑↑ (1992-2000)	
Mid 1990s to mid 2000s		≈≈ (1998-2004)	≈↓ (1996-2004)

Key: see Table 2.

8.2.2. Job control

Of the four types of job control, there are most data on **control over pace/effort***,* where we have over 20 trend comparisons that are shown in Table 4. In general it seems that this form of control declined substantially since the early 1990s. Yet there are also signs – albeit from low-comparability trends – that this followed a *rise* in control in the 1980s.

The picture becomes slightly more consistent when we separate out one particular question from the rest (the trends based on this question are **in red** in the table). Question C2 asks about the respondent's discretion in the context of a longer list of possible influences over the pace of work; it is quite plausible that increases in these other influences would make respondents less likely to say that their discretion influenced their work pace. Even within the Skills Surveys, it seems that trends in C2 do not follow trends in other measures of work intensity in either the 1992-1997 or 2001-2006 periods.

Even ignoring question C2, some apparent contradictions between surveys remain. EWCS supports a considerable decline in autonomy 1995-2000 but not 1991-1995 (although in the earlier period the comparability between EWCS surveys is much more questionable). WERS shows a *rise* in autonomy 1998-2004, which certainly contradicts EWCS (that shows declines 1995-2000 and 2000-2005), and also seemingly disagrees with the Skills Surveys. However, those associated with the WERS surveys believe that the Skills Surveys should be prioritised for looking at trends (see Web Appendix 1a), and Appendix 2A gives good reasons for doing this (due to a sampling methodology that is likely to introduce bias, and a sharply declining response rate over time).

The most plausible account is therefore that there was a decline in control over work pace in particularly the late 1990s – an account that largely (but not completely) depends on the Skills Survey series. Furthermore, there are signs that this comes after an *increase* in this form of control in the late 1980s, although the limited trends that exist are of debatable comparability.

Table 4: Trends in control over work pace/effort
(up arrows refer to *raised* control)

	Comparability		
Period	High	Moderate	Low
Mid 1980s to early 1990s			↑↑ (1986-1992) ↑↑↑ (1984-1996)
Early to late 1990s		↓↓ (1992-1997) ≈≈ (1992-1997)	↑↑ (1991-1995) ↓↓↓ (1992-1996) ↑↑↑ (1996-1997)
Late 1990s to early 2000s	↓↓↓ (1997-2001) ↓↓ (1997-2001)	↓↓ (1995-2000) ↓↓↓ (1997-2000) ↑↑ (2000-2001) ↑ (1998-2004)	
Early to mid 2000s	≈↑ (2001-2006) ↓ (2001-2006)	↓ (2000-2005)	
Mid to late 2000s	↓ (2004-2008)		
Longer periods			
1980s to 2000s			↓ (1986-2006) ↑↑ (1984-2000)
Early 1990s to mid 2000s		↓↓↓ (1992-2006) ↓↓↓ (1992-2006)	↓ (1991-2005)

Key: trends in red text refer to question C2 (see text above). For rest of key, see Table 2.

The second type of job control is **control over how work is done**, shown in Table 5. The trends here are difficult to interpret, but in general it seems that this form of control declined over the 1990s, and possibly to a lesser degree in the 2000s. (Long-term trends beginning in the 1980s have low comparability and conflict with one another, so little can be said about earlier trends).

However, this picture is almost entirely dependent on the Skills Survey series (including SCELI in 1986). The BSA survey series uses a question that would seem to tap similar dimensions of work as the Skills Surveys (*'My job allows me to design or plan most of my daily work'*), but this shows a significant rise in control 1989-1995 (the two rises 1989-1993 and 1993-1995 in Table 5), whereas the Skills Surveys show a sharp decline 1986-1997. Even excluding the more problematic SCELI data, the Skills Survey series shows declines in job control 1992-1997 whereas both BSA and the HSE show stability in job control for brief snapshots of the early 1990s. There is likewise a discrepancy between WERS 1998-2004 and the Skills Surveys 1997-2001/2006.

It is possible to explain away these differences – the Skills Survey trends are more robust, and/or the decline in control may be concentrated in short periods that are only covered by the Skills Survey trend. The only trend that specifically looks at control over *quality standards* at work also shows a sharp decline 1992-2006, with no other data to contradict this. But nonetheless, while the most plausible conclusion is that there was a fall in control over how work is done, there is some uncertainty here.

Table 5: Trends in control over how work is done
(up arrows refer to *raised* control)

Period	**Comparability** High	Moderate	Low
1980s to 1990s		↑† (1989-1993)	↓↓ (1986-1997)
Early to late 1990s		≈≈ (1993-1994) ↑ (1993-1995) ↓↓ (1992-1997) ↓↓↓ (1992-1997)	
Late 1990s to 2000s	↓↓ (1997-2001) ↓↓ (1997-2001) ↑† (1997-2001)	≈↑ (1998-2004)	
Early to mid 2000s	↓ (2001-2006) ≈≈ (2001-2006) ≈≈ (2001-2006)		
Mid to late 2000s	↓ (2004-2008) ↓ (2004-2008)		
Longer periods			
1980s to 2000s			↑ (1984-2000) ↓↓↓ (1986-2006)
Mid 1990s to mid 2000s		↓↓↓ (1992-2006) ↓↓↓ (1992-2006)	

Key: see Table 2.

The third type of job control is **control over task order/choice**, shown in Table 6. There are a relatively small amount of data available, which perhaps explains why the data are relatively consistent: this form of control rose in the 1980s, declined through the 1990s, and started rising again more recently.

Once more, though, it is possible that there are different trends in superficially similar questions. *Rising* control over the longer period is shown by three trends, two of which are based on the question *'Do you decide the specific tasks that you carry out or does someone else?'* It is however possible that the decline in control over task choice is due to constraining *processes* (e.g. customer pressures, targets) rather than supervisors/managers (section 10) – and if this is the case, then this question may show different trends to questions that focus directly on the amount of influence the individual has (with no reference to 'someone else'). Without this explanation, there is a striking conflict between the two moderate quality long-term series, the Skills Surveys (showing a large fall in control) and the BSA/WiB series (showing a large rise).

There is one other conflict in the data, between EWCS 2000-2005 and the two higher-comparability trends (ESS and the Skills Surveys). Again, these questions all refer to slightly different aspects of control over task order/choice ('order of tasks', 'how daily work is organised' and 'what tasks to do' respectively).

Table 6: Trends in control over task order/choice
(up arrows refer to *raised* control)

	Comparability		
Period	High	Moderate	Low
Early to late 1990s		↓↓ (1992-1997)	
Late 1990s to 2000s	↓ (1997-2001)	↓ (1995-2000)	
Early to mid 2000s	≈≈ (2001-2006) ↑† (2002-2004)	↓ (2000-2005)	
Mid to late 2000s	↑ (2004-2008) ↑ (2004-2008)		
Longer periods			
1980s to 2000		↑↑ (1985-2000)	↑↑↑ (1984-2000) ↑↑↑ (1984-2000)
Early/mid 1990s to 2000s		↓↓↓ (1992-2006) ↓↓ (1995-2005)	

Key: see Table 2.

The final type of job control is **say in decisions at work**, shown in Table 7. For this type of control there are more high-quality data for the 1980s than for other forms of control – but coming to general conclusions is difficult as different surveys and questions seem to show different trends.

Many of the trends relate to the question, *'If there was a change in the way you do your job, would you have any say?'* (question C16_1 shown in orange below, and hereafter referred to as 'say over changes'). This is different to other questions like *'I have a lot of say over what happens in my job*' ('say in general') – the former explicitly refers to externally-imposed changes at work, while the latter evokes images of day-to-day decisions (cf. the distinction between participation and discretion in Green 2006:98). It is perhaps unsurprising that these can show different trends, such as for the 1980s where the BSA shows a drop 1985-1987 and stability to 1993 in 'say over changes', while the World Values Survey shows a rise in 'say in general'.

Aside from differences in question wording, there are also conflicts between surveys with the same question. 'Say over changes' also seems to show large differences between surveys in the proportion of responses saying 'it depends' – perhaps reflecting how strongly respondents were (explicitly or implicitly) pressured to avoid this category. The Skills Survey in particular suffers here (see Appendix 2A), and this may explain why it conflicts with the trend in the identically-worded question in BSA over the 1990s and 2000s.

Finally, while it is noted in the table as a 'high-quality trend', the 1985-87 BSA trend may also be slightly problematic – it shows a very large drop in say over changes at work in only two years. Given that 1985 was the first time that this question was used, it is possible that some (unknown) change in the way the question was presented to respondents explains this change. This makes interpreting the long-term BSA trend difficult, as it is dominated by the 1985-87 decline in control, which is far greater than the rise in the early 1990s.

Overall, the balance of evidence is that there was a rise in say over decision at work in the 1990s, but trends both before and after this are uncertain.

Table 7: Trends in say over decisions at work
(up arrows refer to *raised* control, i.e. more say over decisions)

Period	**Comparability** High	Moderate	Low
1980s	↓↓↓ (1985-1987)	↑ (1981-1990) ≈≈ (1987-1993)	
Early to late 1990s		≈↓ (1993-1994) ↑ (1993-1996)	
1990s to 2000s	≈≈ (1996-2005) ≈↑ (1992-2001) ↑↑ (1992-2001)	↑↑ (1992-2000) ↓↓ (2000-2001)	↓↓ (1996-2001) ↓↓ (1996-2001) ↑↑↑ (1996-2001)
Early to mid 2000s	↑↑ (2001-2006)	↓↓ (2001-2006)	
Longer periods			
1980s to 2000s		↓↓ (1985-2005)	
1990s to 2000s	↑↑ (1992-2006) ↑ (1993-2005)	≈↓ (1992-2006)	

Key: trends in orange text refer to question C16_1 (see text above). For rest of key, see Table 2.

8.3. Conclusions

This section has tried to summarise a large amount of information from the review, while at the same time considering a number of technical issues around survey methodology that are critical for the comparisons of surveys. Often there were conflicts between different surveys that were difficult to fully explain, leading to a complex picture in trends in demands and control. If we take a further step back, though, then the following story emerges.

In the 1990s it appears that job demands and control did deteriorate – but different aspects deteriorated to different degrees. The clearest changes can be seen for 'working hard/fast' and 'control over pace/effort', where intensification was likely to have been large and substantial. Smaller/more inconsistent changes can also be seen in 'having enough time to do the job', 'control over how work is done', and 'control over task order'. In contrast, for one aspect – 'say over decisions at work' – there seemed to be a rise rather than a fall over the 1990s.

Outside of the 1990s there is no evidence of a widespread deterioration in job strain. In the 2000s there have been slight improvements in some aspects of work ('having enough time'), worsening in others ('control over how work is done'), but in general there has been stability. There are also suggestions that the 1980s was a period of *rising* job control, in contrast to the belief that this was a period of intensification and declining autonomy (Burchell 2006:5; Green 2008a:2). Much of the evidence for the 1980s is of dubious quality or is contradictory, but there appears reasonable grounds for accepting that 'control over task order/choice' improved (or at least one aspect of it, the extent to which task order/choice is determined by another person), although no convincing trend data are available for demands, and the limited evidence that exists suggest the 1980s were a period of intensification.[37]

Yet while this summary is the most plausible interpretation of the data, it is far from certain. Identical questions in different surveys commonly show different trends, as do similar questions even in the same survey. Some of this is doubtlessly the result of sampling error, which means we would not expect surveys to show identical results. But the degree of difference between trends is far greater than sampling error is likely to explain – a not uncommon finding for attitudinal measures in the social sciences as I noted in the previous chapter when discussing trends in WLD, self-reported health and disability. It seems that such measures are easily influenced by other factors (e.g.

[37] Green provides evidence that the rise in job demands in fact started rising in the 1980s. First, the sources of effort pressure rose 1986-1992 (Green 2006:57). Second, it has been argued that it is not possible to understand productivity growth in early 80s manufacturing in the absence of investment without drawing on intensification (cited in Green 2001:63,72). Green also shows that when looking back over the past five years, employees in the late 1980s said that work had become more intense (p50-52) – but given the social desirability pressures on people, it would be surprising if they responded to such questions in any other way.

questionnaire context effects) which means that the review shows a combination of true trends and random noise.

To cut through this confused picture we need to prioritise some comparisons over others. This generally (although not universally) means prioritising the trends shown in the Skills Surveys over other data – and one of the advantages of the review is that this is done systematically having considered the comparability of the data, rather than because this shows the 'right result'. The Skills Surveys series are simply more comparable than even the two other most widely used surveys of work, WERS and EWCS. WERS samples employees via their employer which not only makes it much less likely to be representative than the Skills Surveys but also seems to become substantially less representative over time; the EWCS is barely comparable 1991-1995 due to changes in its sampling design and even more recently suffers from a weaker sampling methodology than the Skills Surveys (see Appendix 2A).

As I stated at the outset, I expected the review to come to similar conclusions to previous analyses of the Skills Surveys. Despite this, I felt the unusual step of doing a systematic data review was necessary given the importance of these trends for this thesis. Without this, we would have been left with conflicting findings that would require us to arbitrarily prioritise one dataset over another – particularly given the recent claims of 'de-intensification' (Burchell 2010). While the review cannot provide certainty where the data do not justify it, it does provide a systematic basis for comparing studies, and a transparent basis for arguing that job strain *probably* increased over the 1990s.

9. TRENDS IN JOB STRAIN

In the systematic data review, I carefully investigated the trends in specific measures of job demands and control – but this tells us nothing about the trends in job strain itself (i.e. the *joint* distribution of demands and control). In this section, I therefore use the Skills Surveys and the 1992 Employment in Britain survey to look at trends in job strain. As this is similar to Francis Green's figure from the Introduction, this section is brief – although there are very slight differences in the results, as I explain shortly.

9.1.1. Methodology

Across the 1992 EiB survey and the 2001 and 2006 Skills Surveys, we have a total of three questions on job demands and five on job control (see Appendix 2B), with the job control questions also available in the 1997 Skills Survey.[38] While these are analysed separately in the data review, I here combine these into scales of demands and control so that I can then create a single measure of job strain. Analysing trends through these scales has other advantages too; not only does this make the analysis more parsimonious, but the scales will be more sensitive and contain less measurement error (Bartholomew et al 2008).

In previous studies using the Skills Surveys, these questions have been combined into a scale by either simply adding the score from (a subset of) these questions (Gallie et al 2004), or using a Principal Components Analysis (PCA) to create summary scores for how far the different questions seem to be measuring the same thing (Green 2008b). However, PCA is an imperfect technique in this situation – it is designed for continuous measures, and the ordinal variables here are better analysed using Item Response Theory (IRT) models (Zheng and Rabe-Hesketh 2007).

For each of demands and control, I therefore created scales of demands and control using two-parameter IRT models (see Appendix 2B for details). The scales have been standardised so that they have mean=0 and standard deviation=1. There are two main differences between this analysis and Green's figure in the Introduction: I create the scales using IRT rather than PCA, and I use a scale based on three questions on job demands, rather than the single question that was also asked in 1997.

9.1.2. Results

Trends in demands-control are shown in Table 8. As expected, between 1992 and 2006 we can see a sharp rise in demands and a decline in control that is statistically

[38] All of the results using these scales are restricted to the employed, as certain questions on self-control were only asked to employees in EiB.

significant ($p<0.001$). However, there appears little change in either demands or control 2001-2006.

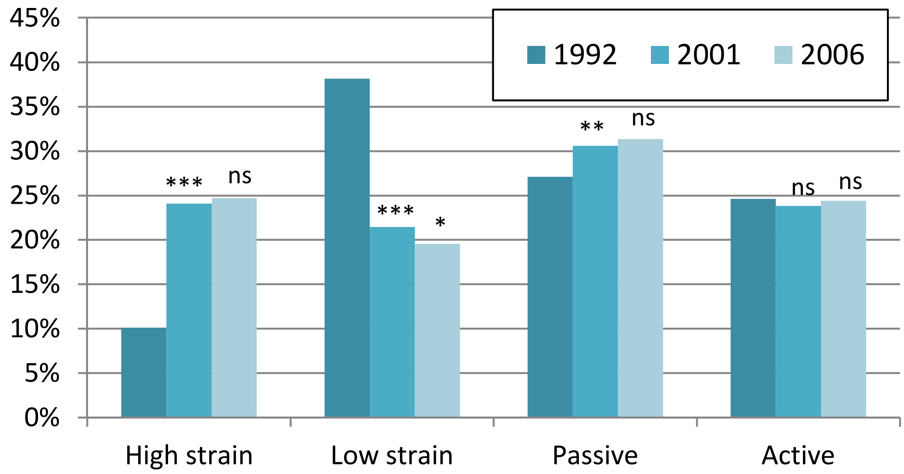
Table 8: Trends in demands-control scales 1992-2006

	Control	Demands
1992 EiB *(reference category)*		
1997 Skills Survey	-0.30**	na
2001 Skills Survey	-0.41**	0.39**
2006 Skills Survey	-0.43	0.41

*Significance level refers to change from previous survey (e.g. the lack of significance for 2006 is compared to 2001, not 1992). Key: ** p<0.01.*

The trend in job strain is shown in Figure 11, using the four-way division described in Chapter 1. This shows a sharp rise in high-strain jobs (from 10% to 25% of jobs), a sharp fall in low-strain jobs (from 38% to 21%), and a smaller rise in passive jobs (from 27% to 31%). Nearly all of these trends occurred 1992-2001; the change from 2001 to 2006 was relatively minor and only statistically significant for a small continued decline in low-strain work.

Figure 11: Trends in job strain in Britain



Job strain categories defined by splitting demands-control at the median value. ***Source****: EiB, SS97 and SS06 surveys (see Appendices). Significance level refers to change from previous survey (e.g. 2001-2006). Key: * p<0.05, ** p<0.01, *** p<0.001.*

These results differ slightly from Green (2009b). In both cases there has been a rise in high-strain jobs strain 1992-2001, but in Green's work this continues after 2001, whereas in the results in Figure 11 there is little change 2001-6. This difference is simply because there was a rise in the single-item measure of job demands that Green uses in this particular figure,[39] but not in the other measures of job demands.

[39] Question D1: My job requires that I work very hard.

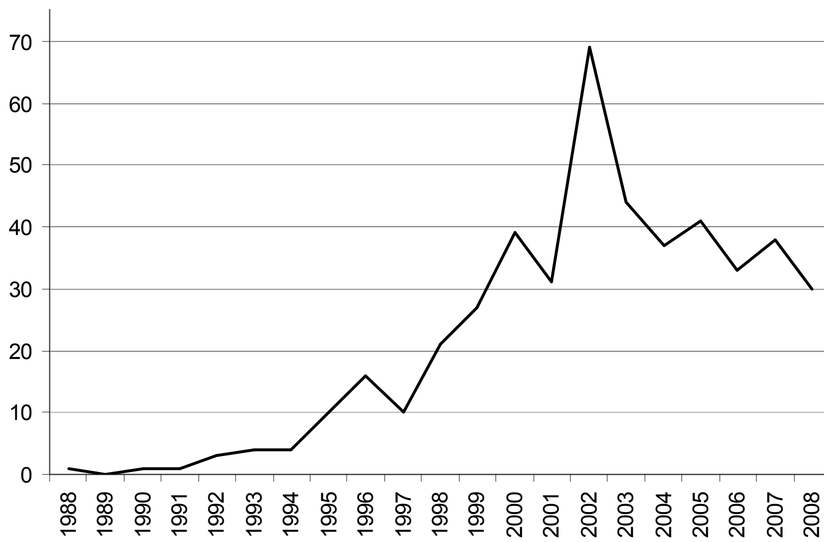
10. HAS THERE REALLY BEEN A RISE IN JOB STRAIN?

When I have presented this to academic audiences, the first question asked is usually 'how do you know that the rise in job strain is real?' Instinctively many academics seem sceptical that job strain has risen – despite often accepting that there may well have been rising job strain for their *own* profession. In this section I consider how far such trends can be taken to show changes in objective working conditions.

Many of the demands-control questions are relative. For example, self-reports of 'working hard' reflect (i) the 'true' levels of demands required in the job; and (ii) people's 'effort norms' that influence their reporting of effort questions (Green 2001:56; Brown et al 2007:947). Green and Brown et al argue that trends can be meaningfully analysed over the medium turn – e.g. around 10 years – if we assume that norms do not change. To the extent that norms change they are most likely to reflect the new status quo, underestimating the real level of change (Green and McIntosh 2001).

The alternative to a real rise in job strain, though, is a shift in people's willingness to report job strain due to the rise of the discourse of work stress. As Figure 12 shows (taken from Wainwright & Calnan), media reports of work stress grew over the very period that self-reported job demands were increasing (Stansfeld et al 2008b:27). This may also be an explanation for the rise in self-reported WLD in outlined in Chapter 1.

Figure 12: Major mentions of Work Stress in UK Newspapers 1988-2008



Source: Figure 2 from Wainwright & Calnan (in press), based on LexisNexis searches.

Wainwright & Calnan (2002:143-157) have argued that this is a contemporary form of the conflict between labour and capital, with conventional trade unionism being marginalised and finding easier expression in the health and safety agenda. Eakin & MacEachen (1998:910) have shown how *"employees' health condition became drawn into the social relations of work and vice versa"*, with conflictual social relations leading workers to attribute health problems to work. There are also historical examples of work-related conditions such as 'telegrapher's cramp' that arose and then vanished, reflecting cultural fads more than genuine epidemics (Henderson 2010).

While a discourse of stress *in itself* could lead to physiological responses and disease (Wainwright and Calnan 2002:161), my concern is with establishing whether there were real (discourse-independent) changes in the nature of work – and this is exceptionally difficult to test. The rise in the discourse of 'stress' may be a cultural phenomena, but it may equally be due to genuinely rising job strain over the same period.

Despite this, there are several reasons to believe that the rise in job strain is genuine, many of which are taken from Green's discussions:

- Self-reported demands have some validity; they correlate with managers' reports (Green 2001), and provide a pay premium (Green 2006:63).
- Case studies show increasing demands in various occupations, including NHS nurses (Adams et al 2000), public sector managers (Morris and Farrell 2007), financial services (McCann 2009) and middle-management across several industries (McCann et al 2008) – although these case studies suggest intensification is not always accompanied by declining discretion (Green 2006:79). This dovetails with descriptions of the contemporary pressures on workers; for example, supermarket managers are subordinated to head office's standardised practices and targets, with their main role now reduced to encouraging employees to work harder (Grugulis et al 2010); while French et al 2008 (cited by McCann 2009:231) describe how decisions on offering loans are now done by computer algorithms rather than bank managers.
- While workers were reporting increased stress-related complaints at work – and indeed, not reporting this in all surveys[40]– they were also reporting *fewer* musculoskeletal complaints caused by work, with the net result that reports of health problems caused by work actually *fell* over the 1990s.[41]

[40] Self-reporting of 3 or more mental health problems due to work decreased substantially 1995-2005 in Britain according to the EWCS surveys, unlike nearly every other EU country (OECD 2008:Figure 4.7; Houdmont et al 2010:659). But reports that 'I find my work stressful' in the BSA surveys increased 1989-2005 (Crompton and Lyonette 2007:61; Olsen et al 2010:234). The HSE surveys find a rise in stress 1995-1998, followed by a smaller decline since (Chandola 2010:47).

[41] From Stansfeld et al (2004) updated with HSE Table SWIT3W12 (accessed via www.hse.gov.uk/statistics/lfs/swit3w12.xls, 1/3/2011).

- There was a rise in *sources* of effort pressure over the 1990s (see section 10.1), which even Wainwright and Calnan (2002:134) accept suggests that the rise in job demands was partly genuine.
- While 'stress' may be a cultural phenomenon, this is somewhat independent of reports of 'strain' based on questions about working to tight deadlines. Furthermore, it is difficult to see how these would influence the way people respond to questions on job control, and section 8.2.2 showed that not all aspects of job control have declined.

It therefore seems unlikely that rising self-reported job strain is fully attributable to the work stress discourse.

10.1. Explaining rising job strain

When people are sceptical about rising job strain, this often seems to come from an *a priori* view that work should be getting better rather than worse, with the old 'bad jobs' in manufacturing being replaced by a 'knowledge-based' service economy (Noon and Blyton 2007:161; Darr and Warhurst 2008). In this view it may well be puzzling to think that job strain has risen. But there are strong reasons to doubt such a Panglossian picture.

Not only are claims about upskilling often overstated (Green 2006:172; Noon and Blyton 2007:164), but there is a strand of Work Sociology that is essentially pessimistic about the future of work. The 'labour process' school traces its lineage back to Braverman's (1974) *Labor and Monopoly Capital* that – to over-simplify – argued that managers continually try to deskill their workforce in order to reduce discretion and maintain capital's control over labour (Noon and Blyton 2007:154; Watson 2008:62,153). Given these conflicting claims, we need to look at what has been happening in practice.

The most important change seems to be the growth of Information and Communication Technologies (ICT), which has contributed to a growing polarisation of work through what is often known as 'skill-biased technological change' (Michaels et al 2010). Complementing this, Francis Green has argued that there is a process of 'effort-biased technological change' – that is, ICT *"differentially increases the productivity of workers who are prepared to, or who can be persuaded to, devote high effort levels"* (Green 2006:69). ICT also increases the ability of managers to monitor (and therefore control) their workers, paradigmatically seen in call centres. Empirically, there is some evidence that ICT leads to increased work effort, although rarely is this effect large enough to explain much of the observed increase in demands (Green 2006:72-3), while mixed effects are found on control depending on whether analyses are cross-sectional (Gallie et al 2004; Green 2006:108) or longitudinal (Green 2006:108).

There are other shifts in the organisation of work (Chapter 1) that may be relevant here – including delayering (Littler et al 2003), lean production (Landsbergis et al 1999), Total Quality Management, and quality circles. Many practices associated with these techniques have become more common in Britain,[42] such as team, incentive and development practices (McGovern et al 2007:149). Yet despite the positive claims that have been made, the impact of these techniques is often detrimental (Green 2006:84) – 'quality circles' (Delbridge and Whitfield 2001:482-3), lean production (Landsbergis et al 1999), high-performance working (Lloyd and Payne 2006a; Belt and Giles 2009:21-2), and general job task interventions (Bambra et al 2007) have all been associated with intensification and/or declining control.

Similarly, while self-managed and self-directing teams have higher discretion, non-self-directed teams are associated with a *reduced* level of all forms of control (Delbridge and Whitfield 2001:Table 3; Gallie et al 2001:21,24; Green 2008a; Gallie et al In Press). Moreover, *all* forms of teamwork are associated with a higher speed of work (Gallie et al In Press). Given that teamwork rose sharply in the 1990s – while the use of self-directing teams fell (Gallie et al In Press) – it is likely that this contributed to rising job strain.

These changing HR practices can be described as a form of 'bureaucratic discipline', with 'massive shifts' in the use of such techniques for managers and professionals (McGovern et al 2007:186). The result is that the number of sources of effort pressure – particularly pressures from machines, colleagues and pay incentives – rose noticeably 1992-1997 if not since (Green 2006:57; Felstead et al 2007:124).[43] At the same time, the normalisation of the threat of redundancy in the 1990s helped to enforce 'market discpline', which was particularly important at the bottom of the occupational hierarchy according to McGovern et al (2007).

These are the likely proximal causes of rising job strain, but behind them lies distal, more structural factors – and foremost amongst these is the rise in global competition (Eurofound 2009:26; Moutsatos 2009:25). Alongside the decline of trade unions, this has reduced the ability of employees to resist changes imposed by managers who have continually been seeking to get more out of their workers. It is not the case that these structural factors acted in isolation – rising job strain in the 90s came *after* the

[42] This does not extend to routine workers where such practices were becoming *less* common (McGovern et al 2007:149-150).

[43] This fits the trends in reported working hard/fast in section 8.2.1, although it grates slightly against the reported decline in control over work pace in the late 1990s.

major declining in trade unionism (Green 2006:80) but *alongside* growing wages and reduced insecurity (Green 2006:85; McGovern et al 2007:151). Instead, working conditions will depend on both the desire and ability of management to intensify work and tightly control their workforce (Green 2006:101). The problem, then, may be that the institutional features of the British labour market allowed employers to use ICT in particular ways (Green 2006:80; Wajcman 2006:777; Green 2009b:20):

> *"The impact of these new information technologies and new management systems on work intensity still depends on the social and political environment; the experience from the United Kingdom can therefore be considered an indication of their potential impact in an environment of low regulation and weak trade unions"* (Rubery and Grimshaw 2001:176).

This fits evidence that lower union power was associated with greater intensification internationally (Green and McIntosh 2001; Gallie 2002), and is also suggested by the relative uniqueness of the decline of autonomy in Britain compared to other countries. I return to this in more detail when considering the policy implications of the thesis as a whole in Chapter 9.

A final factor that cannot be ignored is the role of public service reform, and particular the set of practices known as 'New Public Management' (NPM) that grew in the 1980s and became dominant in the 1990s (Osborne and McLaughlin 2002), itself prompted by wider structural factors (Ferlie and Fitzgerald 2002) and helped by the declining power of public sector unions (as implied by Green 2006:79). NPM by definition seems to imply rising job strain, given that it consists *inter alia* of output controls, privatisation and quasi-markets, performance management, and discipline in resource allocation (Osborne and McLaughlin 2002). Especially for professionals, the rise of NPM, audit culture and subcontracting seem to account for some of the decline in job discretion (Grimshaw et al 2002, cited by Green 2006:109). It may also be the case that the same ideological trends partly explain intensification in the private sector, which occurred even where competitive pressures were less severe (McCann et al 2008:366) and may contribute to declining control which (compared to intensification) is less well-explained (Green 2011:123).

This section has only touched the surface of a vast pool of research and debate, both within Britain and internationally. Nevertheless, I have argued that it is only too plausible that job strain did increase in Britain in the 1990s, as new technologies (and managerial techniques enabled by them) were adopted, combined with an institutional framework where intensification and reduced autonomy made sense, bolstered by a doctrine of public sector reform that pointed in the same direction.

To be clear: I am not claiming that job strain *must have risen* by virtue of this plausible explanation – there are no end of convincing explanations that have been advanced for findings that later turned out to be untrue (Macleod and Davey Smith 2003), and in any debate about empirical findings, it not uncommon for both sides to provide highly plausible explanations for why their results are correct (Davey Smith and Phillips 1992: 759). Instead, having observed empirically the trends in section 8.2, I want to show that the accusation that these trends are *im*plausible is false.

11. Conclusions

In this chapter I have asked whether job strain has genuinely risen in Britain. I began by systematically reviewing all of the available data on demands and control, finding that different surveys show different trends that defy any obvious explanation. Some uncertainty is inevitable in social scientific research, particularly when reaching back into the past. Nevertheless, the weight of evidence from the highest-quality trends – in particular those from the Skills Survey series by Green and colleagues – is that job strain rose in the 1990s, with a more mixed recent picture and substantial uncertainty about the 1980s. The finding does not revolutionise the existing picture, but the review does set out a comprehensive account of the evidence, and provides a robust and transparent basis for prioritising certain trends over others.

While employees report higher demands and lower control, Wainwright & Calnan (2002) have argued that this reflects changing discourses rather than real changes in working conditions. It is exceptionally difficult to provide conclusive proof here, but the suggestive evidence does not support the 'stress discourse' argument. There has been a fall in people's willingness to say that work has caused their health problems, with rises in stress being more than compensated by falls in work-attributable musculoskeletal problems. Case studies show individual examples of workplaces where intensification has occurred. It is also unclear why an epidemic of 'work stress' in public debate would lead people to report declining control over their jobs – particularly when they were simultaneously willing to report improvements in having a say in decisions at work. Moreover, the expectations for a utopian 'knowledge economy' are naive, and there are good reasons why we might expect job strain to have risen.

Two final observations are necessary here. First, low-quality data suggests that job conditions have been deteriorating in the recession (Chandola 2010:15), bearing out earlier research on the impact of economic conditions (Fenwick and Tausig 1994). In the short-term this will be followed by intended intensification in the public sector, and in the long-term many experts believe that intensification will increase due to broader economic trends (Foresight 2008:12; Murphy 2008:5). Second, while I have not covered physical demands in the systematic data review, I mentioned in Chapter 1 that there are no signs that these have declined since 1989 – presumably because rising demands (which partly covers physical demands; Choi et al 2011) have counterbalanced changes in the occupational structure. From the perspective of disabled people, rising job strain has not been counterbalanced by declining physicality.

This lays the foundation on which the rest of the thesis is built – but I return to the uncertainty of these trends once more in the concluding chapter.

12. APPENDICES

This chapter has two Appendices (which appear at the end of the thesis) and one Web Appendix (available from www.benbaumberg.com/thesis.htm):

- **Appendix 2A: Detail of the systematic data review** – details of the pairwise comparisons between measures of demands-control in different years (including question wording, statistical significance, assessment of comparability, size of change, and sample size used for the comparison).
- **Appendix 2B: Construction of the demands-control scales** – how Item Response Models were used.
- **Web Appendix 2a: Studies from the data review included in the pooled data file** – overall study-by-study details of the surveys used in the review (including intended survey population, sample frame, response rate, weights, total sample size, and UK Data Archive Study Number).

CHAPTER 3:
The effect of job strain on work-limiting disability in the Whitehall II study

13. INTRODUCTION

In the last chapter, I established that job strain in Britain rose – or at least, *probably* rose –during the 1990s. The main question for the next five chapters is then, 'what impact does job strain have on work-limiting disability (WLD), health-related job loss and incapacity benefit receipt?' The ideal way to answer this question would be through exogenous sources of variation in job strain (see Chapter 1), but convincing examples of this are hard to find (see also the research recommendations in Chapter 9).

The next best solution would be a data source that (i) contains information on job strain, WLD and incapacity benefits; (ii) contains information on potential confounders such as health and SES; and (iii) is longitudinal, so that we can establish the temporal order of job strain and the various outcomes. Unfortunately such data sources are rare in Britain. The studies that come closest to this ideal are the English Longitudinal Study of Ageing (ELSA), and the influential Whitehall II cohort study ('WII').

I chose the WII study for this study, partly because it is the most influential study on work and health that has been conducted in recent decades, but primarily on the grounds that it has a larger effective sample size – since ELSA is a study of the 50+ population, more than half of the sample is outside of working age and/or already not-working at baseline. [In time ELSA will become a more attractive alternative as more waves accumulate; see Chapter 9].

In this chapter, I use WII to examine whether job strain leads to WLD. The chapter proceeds as follows. First, I describe the WII dataset and how the key concepts (WLD, job demands-control) were operationalised. Second, I describe in more detail the hypothesised links between job strain and WLD, and the statistical models used to test this. Third, I present the results of these models, with particular attention given to the effect of different control variables and the specificity of the effect. Fourth, I look at how far any such relationships are mediated by health or operate through a 'job requirements' pathway. Finally, I discuss the limitations of the study and bring these results together in an interim conclusion.

14. DATA

14.1. Whitehall II

The Whitehall II (WII) study is one of the most widely-cited cohorts used for analysing the influence of job strain on health, from the 1990s (e.g. North et al 1996; Bosma et al 1997; Stansfeld et al 1997) to the present day (e.g. Elovainio et al 2009; Hintsa et al 2010; Virtanen et al 2010). The WII population is made up of civil servants who were aged 35-55 at the baseline wave and working in the London offices of 20 Government departments (Marmot and Brunner 2005). While the WII study began in 1985-1988 and new waves continue to be collected, the data needed for the WLD analysis are currently only available in waves 3-7 (1991-2004). Excluding those who reached the civil service retirement age of 60 by the following wave, this leaves a sample aged 39-57 at wave 3, primarily constituted of middle-aged male London-based central government employees (see Appendix 3B) – who clearly cannot be taken to be typical of the wider population.

The original sample at wave 1 was 10,308 people, but the available sample for the analyses here is lower. This is due to mortality (226 study participants died between waves 3 and 6), attrition (71% of the original sample were remaining at wave 6), respondents leaving the scope of the study (due to ageing and retirement) and missing data (section 15.2.2). The effective sample size for the analyses below is roughly 8,000 observations[44] taken from 6,000 people (exact sample sizes are given for each analysis below).

14.2. Measures available in WII

14.2.1. Job strain

WII contained a number of questions on the psychosocial work environment in waves 3 and 5, based on Karasek's Job Control Questionnaire. This includes four questions on job demands and nine questions on job control (here using the 'decision authority' subscale; see section 3.1.2), which are described in Box 3.1 below. These were supplied in the form of derived 0-100 scales that have been previously validated by the WII team.[45]

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[44] An 'observation' is the combination of data at baseline and follow-up waves (i.e. wave 3 baseline + wave 4 follow-up = 1 observation).

[45] For example, the four job demands questions have a moderate level of internal validity (Cronbach's alpha =0.67), while the 15 decision latitude questions – including the skill discretion as well as decision authority subscales – have a good level of internal validity (Cronbach's alpha =0.84) (Bosma et al 1997).

Box 3.1: Job demands and job control questions in WII

<u>Job control (decision authority sub-scale)</u>

"Concerning your particular work:"

1. Do you have a choice in deciding HOW you do your job?
2. Do you have a choice in deciding WHAT you do at work?

"About your position at work – how often do the following statements apply?"

3. Others take decisions concerning my work
4. I have a good deal of say in decisions about work
5. I have a say in my own work speed
6. My working time can be flexible
7. I can decide when to take a break
8. I have a say in choosing with whom I work
9. I have a great deal of say in planning my work environment.

<u>Job demands</u>

"Concerning your particular work:"

1. Do you have to work very fast?
2. Do you have to work very intensively?
3. Do you have enough time to do everything?

"About consistency and clarity regarding your job"

4. Do different groups at work demand things from you that you think are hard to combine?

[For all questions there were 4 answer categories: often – sometimes – seldom – never/almost never]

For the main regression models, I followed the conventional practice (North et al 1996; Kivimäki et al 2006:434) of recoding the demands and control scales into three tertiles. However, I also conduct sensitivity analyses using continuous scales, which avoids the loss of information inherent in turning these into dummy variables (Austin and Brunner 2004; Altman and Royston 2006; Royston et al 2006).

To test the effect of job *strain* beyond that of demands and control individually as hypothesised by Karasek, I include the interaction between the highest tertile of demands and the lowest tertile of control (North et al 1996; Kivimäki et al 2006:434). In sensitivity analyses, I use the interaction of the continuous scales of demands and control (again to avoid losing information) – and more intuitively, I also use simple dummy variable to compare (i) high-strain jobs to (ii) low-strain jobs and to (iii) all other jobs.

14.2.2. <u>Work-limiting disability (WLD)</u>

In the LFS trends presented in the Introduction and Chapter 1, WLD is measured by a single question on whether the respondent's health interferes with their ability to work. Within WII, however, we instead have *eight* questions that ask about different aspects of WLD, shown in Box 3.2. While I use all eight questions separately in a sensitivity analysis, to facilitate the main analyses I combine these into a single measure of WLD.

Box 3.2: SF-36 questions on Work-Limiting Disability (WLD)

These eight questions are taken from the Short-Form 36 (SF-36) scale (Ware and Sherbourne 1992); bold text is copied from the original WII questionnaire.

Physical health (PH)

*"During the **past 4 weeks**, have you had any of the following problems with your work or other regular daily activities **as a result as a result of your physical health**?"*

1. PH-cut time:*"Cut down on the **amount of time** you spent on work or other activities"*
2. PH-accomplish:*"**Accomplished less** than you would like"*
3. PH-kind: *"Were limited in the **kind** of work or other activities you could do"*
4. PH-difficulty: *"Had **difficulty** performing the work or other activities (for example, it took extra effort)"*

In the remainder of this thesis, these are referred to as PH-cut time, PH-do less, PH-limit type and PH-difficulty respectively.

Mental health (MH)

*"During the **past 4 weeks**, have you had any of the following problems with your work or other regular daily activities **as a result of any emotional problems** (such as feeling depressed or anxious)?"*

5. MH-cut time: *"Cut down on the **amount of time** you spent on work or other activities"*
6. MH-do less: "***Accomplished less** than you would like"*
7. MH-careless: *"Didn't do work or other activities as **carefully** as usual"*

In the remainder of this thesis, these are referred to as MH-cut time, MH-do less, and MH-careless respectively.

Pain

8. WLD-pain: *"During the **past 4 weeks**, how much did **pain** interfere with your normal work (including both work outside the home and housework)?"*
   [Five response options: *"Not at all" / "Slightly" / "Moderately" / "Quite a bit" / "Extremely"*]

In the remainder of this thesis, this is referred to as WLDpain.

The summary measure was calculated using Item Response Theory (IRT; Bartholomew et al 2008) – that is, by seeing how far the different questions seemed to measure the same concept, and weighting them accordingly (Appendix 3A). The summary WLD score was then turned into a single binary variable, with the cut-off value of the scale chosen to reflect the level of prevalence of conventional measures of WLD in the general population.[46]

This somewhat reduces the power of the analyses – again, dichotomising continuous variables loses information. This is outweighed by (i) the need to deal with the extremely skewed nature of the latent WLD scale, (ii) the greater difficulty in explaining the results to policy (and Social Policy) audiences if Negative Binomial models were used to account for this skew, and (iii) the need to ensure comparability with Chapters

[46] Among employees in this age group in the Labour Force Survey, the prevalence is 10%.

6 and 7 where only a dichotomous measure of WLD is available. As will be shown below, even with the binary WLD measure the analyses are sufficiently high-powered to show clear results.

While scales such as this have the advantage of reducing measurement error compared to a single WLD question, the underlying questions are imperfect measures of WLD for three reasons. First, they ask about limitations to ‘work or other regular daily activities’ rather than purely work, which seems to confuse non-working respondents (Mallinson 2001:185-190). Second, they ask about the past 4 weeks rather than longstanding limitations, and so seem to capture acute as well as chronic limitations.[47] Finally, only three questions have unambiguous meanings for those who are not working.[48] I return to the implications of this in the Discussion below, and present empirical evidence on how they are interpreted in Chapter 8.

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[47] 38% of people who are classified as having a WLD do not report a longstanding illness (compared to 69% of those without a WLD), and there is substantial movement in-and-out of WLD states (see section 16).

[48] For example, it is difficult to say that you have ‘accomplished less’ in your job if you do not have a job at all. The questions with relatively unambiguous meanings are PH-cut time, PH-limit type, and MH-cut time.

14.3. Other variables used

The analyses control for an extensive range of baseline health and socio-demographic characteristics, reflecting the many factors that previous empirical research (or *a priori* theoretical reasoning) have suggested may influence WLD (e.g. Banks et al 2006; Kapteyn et al 2008) or labour market participation (e.g. Allebeck and Mastekaasa 2004; Jones 2007). These variables have been split into four groups:

- *demographic/administrative* (lag between waves, age, wave, gender, marital status);
- *socioeconomic status* (grade, financial problems, housing problems);
- *physical health* (SF-36 physical functioning scale, pain, diabetes, respiratory illness, and 14 categories of longstanding illness (LSI)); and
- *mental health* (GHQ caseness, SF-36 mental health scale, and two categories of longstanding illness (depression and mental health (other))).

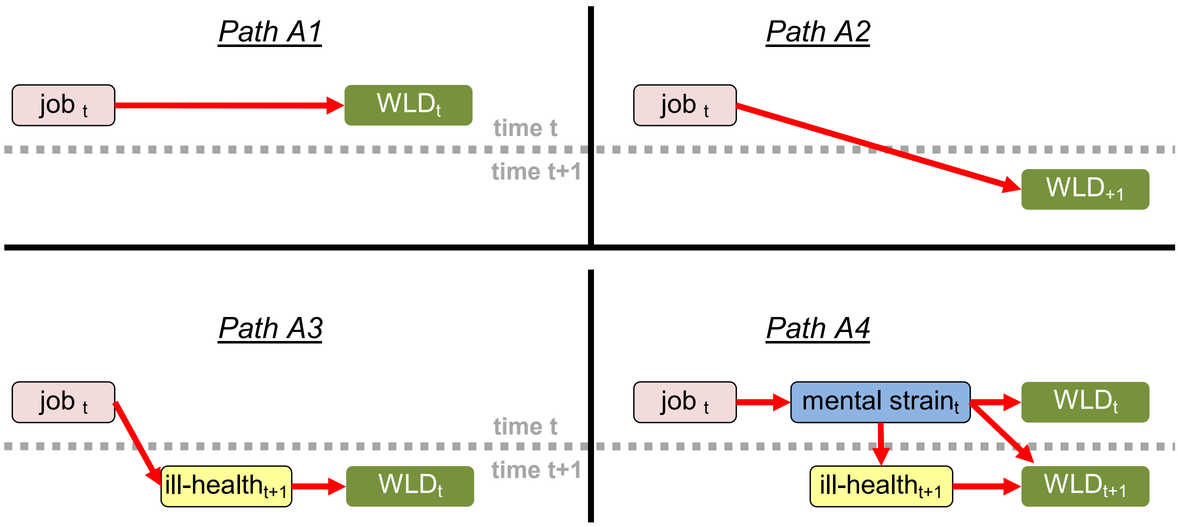
There are a number of variables that may be either confounders or mediators of the relationship (see Chapter 1); I discuss this more in section 15. I include grade (as well as non-work based measures of SES) in the main specifications here, but also test the effect of omitting grade in a sensitivity analysis (Chapter 1). Finally, additional control variables – of which education is the most important – were only available at certain waves, and are therefore considered in sensitivity analyses. Given the large number of variables involved I do not describe these in detail in the main body of the text; full details of the form and derivation of all these variables are instead set out in Appendix 3B, with the descriptive statistics available in Web Appendix 3a.

Before outlining the statistical models used to investigate the relationship of job strain and WLD, I need to be clear as to the hypothesised pathways between the two – partly so that I can specify the appropriate temporal relationship, and partly to decide how to take into account selection effects (confounding) without over-controlling for mediators.

15.1.1. Pathways from strain to WLD

In Chapter 1 I briefly described two pathways from job strain to WLD: a ***health pathway*** (high strain makes you sicker) and a ***job requirements pathway*** (at any given level of health, you are less likely to be fit-for-work in a high-strain job). Thinking temporally, each of these splits into two hypothesised sequences, shown in Figure 13:

Figure 13: Temporal sequences from strain->WLD



- ***Path A1***: at any given moment of time, a person with health problems is more likely to report a WLD if they are in a high-strain job. For example, a person with arthritis may feel they have a WLD if they did a demanding physical job, but may not feel they have a WLD if they did a less demanding, less physical job.
- ***Path A2***: as time goes on, a person in a high-strain job may feel less fit-for-work, even if the job strain is not affecting their health directly. This can occur for several reasons:
    - Their health deteriorates for other reasons (most people are employed when they become disabled; Bardasi et al 2000, cited by PMSU 2005:152);

- They may initially manage to cope with their health problem at work, but this may increasingly become a limitation due to fatigue or the build-up of uncompleted work (Chapter 8);
- It takes a certain amount of time for the additional number of daily failure experiences (Semmer 2011) to become perceived as a fitness-for-work limitation (Daniels 2006:283).

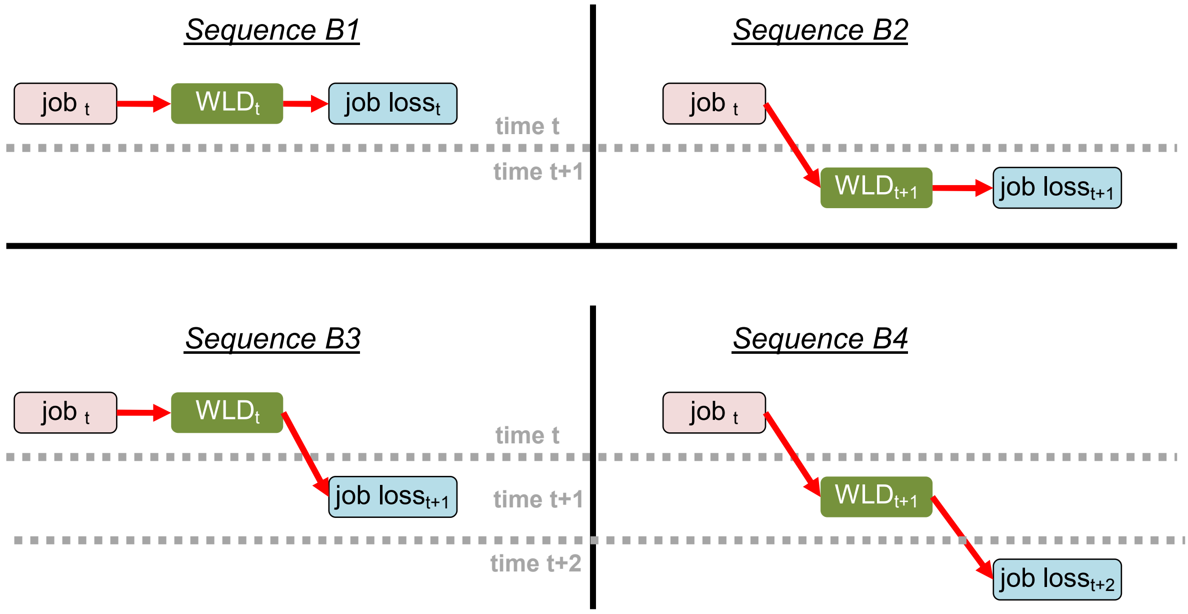
It is however unclear as to exactly how long these processes take (Zapf et al 1996:154; de Lange et al 2003).

- ***Path A3***: job strain may lead to heart disease, mental ill-health and musculoskeletal problems over time (Chapter 1) – although again the exact lag time is unclear.
- ***Path A4***: job strain may lead to mental strain in the short-term (Griffiths et al 2009:9). While a meta-analysis suggests that the strongest impacts of stressors like demands strain comes after 2-3 years (Ford et al 2011), this varies with age, and other studies suggest shorter lags (De Lange et al 2004) or even instantaneous effects (Grebner et al 2005:39-40). Mental strain may then have two impacts:
    - It may lead to ill-health over time, as originally hypothesised by Karasek (1979);
    - It may directly and instantaneously lead people to feel less fit-for-work.

15.1.2. Pathways from WLD to job loss

Having considered the pathways from job strain to WLD, we now need to introduce a further complication – the role of job loss – with four further sequences outlined in Figure 14. (While this should be read alongside the previous figure, to make the figure easier to interpret I do not repeat all of the paths within Figure 13).

Figure 14: Temporal sequences from strain->WLD->job loss



The employee may develop a severe fitness-for-work limitation that instantaneously leads to job loss (Sequences B1 and B2). However, we never observe the full Sequence B1 as we cannot observe job strain (job_t) at the same time as job loss; only Sequence B2 is observable.

Alternatively, there may be a period of time after developing a WLD and before job loss (as found in Oguzoglu 2009) – either because the employee 'struggles on' for a time (Sainsbury and Davidson 2006), or because there is a delay between performance limitations and an employer sacking them (Chapter 8). Sequences B3 and B4 show this situation, including both the instantaneous effect of job strain on WLD (B3) and the lagged effect (B4) as described in Figure 13.

In the absence of selection effects (section 15.1.3), cross-sectional analyses will estimate only a subset of the total pathways – they will pick up Path A1 excluding Sequence B1 (i.e. instantaneous effects of strain on WLD that do not result in instantaneous job loss) and exclude Path A3 (the effect of strain on health over time). With cross-sectional data, it is also impossible to estimate the effect of job strain on job loss.

We must note too that WLD may fluctuate over time; a person who has experienced any of these sequences in the past may, at the time of interview, not report a WLD.

15.1.3. Towards a statistical model

Job strain is not randomly distributed in the population; people are selected into jobs based on a number of characteristics. Partly this reflects ***reverse causality***; health problems could lead people to take *higher-strain* jobs, if people have to take any job they can get; or health may change the way people report their working conditions. Alternatively, health problems could lead to taking *lower-strain* jobs if people look for (or are given) work that they find easier to do. While not all studies agree (De Lange et al 2004; Grebner et al 2005:35), a recent meta-analysis suggests that reverse causality is a real problem: mental strain leads to higher self-reported job demands to a greater extent than higher job demands lead to mental strain (Ford et al 2011:12). There are also ***non-health factors*** that influence people's level of job strain, including their age, gender, ability, education level and work attitudes.

To estimate the causal effect of job strain here, I need to exclude any selection effects that also influence WLD/job loss. Ideally I would do this by using an exogenous (i.e. genuinely random) source of variation in job strain, but these are not available in WII (although I recommend future research in this area in Chapter 9). Instead, I do two things. First, I use the longitudinal structure of the data to look at *changes* in the outcomes (changes in WLD and leaving work). I do *not* look at changes in strain, partly because such changes are arguably even *more* biased by the selection effects described above, particularly at the older ages in this sample (Ford et al 2011; see also Chapter 8). Analyses of change therefore require a fuller analysis of the predictors of levels and changes in job strain (Chapter 9).

Second, I control for as many factors as possible that *prima facie* seem likely to influence both job strain and the outcome variables, in particular numerous health and sociodemographic variables. My assumption is therefore that ***conditional on these observed covariates, there are no differences between people in high-strain and low-strain jobs that influence changes in WLD / health-related job loss / moving from work to incapacity.***

Aside from the plausibility of this assumption (to which I return in the Discussion), the major problem in this approach is that is often unclear whether a variable is a mediator or confounder of the relationship of strain to WLD (Kivimäki et al 2006:439) – or indeed, serves both roles. Christenfeld et al (2004:87) have noted that *"distinguishing a mediator from a confounder must often rely on common sense, intuition, background knowledge, or one's theoretical or ideological persuasion...One person's mediator may be another person's confounder."*

One area where this is particularly complex is for ***mental strain*** measured at the same time as job strain, with debate particularly focusing on negative affectivity (a person's disposition towards feeling negative emotional states). While negative affectivity affects perceived work stress and self-reported health (Judge et al 2000; Spector and O'Connell 1994, cited in D'Souza et al 2006), it is also likely to be a mediator (Karasek et al 1998:350; Spector et al 2000; Spector 2006:226) – in other words, removing Path A4 in Figure 13. Similar issues arise when considering whether to control for other measures of mental strain such as vitality and GHQ caseness.[49] In the main analyses in this chapter I control for general mental ill-health (including GHQ) but *not* specific measures of mental strain – although I test the impact of additionally including these in a sensitivity analysis (cf. Bosma et al 1997; Ferrie et al 2002).

A related issue is whether we should control for **baseline WLD** when looking at the impact of job strain$_t$ on WLD$_{t+1}$. In doing this, we exclude Path A1 in Figure 13 and Sequence B3 in Figure 14 – in other words, how job strain may *instantaneously* lead to WLD among people with pre-existing health problems, which *over time* culminates in job loss. In the main analyses in this chapter, I control for baseline WLD as this may influence job strain (or reports of it), or there may be some unobserved factors (e.g. personality) that influence both WLD and job strain reporting – but in a sensitivity analysis I test the impact of omitting this.

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49 (i) SF-36 vitality includes questions on e.g. feeling 'worn out' in the past four weeks; (ii) GHQ caseness includes questions on feeling 'constantly under strain', 'found everything getting on top of you', 'found at times you couldn't do anything because your nerves were too bad', and 'been satisfied with the way you've carried out your task'. In the case of GHQ caseness, though, the other 26 questions are measures of general mental health.

15.2. Statistical modelling

To estimate the effect of job strain on the composite measure of WLD, I use a logit regression model.[50] The models include the controls mentioned in Section 14.3 – for demographics, SES, physical health and mental health – which are all measured at the baseline wave alongside job strain.[51] For people who remain in the sample I have two observations for each person (the effect of strain at wave 3 on outcomes at wave 4, and the effect of strain at wave 5 on outcomes at wave 6), and I control for the non-independence of the two observed baseline waves within a single respondent by using cluster-robust standard errors. I use two specification tests to check the main model,[52] and the appendices further show pseudo-R^2 as a crude measure of goodness-of-fit (Long and Freese 2006:109).

15.2.1. Mediated effects

Aside from looking at these overall effects, I was also interested in whether any effect of job strain on WLD is mediated by the health or job requirements pathways. To relate this to the terminology used in much of the mediation literature:

- The *indirect effect* is the effect of job strain on WLD that works through health, i.e. the health pathway.
- The *direct effect* is the effect of job strain on WLD that does not work through health, i.e. the job requirements pathway.

The conventional way of looking at mediation is to compare the regression coefficients for the exposure: (i) without adjustments for potential mediators, which shows the overall effect; and (ii) with adjustments for the mediating variables, which shows the direct effect (a technique usually associated with Baron & Kenny (1986)). The difference between the two regression coefficients is an estimate of the indirect effect.

While this method is in widespread use, it suffers from two problems: it does not allow us to conduct inference (e.g. to test the significance levels) of the indirect effect (Shrout

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[50] This used the Stata's LOGIT command, which models the probability of reporting a WLD as:

$$\Pr\left(WLD_{i,t+1} = 1 \middle| \mathbf{x_{it}}\right) = \frac{e^{\mathbf{x}'\boldsymbol{\beta}}}{(1 + e^{\mathbf{x}'\boldsymbol{\beta}})} = \text{logit}(\mathbf{x}'\boldsymbol{\beta})$$

...where y_i refers to the outcome variable (the binary measure of WLD), while the vector $\mathbf{x}$ refers to the observed covariates (e.g. job strain) and the vector $\boldsymbol{\beta}$ refers to the unknown parameters to be estimated.

[51] The model is of the form:

$$\Pr\left(WLD_{i,t+1} = 1 \middle| \mathbf{x_{it}}\right) = \text{logit}(\boldsymbol{\beta_0} + \boldsymbol{\beta_1}\mathbf{STR_{it}} + \boldsymbol{\beta}_2\mathbf{DEM_{it}} + \boldsymbol{\beta_3}\mathbf{SES_{it}} + \boldsymbol{\beta_4}\mathbf{PH_{it}} + \boldsymbol{\beta_5}\mathbf{MH_{it}})$$

where $\mathbf{STR}$ refers to job strain, $\mathbf{DEM}$ refers to a vector of demographic characteristics, $\mathbf{SES}$ refers to a vector of socioeconomic status controls, $\mathbf{PH}$ refers to a vector of physical health controls, and $\mathbf{MH}$ refers to a vector of mental health controls.

[52] These were Pregibon's Link Test (Pregibon 1980; Cleves et al 2008:197) and the Hosmer-Lemeshow test (Long and Freese 2006:155). The main model (model 6 in Table 12) passed both tests comfortably (Hosmer-Lemeshow χ^2(8)=7.65, p=0.47; Pregibon z=-0.89, p=0.37).

and Bolger 2002; Hayes 2009), and it is invalid in the case of logit models, where the addition of new variables raises the coefficients of variables that remain in the model (Mood 2010).

To overcome these problems, I adjust the Baron and Kenny procedure to produce more meaningful results. I construct the two models as above,[53] but additionally:

1. Rather than using the *coefficients* from these models, I instead use *average marginal effects* (AMEs) – that is, using the predicted effect of job strain on the real-world prevalence of WLD, averaged across everyone in the sample.[54] Unlike logit coefficients, these are comparable across different models (Mood 2010).
2. Rather than seeing if job strain *'loses its significance'* when controlling for the mediators, I *bootstrap* the marginal estimate to get a confidence interval for each of the overall, direct and indirect effects. (Bootstrapping is an established way of calculating the standard error of an estimate).[55]

While such estimates seem to be an improvement on most mediation analyses in the literature, I check these against a related method that has previously been peer-reviewed in the statistical literature (Karlson and Holm 2011) in section 17.1.5.

Finally, it is important to note that mediation analyses make additional assumptions beyond the estimates of the overall effect of job strain, which I return to in the Discussion below.

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[53] The model including mediators is of the form:

$$\Pr\left(WLD_{i,t+1} = 1 \middle| \mathbf{x_{it}}, \mathbf{z_{i,t+1}}\right)$$
$$= \mathrm{logit}\left(\boldsymbol{\beta_1}\mathbf{STR_{it}} + \boldsymbol{\beta}_2\mathbf{DEM_{it}} + \boldsymbol{\beta_3}\mathbf{SES_{it}} + \boldsymbol{\beta_4}\mathbf{PH_{it}} + \boldsymbol{\beta_5}\mathbf{MH_{it}} + \boldsymbol{\beta_6}\mathbf{PH_{i,t+1}} + \mathbf{MH_{i,t+1}}\right)$$

where $\mathbf{z_{i,t+1}}$ refers to the vector of physical ($\boldsymbol{\beta_6}\mathbf{PH_{i,t+1}}$) and mental health ($\mathbf{MH_{i,t+1}}$) mediators.

[54] More formally (with thanks to Paul Clarke), in a simple random sample:

$$\hat{E}[Y|\mathbf{x}, STR = 1] - \hat{E}[Y|\mathbf{x}, STR = 0] = \frac{1}{n}\sum_{\mathrm{i=1}}^{\mathrm{n}}\{\mathrm{logit}(\mathbf{x_i}\boldsymbol{\beta} + \gamma STR) - \mathrm{logit}(\mathbf{x_i}\boldsymbol{\beta})\}$$

...where $\mathbf{x}$ is a vector of all the other covariates in the model, $\boldsymbol{\beta}$ a vector of their coefficients, and γ is the coefficient on STR. This can easily be extended to more complex samples using the weighted (rather than simple) sum of the difference in the predicted probabilities.

I here use average marginal effects (AME; see section 17.1.1), and this adds certain additional complications about defining the mediation effect (Hafeman and Schwartz 2009). I get around this by defining the direct and indirect effects in terms of (i) the AME of job strain without controlling for the mediators; to (ii) the AME of job strain holding the mediators $\mathbf{z}$ constant at the observed level, or more formally:

$$AME_{overall} = \left\{\hat{E}[Y|\mathbf{x}, STR = 1] \quad - \hat{E}[Y|\mathbf{x}, STR = 0]\right\}$$
$$AME_{direct} = \left\{\hat{E}[Y|\mathbf{z}, \mathbf{x}, STR = 1] - \hat{E}[Y|\mathbf{z}, \mathbf{x}, STR = 0]\right\}$$
$$AME_{indirect} = \left\{\hat{E}[Y|\boldsymbol{x}, STR = 1] \quad - \hat{E}[Y|\mathbf{x}, STR = 0]\right\} \quad - \quad \left\{\hat{E}[Y|\mathbf{z}, \mathbf{x}, STR = 1] - \hat{E}[Y|\mathbf{z}, \mathbf{x}, STR = 0]\right\}$$
$$= AME_{overall} - AME_{direct}$$

The overall effects therefore by definition represent the sum of the direct and indirect effects – which is an approximation to indirect effects specified in terms the counterfactual distribution of the mediators $\mathbf{z}$, and how this would change depending on the value of STR, i.e. $\left\{\hat{E}[Y|\boldsymbol{x}, STR, z_{str=1}] - \hat{E}[Y|\mathbf{x}, STR, z_{str=0}]\right\}$.

[55] I use non-parametric bootstrapping (Efron and Tibshirani 1993; Carpenter and Bithell 2000) based on the BOOTSTRAP command in Stata with 1,000 replications using bias-corrected estimates.

15.2.2. Sample selection and missing data

These models use a complete case analysis of respondents who were working at the initial wave and who were still younger than 60 by the time they were observed at the following wave. This led to a sample size of 10,010 person-wave observations (from 6,836 people). However, a number of observations are lost due to either incomplete data at the baseline wave or attrition between the baseline and follow-up waves, leaving only 8,140 person-wave observations (from 5,802 people) in the main analyses below.

Not only does this mean that some information is wasted, but it also creates the possibility of bias. I therefore conducted a multiple imputation sensitivity analysis, which imputes missing data based on the responses provided – and does this several times with random error, to reflect the uncertainty we have about the missing variables. The technical details of the multiple imputation are given in Appendix 3C, the results are provided in section 17.1.5, and the validity of the assumptions underlying it are discussed in section 18.1.

16. DESCRIPTIVE RESULTS

Descriptive results for WLD and job strain are presented in Table 9 and Table 11 respectively, based on the 8,140 person-wave main sample who have full information on the main variables used in the analysis,[56] are aged below 60 by the follow-up wave, and are working at the baseline wave. (Descriptive information on the control variables is available in Web Appendix 3a).

Table 9 shows that 10.2% of the sample have a WLD at the baseline wave – which simply reflects that this was the cut-off I chose from the latent WLD scale. The prevalence of the components of the WLD scale vary: some are more common (e.g.mh-do less), while others are less so (e.g. ph-limit type). All WLD variables show increases by the follow-up wave, reflecting the selected nature of the sample at baseline (workers) and deteriorations of health with age.

Table 9: Descriptive statistics for WLD

	Baseline prevalence	Follow-up prevalence
WLD: IRT composite measure	10.2%	16.1%
WLD: mh-cut time	8.6%	10.4%
WLD: mh-do less	17.5%	19.7%
WLD: mh-careless	10.6%	13.0%
WLD: ph-cut time	7.1%	10.9%
WLD: ph-do less	13.2%	18.5%
WLD: ph-limit type	6.9%	12.6%
WLD: ph-difficulty	9.5%	15.6%
WLD: pain (mild or above)	6.5%	9.5%

n=8140, see text for sample details.

For the rest of this chapter, 'WLD' will be taken to refer to the derived binary measure of WLD unless otherwise specified. The wave-to-wave transitions in WLD are shown in Table 10. Here we can see that most people report no WLD at either the baseline or follow-up waves (78.2%), but sizeable numbers both move into (11.6%) or out of (5.8%) WLD, or stay at WLD in both baseline and follow-up waves (4.4%).

It is perhaps surprising that among those reporting a WLD at the baseline wave, the majority do *not* report a WLD at the following wave. The extent to which this is seen for other measures of WLD that ask about 'longstanding' problems (rather than problems in the past four weeks, as asked here) is seen in Chapter 6. Even for reports of

[56] That is, the variables used in model 6 of Table 12, plus job satisfaction, social support and skill variety.

'longstanding illness' in WII though, we see that chronic conditions are not reported at every wave; for example, 43% of those with a major heart longstanding illness do not report it at the following wave, representing a combination of measurement error and genuine fluctuations.

Table 10: Wave-to-wave transitions in WLD

% of total sample

(% of baseline WLD category)

	WLD at follow-up	
Baseline WLD	No WLD	WLD
No WLD	78.2% (87.1%)	11.6% (12.9%)
WLD	5.8% (56.5%)	4.4% (43.5%)

n=8140, see text for sample details.

Table 11 shows how the tertiles of job demands-control compare to the continuous scale measures. The scales tend to be skewed such that only small number of people have the lowest levels of both demands and control; for example, the lowest tertile of demands covers people with scores of 0 to 50 on the continuous scale, but the mean score is 40, suggesting that few people have the lowest scores.

Table 11: Descriptive statistics for demands-control

		Continuous demands-control scores			
	n	Mean	Std Dev	Min	Max
Job demands tertile					
Low	2564	40.4	10.8	0	50
Medium	2680	62.8	4.2	55.6	66.7
High	2896	82.9	8	75	100
Job control tertile					
Low	2766	47.4	11.7	0	59.3
Medium	2747	68.8	4.1	62.5	75
High	2627	84.5	5.9	77.8	100

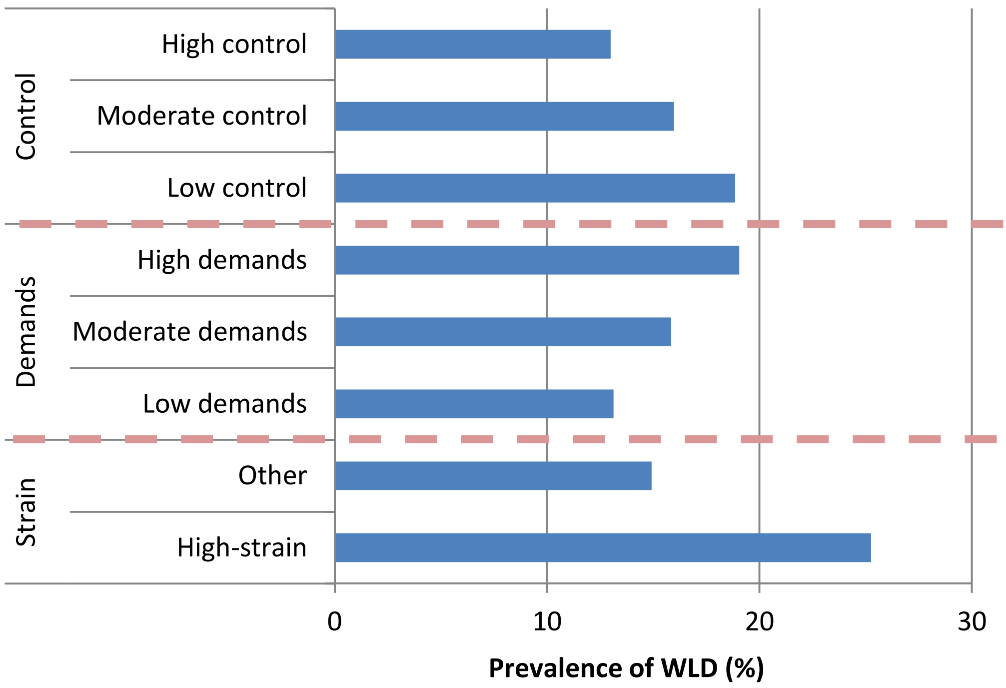
n=8140, see text for sample details.

Job strain – the combination of high demands and low decision authority – was reported by 11.0% of the sample. This was similar across Civil Service grades (9-12% in each of administrative (the highest grade), professional/executive and clerical/support grades). In contrast, the prevalence of jobs with the lowest control and lowest demands ('passive' jobs) was much higher in the lower grades (42% vs 3%), while high-control high-demands ('active') jobs were much more common in the higher grades (1% vs 22%) – a situation that is similar for the occupational gradient more broadly (Chapter 5).

16.1.1. Bivariate associations of job strain and WLD

From the literature review, I hypothesised that high demands and low control will be associated with higher levels of WLD at the following wave. A simple bivariate analysis supports this hypothesis, as shown in Figure 15. Higher demands and lower control are associated with greater levels of later WLD, and those initially in high-strain jobs have more than double the level of later WLD as those in low-strain jobs (25.4% vs. 14.9%).

Figure 15: Unadjusted association of baseline demands-control with WLD at follow-up



n=8140, see text for sample details.

The next section investigates whether this unadjusted relationship persists after adjustment for potential confounders.

17. REGRESSION RESULTS

The effects of job strain on WLD after controlling for different groups of confounders are shown in Table 12. These figures are odds ratios, where 1 indicates no effect, >1 indicates an increased risk of WLD, and <1 indicates a reduced risk. For example, in the empty model (model 1), the odds of reporting a WLD are 41% higher [(1.41 – 1)*100] for those reporting high vs. low demands, and 41% lower [(1/0.71 – 1)*100] for low vs. high control, both of which are significant at the 0.1% level.

In the empty model – which is similar to the bivariate analyses in Figure 15 – we can see that all the coefficients are significant in the predicted direction: high control lowers the odds of reporting a WLD, while high demands raises the risk. Moreover, the interaction between low control and high demands is significant, *beyond* the separate impacts of the terms for low demands and high control. (The coefficients for demands, control and strain are mutually adjusted in all models).

As we add the groups of control variables in Table 12, the impact of control is quickly attenuated and becomes non-significant. High demands is also attenuated by controls for physical health and particularly mental health, suggesting that people with poor mental health are particularly likely to report high demands and a later WLD. However, demands remains significant in the full model, and the effect of job strain remains significant and unchanged in size in all of the models.

Table 12: Regression of WLD on baseline job strain [odds ratios]

	[m1] Empty	[m2] Initial	[m3] m2 + SES	[m4] m2 + PH	[m5] m2 + MH	[m6] Full
Low control (not high demands): base						
Moderate control	0.90	1.03	1.12	1.07	1.11	1.17
High control	0.71***	0.88	1.01	0.94	0.99	1.11
Low demands: base						
Moderate demands	1.30***	1.27**	1.30**	1.23*	1.21*	1.21*
High demands (not low control)	1.41***	1.36**	1.44***	1.31**	1.21*	1.26*
Low control * high demands	1.42**	1.41**	1.42**	1.46**	1.38*	1.41*
N	*8140*	*8140*	*8140*	*8140*	*8140*	*8140*
Num persons	*5802*	*5802*	*5802*	*5802*	*5802*	*5802*
pseudo R-sq	*0.01*	*0.07*	*0.08*	*0.11*	*0.09*	*0.13*

*NOTES: Significance levels: † p<0.10; * p<0.05; ** p<0.01; *** p<0.001. Controls are:*
- Empty*: no controls*
- Initial*: baseline WLD, demographics (age, gender, marital status), wave, lag between waves*
- SES*: m2 + socioeconomic status (grade, money problems, housing problems, class of non-Civil Service job)*
- PH (physical health)*: m2 + physical health(SF-36 physical functioning, pain, diabetes, respiratory illness, and 14 categories of longstanding illness (LSI))*
- MH (mental health)*: m2 + mental health (GHQ caseness, SF-36 mental health, LSI depression, and LSI mental health (other))*
- Full*: all controls from the previous models.*

The coefficients on the other covariates in the full model (m6) are presented in Web Table 3.Covariates. This shows that the odds of WLD are higher for those with a baseline WLD, who are female, have housing problems, money problems, several types of longstanding illness (gastrointestinal, brain, heart(major/angina), respiratory, musculoskeletal), bodily pain, poor physical functioning, poor mental functioning or GHQ caseness.

17.1.1. Interpreting these results

Odds ratios are difficult to interpret – they are often interpreted as risk ratios, but this is an approximation.[57] Additionally, odds ratios are not comparable between models: as the pseudo-R^2 increases, the odds ratios will increase even if the strength of the relationship is unchanged (Hoetker 2007; Mood 2010). As a result, most of the rest of the results in this chapter are presented in terms of 'average marginal effects' (AME).[58]

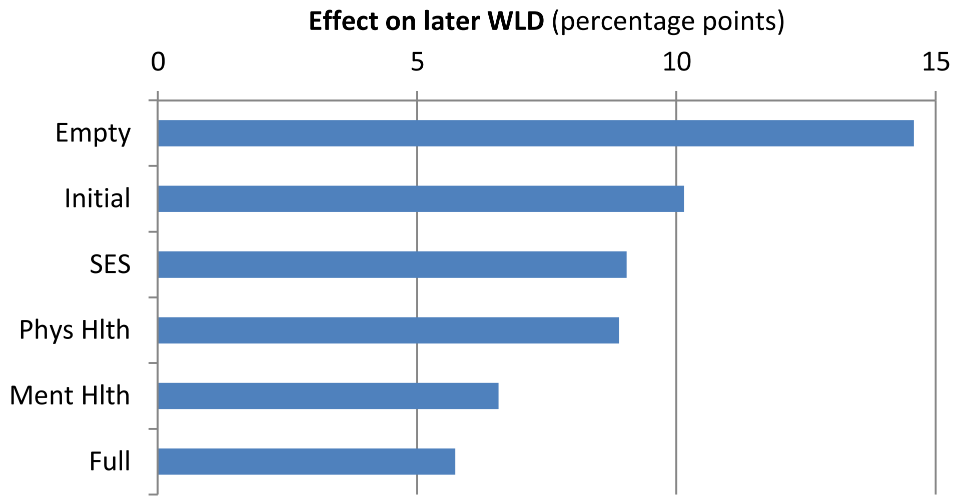
[57] Odds ratios are an approximation to relative risks for small probabilities – but as the probability of an event becomes more common, the gap between an odds ratio $\frac{p_1/(1-p_1)}{p_2/(1-p_2)}$ and a relative risk $\frac{p_1}{p_2}$ becomes larger (given that 1-p_1 tends to 0). Hence a relative risk of 1.25 at a base probability of 0.05 gives an almost identical odds ratio of 1.27, whereas at a base probability of 0.50 the risk ratio of 1.25 becomes an odds ratio of 1.67 – and at a base probability of 0.75 the odds ratio reaches 5. At the average prevalence of WLD in this sample (16%), a risk ratio of 1.25 becomes an odds ratio of 1.31. Both odds ratios and relative risks also present relative effects, while marginal probabilities present absolute percentage point effects.

[58] AME's are calculated using the MARGINS command in Stata 11.

These are the average changes in WLD if everyone in the sample moved from low-strain to high-strain jobs.

When doing this, it is simpler to do comparisons between low-strain and high-strain jobs (rather than looking at the separate sets of coefficients for demands, control, and their interaction).[59] The AMEs are shown in Figure 16, based on the models shown in Table 12. Figure 16 demonstrates clearly how the various controls – particularly demographics, baseline WLD, and mental health – attenuate the relationship between job strain and later WLD. Nevertheless, the size of the effect in the fully-adjusted model is large at 5.8% (95% CI 2.8-8.9%, p<0.001), giving an estimated prevalence of WLD of 19.9% for high-strain and 14.1% for low-strain jobs.

Figure 16: Estimated effect of baseline job strain on later WLD



All effects are statistically significant at the 0.1% level. Estimates are population-averaged marginal effects, based on the same model as the previous table. Sample average WLD at follow-up is 16.1%.

In this chapter I primarily present the results in terms of the contrast between high-strain and low-strain jobs, based on models that include tertiles of demands and control and an interaction of high demands*low control. These models fit with the previous literature, and allow me to investigate whether demands, control or their combination seem to be important. However, the results will be a slight underestimate of more straightforward comparisons of high-strain jobs, low-strain jobs and other jobs. A model that simply uses dummy variables for these categories finds estimated levels of WLD in high-strain jobs that are 6.1 (rather than 5.8) percentage points higher than in low-

[59] This contrast is a combination of the coefficients for low (vs. high) control, high (vs. low) demands, and the interaction low control*high demands.

strain jobs. The simplified model also estimates that people in high-strain jobs are 4.1 percentage points more likely to report a later WLD compared to people in all other jobs (excluding those in low-strain jobs).

Returning to the main research question, the main models largely bear out the original hypothesis that baseline job strain leads to later WLD, even after controlling for an extensive array of health and sociodemographic controls. The estimated real-world effect of this is moderately large.

17.1.2. ***Confounders and mediators***

While this effect is suggestive, it is important to check whether this simply reflects uncontrolled confounding for variables that are not included in the main models. To do this, I first checked the impact of controls that are likely to be confounders but which are only available for one of the two baseline waves: this includes education, council housing, car availability, low income, assets, and several additional health controls (see Appendix 3B for variable details, and Web Table 3.ControlsB for results).

While the subsamples for these analyses are different – and therefore, the results are different to those in Figure 16[60] – the impact on the strain effect was small. For example, following controls for education, council housing and car availability,[61] the effect of moving from low-strain to high-strain jobs was to raise WLD by 5.6%, compared to 5.3% without these controls; only housing tenure was significant itself. Given that none of these controls noticeably affected the findings or even consistently attenuated them, the main results do not seem to reflect confounding by SES.

Beyond this, however, there are a number of variables where it is uncertain if they are confounders or mediators (section 15): (i) variables included in the main models that might be over-controlling (baseline WLD, GHQ caseness and grade); (ii) other psychosocial work characteristics, including job satisfaction, job social support, and job variety/skill use (known as 'skill discretion'); (iii) negative affectivity; and (iv) self-reported health and vitality. These results are split into Figure 17 and Figure 18 (see Web Tables 3.controlsA and 3.controlsB), reflecting the different samples used in the analyses (due to data availability).

Looking first at Figure 17, we can see that the strength of the relationship between job strain and WLD is not much increased by excluding controls for baseline WLD, GHQ

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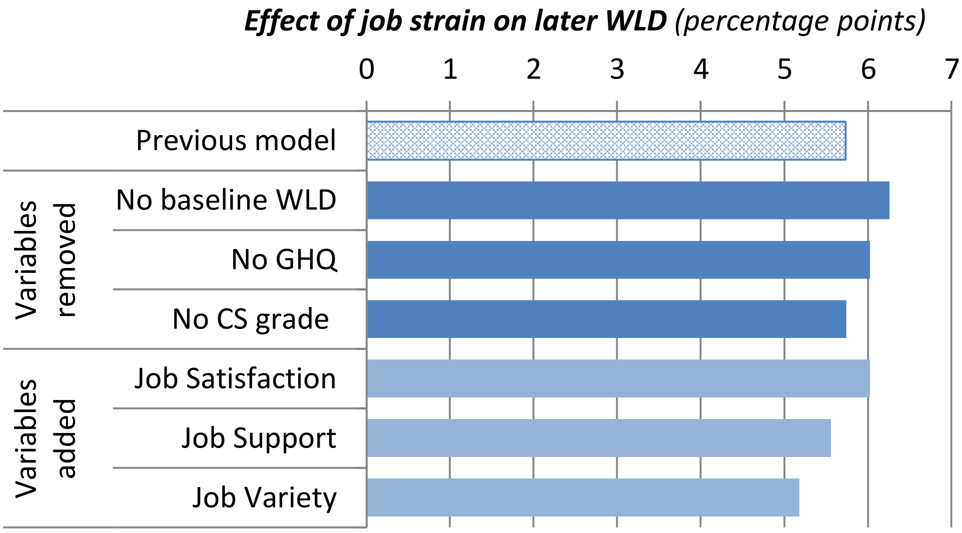
[60] For example, job strain was highly significant when looking at the wave 3 baseline, but smaller and non-significant when looking at the (lower-powered) wave 5 baseline. However, the difference between waves (jointly testing the coefficients for low vs high control, low vs high demands, and job strain) was not significant (chi2(3)=5.4, p=0.15).

[61] Education was only asked at wave 5, while council housing and car availability were only asked at wave 3. For this analysis, these were copied across to the other baseline wave (i.e. I controlled for baseline education at wave 3 as well as wave 5).

caseness or grade – but nor is it much *decreased* by additional controls for job satisfaction, job social support and job variety (none of which have a significant effect on WLD). Despite the theoretical controversy over whether these should be considered mediators or confounders, in this analysis this decision is rendered moot.

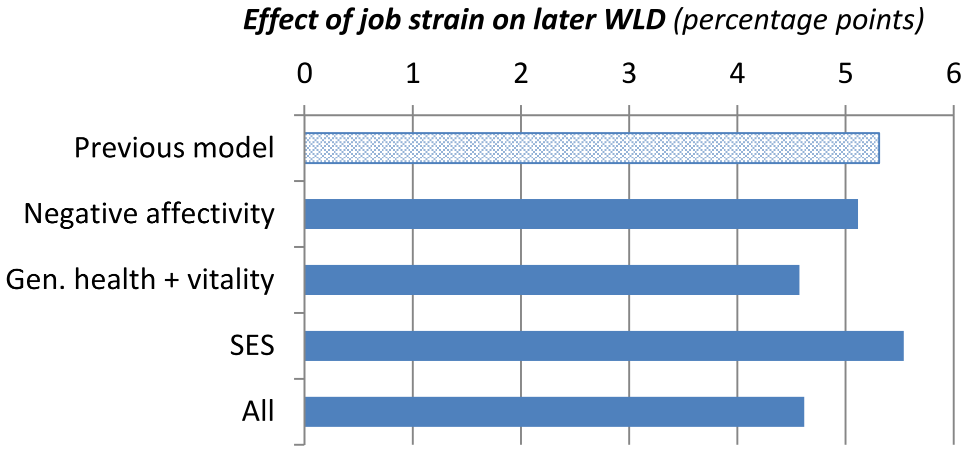
Figure 17: Effect of controlling for other psychosocial work characteristics



All models include the controls for m6 in Table 12 (except for the variables removed or added as specified), using the same sample. Effect of job strain in all models is significant at the 1% level.

Turning to Figure 18, we can see that the effect of job strain on WLD is slightly attenuated when adjusting for negative affectivity, education and particularly self-reported health and vitality (all of which were individually significant). Nevertheless, the main finding is that the relationship between job strain and WLD is strong and statistically significant even with their inclusion – despite the possibility that these are on a causal pathway between job strain and WLD.

Figure 18: Effect of controlling for other potential mediators/confounders



Sample is different to that in previous Tables/Figures (due to the timing of the questions on negative affectivity and education); n=6944, n(persons)=4678. All models include the controls for model 6 in Table 12. 'SES' models include education (at wave 5), council housing, car ownership, and number of children (all at wave 3), all copied across to the other baseline wave. Effect of job strain in all models is significant at the 1% level.

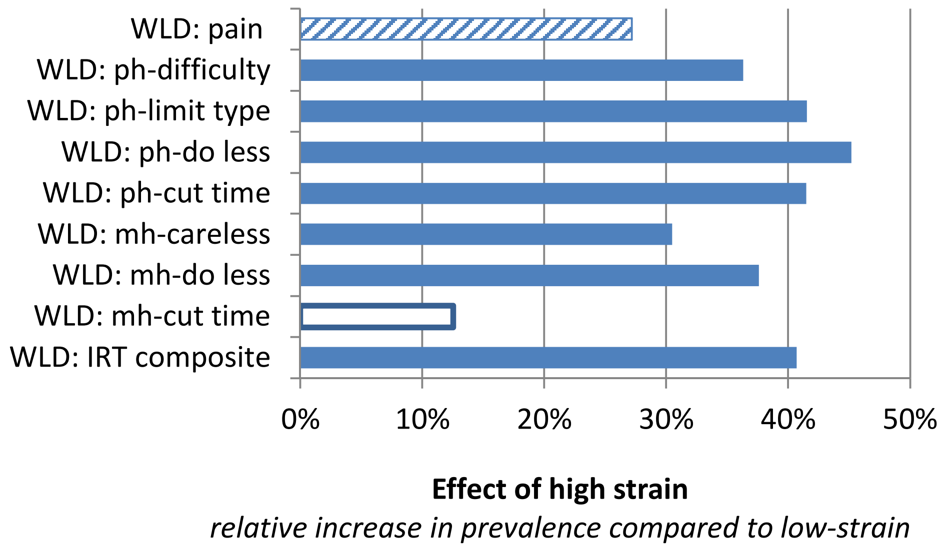
In conclusion, this section has shown that the relationship between job strain and WLD is highly robust to the inclusion of a range of additional controls – even those that are potentially on a causal pathway between job strain and WLD.

17.1.3. Specificity

A further test of whether the association of demands-control and WLD is due to unmeasured confounding is whether it is 'specific' (Bradford-Hill 1965; Macleod et al 2001); that is, whether other outcomes that are not expected to be related show similar effects, which would suggest generalised confounding by unmeasured factors. In this case, I would expect demands-control to influence mental and physical health to some extent (Chapter 1), but to have a greater effect on all aspects of WLD. I therefore conduct parallel analyses for the eight individual WLD questions covering different aspects of WLD, and for three other measures of health.

The eight different measures of WLD are described in section 14.2.2, and the results shown in Figure 19. This shows that job strain has a significant effect on seven of the eight measures of WLD, the only exception being cutting the amount of time at work or other regular daily activities due to emotional problems. Moreover, the effect sizes are similar across most measures, with high-strain jobs leading to levels of WLD that are 30-45% larger than in low-strain jobs. There was however some variation in whether job demands or the demands*control interaction was most important (see Web Table 3.Outcomes), although there was little sign of any effect for job control alone.

Figure 19: Effect of job strain on different measures of WLD



Solid bars significant at 5% level; diagonally shaded bar significant at 10% level; empty bars are non-significant. Models use the same sample and controls as model 6 in Table 12. Sample averages at follow-up are: 9.5% (pain), 10.9% (ph-difficulty), 18.5% (ph-limit type), 12.6% (ph-do less), 15.6% (ph-cut time), 10.4% (mh-careless), 19.7% (mh-do less), 13.0% (MH-cut time), and 16.1% (WLD IRT).

For the health measures that are less related to fitness-for-work, there were apparent effects of job strain but these were smaller in size than the effects on WLD. Those in high-strain jobs were slightly more likely to report poor health at the follow-up wave, but this effect was relatively small (a 10% reduction in 'excellent health' from 12.4 to 11.2 percentage points) and only marginally significant ($p<0.10$). For the continuous mental and physical functioning scales the power of the analyses was higher than when looking at binary WLD, so the effect of job strain was significant in both cases. Yet the sizes of these effects were small, with the difference between high- and low-strain jobs being only 0.1 standard deviations.

There is therefore some evidence that job strain impacts on fitness-for-work to a greater extent than health. The next section tests these pathways explicitly.

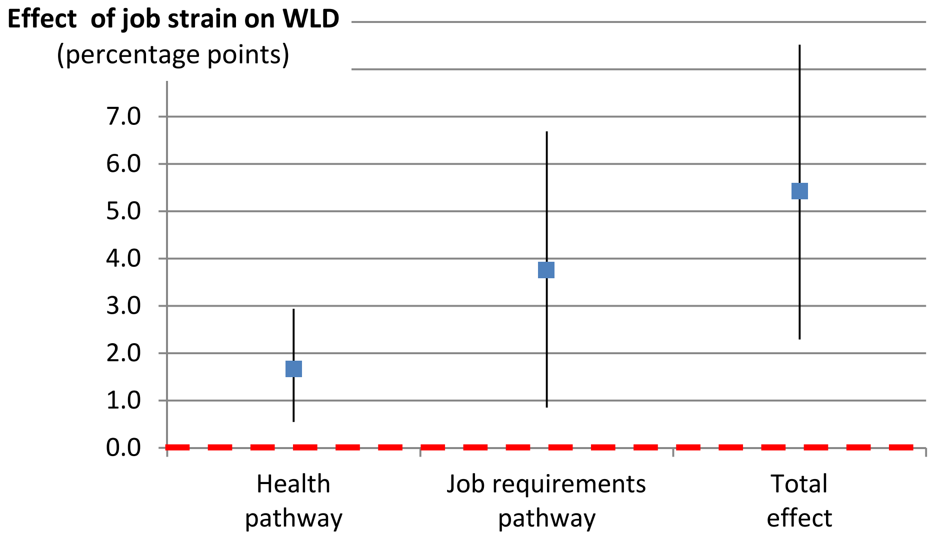
17.1.4. Pathways between job strain and WLD

As described in section 15, I hypothesised that there are two potential pathways between job strain and WLD. First, through the job requirements pathway, people could believe that some jobs are more difficult for them to do even at a constant level of health. Second, through the health pathway, jobs could affect people's health over time, which then affects WLD.

In this section, I investigate the relative strength of these two pathways using the mediation models described in section 15.2.1. Any relationship between baseline job strain and WLD at follow-up that is not explained by health at follow-up is assumed to be part of the job requirements pathway – an assumption that is considered in more detail in the Discussion below.

The results are shown in Figure 20. This shows that both the health pathway and the job requirements pathway are statistically significant – that is, the 95% confidence intervals exclude zero.

Figure 20: Pathways between job strain and WLD
Marker shows bias-corrected estimate; bar shows 95% confidence interval



Estimates are bias-corrected bootstrap estimates with 1000 replications. All models include the controls for model 6 in Table 12; the total effect is equivalent to that model on a slightly smaller sample (n=8157, n(persons)=5751) and using bootstrap rather than normal-based confidence intervals.

The mediators included in the health pathway are SF-36 physical functioning scale, diabetes, 14 categories of physical Longstanding Illness (LSI), SF-36 mental health scale, LSI Depression, and LSI Mental Health (other) at the follow-up wave. The job requirements pathway is the direct effect of job strain on WLD after controlling for the health pathway (see section 15.2.1 for details)

It is interesting to see that the job requirements pathway is around twice the size of the health pathway, at 3.8 vs 1.7 percentage points of WLD. This is estimated with considerable uncertainty and the difference is not significant at conventional levels – although the job requirements pathway seems to be larger than the health pathway, there is a reasonable possibility that this is due to chance. However, when the pathways models were repeated comparing high-strain jobs to all other jobs (rather than just low-strain jobs),[62] then the job requirements pathway was larger than the health pathway at the 10% significance level.

In conclusion, the pathways analysis not only shows that the job requirements pathway is statistically significant, but also that it may be larger than the health pathway – which is also suggested by the previous finding that the effect of strain on WLD is greater than on health outcomes (section 17.1.3).

[62] i.e. a single dummy term for job strain was used, excluding the four other dummy terms for moderate/high demands/control.

17.1.5. Methodological sensitivity analyses

A number of other sensitivity analyses were conducted to test the robustness of the findings to various methodological decisions, including different forms of demands, control and strain; transformed versions of continuous covariates; and multiple imputation for missing data. The findings above were robust to all of these checks, and the results are presented in Appendix 3D. This Appendix also gives information on an alternate method of binary mediation analysis; the job requirements pathway was once again significant, but unlike section 17.1.4 the health pathway was not.

As a further check, I tested whether the main effect was also found for different subgroups (men and women, older and younger workers). Stratified analyses are common but also result in spurious differences (Brookes et al 2001), so instead of stratifying, I tested the significance of interaction terms with group*(demands & control) and group*(covariates). No significant differences were found by gender, but there were marginally significant differences in the effect of strain by age (AME=2.9% for younger workers (39-50), 8.3% in older workers, p=0.08).

A final analysis looked at the cross-sectional association of job strain and WLD. This also found a strong effect of job strain (8.0% WLD in low-strain jobs vs. 11.6% high-strain, p=0.001), but unlike the main analyses found effects (p<0.10) of control and demands but not demands*control. I return to this finding in Chapter 7.

18. DISCUSSION

18.1. Limitations

There are four principal limitations of this study. *First*, the associations shown in regression models can only be given a causal interpretation if we accept the assumptions underpinning them; I consider this further in the following section. *Second*, this is a sample of middle-aged civil servants working in London rather than a nationally representative sample, and while the underlying processes are likely to be similar, the exact results are unlikely to be generalisable (Chapter 8).

Third, the measure of WLD is not ideal, as described in section 14.2.2. In particular, the questions talk about health affecting 'work or other regular daily activities' rather than just work. While these sub-scales of the SF-36 capture something different to the questions on health per se (Keller et al 1998), there might still be differences in reporting between workers and non-workers: workers are likely to focus on the job-related part of this question, but non-workers may report limitations that would not interfere with work (Wardle et al 2007), as I explore in Chapter 8. Differences in interpretation are also likely due to the wording of several questions on limitations in the past four weeks, which are difficult to respond to affirmatively for those who are out-of-work (see footnote 47 and Chapter 8).

While the restriction of the question to the past four weeks is likely to depress the reporting of WLD by non-workers, the consideration of 'other daily activities' may increase it – to which we must add the incentives to justify worklessness described in Chapter 1. However, the relationship between job strain and later WLD is found even when the sample is restricted to those working at the follow-up wave.[63] This suggests that employment status-related biases in WLD can be ignored for this chapter.

Fourth, multiple imputation can only take account of attrition *if we can assume that the drop-out is missing at random after taking into account a person's known characteristics* (Carpenter and Kenward 2008; Rose and Fraser 2008). In practice, it is likely that this assumption will not be met; people who have left the Civil Service or whose health has deteriorated may be more likely to drop out of the study, even after taking account of their original health, SES, working conditions etc (as suggested by Ferrie et al 2009). Nevertheless, it is usually argued that the extent of bias after taking account of known characteristics will be small; to that extent, it is reassuring that the missing data analysis and the main analysis show very similar results.

[63] The effect of job strain on WLD is estimated at 6.3% among workers, rather than the 5.8% in the full model of Table 12 (see Web Table 3.ControlsA).

At the same time, this study has a number of strengths. The WII cohort is ideal for examining the relationship between job strain and health-related outcomes, given the large amount of data on both working conditions and health. I include a large number of covariates (all included on theoretical grounds), particularly compared to most previous studies of job strain. I carefully consider the impact of potential mediators/confounders, running many additional models to examine their impact. And I look at potential pathways between job strain and WLD, using robust methods of binary mediation analysis.

18.2. Main findings

There are three findings from this chapter that merit further discussion:

1. High-strain jobs at a baseline wave lead to a higher risk of reporting a WLD at the following wave.
2. While the overall contrast between high- and low-strain jobs is significant, other predictions from the Karasek model are not borne out – particularly the absence of the predicted effect of low job control.
3. The effect of job strain is primarily due to a health-independent 'job requirements' pathway.

18.2.1. Finding 1: Job strain and WLD

Compared to being in a low-strain job, those in high-strain jobs at the baseline wave were more likely to report a WLD at the following wave – a finding that was significant at the 0.1% level after controlling for an extensive array of health and socio-demographic variables. This effect was moderately large, with 14.4% of those in low-strain jobs predicted to report a WLD, compared to 19.9% in high-strain jobs.

Estimating the effect of job strain is difficult due to the number of factors that are potentially both mediators and confounders of the relationship (see section 15). Yet the impact of these factors was generally minimal; strong relationships between job strain and WLD were found even after adjusting for negative affectivity, self-reported general health, vitality, job satisfaction, job social support and job skill discretion. The relationships were also unchanged after adjusting for a range of further health and sociodemographic confounders only available at certain waves.

18.2.2. Finding 2: bearing out the Karasek model?

While the overall hypothesis of the Karasek model was supported (on job strain), the specific predictions for job control were not. While job control had a significant effect on WLD in the empty model, this lost significance as soon as I controlled for baseline

WLD, and became even smaller in size after adjusting for SES. Furthermore, it was not significant when I omitted job grade or baseline WLD from the full model, or when job demands was removed from the model, or for any of the eight individual WLD questions. The analyses here therefore show little support for any relationship between job control and WLD – although the *interaction* of low control*high demands is often significant, and control was significant in the cross-sectional analysis (where the demands*control interaction did not have an effect).

The most likely explanation for the unimportance of control in the absence of high demands is that the estimated size of the job control effect in meta-analyses is small (odds ratios of around 1.2), there is statistically significant heterogeneity between studies (Kivimäki et al 2006:437), and some recent systematic reviews suggest no effect of job control (Eller et al 2009). Previous analyses of WII data have similarly found inconsistent results (on general health in Kivimaki et al 2004) (and the full SF36 scale in Stansfeld et al 1998). The degree of divergence from the prior literature should not be over-stated: in the full model in Table 12, the limit of the 95% confidence interval is that high control lowers the odds of WLD by 13%.

18.2.3. Finding 3: the health versus job requirements pathways

There are two hypothesised pathways between job strain and WLD (section 15): job strain could increase ill-health which in turn increases WLD (the 'health pathway'), or strain could increase the reporting of WLD even among people with a constant level of health (the 'job requirements pathway'). While the job requirements pathway is common-sense to occupational health practitioners (Chapter 1), to my knowledge, no previous studies have explicitly investigated the size of these two pathways; the study that comes closest suggests that relatively little of the effect of demands-control on work ability is mediated through health (Alavinia et al 2007).

In this chapter, the job requirements pathway was not only significant (as hypothesised) but was also considerably larger than the health pathway. The health pathway itself is unclear; it was significant in the main analyses but not significant in the sensitivity analyses. The specificity analyses however suggested that the effects of job strain on WLD were greater than the effects on health itself.

To understand these results, though, we need to consider a number of caveats. The relative size of these two pathways will vary as we look at longer lags between job strain and WLD; Eller et al (2009) suggest a 10 year gap is necessary for plausible health effects of workplace psychosocial factors on ischaemic heart disease (Eller et al 2009). Moreover, the way in which I attempt to separate out these two pathways depends on three critical assumptions. First, mediation analyses assume that the people for whom job strain affects health are the same people for whom health affects

WLD (Gellman and Hill 2007:191); I discuss this at greater length in the following chapter. Second, I can only interpret the residual effect of job strain after controlling for health as a 'job requirements pathway' if I fully control for health at follow-up. To the extent that job strain affects health in ways that I do not capture, I will underestimate the size of the health pathway.

Finally, I assume there are no other causal pathways between strain and WLD, such that the non-health pathway can be interpreted as 'job requirements'. My view is that other pathways are unlikely – particularly given the minimal effect of controlling for job satisfaction and social support – but they cannot be ruled out, and the reader must come to their own judgement as to the strength of inferences around the job requirements pathway.

18.3. A causal relationship?

To interpret these associations as causal effects, we have to assume (section 15.1.3) that, conditional on the observed covariates, the only relevant differences between people in high- and low-strain jobs are their psychosocial working conditions. We therefore need to consider whether the apparent effects are the result of omitted or poorly measured confounders (Christenfeld et al 2004; Fewell et al 2007). There are six main sets of possible confounders:

- **Health**: reverse causality was explicitly mentioned in section 15.1.3, but seems highly unlikely in this study. While some of the health measures used here are imperfect (GHQ in Stansfeld and Marmot 1992; self-reported conditions in Johnston et al 2007), the large number of health covariates (and minimal effect of additional covariates) makes residual confounding unlikely.
- **Depression and negative affectivity**: there is an ongoing debate as to whether self-reported job strain reflects working conditions or the reporting tendency of individuals. I here control for four measures of baseline mental ill-health and, in a sensitivity analysis, for negative affectivity – and while the estimated effect of demands is reduced, demands is still significant and the effect of the demands*control interaction is unchanged.
- **Sociodemographic characteristics**: in various analyses I control for last Civil Service grade, class of non-Civil Service job, age, gender, education, money problems and housing problems, and still find an effect of strain on WLD. It is possible that early-life factors influence people's selection into different jobs. However, several studies have investigated this, and – where they have already adequately controlled for later-life factors – tend to find minimal changes on the

effect of job strain (Brunner et al 2004; Hintsa et al 2008; Stansfeld et al 2008a; Hintsa et al 2010).

- **Health risk factors:** may be greater in high-strain jobs, but any effect on WLD will be restricted to the (small) health pathway. Moreover, conditional on other characteristics, health risk factors may be a mediator rather than a confounder of the relationship (Christenfeld et al 2004; Siegrist and Rödel 2006).
- **Work commitment:** people in high-demand jobs may be more committed than those in other jobs – but if this was a confounder, then we would expect that controlling for job satisfaction would attenuate the effect of strain, and this is not the case.
- **Work-life conflict** may be worse in high-strain jobs. However, neither marital status nor number of children has any significant effect on WLD, and this is true even when stratifying the sample by gender (results not shown).
- **Coping:** workers with high coping abilities may not only perceive their job demands as lower but also enact (Daniels 2006:280-1) or 'craft' (Wrzesniewski and Dutton 2001:185; Grant 2009:33) their job in a less-demanding way (see also Karasek and Theorell 1992:99 and Chapter 8). Coping is controlled for to the extent that it is associated with observed covariates (negative affectivity, health), but coping is difficult to measure directly and there is a possibility of residual confounding.

Beyond this, the present study has several strengths that make a causal relationship more plausible. The longitudinal data allow us to confirm that job strain predicts *changes* in WLD. Compared to much of the existing literature, I use a large number of control variables. I subject the analyses to a number of sensitivity analyses covering everything from missing data to functional form. And the specificity of the effect is strong: job strain predicts 7 of 8 WLD measures to a greater degree than 3 general health measures.

Nevertheless, uncontrolled confounding is still always possible in observational research, and it is difficult to control for coping abilities. I therefore interpret my results as suggestive *but certainly not definitive* evidence of a causal effect. In Chapter 6 I therefore replicate this analysis using a more 'objective' measure of demands-control that should be robust to individual differences in coping, and Chapter 9 recommends further research to provide a greater surety of causal inference.

18.4. Conclusion

In this chapter, I considered whether job strain affects self-reported fitness-for-work – an important step connecting the rises in job strain considered in Chapter 2, and the rises in WLD and incapacity benefit receipt shown in Chapter 1. The results are consistent: people in high-strain jobs are more likely to report a work-limiting disability 2-3 years later.

This effect was moderately large, with 14.4% of those in low-strain jobs predicted to report a WLD, compared to 19.9% in high-strain jobs. The effect is also robust to a very large number of controls and specifications; in nearly every case I find a significant effect of job strain. While less certain, it also seems that this effect is *not* simply because job strain is harmful for people's health; for any given level of health, being in a high-strain job made respondents more likely to report a WLD, which I interpret as evidence for a 'job requirements' effect of job strain on fitness-for-work.

If job strain affects WLD, then it seems likely to also influence health-related job loss. The next chapter tests this explicitly using WII, and if there is an effect, considers how far this is explained by the effect of job strain on WLD.

19. APPENDICES

This chapter has four Appendices (which appear at the end of the thesis), one Web Appendix, and several Web Tables (available from www.benbaumberg.com/thesis.htm):

- **Appendix 3A: Construction of WLD scale** – how Item Response Theory models were used to create these scales.
- **Appendix 3B: Description of control variables** – details of the each covariate included in the models (including how these were grouped, and how they were derived from the supplied data).
- **Appendix 3C: Multiple Imputation** – details of the multiple imputation by chained equations (ICE) analysis.
- **Appendix 3D: Results of sensitivity analyses** – summaries of the results of each sensitivity analysis referred to in section 17.1.5.
- **Web Appendix 3a: Descriptive statistics for control variables.**

CHAPTER 4:
The effect of job strain on health-related job loss in the Whitehall II study

20. INTRODUCTION

The previous chapter used WII to show that people in high-strain jobs are more likely to report a WLD two-to-three years later compared to those in low strain jobs. In this chapter, I develop this analysis to investigate whether job strain increases the chances of health-related job loss and long-term sickness – and the extent to which this occurs through WLD. Unfortunately WII contains no measures of incapacity benefit receipt itself, but health-related job loss is an intervening step on the hypothesised causal chain outlined in Chapter 1.

The chapter is structured similarly to the analysis of WLD. First, I describe how health-related job loss was operationalised. Second, I recap the theoretical model, and present the statistical models used to test it. Third, I present the analysis of the relationship of strain with health-related job loss. Fourth, I look at whether these relationships are mediated by WLD. Fifth, I discuss the robustness of these results to various sensitivity analyses. Finally, I discuss the limitations of the study, and consider how to interpret the findings as a whole.

21. DATA

The WII data are described in the previous chapter, including details of how job strain and WLD were operationalised, together with descriptive statistics of all variables. This section adds a description of the measures of health-related job loss used in this chapter (as used in Virtanen et al 2010) and other outcomes used in sensitivity analyses.

21.1.1. Health retirement (and other early retirements)

The main measure of health-related job loss available in WII is ‘health retirement’ – a measure that is derived in waves 4-6 from respondents’ descriptions of their retirement pathway as *‘on health grounds’* (Box 4.1). This was unavailable at wave 3, when respondents were instead asked to describe their ‘reason for stopping work’ in their own words. This was coded by the Whitehall II team into six different categories, of which one was *‘retired on health grounds’*. Respondents also gave the year in which they stopped work, which allowed me to construct measures of health retirement by wave 2.

In sensitivity analyses, I also look at early retirements (excluding health retirements) and long-term sickness absence. Early retirement is available at waves 4-6 and includes *'voluntary early retirement'*, *'voluntary compulsory redundancy'* and *'redundancy'*. Sickness absence is counted in two periods (waves 1-3 and 3-5); I restrict the analysis to sickness absence >7 days requiring medical certification, and look at both absence per se and the number of days of absence per year in the Civil Service.

Although the WII retirement data has been used elsewhere (Hyde et al 2004; Jokela et al 2010), we must note that:

- Ill-health retirements overlap with early retirements in the civil service. This is because the definition of a retirement as an 'ill-health retirement' or 'early retirement' partly reflects the outcome of a struggle between the Department and the Treasury.
- Women are much more likely to be health-retired than men. This may be because they have lower pension entitlements, which makes it harder for them to take early retirement.
- The data refer to retirement *from the Civil Service.* As a result, relatively high minorities of 'retired' respondents are still working, but outside of the Civil Service.[64] Health retirement is here used as an indicator of health-related job loss, not of non-work due to incapacity (for which 'long-term sickness' is used; see below).

Despite this, previous analyses have shown that people taking health retirement have substantially worse health than those taking early retirement (Jokela et al 2010) – and there is therefore a meaningful distinction to be made between these two forms of retirement.

[64] For example, at wave five, 741 of the 2083 who report taking health/early retirement were still working but outside of the Civil Service, as were 133 of the 941 people who report retiring at the normal age.

Box 4.1: Retirement pathway in WII

Waves 4-6

"By which route did you leave the civil service? (Please mark one box only)."

- "Retirement on health grounds"
- "Retirement at 60"
- "Voluntary early retirement"
- "Voluntary compulsory redundancy"
- "Redundancy"
- "Transfer to company through privatisation"
- "Left to take a post outside the civil service"
- "Left to become self-employed"
- (Wave 5 only): "Transfer to an NDPB" (Non-Departmental Public Body)
- "Other (please specify)"

Wave 3

"If you have left the civil service; please give your last civil service grade title and your leaving date. Please also state if you are working elsewhere, your current occupation and industry."

[Spaces are supplied for leaving date, last grade, current occupation, and industry].

"If retired, please give your last civil service grade title and your leaving date. Please also state your retirement date and reasons for stopping work."

[Spaces are supplied for retirement date, last grade title, and reason for stopping work. This was recoded by the WII team into:

- *"Retired early due to ill health"*
- *"Retired early to take up another job"*
- *"Retired early due to work circumstances" (inc. those retiring >6mths before 60)*
- *"Retired at normal retirement age" (inc. those retiring <6mths before 60)*
- *"Sacked/"retired" on inefficiency grounds"*
- *"Under 50 and state "redundancy""].*

21.1.2. Long-term sickness (and non-employment)

At waves 4-6, individuals reported their employment status in a standard form (Box 4.2), with long-term sickness given as one of the response options. At wave 3, however, respondents were not asked directly about their employment status, instead being asked if they were working and their reason for stopping work (if they have left the civil service).

Wave 3 employment status is needed to exclude non-workers from the models, and has been inferred from the data.[65]

[65] From the questions in Box 4.1:

- ...people were classified as civil service employees if they took part in wave 3 and did not say that they had left the civil service or retired.
- ...people were classified as non-civil service employees if they gave a date for having left the civil service or retired, and simultaneously responded to the job demands/control questions at wave 3.
- ...people were classified as non-employed if they responded to the survey, said they had left the civil service or retired, and were missing data on job demands/control.

Box 4.2: Employment status in WII

Waves 4 and 6
"Are you currently in paid employment?" [Yes/No; a note instructs the respondent to move to a different question if they answer 'yes'. The following question is then:]

"If you are not currently in paid employment, would you classify yourself as:" [Unemployed – Retired – Long term sick – Other (please specify. Respondents were instructed to only tick one box]

Wave 5
"Are you in paid employment at present?" [Yes/No; 'if you are in paid employment please go to Question 1.5. If you are NOT in paid employment at present:]

"How would you classify yourself? (Please mark one box only)" [Unemployed – Retired – Long-term sick – Housewife/husband – Student – Other (please specify)]

22. METHODOLOGY

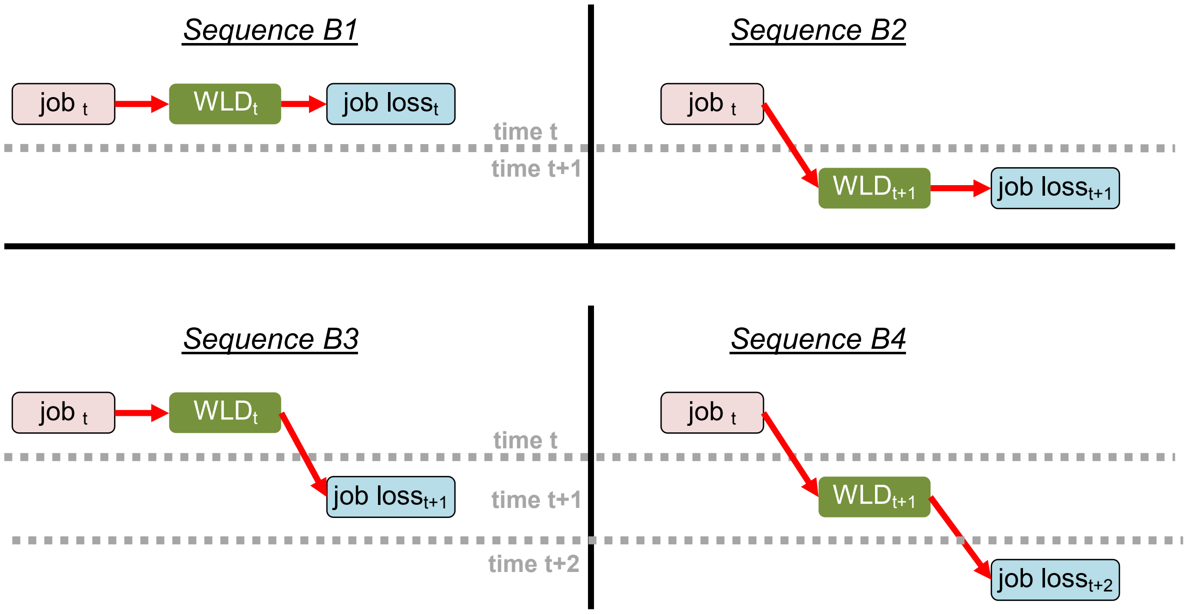
22.1. Theoretical model

The previous chapter described a number of potential paths between job strain, WLD and health-related job loss, which were grouped into a ***health pathway*** (strain leads to worse health) and a ***job requirements*** pathway (strain leads to reduced fitness-for-work at any given level of health). Aside from these WLD pathways, strain may also lead to job loss for other reasons (e.g. lower job satisfaction), which I term the ***'job effects pathway'***.

In general the same considerations apply in this chapter as the previous one. One additional issue is that when looking at how far WLD mediates the effect of strain on job loss, we can look at a number of pathways (seen in Figure 21 overleaf). Sequence B1 is not observed. In the main mediation analysis, I look at the combined effect of Sequences B2 and B3 (i.e. how far WLD_t and WLD_{t+1} jointly mediate the relationship). However, different sequences are subject to different biases – Sequence B2 will be biased by the effect of employment status on WLD reporting (Chapter 1), while Sequence B3 does not enable us to look at *changes* in WLD between waves.

In section 24.1.4, I therefore further look at Sequences B2, B3 and B4 separately. I restrict Sequence B4 to respondents who are in work at time t+1 (to avoid biased WLD reporting), and this therefore does *not* encompass the job losses in Sequences B2; the sample also changes to exclude those who turn 60 or drop out by t+2. While Sequence B2 may well be an overestimate due to reporting biases in WLD, it is also true that Sequences B3 and B4 only estimate part of the full pathway and may therefore be underestimates (Chapter 3).

Figure 21: Temporal sequences from strain->WLD->job loss
[This is identical to Figure 14, and is repeated here for convenience]



22.2. Statistical modelling

The statistical models here are similar to the previous chapter. To estimate the effect of job strain on health-related job loss, I use a logit regression model. Again, the longitudinal nature of the data enables me to look at *changes* over time – in this case, from people being in-work at the baseline wave, to their employment status at the follow-up wave 2-3 years later.

The models include the controls mentioned in the previous chapter – for demographics, SES, physical health and mental health – which are all measured at the baseline wave alongside job strain.[66] Unlike the analysis of job strain and WLD, I had to deal with certain computational problems due to the rarity of long-term sickness and health retirement (see section 23).[67] As before, I also control for the non-independence of the

[66] The model is of the form:

$$\Pr\left(job\ loss_{i,t+1} = 1 \middle| \mathbf{x}_{\mathbf{it}}\right) = \mathrm{logit}(\boldsymbol{\beta}_1\mathbf{JOB}_{\mathbf{it}} + \boldsymbol{\beta}_2\mathbf{DEM}_{\mathbf{it}} + \boldsymbol{\beta}_3\mathbf{SES}_{\mathbf{it}} + \boldsymbol{\beta}_4\mathbf{PH}_{\mathbf{it}} + \boldsymbol{\beta}_5\mathbf{MH}_{\mathbf{it}})$$

where $\mathbf{JOB}$ refers to job strain, $\mathbf{DEM}$ refers to a vector of demographic characteristics, $\mathbf{SES}$ refers to a vector of socioeconomic status controls, $\mathbf{PH}$ refers to a vector of physical health controls, and $\mathbf{MH}$ refers to a vector of mental health controls.

[67] This causes a problem of 'quasi-complete separation'; that is, one of the levels of a dummy variable completely determines whether a person is long-term sick. This tended to occur due to the rarity of both the dummy variable and the outcome rather than due to a substantive relationship – for example, none of the 27 people with a longstanding infectious disease became long-term sick between waves. The conventional response is to drop these 27 individuals from the analysis (Long and Freese 2006), but given that the association seems to be spurious rather than genuine, I merged categories together to ensure that separation did not occur. I also checked that none of these cases of separation represented statistically

two baseline waves using cluster-robust standard errors, use two specification tests,[68] and show pseudo R^2 as a crude measure of goodness-of-fit.

22.2.1. Mediated effects

In this chapter I was particularly interested in whether any effect of job strain on health-related job loss is mediated by WLD. I used the same method of investigating mediation as in the previous chapter, comparing the models both with and without WLD in terms of their average marginal effects. These estimates were then bootstrapped to create confidence intervals. As described in section 22.1, various different specifications of the mediation model are used.[69]

These mediation analyses make additional assumptions that are discussed at length in section 25.2.4.

22.2.2. Sample selection and missing data

The models for long-term sickness use a complete case analysis of respondents who were working at the initial wave and who were still younger than 60 at the following wave. As before, I conduct a multiple imputation analysis to test if the complete case results are biased by missing data.

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significant relationships by using a different – and much slower – form of logit estimation called 'penalized maximum likelihood estimation' (Heinze and Schemper 2002; Zorn 2005), which confirmed that separation did not reflect genuine associations.

[68] While the models for long-term sickness passed both tests (Hosmer-Lemeshow $\chi^2(8)=3.67$, p=0.89; Pregibon z=-0.13, p=0.90), the main health retirement model failed the link test (Pregibon z=-2.2, p=0.03) indicating likely model mis-specification.

To see if this mis-specification produced biased results, I re-ran the health retirement model including a nonlinear transformation of a latent general health index. (The health index was created by regressing the full list of health controls on self-reported general health using an ordinal logit model, and then estimating the latent level of general health for each respondent). Multivariable fractional polynomial modelling (Royston and Sauerbrei 2008) suggested that a cubic form of this latent health index best-fitted the health retirement model, when included alongside the full list of other controls.

With this additional cubed health index term, the health retirement model now passed the link test (z=-0.9, p=0.36). Comparing this better-fitting model with the full model reported below, the results are very similar. The lack of fit of the main models therefore does not bias the results presented in the main body of the chapter.

[69] The model including mediators is of the form:

$$\Pr\left(job\ loss_{i,t+1} = 1 \middle| \mathbf{x_{it}}, \mathbf{z_i}\right) = \mathrm{logit}(\boldsymbol{\beta_1}\mathbf{JOB_{it}} + \boldsymbol{\beta_2}\mathbf{DEM_{it}} + \boldsymbol{\beta_3}\mathbf{SES_{it}} + \boldsymbol{\beta_4}\mathbf{PH_{it}} + \boldsymbol{\beta_5}\mathbf{MH_{it}} + \boldsymbol{\beta_6}\mathbf{z_i})$$

where $\mathbf{z_i}$ variously refers to either $WLD_{i,t}$ (sequence B3), $WLD_{i,t+1}$ (sequence B2), or both $WLD_{i,t}$ and $WLD_{i,t+1}$ (combining both paths). Finally, I investigated temporal order B4 by using exactly the same models but looking at the outcome $job\ loss_{i,t+2}$

23. DESCRIPTIVE RESULTS

Descriptive results for health retirement and long-term sickness are presented in Table 13. The sample size for long-term sickness is larger than for WLD due to item-missingness in WLD at follow-up. The sample for health retirement is however smaller as it is restricted to those who are still working in the Civil Service at baseline. (Other descriptive information is available in the previous chapter).

Table 13: Incidence of job loss by follow-up among those working at baseline

	% job loss	n job losses	Total sample
Main outcomes			
Health retirement	1.9%	152	8005
Long-term sickness	0.7%	63	8935
For sensitivity analyses only			
Early retirement	13.0%	1039	8005
Non-employment	10.2%	835	8935

See text above for sample details.

Table 13 shows that both the prevalence of workers taking health retirement or becoming long-term sick between waves is low – only 1.9% for health retirement, and 0.7% for long-term sickness. This leads to small numbers of observations for the analyses below (152 health retirements and 63 long-term sicknesses), which cannot be much increased by combining the outcomes.[70] This makes the regression models relatively low-powered; that is, there is a greater chance of *not* finding a statistically significant relationship even though a relationship really exists (a 'Type II error').

In sensitivity analyses, I increase the number of health retirements in two ways. First, I use additional baseline data from waves 1 and 2 – waves in which respondents were asked about demands-control but not WLD. This raises the number of health retirements to 300 (1.4% of the larger sample). Second, using this larger sample, I look at health retirements over a longer period (two waves rather than one),[71] which raises the number of health retirements to 584 (2.8% of the larger sample). These analyses use fewer controls and cannot look at the mediating effect of WLD, but they do provide more power for looking at the overall effect of strain on health retirement.

[70] Combining the two outcomes (cf. Virtanen et al 2010:462) would only increase the heath retirement sample from 152 to 163.

[71] Given that I only observe working conditions at waves 1, 2, 3 and 5, this adds those health retirements that occur between waves 4 & 5 and 6 & 7.

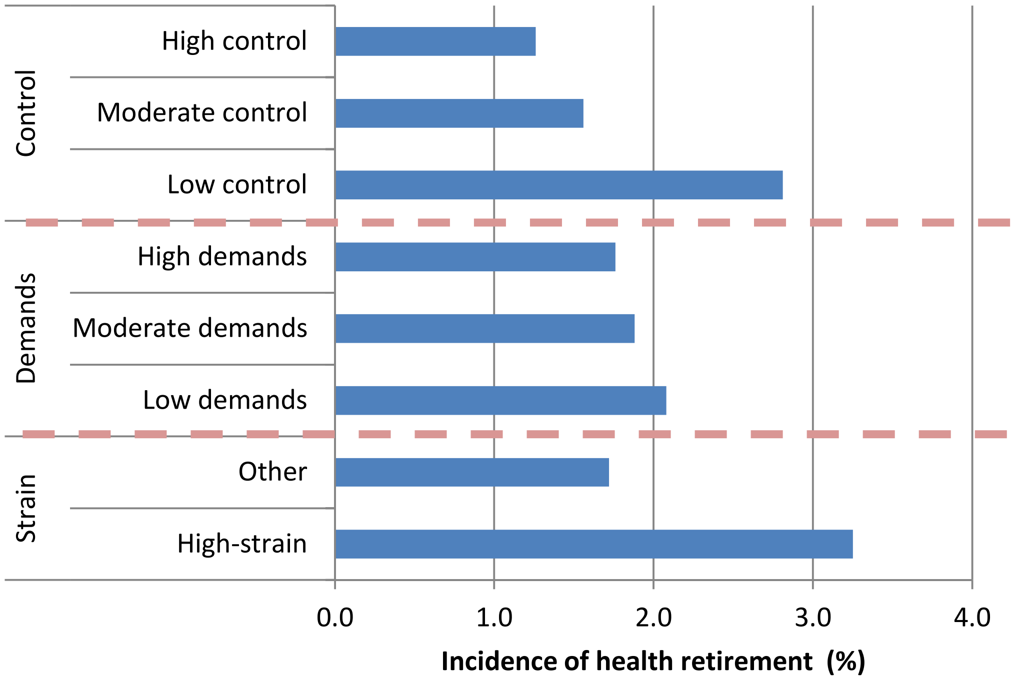
The reverse transition – from long-term sickness to employment – is much rarer. Of the people at wave 3 who also respond at wave 5, only one of the 35 people moving from work to long-term sickness then returns to work by the following wave. (Health retirement is by definition an absorbing state as it refers to the first point someone leaves the Civil Service). The clustering of observations in individuals is therefore primarily among the selected group of those who do not have a health-related job loss by wave 5.

23.1.1. Bivariate associations

It is also useful to look at the unadjusted relationship between job strain and health-related job loss, and this is shown in Figure 22 and Figure 23. My original hypothesis is that people with high job strain would be more likely than others to have a health-related job loss by the following wave – and this is borne out by the bivariate relationship for both health retirement (3.3% for high strain vs. 1.7% for other jobs) and long-term sickness (1.1% vs. 0.7%).

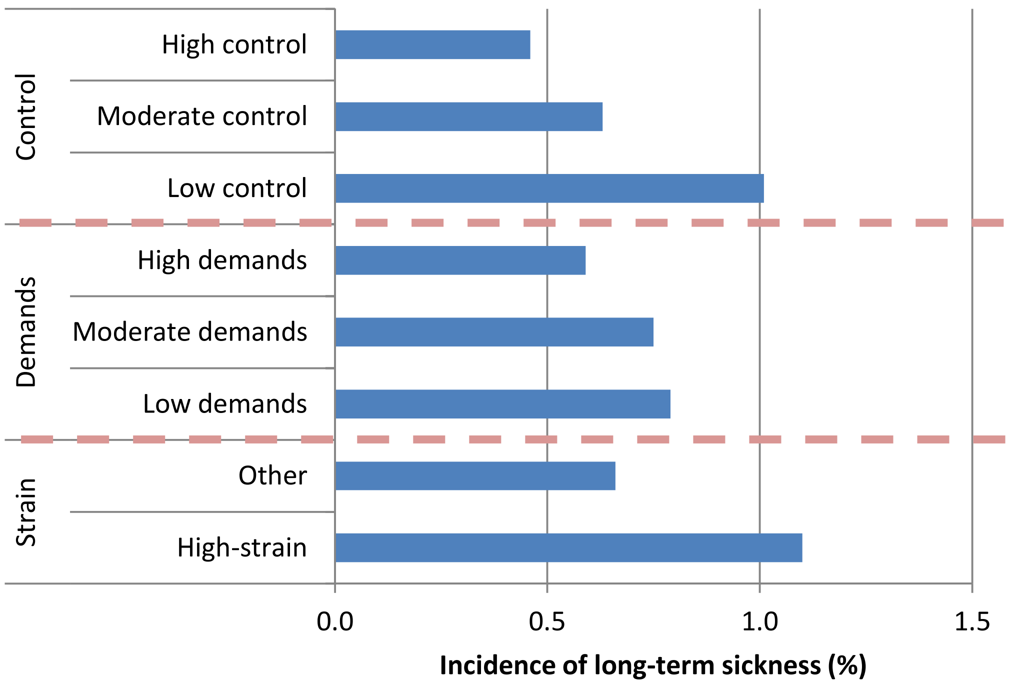
However, while the relationship with job control is also strong and in the expected direction, there is little relationship with job demands for either outcome. Indeed, to the extent that there is any pattern at all, it is for high-demand jobs to have *lower* levels of health-related job loss, the opposite direction to that hypothesized. In the next section, I investigate the extent to which these results change when we control for the large number of potential confounders.

Figure 22: Unadjusted association of <u>health retirement</u> with job demands-control



n=8005, see text for sample details.

Figure 23: Unadjusted association of <u>long-term sickness</u> with job demands-control



n=8935, see text for sample details.

24. REGRESSION RESULTS

The effects of job strain on health-related job loss after controlling for various groups of confounders are shown in Table 14 (for health retirement) and Table 15 (for long-term sickness). Again, these figures are odds ratios, where 1 indicates no effect, >1 indicates an increased risk of WLD, and <1 indicates a reduced risk.

Beginning first with health retirement in Table 14, the empty model shows that high control is protective against health retirement (significant at the 5% level), and that job strain increases the risk (significant at the 10% level). The protective effect of high demands suggested by Figure 22 is more marked when controlling for the demands*control interaction, but this is not statistically significant.

As we add the various groups of controls, the coefficients for demands and strain are essentially unchanged. In the final model, this means a strong but non-significant protective effect of high demands (64% lower odds, $p>0.10$) and a very strong and marginally significant increased risk for high strain (95% greater odds, $p<0.10$). However, the effect of job control is strongly attenuated – particularly by SES – and in the full model there is no effect of job control.

Table 14: Regression of health retirement on baseline job strain [odds ratios]

	[m1] Empty	[m2] Initial	[m3] m2 + SES	[m4] m2 + PH	[m5] m2 + MH	[m6] Full
Low control (not high demands): base						
Moderate control	0.68+	0.76	0.88	0.81	0.85	0.94
High control	0.56*	0.65+	0.86	0.75	0.79	1.02
Low demands: base						
Moderate demands	0.94	1.03	1.13	0.92	0.94	0.94
High demands (not low control)	0.64	0.71	0.87	0.60+	0.57+	0.61
High-strain	1.93+	2.05*	2.06*	2.03+	1.93+	1.95+
N	*8005*	*8005*	*8005*	*8005*	*8005*	*8005*
Num persons	*5877*	*5877*	*5877*	*5877*	*5877*	*5877*
pseudo R-sq	*0.01*	*0.05*	*0.06*	*0.14*	*0.10*	*0.18*

*NOTES: Significance levels: † p<0.10; * p<0.05; ** p<0.01; *** p<0.001. Controls are:*
- *Empty: no controls*
- *Initial: baseline WLD, demographics (age, gender, marital status), wave, lag between waves*
- *SES: initial + socioeconomic status (grade, money problems, housing problems, class of non-Civil Service job)*
- *PH (Physical Health): initial + physical health(SF-36 physical functioning, pain, diabetes, respiratory illness, and 13 categories of longstanding illness (LSI; compared to the WLD models, LSI(migraines) is excluded as it causes separation problems; see section 22.2))*
- *MH (Mental Health): initial + mental health (GHQ caseness, SF-36 mental health, LSI depression, and LSI mental health (other))*
- *Full: all controls from the previous models.*

Turning to long-term sickness in Table 15, the empty model shows a similar pattern: high control reduces the risk of long-term sickness (72% lower odds), job strain increases the risk (95% greater odds), and – contrary to the predictions of the Karasek model – high job demands reduces the risk (61% lower odds). However, none of these terms are significant, reflecting the low statistical power of these analyses due to the rarity of long-term sickness in this sample.

As we add the potential confounders to the model, we also see a similar pattern to that of health retirement. The protective effect of high control disappears when we control for SES, while the effects of high demands and job strain remain (albeit with some attenuation). In all of these analyses, though, none of the individual terms are statistically significant even at the 10% level.

Table 15: Regression of long-term sickness on baseline job strain [odds ratios]

	[m1] Empty	[m2] Initial	[m3] m2 + SES	[m4] m2 + PH	[m5] m2 + MH	[m6] Full
Low control (not high demands): base						
Moderate control	0.77	0.88	1.23	0.86	0.97	1.10
High control	0.57	0.70	1.20	0.74	0.84	1.04
Low demands: base						
Moderate demands	0.98	1.09	1.40	0.96	1.05	1.13
High demands (not low control)	0.56	0.64	0.98	0.56	0.54	0.72
High-strain	2.03	2.13	2.22	1.96	2.08	1.93
N	*8935*	*8935*	*8935*	*8935*	*8935*	*8935*
Num persons	*6260*	*6260*	*6260*	*6260*	*6260*	*6260*
pseudo R-sq	*0.01*	*0.04*	*0.09*	*0.17*	*0.08*	*0.22*

*NOTES: Significance levels: † p<0.10; * p<0.05; ** p<0.01; *** p<0.001. Controls as per previous table, with the exception of PH (m4/m6), where four categories of LSI (genitourinary, cancer, migraines and infectious diseases) are merged with LSI(other) to avoid separation problems.*

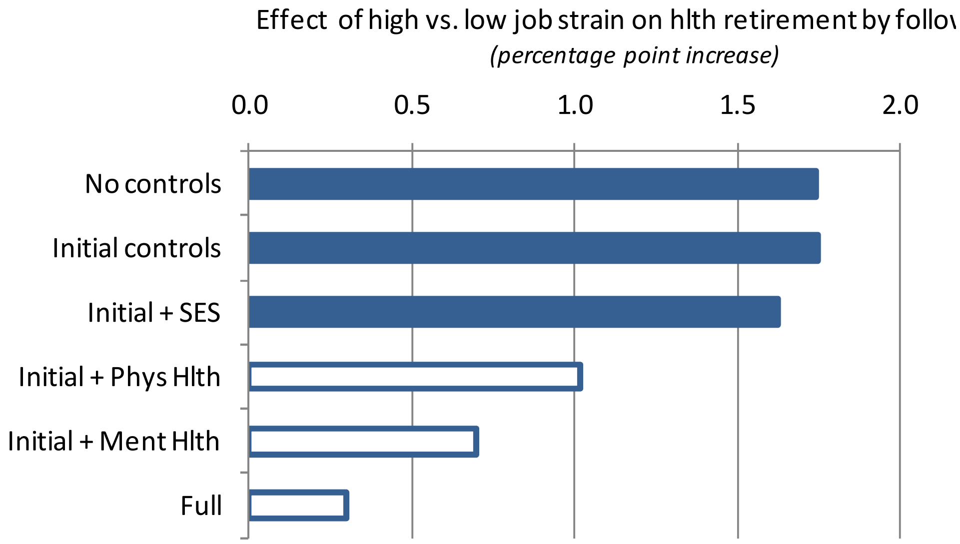
The coefficients on the other covariates in the full models (m6) are presented in Web Table 4.Covariates. This shows that long-term sickness is higher for older (p<0.10) and unmarried people, the top and particularly the bottom Civil Service grade, those in lower-class non-Civil Service jobs, with certain longstanding illnesses (brain, musculoskeletal, ear, depression, other), with poor physical functioning, and after wave 3 (vs. wave 5). Health retirement is more common for older people, for lower grades (p<0.10), with certain longstanding illnesses (brain, musculoskeletal, ear, depression, mental health (other)), with diabetes, poor physical functioning, poor mental functioning, and after wave 3 (vs. wave 5).

24.1.1. Interpreting these results

It is again helpful to present the results in terms of the estimated change in the prevalence of health-related job loss, in the situation that everyone in the sample moved from low-strain to high-strain jobs (population-averaged marginal effects, or 'AME'; odds ratios and standard errors are available in the Web Appendices).

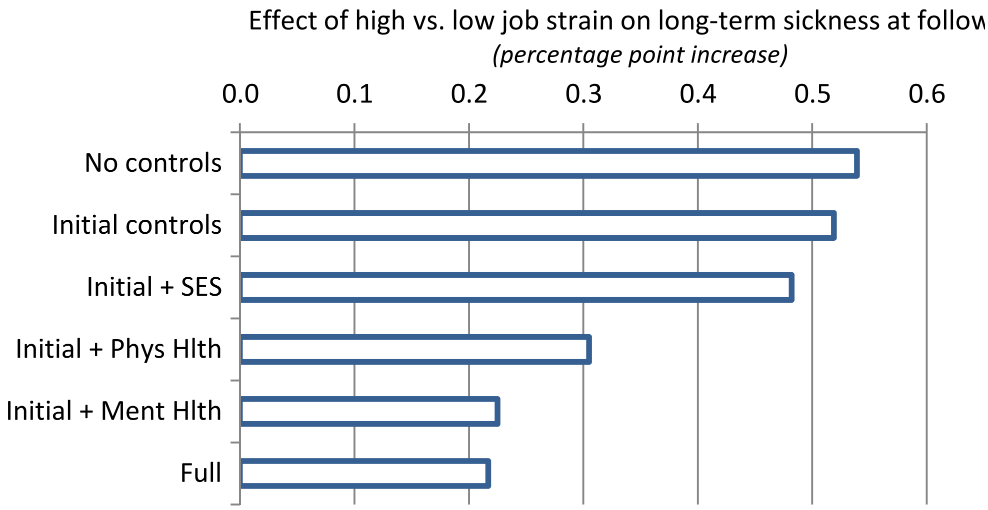
AMEs are shown in Figure 24 and Figure 25. For health retirement, we can see the difference between high-strain and low-strain jobs is large (1.7 percentage points) when controlling for SES and demographics, but this declines and becomes non-significant when baseline health is controlled for. The same is true for long-term sickness, except that the analyses are even lower-powered; even in the unadjusted models, the effect of job strain is non-significant.

Figure 24: Estimated effect of baseline job strain on health retirement by follow-up



Average incidence of health retirement by follow-up is 1.9%. Solid bars are significant at the 5% level; bars shaded with hashes are significant at the 10% level; empty bars are non-significant. Estimates are population-averaged marginal effects, based on the same models as Table 14.

Figure 25: Estimated effect of baseline job strain on long-term sickness at follow-up



Average incidence of long-term sickness by follow-up is 0.71%. Solid bars are significant at the 5% level; bars shaded with hashes are significant at the 10% level; empty bars are non-significant. Estimates are population-averaged marginal effects, based on the same models as Table 15.

As in Chapter 3, these comparisons between high-strain and low-strain jobs are based on the combination of the individual coefficients on high demands, low control, and the high demands*low control interaction. This enables me to compare my results to previous studies and to separate the roles of demands, control and the demands*control interaction – but this produces underestimates compared to a simpler model that just compares the broader groups of 'high-strain', 'low-strain', and 'all other' jobs. In this simpler case, the effect of high- vs. low-strain on health retirement is 0.43 not 0.30 percentage points, and on long-term sickness is 0.30 not 0.22 percentage points. Yet neither these comparisons nor the contrast between high-strain and all other jobs are statistically significant.

In conclusion, these results provide weak evidence on the original hypotheses. Compared to low-strain jobs, those in high-strain jobs have a raised chance 2-3 years later of taking a health retirement or becoming long-term sick – but these effects are far from statistical significance after I include extensive controls for baseline ill-health. Looking at the coefficients separately, the interaction of low control*high demands was marginally significant (p<0.10) for health retirement and consistently large in all models, but this was counteracted by an apparent (although non-significant) *protective* effect of high demands. In the remainder of this chapter, I investigate the robustness of these null findings to different model specifications and different assumptions, and try to increase the statistical power of these analyses.

24.1.2. Confounders and mediators

As an initial check, I included confounders that are only measured at one of the two baseline waves, including education, council housing, car availability, low income and assets (see Appendix 3.A of the previous chapter for variable details). Given that the subsamples for these analyses are different it is no surprise that the specific results differ, but compared to models using the same controls as Table 14 and Table 15 above, the impact of including these additional variables is negligible. Indeed, the only impact that was discernable at all was due to the inclusion of controls for income and assets,[72] and this was far from statistical significance.[73] Our conclusion of a null effect is unchanged by more extensive controls for SES.

It is possible that the effect of job strain is attenuated by controlling for civil service grade and GHQ in the analysis (see section 3.1 in the previous chapter). However, removing grade had no effect on the size of the estimates (or even led to smaller

[72] For health retirement this slightly attenuated the already weak and non-significant effect of job strain; while for long-term sickness it slightly increased it.

[73] Jointly testing the coefficients for low vs high control, low vs high demands, and job strain, the impact of the additional controls was not significant (chi2(3)=2.0, p=0.58 for health retirement, chi2(3)=1.5, p=0.69 for long-term sickness).

estimated effects), while removing GHQ increased them – but not to the point that the effects were close to statistical significance.

In conclusion**,** the null finding for job strain does not appear to be due to over-controlling for grade and GHQ, nor to residual confounding by SES.

24.1.3. Greater power from additional data

As described in section 23, I increased the number of health retirements by adding data from waves 1 and 2, which gives the analyses considerably more statistical power. After adding this additional data, I find a significant effect of job strain on health retirement (p=0.01; see Web Table 4.Waves), even with an extensive – if smaller – number of controls.[74] This is a large effect; I estimate that 1.3% of people in low-strain jobs will take a health retirement by the following wave, compared to 2.1% of people in high-strain jobs. (If I use simple dummy variables for high- and low-strain jobs, I find an identical effect).

If I increase the number of health retirements yet further by looking at the effects of job strain two waves into the future, then I find a marginally significant effect of job strain on health retirement (p=0.06). Here I estimate that 2.5% of people in low-strain jobs will have retired on health grounds after two waves, compared to 3.4% of people in high-strain jobs (see Web Table 4.Waves). The smaller effect here is not unexpected; the additional number of health retirements of this analysis has to be balanced against the increasing likelihood of changing working conditions between the baseline wave and the point of health retirement. This model also shows a possible effect of job control (30% greater odds of health retirement, p<0.10) that does not depend on the interaction of high demands*low control – a finding that is unimportant in the context of this chapter, but to which I return to in Chapter 7.

This suggests that there is a genuine effect of job strain on health retirement, but that the main analyses above are under-powered for detecting such an effect.

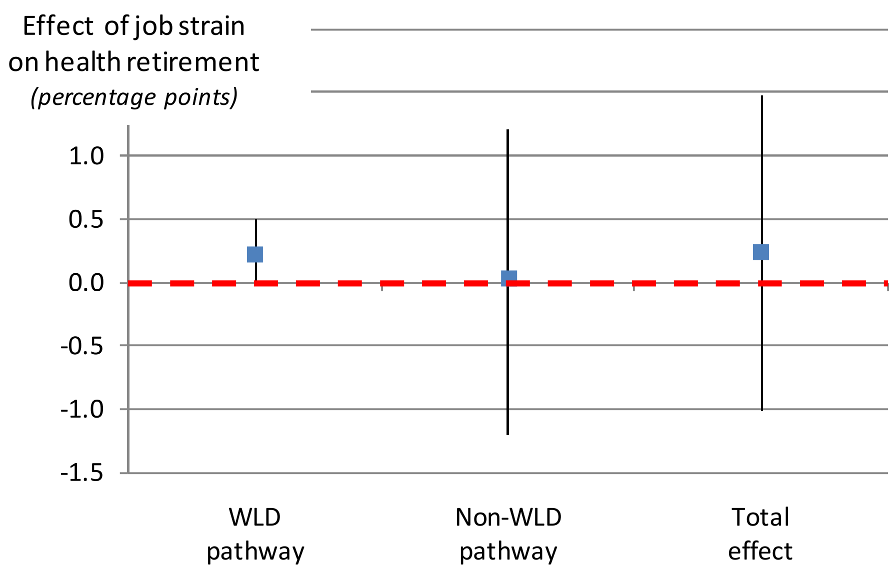
24.1.4. Pathways between job strain and health-related job loss

Another way of dealing with lower power is to look at particular *pathways* between job strain and health-related job loss, an analysis that can be much more high-powered as each component can be estimated much more precisely – a finding that has been confirmed in several simulation studies (Shrout and Bolger 2002; MacKinnon et al 2007; Hayes 2009).

[74] Four controls are not available at baseline waves 1/2 – social class of non-civil service job, physical functioning, mental functioning, and bodily pain. If these are removed from the baseline waves 3/5 analyses, then the effect of job strain appears slightly stronger but is still non-significant (AME_{strain}=0.71 percentage points vs. 0.30 percentage points in Figure 24; p=0.27 vs. 0.66 respectively). This difference is due to fluctuations in the non-significant coefficient on job control.

I therefore estimated the impact of job strain on health-related job loss *through WLD* using the method described in section 15.2.1, given the evidence in the previous chapter that job strain strongly affects WLD. (In results not shown, I also confirmed that WLD predicts health-related job loss). The results of these mediation analyses are shown in Figure 26 and Figure 27.

Figure 26: Pathways between job strain and health retirement
'Job strain' is the contrast between high-strain and low-strain jobs
Marker shows bias-corrected estimate; bar shows 95% confidence interval



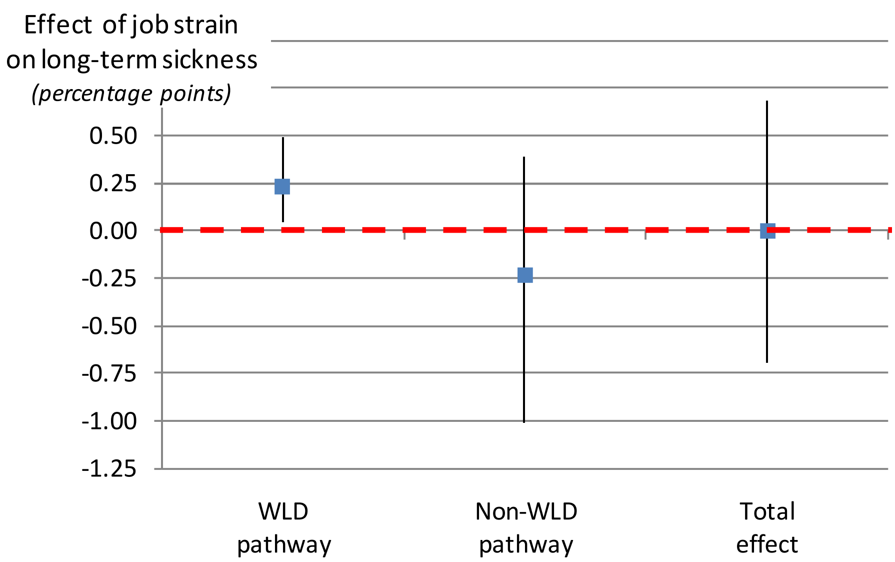
Average incidence of health retirement in this sample is 1.7%. Estimates are bias-corrected bootstrap estimates with 1000 replications (n=7327, n(persons)=5439). All models include the controls for model 6 in Table 14, with the exceptions of several rare variables that were combined into three groups to avoid estimation problems: (i) LSI eye and LSI ear; (ii) LSI brain and LSI cancer; (iii) LSI other, LSI infectious disease, LSI genitourinary and Diabetes.

The mediators included in the WLD pathway are WLD measured at both baseline and follow-up. The non-WLD pathway is the direct effect of job strain on health retirement after controlling for the WLD pathway.

Looking at health retirement in Figure 26, we can see that there is a small effect of job strain on health retirement through WLD that is significant at around the 5% level. This effect is similar in size to the estimated total effect (0.24 percentage points, compared to 0.27 percentage points for the total effect – about one-seventh of the average incidence of health retirement of 1.7 percentage points across the sample). In contrast, the non-WLD pathway is estimated very imprecisely; the 95% confidence interval includes both -1.0% and +1.0%.

Turning to long-term sickness in Figure 27, we again see that there is an effect of job strain through WLD that is significant at the 5% level. This WLD pathway is moderately sized – 0.22 percentage points, about one-third the average incidence of long-term sickness of 0.63 percentage points in the sample. While it may seem surprising that the WLD pathway can be moderately large and significant when the total effect appears to be zero, this is explained by the reverse effect of the imprecisely estimated non-WLD pathway.

Figure 27: Pathways between job strain and long-term sickness
'Job strain' is the contrast between high-strain and low-strain jobs
Marker shows bias-corrected estimate; bar shows 95% confidence interval



Average incidence of long-term sickness in this sample is 0.63%. Estimates are bias-corrected bootstrap estimates with 1000 replications (n=8287, n(persons)=5842). All models include the controls for model 6 in Table 15 with the exceptions of the following changes to avoid estimation problems:

- *The longstanding illness variables that were grouped into three categories: LSI heart (minor+major); LSI musculoskeletal; and LSI other;*
- *Bodily pain, where the two categories with least pain were combined;*
- *The three binary mental health questions, which were combined into a single binary indicator (GHQ caseness, LSI depression and LSI mental health (other));*
- *Age, where the two youngest ages were combined.*

The mediators included in the WLD pathway are WLD measured at both baseline and follow-up. The non-WLD pathway is the direct effect of job strain on health retirement after controlling for the WLD pathway.

These analyses look at the mediating effect of WLD measured at *both* baseline and follow-up on health-related job loss at the following wave. However, section 22.1 described three different temporal sequences between job strain and health-related job loss via WLD, each of which have strengths and weaknesses.

To test the robustness of the WLD pathway to different assumptions, I re-did the mediation analyses for each ordering individually, as shown in Table 16 (the first line of which repeats the analyses above). Across six new pathways (three sequences x two outcomes), four are statistically significant at the 5% level and a further one at the 10% level. Moreover, among the more conservative sequences that look only at WLD reported among workers (Sequences B2 and B4), two of the four possible pathways are significant at the 5% level and a further pathway is significant at the 10% level.

Table 16: WLD pathway between job strain and health-related job loss
Sequences refer to Figure 21

Temporal ordering	***Health retirement*** Estimate	95% CI	***Long-term sickness*** Estimate	95% CI
(Overall incidence for one-wave transitions)	*1.69%*		*0.62%*	
$Strain_t$ ⇨ $WLD_{t,\ t+1}$ ⇨ Job $loss_{t+1}$ **Sequences B2 + B3**, shown in Figure 26/27	0.22%*	0.005; 0.51	0.23%*	0.04; 0.49
$Strain_t$ ⇨ $WLD_{\mathbf{t}}$ ⇨ Job $loss_{t+1}$ **Sequence B2**	0.12%†	-0.01; 0.38	0.07%*	0.01; 0.20
$Strain_t$ ⇨ $WLD_{\mathbf{t+1}}$ ⇨ Job $loss_{t+1}$ **Sequence B3**	0.20%*	0.04; 0.48	0.22%*	0.03; 0.46
(Overall incidence for two-wave transitions)	*1.49%*		*0.82%*	
$Strain_t$ ⇨ WLD_{t+1} (workers only) ⇨ Job $loss_{\mathbf{t+2}}$ **Sequence B4**	0.34%*	0.11; 0.67	0.00%	-0.33; 0.48

** = p<0.05 (using bias-corrected estimate of 95% CI), † = p<0.10 (using percentiles of individual bootstrap replications. Estimates are for the difference between high-strain and low-strain jobs, and are bias-corrected bootstrap estimates with 1000 replications. Number of observations varies by model: for health retirement, n=7929 (n(persons)=5839) for (1) and (2), n=4230 for (3)); for long-term sickness (n=8234, n(persons)=5792). All models include the same controls as in Figure 26 (health retirement) or Figure 27 (long-term sickness), with the exception of Sequence B4 where the smaller sample size creates additional separation problems:*

- *For health retirement: LSI(skin/allergy) was excluded, LSI(depression) and GHQ caseness were combined; and the LSI(other) category was combined with several rare LSIs (LSI(eye), LSI(ear), LSI(brain), LSI(cancer) and LSI(MH other)).*
- *For long-term sickness: the age groups were grouped further (39-50 vs. 50-60), and lag + wave were excluded.*

In conclusion, this section has shown a significant effect of job strain on both forms of health-related job loss operating through the WLD pathway. This effect is much more precisely estimated than the overall effect, although it requires auxiliary assumptions from the main analysis – an issue I return to in the Discussion below. Much of this effect comes from the effect of job strain on WLD measured at the same time as health retirement/long-term sickness, which is particularly subject to endogeneity – but some pathways are significant that use the effect of strain on WLD only among workers, where this endogeneity does not apply.

24.1.5. Other working conditions

The past two sub-sections have provided some signs of a statistically significant effect of job strain on health-related job loss, both in higher-powered analyses using waves 1/2, and when looking at the WLD pathway. Yet it is possible that demands and control are proxies for other aspects of working conditions that influence labour market behaviour – although equally they may be mediators and/or collinear variables that attenuate the effects of strain (see section 22.1 and Discussion). I therefore repeat the analysis of the previous two sections with the addition of controls for job satisfaction, job social support and job skill use:

- With the addition of these other job variables to the wave 1/2 data, the effects of job strain on health retirement become smaller and non-significant whether looking one wave (AME_{strain}=0.43% vs. 0.86%, revised p=0.22) or two waves (AME_{strain}=0.35% vs. 0.91%, revised p=0.49) into the future.
- In the pathways analyses the effect of including these other job variables is smaller, and varied between the different sequences/outcomes (Table 17). Comparing this to Table 16, we can see that while the WLD pathways are often slightly smaller, three of four pathways are still significant at the 10% level for both health retirement and long-term sickness.

Table 17: WLD pathway between job strain and health-related job loss, controlling for job satisfaction, job social support, and job skill use
Sequences refer to Figure 21

Temporal ordering	*Health Retirement* Estimate	95% CI	*Long-term sickness* Estimate	95% CI
(Overall incidence for one-wave transitions)	*1.70%*		*0.64%*	
$Strain_t$ ⇨ $WLD_{t,\ t+1}$ ⇨ Job $loss_{t+1}$ **Sequences B2 + B3**	0.17%*	-0.09; 0.43	0.20%*	0.002; 0.52
$Strain_t$ ⇨ $WLD_{\mathbf{t}}$ ⇨ Job $loss_{t+1}$ **Sequence B2**	0.15%†	-0.01; 0.40	0.07%*	0.004; 0.23
$Strain_t$ ⇨ $WLD_{\mathbf{t+1}}$ ⇨ Job $loss_{t+1}$ **Sequence B3**	0.12%	-0.07; 0.36	0.19%*	0.003; 0.49
(Overall incidence for two-wave transitions)	*1.52%*		*0.81%*	
$Strain_t$ ⇨ WLD_{t+1} (workers only) ⇨ Job $loss_{\mathbf{t+2}}$ **Sequence B4**	0.25%*	0.01; 0.58	0.07%	-0.07; 0.30

See Table 16 for details.

Furthermore, it is striking here that all of satisfaction, variety and social support show relatively large effects on both forms of health-related job loss, and these are significant in the case of job satisfaction (where low satisfaction is associated with increased risks of health retirement) and job variety (where increases in variety reduce the risk of health retirement).[75] As previously discussed, one possibility is that these are mediators rather than confounders of the effect of job strain. But even if this is the case, then this means much of the apparent effect of job strain is not working through the WLD pathway, given that satisfaction/variety/support have no impact on WLD.

In other words, there appear to be two processes occurring simultaneously. On the one hand, there is a WLD pathway from job strain to health-related job loss, which is relatively robust to controls for other working conditions. On the other hand, there is a non-WLD pathway from working conditions to health-related job loss, which appears to be more associated with job satisfaction/variety. I develop this interpretation in the Discussion below.

[75] Net of job strain for health retirements by the following wave, $AME_{satisfaction}$=0.74%, $p<0.05$; $AME_{support}$=0.37%, $p<0.10$; $AME_{variety}$=0.82%, $p<0.01$).

24.1.6. Specificity

A further criterion to help establish causal effects is specificity; that is, where effects are restricted to particular hypothesized outcomes and are not found for theoretically unrelated outcomes. To establish specificity, I look at the effects of job strain on one similar outcome (long-term sickness absence) and two other forms of job loss, non-health early retirements and non-employment. While I expect strain to be associated with non-health job loss (e.g. through mental strain or job satisfaction), I expect the effect on health-related job loss to be greater and more associated with the WLD pathway.

The overall effects of job strain on the different types of job loss are shown in Figure 28. The effects do not look specific to health-related job loss; the level of job loss is greater in high-strain jobs for all four outcomes, including the two outcomes less associated with health. This is close to statistical significance for the most common outcome (and therefore highest-powered analysis), non-employment (p<0.06).

Looking separately at ***sickness absence***, high control reduces the odds of *any* long-term sickness absence (by 20%), but high demands *also* reduces the odds (by 14%) – the reverse direction to that predicted by the Karasek model – meaning that there is no significant effect of job strain as a whole (p>0.50; see section 24.1.8). There is slightly greater evidence that job strain increases the *number of days*[76] of long-term sickness (p<0.10), with the model estimating 5.2 days/year over the following two waves for those in low-strain jobs compared to 6.4 days/year for those in high-strain jobs, but again there are some (non-significant) signs of a protective effect of high job demands.

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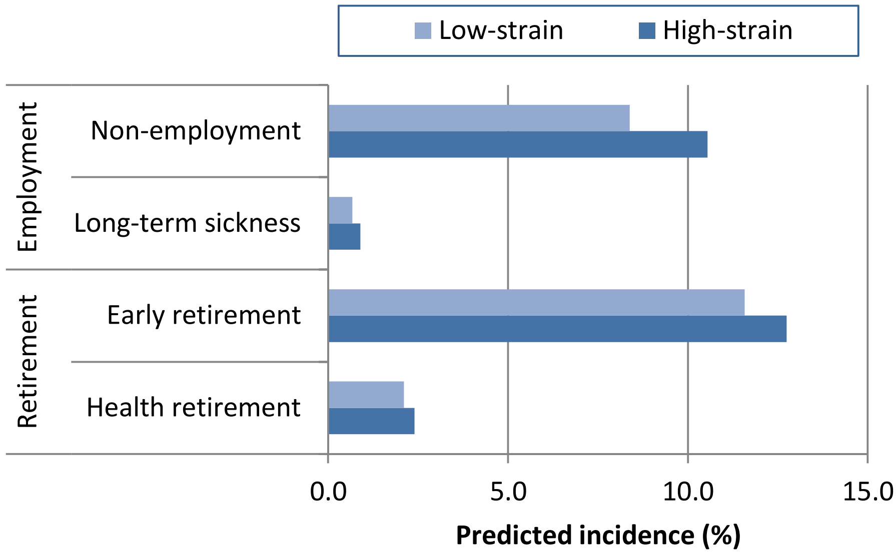
[76] Unlike the other models, this uses a negative binomial model to deal with the skewed nature of the days of absence, where (Cameron and Trivedi 2009:555):

$$\Pr(\mathrm{Y_{it}} = y_{it}|\mu_i, \alpha) = \mathrm{Negbin}(\mu_i, \alpha)$$

$$\mu_{\mathrm{it}} = \exp\left(\boldsymbol{\beta_1 \mathrm{JOB}_{it}} + \boldsymbol{\beta}_2\mathbf{DEM_{it}} + \boldsymbol{\beta_3}\mathbf{SES_{it}} + \boldsymbol{\beta_4}\mathbf{PH_{it}} + \boldsymbol{\beta_5}\mathbf{MH_{it}}\right)$$

where Γ is the gamma integral, α is a constant, y_{it} is the days of sickness absence for person i in time period t (either waves 1-3 or 3-5), and there are vectors of covariates $\mathbf{JOB}$ (job strain), $\mathbf{DEM}$ (demographic characteristics), $\mathbf{SES}$ (socioeconomic status), $\mathbf{PH}$ (physical health), and $\mathbf{MH}$ (mental health).

Figure 28: Effect of job strain on different types of job loss



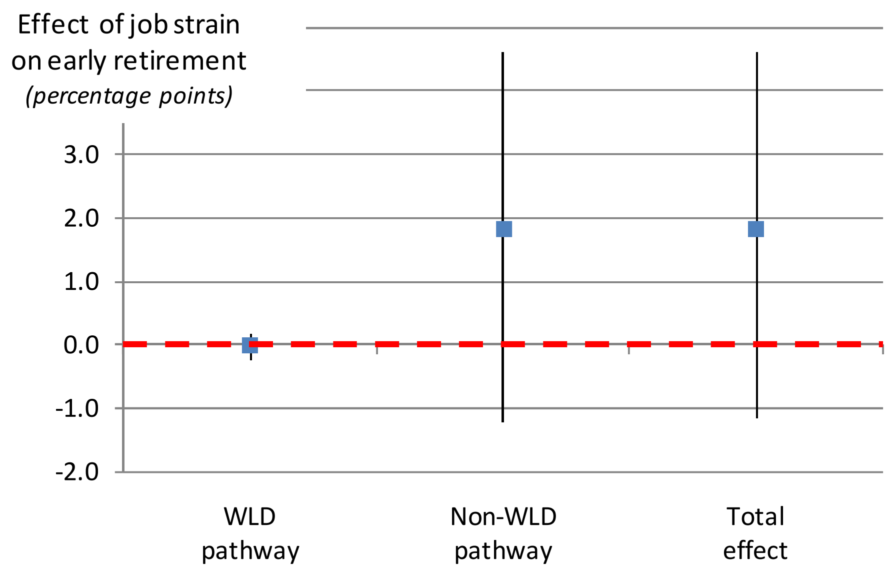
Models are conducted with the same controls/sample as model 6 in Table 14 (retirement) or Table 15 (employment).

If I control for other working conditions (satisfaction, variety, support), then the apparent effects of job strain on early retirement and non-employment are substantially attenuated (and in the case of the only significant effect (non-employment), declines to non-significance). This mirrors what I found for health retirement with the addition of the wave 1/2 data in section 24.1.5.

At the same time, there are signs that the WLD pathway is specific to health-related job loss. The results for the WLD pathway for *non*-health-related outcomes are shown in Figure 29 and Figure 30. Both analyses show a negligible pathway between job strain and non-health-related job loss via WLD (this is significant for non-employment at the 10% level, but small in size).

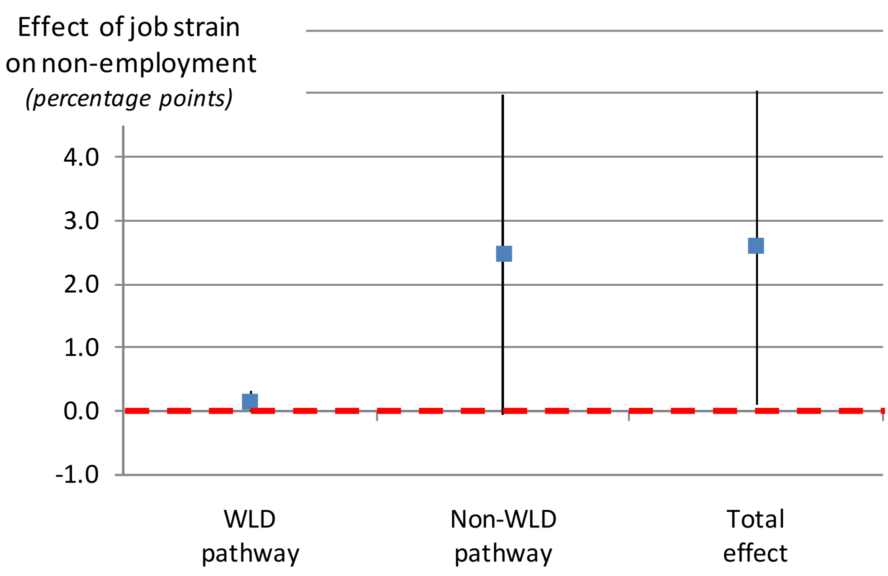
This supports the interpretation of the previous section: that there is an effect of job strain on job loss via the WLD pathway that only applies to health-related outcomes; and an apparent effect of job strain on all forms of job loss that is associated with strain alongside other working conditions (which can be interpreted as either a separate causal pathway or confounding).

Figure 29: Pathways between job strain and early retirement
Marker shows bias-corrected estimate; bar shows 95% confidence interval



The average incidence of non-health early retirement in this sample is 10.8%. Sample, controls and mediating variables are the same as those in Figure 26.

Figure 30: Pathways between job strain and non-employment
Marker shows bias-corrected estimate; bar shows 95% confidence interval



The average incidence of non-employment in this sample is 9.5%. Sample, controls and mediating variables are the same as those in Figure 27.

24.1.7. Sensitivity analyses

A number of other sensitivity analyses were conducted to test the robustness of the findings to various methodological decisions, including different forms of demands, control and strain; transformed versions of continuous covariates; and multiple imputation for missing data. The findings above were robust to all of these checks, with the results presented in Appendix 4A. I also used alternate methods of binary mediation analysis; while there is some disagreement between different methods, all the methods provide support to some extent for a WLD pathway between job strain and health-related job loss.

As a further check, I tested whether the effect was also found for different subgroups (men and women, older and younger workers). There was suggestive evidence that strain only had an effect on health retirement at younger ages (under-50), but this was not significant either when looking one wave or two waves into the future (e.g. for health retirement by two waves after baseline, AME=1.5% for under-50s but -0.1% for 50-60 year olds; p(difference)=0.16). The evidence on differences by gender was more mixed; there was a marginally significantly greater effect among men compared to women when looking one wave into the future (p=0.06), but a reverse, non-significant pattern when looking two waves into the future (p=0.47).

24.1.8. 'Active' jobs and the role of job demands

It appears from Table 14 and Table 15 that as long as control is at least moderate, high demands *reduces* rather than raises the chances of health-related job loss (64% lower odds for health retirement, 23% lower odds for long-term sickness) – in contrast to the original hypothesis. This large effect is not significant in those models, but it does become significant when simultaneously using the additional wave 1/2 data and looking two waves into the future.[77] It is also seen to a smaller degree when looking at long-term sickness absence and early retirement.[78]

This suggests that the contrast between high-strain and low-strain jobs might be the wrong comparison to make; instead, the largest differences seem to be between high-strain and *active* jobs – that is, jobs that have high demands (like high-strain jobs), but combine this with high levels of control. If I re-do the analyses above comparing high-strain to active jobs, then I find a marginally significant effect on health retirement ($AME_{strain\ vs\ active}$=1.1%, p=0.08) which becomes highly significant when I add the wave 1/2 data (AME=1.0%, p<0.01). I also find significant effects on each of non-employment, the

[77] People in high-demand jobs have 43% lower odds of a health retirement by two waves time (p<0.05). When looking one wave into the future, high demands reduced the odds of health retirement by 14%, but this was very imprecisely estimated and non-significant.

[78] The odds of long-term sickness absence and early retirement are both reduced by 14%; the exponentiated coefficient on the days of long-term sickness absence in the negative binomial model is 0.89.

number of days of long-term sickness absence, and when looking two waves into the future for health retirement (as well as marginally significant effects on early retirement and having any long-term sickness absence).[79]

Should we then conclude that job strain affects health-related work outcomes – but just in a different form to the one that was expected? Caution is needed for unplanned tests that seize upon chance variations in the data (Rothwell 2005), although a planned test following such a hypothesis would not have been a complete break from previous work; as noted in Chapter 1, Karasek's original model suggested that active jobs might be a form of 'good stress' that has protective effects on certain outcomes. However, aside from the limited evidence in favour of a protective effect in the reviews I considered in Chapter 1, I assumed on a priori theoretical grounds that any beneficial effects of demands would *not* operate via fitness-for-work but rather through e.g. job satisfaction or mental stimulation.

This expectation is borne out in the data; job demands *raised* rather than lowered the risk of WLD in Chapter 3, and in this chapter the effect of job strain (vs. active jobs) did not significantly operate through the WLD pathway,[80] unlike when comparing high-strain vs. low-strain jobs in section 24.1.4. The effects also tended to be halved in size and non-significant when controlling for other job characteristics (shown in Figure 31). This contrast therefore seems to be capturing other aspects of work than demands and control, and worked through WLD-independent pathways.

It is nevertheless puzzling that active jobs seemed to be particularly protective against *health-related* job loss – a change from active to high-strain jobs was estimated to raise health retirement and long-term sickness by 81% and 78% respectively, compared to only 22% and 30% for other early retirement and non-employment. Given the possibility of spurious effects I do not emphasise these results in the Discussion below, but return to them briefly in Chapter 9.

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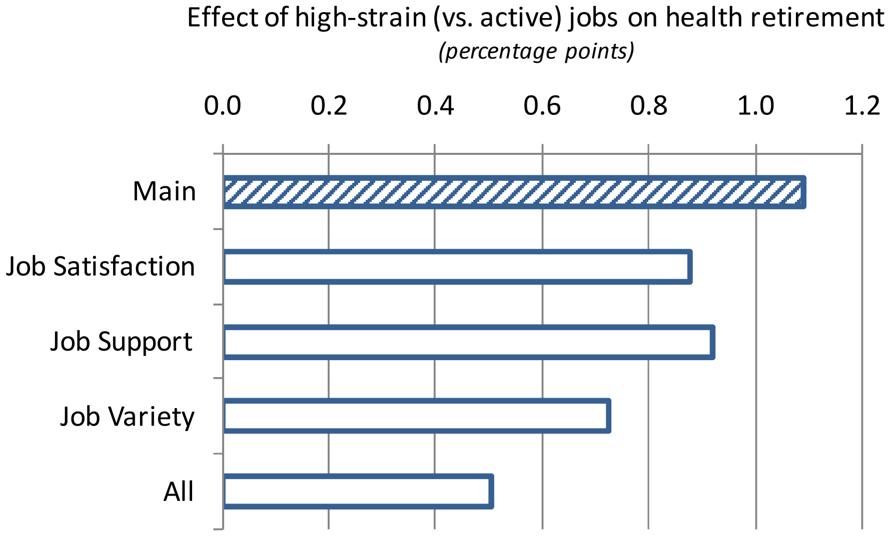
[79] For non-employment the estimated difference was 10.5% (high-strain) vs. 8.1% (active), $p<0.05$; for early retirement 12.7% vs. 10.4%, $p<0.10$; for any long-term sickness absence 35.4% vs. 32.4%, $p<0.10$; for days of long-term sickness absence 6.4 days vs. 4.7 days, $p<0.05$; for long-term sickness 0.89% vs. 0.50%, $p=0.30$; and for health retirement two waves later, 3.4% vs. 1.8%, $p<0.001$.

[80] The overall WLD pathway for health retirement was estimated as 0.10% (95% CI -0.32; 1.77) and for long-term sickness as 0.12% (95% CI -0.06; 0.34), in both cases around half the size of the equivalent WLD pathway when comparing high-strain and low-strain jobs.

Figure 31: Effect of controlling for other psychosocial work characteristics on the contrast between high-strain vs. active jobs



Solid bars are significant at the 5% level; bars shaded with hashes are significant at the 10% level; empty bars are not significant. Estimates use same main sample and controls as model 6 in Table 14.

25. DISCUSSION

25.1. Limitations

This chapter has the same limitations as discussed in the previous chapter: a lack of representativeness, problems with the measures of WLD, missing data, and the need to make several assumptions in order to give the results a causal interpretation (see below). Beyond this, it has the additional limitation that health-related job losses between waves are rare in WII; there are only 152 health retirements and 60 long-term sicknesses in the main analyses, although in sensitivity analyses I have up to 584 health retirements.

At the same time, this study has the same strengths as the previous chapter – the value of the WII cohort itself, the extensive controls, the careful analyses of potential confounders/mediators, and the use of improved techniques of mediation analysis.

25.2. Main findings

There are three findings from this chapter that merit further discussion:

1. People in high-strain jobs are more likely to have a health retirement by the following wave.
2. This effect is not specific to health-related outcomes, and becomes halved in size and non-significant when I control for job satisfaction, social support and variety.
3. If we focus only on the effect of job strain *via WLD*, then job strain has a significant and specific effect on health-related job loss.

I here discuss each finding in turn.

25.2.1. Finding 1: Overall effects

The main hypothesis in this chapter was that people in high-strain jobs would have a raised risk of health-related job loss compared to those in low-strain jobs. The initial models found a raised but non-significant risk of health retirement and long-term sickness, but these analyses were low-powered for detecting a true effect; indeed, any effect that was statistically significant would probably have been an over-estimate (Gelman and Weakliem 2009). When I added additional data on health retirement following waves 1/2 to increase statistical power, I then found a significant effect of job strain, estimating that 1.3% of people in low-strain jobs will take a health retirement by the following wave compared to 2.1% in high-strain jobs.

25.2.2. Finding 2: Strain or satisfaction?

I originally hypothesised that job strain causes particular difficulty for people with health problems, therefore having a larger impact on health-related job loss than other forms. However, this was not borne out in practice; similar results were found for non-health early retirements and non-employment at waves 3-6. Moreover, when I controlled for job satisfaction, variety and support, the estimated effects of strain on all outcomes were halved and became non-significant.

Both job satisfaction (Chapter 3) and social support (Chapter 8) are plausibly mediators rather than confounders, while job variety and control are sometimes considered such closely-related concepts that they are combined into Karasek's single measure of decision latitude (Chapter 1). The results could mean that strain simply serves as a proxy for causally important aspects of work (satisfaction, support and variety) – but they could also mean that the causal effect of strain works through these other work dimensions. As discussed in the previous chapter, one researcher's mediator is another's confounder, and it will take further research suggested in Chapter 9 to determine which of these possibilities is correct.

25.2.3. Finding 3: WLD pathways

When I looked specifically at the WLD pathway I found a statistically significant effect of job strain on health retirement and long-term sickness, even at waves 3-6 where the main analyses showed non-significant results. The size of this WLD pathway was moderate for long-term sickness (where people in high-strain jobs are estimated to be one-third more likely to become long-term sick than those in low-strain jobs), and small for health retirement (those in high-strain jobs being one-eighth more likely to retire on health grounds than those in low-strain jobs).

This effect was specific to health-related job loss, with little sign of such a pathway operating for early retirements and non-employment. Moreover, this effect could be seen even when controlling for other working characteristics. However, there were some differences when I used alternate methods in sensitivity analyses; binary mediation is an emerging area of statistical research, and some additional caution is therefore necessary here. While few studies have tested such pathways, Alavinia et al (2009a) also suggest that work ability mediates the effect of job control on incapacity claims.

25.2.4. A causal effect?

The association of job strain with health retirement can only be given a causal interpretation if we assume that there are no other relevant differences between those in high- and low-strain jobs. As described above, there are other differences in working conditions associated with job strain, and it is difficult to tell if the apparent effect of job strain reflects strain itself or other facets of work. However, as discussed in Chapter 3, it seems likely that some aspect of work has a causal effect, given the large number of covariates I control for. The mediation models also suggest that job strain has a non-work-specific effect on health-related job loss via its effect on WLD, which is robust to controls for other work characteristics.

This chapter therefore provides some evidence for causality, but this is subject to two caveats. First, unobserved confounding is a common affliction of regression models and may apply here, particularly given suggestions of *protective* effects of high job demands (in the absence of low control). In Chapter 7 I therefore replicate this analysis using an alternate method on a different dataset.

Second, mediation models rely on two assumptions about the homogeneity of causal effects (Gellman and Hill 2007:191):

- They assume that the people whose WLD is affected by job strain *are the same people* as those for whom WLD leads to health-related job loss. If job strain leads to WLD only in men, and WLD only leads to health-related job loss among women, then the mediation model will come to incorrect conclusions.
- They assume that *the part of WLD that is affected by job stra*in is the same part of WLD that influences health-related job loss. For example, we assume that WLD caused by quite bad health in high-strain jobs has the same effect as WLD caused by worse health in low-strain jobs. There is more than a theoretical possibility that such assumptions go awry (Maitra 2010:805).

Some caution is therefore needed in interpreting these results causally, and in Chapter 9 I recommend possible avenues for future research to provide greater certainty.

25.3. Conclusion

In the previous chapter I showed that job strain has a strong and robust effect on WLD. This chapter has gone on to look at the relationship between job strain and health-related job loss, and the extent to which any effect is mediated by WLD.

The results suggest that there are two processes that link job strain to health-related job loss. First, there is a relatively large effect of job strain on all forms of job loss, which is associated with job satisfaction, variety and support. Second, there is a fitness-for-work pathway that is specific to health-related outcomes: job strain raises the risk of WLD which in turns raises the risk of job loss. While the evidence is suggestive of causality, both conclusions require assumptions that may ultimately prove to be invalid.

However, health-related job loss is only one possible step on the way to incapacity benefit receipt. Moreover, I have so far only looked at Whitehall civil servants, and have depended on self-reports of job strain – and as I discussed in Chapter 1, some researchers have suggested that self-reports may be unreliable. In the following three chapters, I therefore conduct similar analyses using a different method, conducted on a different (nationally representative) population, and including an analysis of incapacity benefit receipt itself. The next chapter describes the overall method, before Chapters 6 and 7 provide the empirical results analogous to Chapters 3 and 4.

26. APPENDICES

This chapter has one Appendix (which appears at the end of the thesis) and several Web Tables (available from www.benbaumberg.com/thesis.htm):

- **Appendix 4A: Results of sensitivity analyses** – summaries of the results of each sensitivity analysis referred to in section 24.1.7.

CHAPTER 5:
Demands, control and occupations – Imputing demands-control into a longitudinal survey

27. INTRODUCTION

The previous two chapters have investigated the effect of job strain among civil servants. These analyses have a number of strengths – particularly the extensive range of health covariates that are available – but also several weaknesses, including their restriction to a non-representative sample, the low number of health-related job losses, and biases that arise from using people's own self-reports of their job demands and control.

In the following two chapters, I conduct parallel analyses of the effects of job strain using a nationally representative panel survey. Unfortunately no nationally representative panel survey has asked participants about their levels of job strain (see Chapter 3). Instead, I impute demands-control to each person on the basis of the job they do – meaning that I use the demands-control that is typical for that type of person, rather than their (possibly biased) own perceptions of their working conditions. This technique is relatively common in the occupational health field, where it sometimes called a 'Job Exposure Matrix'.

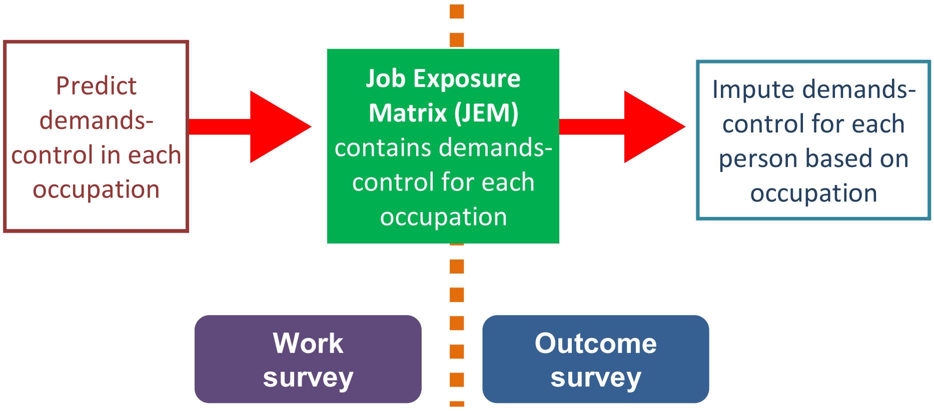
This chapter explains the process of imputation across three sections. First, I discuss the imputation methodology – its strengths and weaknesses, and how it has previously been applied. Second, I describe and justify my own particular form of the imputation, including both the source (work) and target (outcome) surveys. Finally, I show the patterns that underlie the imputations, by presenting a map of demands and control across British occupations.

28. UNDERSTANDING IMPUTATION METHODS

28.1. What is a 'Job Exposure Matrix'?

A Job Exposure Matrix ('JEM') is simply a way of linking information on *jobs* in one data source to information on *outcomes* in a different data source. In its simplest form, we have a cross-sectional survey with information on working conditions and a detailed occupational coding. Alongside this, we have an outcome survey that has the same occupational coding, but no information on working conditions. We then estimate the average demands-control in each occupation in the work survey, and impute these occupational averages into the outcome survey (see Figure 32).

Figure 32: A simple imputation technique using a Job Exposure Matrix



While the term 'Job Exposure Matrix' would be unrecognisable in most disciplines, the practice of imputing data based on occupation is widespread. It is used by economists (e.g. Wasmer 2008:9; Autor and Handel 2009) and is common in Sociology (e.g. Geronimus et al 1996:529; Rose 2003), particularly when using empirically-derived measures of occupational status (Chan and Goldthorpe 2007:517-8). Imputation can also be done based on other characteristics (Longhi and Taylor 2010:4-6); many JEM studies use both occupation and characteristics like age/gender, as I discuss in section 29.4.

28.2. The strengths and limitations of JEMs

Imputation is often used because no single survey has data on all relevant variables. Yet for psychosocial working characteristics, it has been argued that imputation has strengths over conventional studies, as individual self-reports of working conditions may be biased by respondents' personality, situation and expectations (Chapters 1 and 3). In this sense aggregated scores are more 'objective' than individuals' self-reports (Schwartz et al 1988; Spector 1994; Bonde et al 2009b:2) and in practice correlate better with external assessors (Spector 1992:128). They may however still be affected to some degree by broader differences in the type of people in different occupations (Landsbergis et al 2000:174; Stansfeld et al 2004:49), or by different effort norms in different jobs (Green and McIntosh 2001).

Such advantages of JEMs come at the cost of measurement error, which arises for three reasons:

1. While people in the same occupation will tend to have similar working conditions, there will still be a large amount of ***within-occupation variation***. Indeed, previous psychosocial JEMs have found that only 20-25% (control) and

4-11% (demands) of the variation in individuals is explained by a detailed occupational classification – although we must remember that some of this individual variance is due to both random variation and the very biases that we are trying to avoid.[81]

2. We often only have small numbers of people within each occupation, which means the resulting occupation-level estimates are subject to high levels of ***sampling error***. One of the most influential JEM studies had an average sample size in the work data of 15 observations per occupation, with the smallest occupations containing just 3 people (Karasek et al 1988:911). Some JEM studies therefore group (or simply exclude) rare occupations containing less than 4-10 people (e.g. Johnson and Stewart 1993; Wieclaw et al 2008).

3. Measurement error will be exacerbated by ***errors in occupational coding*** in both work and outcome datasets (Pilorget et al 2003). This means that many of the apparent changes in imputed demands-control for any given individual will be due to measurement error (Longhi and Brynin 2010:656-8). At the three-digit occupational level – the level used here – agreement between occupational coders is 70-90% in survey data and 60-70% in epidemiological data (t'Mannetje and Krombout 2003:425). This is worsened if occupational codes from the JEM need to be recoded to a different classification scheme in the main data (t'Mannetje and Krombout 2003:42), as discussed in section 29.3.1.

This causes problems for regression models because measurement error leads to 'attenuation bias' – that is, the effect of working conditions will be underestimated (Schwartz et al 1988; Kauppinen et al 1992; Spector 1992; Stromberg and Bjork 2002), particularly for job demands where the measurement error is greater (Theorell and Hasselhorn 2005), and leading to possibly substantial biases (Aydemir and Borjas 2011:84). Standard errors will also be incorrect and probably too small (see the simulations in Gilks and Richardson 1992), and the statistical power of the analyses will be lowered. These problems will be even more pronounced when we are trying to isolate job demands from job control and potential confounders such as physicality (De Jonge et al 1999:104), given the high correlation between these at the occupational level.

If a survey contains self-reported as well as imputed data on demands-control, the attenuation bias can be overcome by using imputed data as an instrument for individual reports (Henseke 2011), and I recommend applications of this in Chapter 9.

[81] Schwartz et al (1988:906) look at how much occupation explains the *reliable* variance in working conditions (i.e. the variance in the underlying scale rather than in individual indicators), finding occupation explains 36% (control) / 7% (demands) of reliable variance, rather than 25% / 4% respectively of total variance.

Elsewhere, it is still possible to overcome some of these problems by using more sophisticated forms of imputation, such as those that account for uncertainty by using multiple imputations (Brown 2002; Schenker and Raghunathan 2007; Tarozzi 2007; Schenker et al 2010). While not used here, I am developing related techniques outside the thesis later in 2011.[82]

28.3. Previous JEM studies of demands-control

JEMs including job strain have been created in Sweden, Denmark, Finland, Canada, the US, and even twice in the UK (Rose 2003; Blekesaune et al 2008:67), although in both cases strain was not a major focus of analysis.[83] Given the potential strengths and weaknesses of JEMs compared to conventional studies, it is helpful to compare previous JEM results to studies using self-reported demands-control (Chapter 1). I therefore reviewed a subset of 27 previous JEM studies across a variety of outcomes – particularly those related to heart disease (13 studies), but also outcomes as varied as low birthweight, dementia and self-reported health.[84]

From this review, we can partially affirm the conclusions of an earlier review (Schnall et al 1994) that most JEM studies find significant effects of job demands-control on health. In this updated review, only four of the 27 studies do not find any significant results (Reed et al 1989; Klonoff-Cohen et al 1996; Andersen et al 2004; Hemmingsson and Lundberg 2006). However, this is a slightly misleading way of presenting their findings. Many of the studies find significant results for only certain analyses – for example, for job control but not for demands (Hemmingsson and Lundberg 1998), or for certain subgroups but not the sample as a whole (Homer et al 1990).

A more balanced way of presenting this is to split the results for job demands from those for job control, and to look at the total number of contrasts being made. If we look for job control, then we find that 21 of 46 contrasts were significant. Even for heart disease, where self-reports often – but not universally – show an impact of control

[82] I am working with biostatisticians at Imperial College London to both reduce the level of measurement error (using techniques commonly associated with small area estimation) and properly account for the uncertainty in the imputed values (Gilks and Richardson 1992; Jackson et al 2008).

[83] Blekesaune et al focus on later life employment. They do *not* assign demands/control to people based on their present job, but rather on the last job they had for at least two years before reaching 60. Rose (2003) imputes control (but not demands) from the 1997 Skills Survey into BHPS, focusing on the predictors of imputed job satisfaction.

[84] This review is not 'systematic' – I know of at least 19 other potentially relevant JEM studies – but is a wider review than has previously been conducted. The studies reviewed are (Alfredsson et al 1982; Pieper et al 1989; Reed et al 1989; Homer et al 1990; Muntaner et al 1991; Theorell et al 1991; Alterman et al 1994; Johnson et al 1996; Klonoff-Cohen et al 1996; Steenland et al 1997; Hammar et al 1998; Hemmingsson and Lundberg 1998; Muntaner et al 1998; Theorell et al 1998; Marcoux et al 1999; Amick III et al 2002; Andersen et al 2004; Cohidon et al 2004; Seidler et al 2004; Blekesaune and Solem 2005; Hemmingsson and Lundberg 2006; Blekesaune et al 2008; Boedeker et al 2008; Gisselmann and Hemström 2008; and Wieclaw et al 2008; Jansson et al 2009).

(Hemmingsson and Lundberg 2006:617), only 7 of 16 contrasts were significant. As I will return to in section 36.2.1, interpretation of non-significant findings is difficult and depends on the precision of the null results; this led the earlier review to conclude that significant JEM results *"provide strong support for the model, while negative studies may result, in part, from loss of power"* (Schnall et al 1994:397).

When we come to job demands, we find a different picture. Of the 19 studies that investigate job demands, the majority find it to be non-significant, but 6 studies found at least one significant result. However, in five of these six cases this was in the *reverse* direction to that predicted by the Karasek model. That is, high job demands was associated with lower risks of heart disease (Theorell et al 1998), mental health disorders (Wieclaw et al 2008) and schizophrenia (Muntaner et al 1991), better self-reported health (Niedhammer et al 2008), and fewer disability retirements (Boedeker et al 2008). Only one study found a significant detrimental effect of high demands (Karasek et al 1988).

Using externally-assessed measures of demands-control produces similar results (Chapter 1), as does aggregating demands-control within work units rather than occupations. Bonde and colleagues looked at 700 work units in Denmark (Bonde et al 2009a; Bonde et al 2009b; Kolstad et al 2011) and found no effect of demands/strain. Other studies use the same method for looking at other psychosocial work characteristics such as emotional demands, burnout, role conflict (Borritz et al 2010), job satisfaction (Jensen et al 2010) and social capital (Kouvonen et al 2008), and inconsistently find significant results.

Even restricting ourselves to the JEM studies, these are highly variable in both their outcomes and their methods – many are likely to be highly biased due to their design (e.g. cross-sectional studies) or lack of controls (e.g. only adjusting for age). Nevertheless, the review provides two lessons for the JEM studies undertaken here. First, the JEM study may find different results for job demands from the conventional study using Whitehall II. Second, there is a risk of null findings even if a genuine relationship exists, due to the lower power of JEM studies. Yet neither of these risks invalidate the use of the technique. Many conventional studies are low-powered and produce heterogeneous results (Kivimäki et al 2006). Moreover, there is an ongoing debate about imputed vs. self-reported job strain, and in the following chapters I am able to investigate several aspects of this in further detail. I return to these issues repeatedly over the course of the next two chapters, and also in Chapter 9.

29. APPLYING THE IMPUTATION METHOD

In the previous section I described how the imputation method worked in general; here I explain how I apply this in practice. I focus on five issues: (i) the choice of datasets; (ii) the measures of demands, control and strain; (iii) the way that these are estimated for each occupation; (iv) whether other covariates such as gender and education should be used in the imputation; and (v) how to impute changing demands-control over time.

29.1. Choosing datasets

The first decision I had to make was to choose the work dataset and the outcome dataset. Having systematically reviewed the available work data in Chapter 2, I decided to obtain work data from the 1992 Employment in Britain survey and the Skills Surveys of 1997, 2001 and 2006. These are recognised as the highest-quality trend data available, and allow me to construct scales of demands and control.

For the outcome data, I used the British Household Panel Survey ('BHPS'). This is the only long-running nationally representative panel survey in Britain, and contains data on incapacity benefit receipt as well as an extensive range of sociodemographic data within the household. As such, it is one of the most frequently used datasets in UK social science, including for analysis of incapacity benefit receipt (Peasgood et al 2006; OECD 2009; e.g. Benitez-Silva et al 2010:501; Whittaker and Sutton 2010), and one study has even used it to study distant proxies of demands-control (Jones et al 2010).

BHPS is primarily a nationally representative sample of *households* in the British population in 1991, who have been interviewed every year along with other members of their (changing) households and their children's households (Lynn 2006). The original sample is therefore representative *of people living in Britain in 1991 (and their children)*, rather than of the British population in each year of the survey.[85] Additional samples from Scotland, Wales (from 1999) and Northern Ireland (2001) have also been added to the survey and are used in the analyses in the following chapters unless otherwise specified.[86] Further detail on BHPS is given in Chapter 6.

29.2. Measuring job demands, control and strain

It is preferable to measure job demands and control through scales rather than single questions: not only does this make the analysis more parsimonious, but it makes the

[85] BHPS also excludes people living in institutions (including student halls of residence), those living north of the Caledonian Canal, and people living in addresses not on the Postcode Address File (Lynn 2006:66-7).

[86] The ECHP subsample in BHPS has been excluded from the analyses.

scales less subject to measurement error (Bartholomew et al 2008). I therefore use the same scales of demands and control that I created in Chapter 2 (see Appendix 2B). As in WII, I turn these scores into tertiles (low, moderate, high) to pick up nonlinearities, and use the continuous imputed score in sensitivity analyses.

There are two different ways of using these scales to impute the demands*control interaction:

1. ***'Interact-then-impute'***: interact demands*control in the work survey to create either a continuous score (e.g. demands-control) or categories ('high-strain jobs' defined as above-median demands and below-median control). Job strain in each occupation can then be imputed into BHPS.
2. ***'Impute-then-interact'***: impute demands and control separately into BHPS, and *then* look at the interaction of average demands*average control.

The difference between strain at the individual level and the occupational level becomes clearer through an example. In a hypothetical occupation, there are two equally large groups of workers: (i) those in passive jobs with low control (-0.6) and moderately low demands (-0.2), and (ii) those in active jobs with moderately high control (+0.2) and high demands (+0.6). On average, people in this occupation have moderately low control (-0.2) and moderately high demands (+0.2) – which means this appears to be a 'high-strain job'. But no individual in this occupation is actually working in a high-strain job!

The interact-then-impute method therefore seems to better capture job strain – but in practice, nearly all JEM studies impute-then-interact (e.g. Alfredsson et al 1982; Klonoff-Cohen et al 1996; Marcoux et al 1999; Blekesaune and Solem 2005; Hemmingsson and Lundberg 2006; Jansson et al 2009). A problem with interact-then-impute is that the categorical quadrant model of job strain is often preferred, and categories are even less precisely estimated at the occupational level than continuous variables. Yet continuous measures of job strain have been created (both ratios and products are used; Landsbergis et al 2000:170) (see also Reed et al 1989), and these continuous measures have still been incorporated in JEMs using the interact-then-impute method (Karasek et al 1988; Pieper et al 1989; Reed et al 1989; Theorell et al 1991; Alterman et al 1994; Amick III et al 2002; Choi et al 2008).

So far I have only found one study that actually imputes strain directly, rather than using impute-then-interact (Wieclaw et al 2008). In the following two chapters, I use *both* of these measures of imputing job strain and compare the results.[87] While not a

[87] For the impute-then-interact measure, I follow traditional practice in defining job strain as jobs that are in the highest tertile of demands and the lowest tertile of control. For the interact-then-impute measure, I define strain as the ratio of demands to control, impute this continuous measure into BHPS, and then

major focus of the following chapters, to my knowledge this is the first time that such a comparison has been done, and in Chapter 7 I find this can lead to different results.

29.3. Occupational codes

In the 2001/2006 Skills Surveys, occupations are coded to the 2000 Standard Occupational Classification ('SOC 2000'). Each occupation is classified according to a nested series of categories:

> 9 *major* groups (one-digit codes)...
>
> ...divided into 26 *sub-major* groups (two-digit)...
>
> ...in turn divided into 79 *minor* groups (three-digit)...
>
> ...and finally divided into 343 different *unit* groups (four-digit).

For example, the unit group *'stock control clerks'* (4133) is nested within minor group *'administrative occupations: records'* (413), which in turn is part of sub-major group *'administrative occupations'* (41), which is within major group *'administrative and secretarial occupations'* (4).

However, at the most detailed level this leaves an average of only 33 people per unit group, and because some occupations are much more common than others, we in fact find that we have less than 30 observations for most occupations – which makes estimates at this level unreliable. One alternative is to use the less-detailed minor groups, but this captures noticeably less of the variation in job conditions, and still has seven (of 79) categories with less than 30 people.

Instead, I created an occupational classification based on 4-digit unit groups. Larger unit groups were unchanged, but smaller unit groups were combined with similar[88] occupations until they contained at least 30 people. For example, unit group 8131 ('*assemblers (electrical products)*') was combined with unit group 8132 (*'assemblers (vehicles and metal goods)'*) on the basis that these were both within the minor group 813. This bespoke classification consists of 119 occupational categories. Full details of these categories and their relation to SOC 2000 are given in Web Appendix 5a.

In the 1992 Employment in Britain survey and pre-2001 BHPS, the earlier SOC version is used (SOC90). This similarly uses nested categories: 374 unit groups (3-digit) nested within 77 minor groups (2-digit) nested within 9 major groups (1-digit). Even

---

divide this imputed measure of strain into tertiles. To create a positive continuous measure, demands and control were first scaled to a standard deviation of 1 and a minimum of 1.

[88] 'Similar' here meant that they were within the same three-digit SOC 2000 code. Where there were several possible merges, similarity was determined on the basis of my own prior knowledge, together with the levels of demands and control in the WERS 2004 data.

with the additional EiB data the sample sizes are still too small at the 3-digit level, so I have created a bespoke occupational classification of 135 categories – details are given in Web Appendix 5b.

Finally, I divided the larger SOC90 bespoke occupations by industry if there were more than 30 people in each resulting subgroup. This produced 46 additional groups (181 in total), which are used in sensitivity analyses in Chapter 6-7, with details given in Web Appendix 5c.

29.3.1. Creating a consistent occupational classification

The JEM method depends on having the same occupational codes in all datasets. Unfortunately this is not the case; we have SOC90 in EiB and BHPS pre-2001; and SOC 2000 in the 2006 Skills Survey (BHPS post-2001 and the 2001 Skills Survey have both). These different classifications cannot be neatly recoded into one another, and manually recoding the underlying information would be an overwhelming task.

To create a consistent occupational classification, we instead have to use pre-existing data that has been coded to both classifications ('dual-coded data') in one of four ways (Williams 2011:187):

1. **Modal allocation** – SOC 2000 occupations are assigned to the SOC90 code that they are most likely to match to (i.e. the mode within each SOC 2000 group) (Oesch and Rodriguez Menes 2011). This is easy to use, but works poorly at the most detailed level where each occupation maps onto many other occupations.
2. **Random allocation** – the SOC90 codes are randomly assigned to each person's SOC 2000 code according to the probabilities in the dual-coded data (Goos and Manning 2007). Again, this performs poorly when occupations map onto many codes and the within-occupation sample size is small.
3. **Weighting** – duplicate observations are generated for each person, with one observation generated for each SOC90 code that matches the respondent's SOC 2000 code. For example, for a person in a SOC 2000 code that matches five SOC90 codes, five copies of this person would be generated. Each duplicate observation is then given a weight according to the probabilities in the dual-coded data, with the total weight adding to 1 (Weeden 2005; Weeden and Grusky 2005). The mean values from this recoded data will be appropriate; however, the results do not take into account the uncertainty involved in converting between classifications.
4. **Multiple imputation** – rather than just assigning occupations once (as in random allocation) or providing a fixed weight (as in weighting), multiple

imputation creates several different versions of the dataset where the SOC90 code for each person with a SOC 2000 code is predicted differently in each version (Schenker et al 1993). It then analyses all these different datasets simultaneously, taking into account the uncertainty in assigning occupational codes. However, care needs to be taken to ensure that the (multinomial logistic) model is working adequately, particularly at the fine-grained occupational level with several hundred outcomes; this also imposes considerable costs in terms of computation time and the viability of other statistical techniques.

Ultimately, I used the weighting method to create a merged dataset where the 2006 Skills Survey was coded to SOC90 to match the other datasets (Web Appendix 5d). The dual-coded data was supplied by the Office of National Statistics based on the 2000 Labour Force Survey. This means that occupational demands-control from 2006 has greater measurement error than preceding years.

For some analyses below, I also need to recode the *industrial* classification available in EiB 1992 (SIC80) with the classification available in the Skills Surveys and BHPS (SIC92). However, no dual-coded data are available for this. I therefore created my own proportional mapping matrix from the dual-coded data in BHPS. Rather than using the weighting procedure, it was here feasible to use the model allocation method, as the industrial classifications map more closely to one another (the accuracy of this technique in the dual-coded data was around 94%). Again, the industrial classifications in EiB 1992 will have a greater level of measurement error than those in other years.

29.3.2. ***Estimating demands-control within occupations***

Even when grouping the smallest occupations together, the sample size within each occupation is often relatively small, and this makes the estimates imprecise. One way of getting more accurate estimates is to use 'Empirical Bayes' estimation ('EB', Rabe-Hesketh and Skrondal 2008), based on multilevel random effects models. In these models, the level of demands or control in an occupation is treated as a 'random effect' deviating from what we would otherwise expect. We can then predict the true level of these random effects as a compromise between our prior expectation about the distribution of the random effect, and the occupational average we actually see in the data.

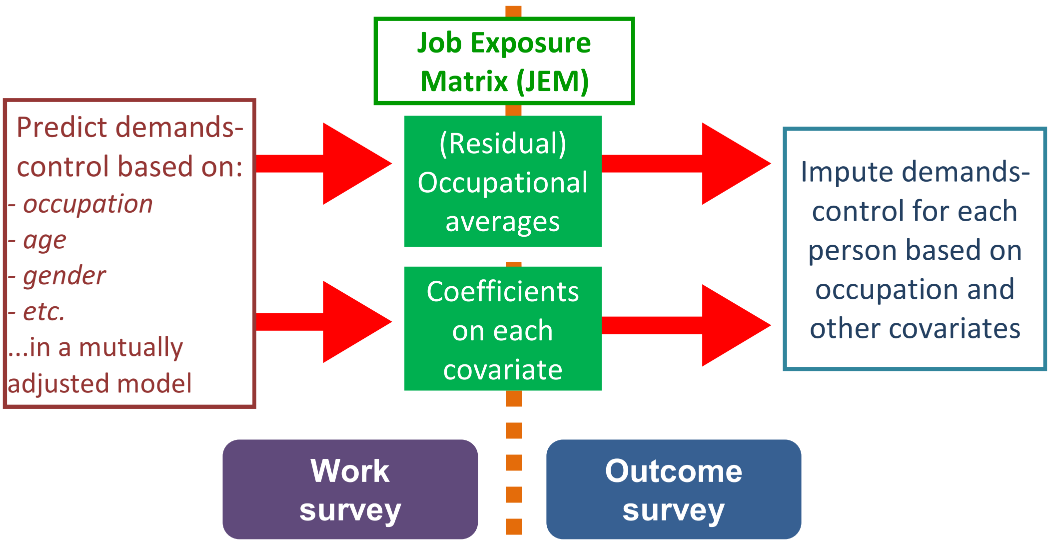
Here, the prior expectation is that each occupation has the level of demands-control in the larger SOC major group that encompasses it. The resulting EB estimates are a compromise between (i) the precise estimates of the relatively large major groups; and (ii) the less precise but more detailed estimates of each unit group. This compromise is different depending on how confident we are about the estimate within the unit group: where we are more confident (e.g. occupations with larger sample sizes), the EB estimate is weighted towards the unit group, while when we are less confident the EB estimate is weighted towards the major group. Examples and further details are given in Appendix 5B.

This greater accuracy comes at a price, however: the estimates are biased towards the average in each major group (causing problems in Chapters 6 and 7), and the methodology is less intuitive than normal imputations. The EB estimates are therefore used as a sensitivity analysis to see if the reduced level of measurement error enables stronger inferences, EB methods not having previously been used for JEMs.

29.4. *Imputing based on other covariates*

In its simplest form, a JEM imputes demands-control based only on a person's occupation. In practice, though, many researchers have used several covariates to construct their JEM; an influential US JEM included gender, age, education, race, marital status, region, urban vs. rural, and self-employment status (Schwartz et al 1988:907). Extending the simple model of Figure 32, we can represent this procedure as follows:

Figure 33: Imputation using covariates as well as occupation



Other JEMs that include covariates include those in Sweden (Johnson and Stewart 1993; Jansson et al 2009) and Canada (Marcoux et al 1999). The argument of Schwartz et al and others is that job strain may be affected by e.g. gender (Kennedy and Koehoorn 2003) or age (Villosio et al 2008:38), and the imputations are therefore more accurate (and less subject to the problems in section 28.2) if these are included.

The counterargument is that these covariates introduce the very biases that the imputation method is designed to avoid. Are differences by age a reflection of the different jobs in which different cohorts find themselves – or do they partly reflect the increased prevalence of ill-health with age?

Strangely, these discussions are absent from nearly all JEM studies, and I know of no study that compares these ways of imputing job characteristics. In the following two chapters I do precisely this, using only occupation in the main analyses, but considering JEMs using occupation and two sets of covariates in the sensitivity analyses. (The first set of covariates includes those characteristics that are most likely to reflect genuine differences in work characteristics – namely industry and qualifications – while the second set also includes age, gender, and working hours).

We have already seen that demands-control in the Skills Surveys change over time (Chapter 2). The question is: how do we account for this when imputing into BHPS?

One possibility is that the changes in demands-control simply reflect changes in the distribution of occupations in Britain – for example, a rise in low-control jobs like call-centre work (Sprigg et al 2003:39-51). To test this, I decompose the trends in demands, control and strain into (i) changes in the prevalence of different occupations; and (ii) changes in job demands-control within each occupation, using a Blinder-Oaxaca decomposition analysis.[89]

The results in Table 18 are striking. The contribution of changes in the prevalence of different occupations is effectively zero. Instead, the entire trend is driven by the differences *within* occupations, for all three outcomes. This suggests that we need to take into account within-occupation changes in demands-control, or face yet-further measurement error in the JEM.

Table 18: Decomposition of trends in job demands-control/strain, 1992-2006
into changes in prevalence of occupations vs. other changes

	Total change	Change due to occupational prevalence	Change due to within-occupation change
Control standardised scale	-0.40 (-0.45, -0.35)	0.03 (0.01, 0.05)	-0.43 (-0.48, -0.39)
Demands standardised scale	0.38 (0.34, 0.43)	0.02 (0.00, 0.43)	0.36 (0.32, 0.41)
Strain log odds ratios	0.15 (0.13, 0.16)	0.00 (-0.01,0.00)	0.15 (0.13, 0.17)

Working-age sample using EiB 1992 and the 2006 Skills Survey. Demands and control use the scale outlined in Chapter 2; strain is based on the highest tertile of demands and the lowest tertile of control. See footnote 89 for further details.

[89] In a Blinder-Oaxaca decomposition, we can decompose a difference (in this case, between years) into (i) a part that is 'explained' by characteristics (in this case, occupation); and (ii) an unexplained component. More formally (Jann 2008):

$$E(Y_{2006}) - E(Y_{1992}) = [E(X_{2006}) - E(X_{1992})]'\beta^* + [E(X_{2006})'(\beta_{2006} - \beta^*) + E(X_{1992})'(\beta^* - \beta_{1992})]$$

...where $E(Y_{year})$ is the expected value of demands/control in that year, $E(X_{year})'$ is the occupational distribution in that year, β_{year} are the coefficients of each occupation in the year, and β^* are the coefficients averaged across all years.

For control and demands I use the Stata command OAXACA; for the binary indicator of high-strain jobs I used the Stata command NLDECOMPOSE. (I also checked results using the LDECOMP command, but this does not allow the use of weights).

Oaxaca-Blinder decompositions vary slightly according to which year is the base category (Jann 2008:10); however, these results are almost identical to both specifications. I here use EiB 1992 and the 2006 Skills Survey, together with the method of converting between occupational classifications described in section 29.3.1.

This still leaves open the possibilities that demands-control changed considerably in some occupations but not in others – or that the trends in demands-control could be seen across the occupational distribution. Ideally we would check this at the fine-grained level of the 135 SOC90-based occupations described in section 29.3, but many of the comparisons across years would be based on very small sample sizes, and we would expect to find that six of these comparisons would be significant at the 5% level just by chance.

Rather than expending considerable effort using recently-developed techniques to detect unusual temporal patterns in small groups (Abellan et al 2008; Li et al 2010), I restrict myself to looking at trends in the broad occupational (SOC90 1-digit) major groups where the sample size is large enough for reasonable comparisons. The results of this comparison are shown in Table 19.[90]

Table 19: Trends in demands and control by broad occupation, 1992-2006

	Control			Demands		
	1992	2006	**Change**	1992	2006	**Change**
Managers and senior officials (reference category)	0.68	0.28	**-0.40**	-0.16	0.28	**0.43**
Professional occupations	0.38	-0.02	**-0.40**	-0.24	0.33	**0.57**
Associate prof / technical occs	0.49	-0.03	**-0.51**	-0.03	0.30	**0.33**
Admin / secretarial occs	0.16	-0.24	**-0.40**	-0.28	-0.01	**0.27***
Skilled trades occs	0.01	-0.19	**-0.19***	-0.33	-0.06	**0.28†**
Personal service occs	0.29	-0.23	**-0.52**	-0.37	0.15	**0.52**
Sales / customer service occs	0.21	-0.39	**-0.59†**	-0.42	-0.12	**0.30**
Process, plant & machine operatives	-0.24	-0.66	**-0.42**	-0.33	-0.23	**0.10****
Elementary occs	-0.08	-0.57	**-0.49**	-0.60	-0.24	**0.37**

*Working-age sample using EiB 1992 and the 2006 Skills Survey. Demands and control use the scales outlined in Chapter 2. Occupation in the 2006 Skills Survey has been recoded to SOC90 using the technique described in section 29.3.1. Statistical significance for comparison to managers and senior officials † p<0.10, * p<0.05, ** p<0.01.*

This shows that declines in control and increases in demands can be seen across the occupational spectrum, with no systematic trend for this to be greater at the top or the bottom of the occupational gradient. It does appear that some occupational groups have seen slightly different trends: for example, demands rose less in *administrative/secretarial occupations* and particularly *process, plant and machine operatives* than it did for *managers and senior officials*. *Skilled trades occupations* also

[90] This is based on simple OLS regression models that include dummies for each occupational group, and dummies for the interaction of each group with the time trend, i.e.

$$y_{ij} = \beta_1 + \sum_{k=2}^{9} \beta_{2k}\mathrm{majorSOCgroup}_k + \beta_3\mathrm{year}_{2006} + \sum_{k=2}^{9} \beta_{3k}\mathrm{majorSOCgroup}_k * \mathrm{year}_{2006} + \epsilon_{ij}$$

saw less of a decline in control and less of a rise in demands than managers did. These results fit reasonably closely with those reported previously (Felstead et al 2007:129; Gallie 2007:119; Green 2008b:124), although there are slight differences, perhaps reflecting the different ways of making occupation codings comparable over time.[91]

In deciding how to impute demands-control over time, Table 19 is ambiguous enough that it can be used to argue two opposite courses of action. On the one hand, the overall trend of rising strain can be seen in most occupations, so we may simply want to take account of a constant time trend across *all* occupations. On the other hand, the statistical significance of the interaction between the major occupational groups and survey year is high, even after controlling for the average level of demands-control in the 135 more detailed occupations ($F_{8,26703}$=1.81, $p<0.07$ for the control interaction, $F_{8,26703}$=3.09, $p<0.01$ for demands), which suggests that the imputation should take account of the different trends in different occupational groups.

I therefore followed the same approach as for the other decisions in this chapter: one method was selected for the main analyses, but other methods of imputation were checked in the sensitivity analyses. The constant change across all occupations was used for the main analyses for simplicity, but I also test both (i) ignoring the trends entirely and using the averages across the entire 1992-2006 period, and (ii) imputing based on different trends in each of the nine major occupational groups.

The final problem was that I needed to impute data for the years *between* the Skills Surveys, and this requires an assumption about the path of trends in years that are not observed. In the absence of better data, I linearly interpolated for the missing years 1993-2000 (assuming a linear trend 1992-2001) and 2002-2005 (assuming a linear trend 2001-2006).

[91] The occupational-group-level correlation between the results for job control reported here and in Felstead et al is 0.87. Felstead et al's results also suggest that the starkly greater decline in discretion seen in professionals vis-à-vis managers 1992-2001 (Green 2006:105) is not as visible for the 1992-2006 comparison, and it is even less visible here – whether this is due to changes 2001-2006 or simply sampling error is unclear. Gallie 2007 more broadly suggests a slight polarization in job control 1992-2001, but this is barely visible in the 1992-2006 trend here.

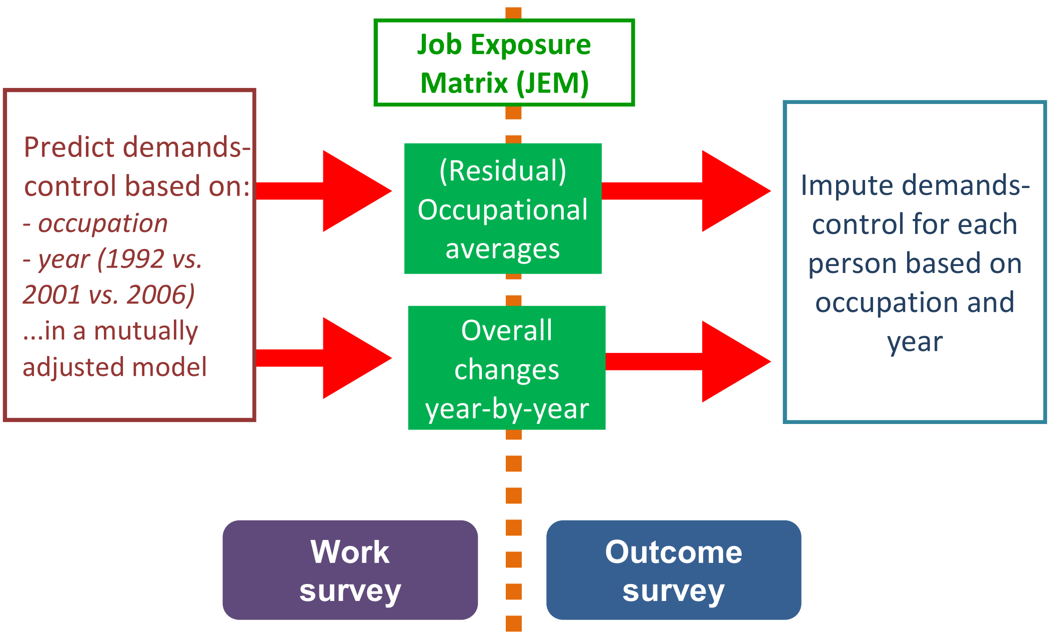
For demands, Green 2008 uses a slightly different measure of 'high effort', which is the prevalence of scores >1 SD above the mean on the demands scale score (rather than the mean level), and this shows much smaller rises in demands among personal service occupations than shown above.

29.6. Conclusions

In this section I have described how I imputed data on job demands and control into the BHPS. The idea is simple: I take the average demands-control in each occupation from the work surveys, and apply this to each employee in the BHPS. Very few of the 27 JEM studies I reviewed consider methodological choices in detail (perhaps for reasons of space); here I have considered the methodological strengths and weaknesses of a variety of options. The JEM used in the main analyses in the next two chapters therefore uses:

- The weighting method for recoding SOC 2000 to SOC90;
- 135 occupational groups (sensitivity: 181 industry-occupation subgroups);
- Mean values in each occupation (sensitivity: Empirical Bayes estimates);
- Occupation only (sensitivity: uses occupation + other covariates);
- Trends across the workforce 1992-2006 (sensitivity: ignores trends / uses more detailed trends within major occupational groups);
- The conventional impute-then-interact method for the interaction of demands*control (sensitivity: interact-then-impute).

Figure 34: Summary of the main Job Exposure Matrix



30. A Map of Job Strain Across Occupations

In the following two chapters, I use these imputed values to look at the relationship between demands-control, WLD and incapacity benefit receipt. Before this, it is helpful to provide a 'map' of how job demands-control varies across British occupations, to 'make concrete' (Karasek and Theorell 1992:40) the range of variation in demands-control that underlies Chapters 6 and 7. I here make a slight change to the JEM used in the following two chapters, in that I use the 119 SOC 2000 occupations rather than 135 SOC90 occupations,[92] and also use the more accurate Empirical Bayes estimates (Appendix 5B) – but essentially the methods are the same.

The occupational map is also interesting in its own right and has been created from other JEMs (Karasek et al 1988:Figure 1) and within the UK for mental health problems (Stansfeld et al 2011), work-related stress (Johnson et al 2005), 'knowledge work' (Brinkley et al 2010) and (on a smaller scale) demands-control itself (Stansfeld et al 2004:24).

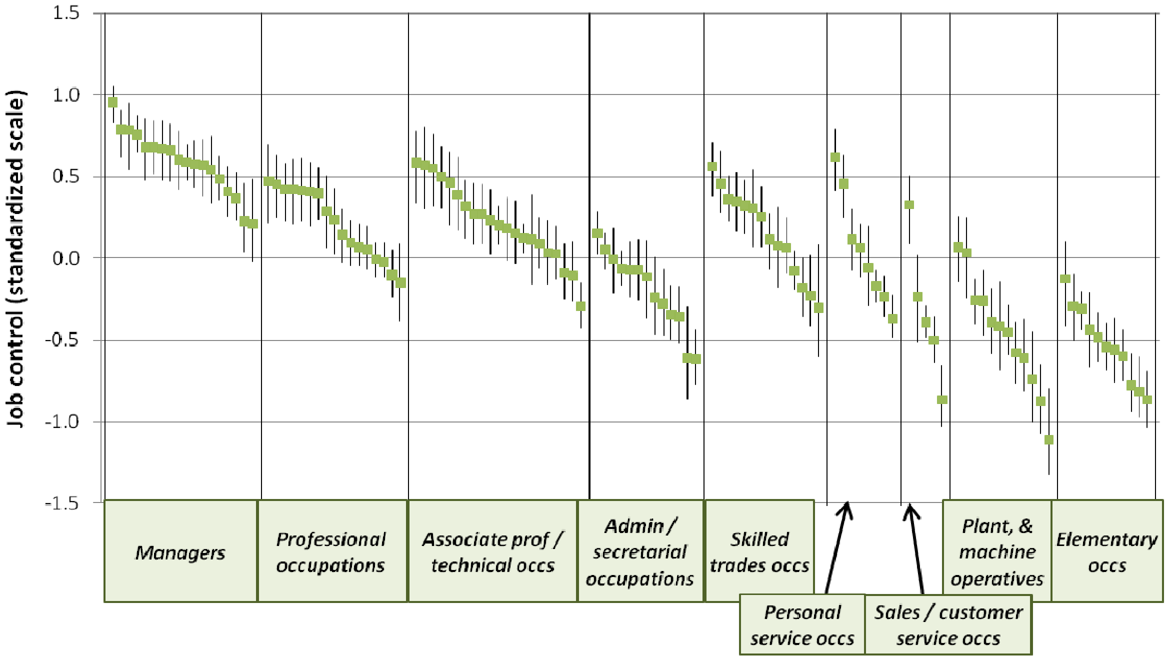
30.1. Control across British occupations

The estimates of job control for each of the 119 occupational groups are shown in Figure 35. The bars each represent a single occupation, and the eight dividing lines divide the occupations into the major SOC2000 groups. For example, the first 18 bars each show an occupation within major group 1 (*'managers and senior officials'*).

There are three main points to take from Figure 35. First, there is considerable variation in control between occupations. The control scale has been standardised so that the difference between 0 and 1 is one standard deviation *at the individual level*. With this in mind, we can see that the differences between occupations can be large – the largest gap between occupations is over 2 standard deviations, and many occupations are separated by around 1 standard deviation.

[92] The contemporary picture is better-described by the updated occupational classification SOC 2000 rather than the older classification SOC90; the analyses in this section are therefore restricted to the Skills Surveys 2001 and 2006. This also allows more fine-grained scales of demands and control to be used, which are described in Appendix 5A.

Figure 35: Control across British occupations 2001-6
Dots show estimates; lines show 95% confidence interval



Source: Skills Surveys 2001 and 2006. Estimated values are Empirical Bayes estimates (see section 29.3.2); confidence intervals are obtained via bootstrapping.

Second, there is an overall gradient across the SOC major groups so that the higher categories (e.g. managers) have greater control than the lower categories (e.g. elementary occupations) – as has been found internationally (Laaksonen et al 2006; Rahkonen et al 2006), and partly reflects the very definition of occupational class (Rose and Pevalin 2005).

Third, there is also considerable variation *within* each major group. For example, at the top-left of *plant and machine operatives*, occupation 810 ('taxi, cab drivers and chauffeurs') has a level of control just above 0. In contrast, at the bottom-right of this group we have occupation 809 ('bus and coach drivers') with a level of control of -1.1. Even within the relatively homogenous major groups like managers (major group 1), we can see statistically significant differences between occupations.

The specific occupations with the lowest and highest levels of control are shown in Table 20. Again, levels of control reflect the position in the occupational hierarchy, but some occupations are noticeably better/worse than other occupations in the same major group. For example, counter clerks (major group 4) have surprisingly low levels of control considering their major occupational group, whereas childminders (major group 6) have surprisingly high levels.

Table 20: Highest and lowest levels of job control in British occupations, 2001-2006

Occupations with *lowest* control	Estimate (95% CI)	Occupations with *highest* control	Estimate (95% CI)
809.Bus and coach drivers	-1.11 (-1.33; -0.80)	118.Mangers & proprietors in services (other)[2]	0.95 (0.83; 1.05)
800.Food, drink & tobacco operatives	-0.88 (-1.07; -0.65)	112.Retail & wholesale managers	0.79 (0.62; 0.91)
709.Retail cashiers	-0.87 (-1.03; -0.66)	119.Shopkeepers + wholesale & retail dealers	0.78 (0.54; 0.95)
904.Postal workers & couriers	-0.86 (-1.04; -0.69)	102.Production & maintenance managers	0.76 (0.65; 0.88)
911.Elementary sales occs	-0.82 (-0.97; -0.61)	111.Storage & distribution managers	0.68 (0.48; 0.86)
902.Process/plant labourers (packers)	-0.78 (-0.94; -0.59)	106.HR and R&D managers	0.68 (0.52; 0.84)
804.Assemblers (electrical/metal)[1]	-0.74 (-1.00; -0.45)	116.Leisure sector managers[3]	0.67 (0.48; 0.84)
404.Counter clerks	-0.62 (-0.77; -0.44)	114.Restaurant & catering managers	0.66 (0.47; 0.83)
801.Process operatives (other)	-0.61 (-0.82; -0.37)	603.Childminders & related occs[4]	0.62 (0.41; 0.79)
408.Telephonists and related occupations	-0.61 (-0.86; -0.30)	199.CEOs, directors and senior officials[5]	0.60 (0.42; 0.78)

[1] *Occupation 804 includes vehicle assemblers*

[2] *Occupation 118 includes inter alia proprietors of garages, hairdressers, and farms*

[3] *Occupation 116 includes hotel & pub managers as well as other leisure sector managers*

[4] *Occupation 603 includes playgroup workers*

[5] *Occupation 199 includes senior officials in local/national govt, plus CEOs/directors in major organisations*

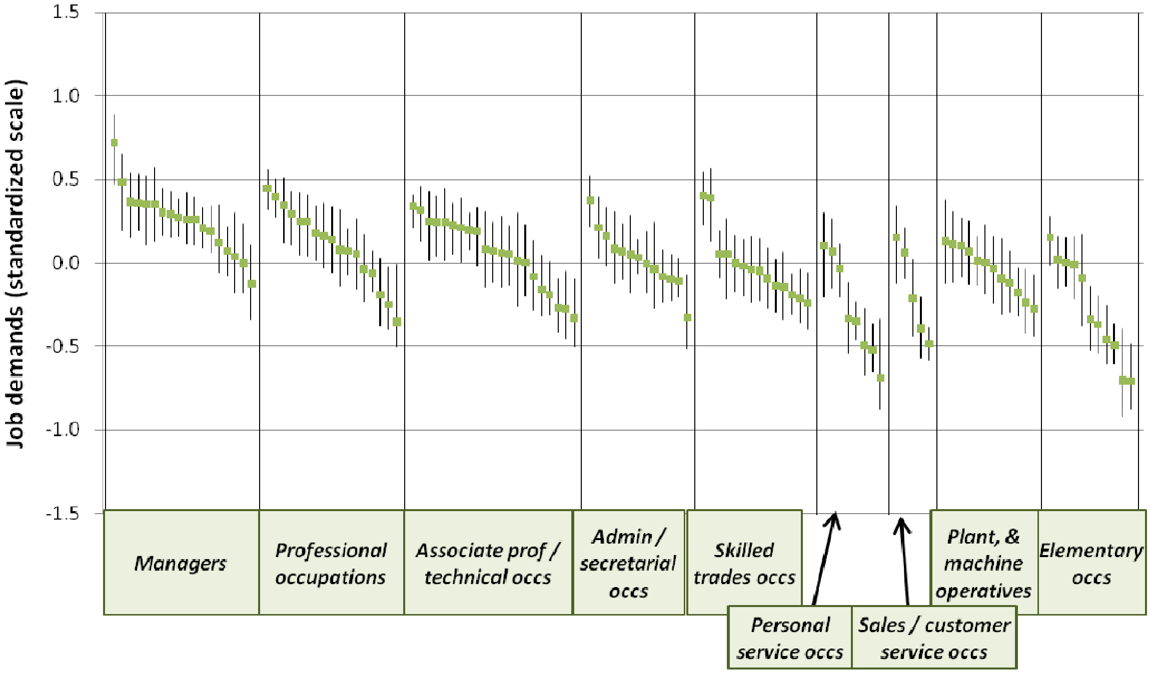
***Source**: Skills Surveys 2001 and 2006. Estimated values are Empirical Bayes estimates (see section 29.3.2); confidence intervals are obtained via bootstrapping.*

It is also worth noting that different occupations in the same sector have very different levels of control – for example, retail cashiers (-0.87) and retail managers (+0.79).

30.2. Demands across British occupations

The equivalent estimates for job demands are shown in Figure 36. This uses the same scale as Figure 35, which demonstrates that the extent of variation between occupations is lower for demands than control; the difference between the most-demanding and least-demanding occupations is now only 1 standard deviation, and 79 of the 119 occupations are within 0.5 standard deviations of each other. Nevertheless, we can still see that there is a rough overall gradient such that jobs become less demanding as we go down the occupational hierarchy. Similar to control, we can also see that there is substantial variation *within* the major occupational groups.

Figure 36: Job demands across British occupations 2001-6
Dots show estimated value; thin lines show 95% confidence interval



Source: Skills Surveys 2001 and 2006. Estimated values are Empirical Bayes estimates (see section 29.3.2); confidence intervals are obtained via bootstrapping.

The specific occupations that have the highest and lowest levels of job demands are shown in Table 21. Compared to job control, this reflects the greater levels of variation within major occupational groups: three of the least-demanding occupations are within major group 6 (childminders, educational assistants and nursery nurses), while two of the most demanding occupations are in major group 5 (chefs & cooks; printers & bookbinders) and another is major group 4 (medical, legal and school secretaries). We must bear in mind that demands refers only to *quantitative* demands (working hard/at high speed/to tight deadlines/under tension); jobs with low quantitative demands may be demanding in other ways (cognitively/emotionally/physically etc).

Table 21: Highest and lowest levels of job demands in British occupations, 2001-2006

Occupations with ***lowest*** demands	Estimate (95% CI)	Occupations with ***highest*** demands	Estimate (95% CI)
910.Elementary security occs (other)[1]	-0.71 (-0.87; -0.49)	114.Restaurant & catering managers	0.72 (0.47; 0.89)
908.Security guards	-0.70 (-0.92; -0.40)	103.Managers in construction	0.49 (0.19; 0.65)
603.Childminders & related occs (inc. playgroup workers)	-0.69 (-0.87; -0.34)	210.Secondary school teachers[3]	0.45 (0.32; 0.56)
604.Educational assistants	-0.52 (-0.65; -0.37)	512.Chefs & cooks	0.40 (0.23; 0.54)
602.Nursery nurses	-0.49 (-0.67; -0.28)	211.Primary/nursery school teachers	0.40 (0.28; 0.50)
907.Cleaners	-0.49 (-0.61; -0.37)	510.Printers & bookbinders	0.39 (0.13; 0.57)
708.Sales & retail assistants	-0.49 (-0.58; -0.39)	411.Medical, legal & school secretaries	0.38 (0.22; 0.52)
901.Labourers in construction	-0.46 (-0.61; -0.25)	111.Storage and distribution managers	0.37 (0.15; 0.54)
709.Retail cashiers	-0.39 (-0.57; -0.20)	113.Managers nec	0.36 (0.19; 0.53)
905.Bar staff	-0.37 (-0.54; -0.20)	109.Financial institution managers	0.35 (0.11; 0.53)

[1] *Occupation 910 primarily includes school mid-day assistants*

[3] *Occupation 210 includes SEN teaching professionals*

[4] *Occupation 113 includes managers in health/social care and police/security*

Source*: Skills Surveys 2001 and 2006. Estimated values are Empirical Bayes estimates (see section 29.3.2); confidence intervals are obtained via bootstrapping.*

A small number of occupations feature in both Table 20 and Table 21. Retail cashiers (#709) have low control and low demands; childminders have high control and low demands; and managers in storage & distribution (#111) and restaurants & catering (#114) have high control and high demands. The next section turns to a more systematic analysis of the *combination* of different levels of demands and control.

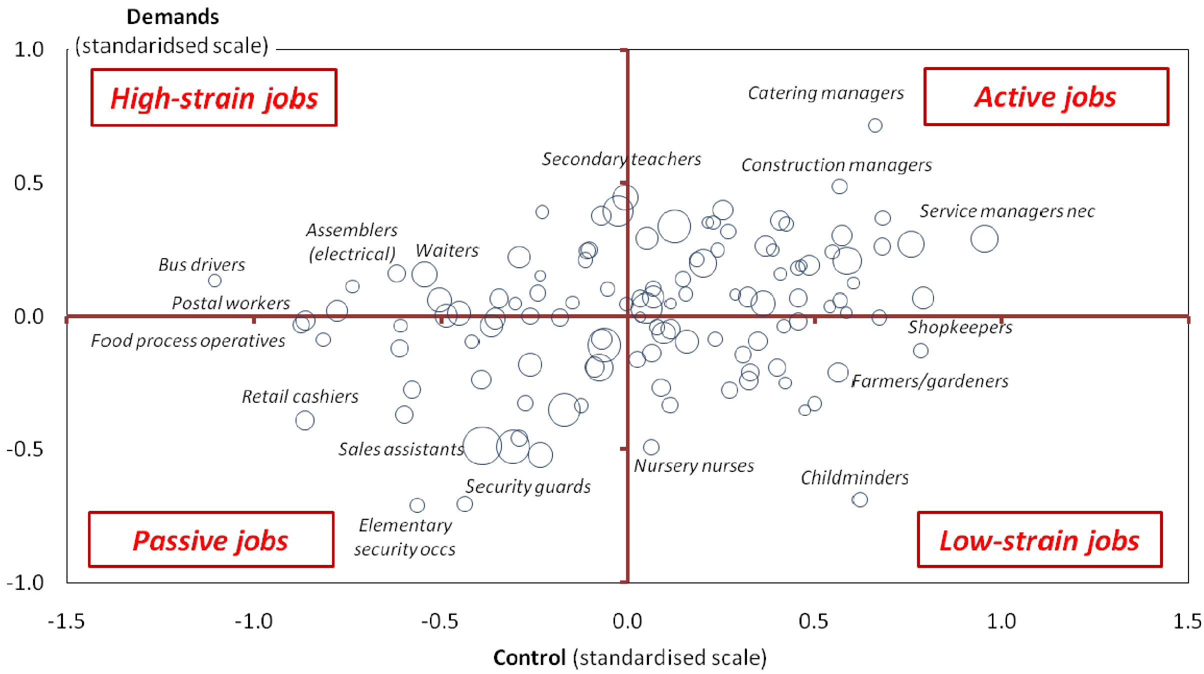
30.3. Results for job strain

The joint distribution of demands and control across occupations is shown in Figure 37. The size of each circle shows the number of people in that occupation in the Skills Survey sample; the position shows the level of demands and control.

It is possible to see on Figure 37 the positive correlation between demands and control (r=0.30) at the occupational level, which is much stronger than at the individual level (r=0.06). Nevertheless, a number of occupations fall into the off-diagonal quadrants of the chart that contain low-strain and particularly high-strain jobs. Selected occupations are labelled on Figure 37, revealing that farmers/gardeners, shopkeepers, nursery nurses and childminders are particularly low-strain occupations; whereas secondary teachers, waiters/catering assistants, assemblers of electrical products, and bus drivers are more high-strain occupations.[93]

Figure 37: The joint distribution of demands-control across occupations, 2001-2006

The size of the circle shows the prevalence of that particular occupation



Source: Skills Surveys 2001 and 2006. Demands and control estimates are taken from Figure 35 and Figure 36.

Yet although this shows the relationship of the *average* levels of demands and control in occupations, it shows nothing about the combination of high demands and low control at the *individual* level, as I described in section 29.2. To illustrate this gap, I classified occupations as high-strain on the aggregate level according to Figure 37, and then compared this to the occupations that had the highest-proportion of high-strain jobs[94] at the individual level. The results confirm that the analyses at the aggregate level can be misleading; not only are the majority of people in 'high-strain occupations' not actually in high-strain jobs,[95] but the ranking of the different occupations also

[93] Bear in mind that the designation of a job as 'high-strain' throughout this section is somewhat arbitrary – Figure 3 splits jobs at their mean value of demands and control (i.e. zero on the standardized scales).

[94] Defined as in Figure 37 as above-median demands and below-median control.

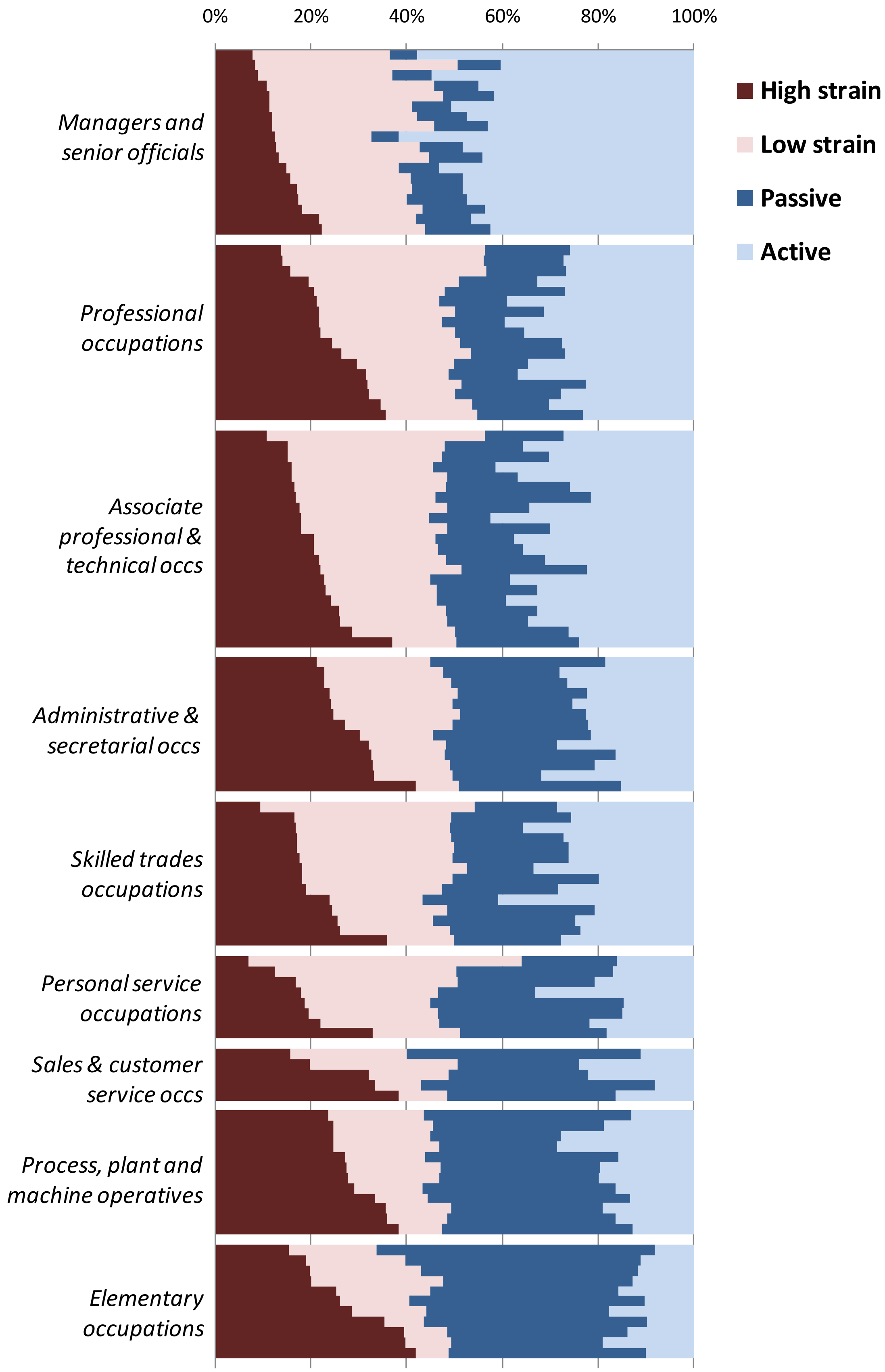
[95] Aggregate high-strain occupations are more likely to contain *individuals* with high-strain jobs, but a majority of people are still *not* in high-strain jobs (33% are high-strain, 16-28% in the other three categories). The same is true for aggregate low-strain, passive, and active occupations, where only 35-40% of individuals have the type of jobs that their occupation is classified as at a whole (e.g. around 35% of individuals in jobs that are low-strain at the aggregate level actually have low-strain jobs themselves).

changes slightly.[96] In fact, the occupation with the second-highest level of high-strain jobs at the individual level is estimated to be shelf fillers (#911), which is not classed as 'high-strain' at the aggregate level at all.

The four Karasek quadrants of job strain are shown in Figure 38 (analogous to Figure 35). To a certain degree we can make the same two observations about job strain that we have made for demands-control previously: there is a gradient as we go down the occupational hierarchy, and also substantial variation within major groups. Yet the pattern in Figure 38 is more complex than this; the occupational gradation is very consistent in active and passive jobs, but less visible for high-strain jobs where there is effectively no gradient outside of the lower job strain of managers/professionals. There is therefore much more within-major-occupational-group variation in job strain than active vs. passive jobs.

[96] For example, the 10th greatest proportion of high-strain jobs is estimated to be 36% among assemblers and routine operatives (#805). Goods-handling & storage occupations (#903) have similar aggregate levels of demands and control (-0.5 and 0.0 respectively), but only 29% of workers are estimated to be in high-strain jobs.

Figure 38: Job strain across British occupations, 2001-2006



30.3.1. Working conditions in the Civil Service

In the following chapters I replicate the WII analyses from Chapters 3 and 4. It therefore helps to know how demands-control in Whitehall civil servants compares to the rest of the population, using the 169 employees in public administration in the public sector in London and the South-East in the 2001/2006 Skills Surveys. Among this subsample, demands and control are both slightly higher than among the rest of the population – but the differences are relatively small, and at least in this sense, Whitehall Civil Servants seem roughly average.

30.4. Job strain and qualifications

We can also use the work data to look at how demands-control relate to qualifications, given that we saw in Chapter 1 that incapacity benefit receipt is primarily a phenomenon of the low-skilled. To do this, I return to the 1992-2006 data described in section 29.6 and Chapter 2. The results of this analysis are shown in Table 22.

Table 22: Levels and changes in demands-control by qualifications 1992-2006

	Control			Demands		
	(m1)	**(m2)**		**(m1)**	**(m2)**	
Qualifications (NVQ equivalent)	Pooled 1992-2006	1992	Change to 2006	Pooled 1992-2006	1992	Change to 2006
Degree (Level 5) – reference category	*ref*	*ref*	*-0.37*	*ref*	*ref*	*0.50*
Professional (e.g. teaching) (Level 4)	0.06†	0.19*	-0.55†	0.09*	0.21*	0.40
A-levels (Level 3)	-0.11**	-0.01	-0.52	-0.12**	0.04	0.33
GCSE A*-C (Level 2)	-0.13***	-0.06	-0.46	-0.13***	-0.02	0.35
Other quals (Level 1)	-0.31***	-0.23*	-0.45	-0.21***	-0.04	0.28
No quals	-0.49***	-0.41***	-0.52	-0.26***	-0.09	0.24*

*Model 1 includes dummies for qualifications and for survey year; model 2 additionally includes the interaction of qualifications and year. Analysis of weighted EiB92 and Skills Survey 2006 data, 20-60 year old employees in GB, n=9390. Key: *** p<0.001, * p<0.05, † p<0.10. Significance levels are calculated compared*

Looking first at the pooled models (m1), we can see that those with no qualifications have both less control (-0.49) and lower demands (-0.26) than those with degrees. The net effect of these combined trends is that there is no association between qualification level and whether a person is in a high-strain job (defined as below-median control and above-median demands; analyses not shown). While I do not present the models here, it is also interesting that the association between qualification level and demands-control almost entirely works through occupation and industry; low-skilled people in any given job have only slightly less control and the same level of demands as high-skilled people in the same occupation.

When we look at the levels of change by qualification level, we can see that those holding a degree-level qualification saw the least decline in control (-0.37 vs. -0.45 to -0.55 for other qualification levels) but the most rise in demands (0.50 vs. 0.24 to 0.40). These differences should not be over-interpreted however, as the set of five interaction terms of education*year as a whole are not significant for either outcome ($F_{5, 9378}$=1.31, p=0.26 for control, $F_{5,9378}$=1.37, p=0.23 for demands).

This brief analysis suggests that job strain is not more common for the low-skilled, that the intensification of work if anything took place to a lesser extent among the low-skilled than the highest-skilled, and that declines in autonomy can be seen for most levels of qualification. For the time being I will place this issue to one side, only to return in Chapter 8 where I develop a conceptual model that explains why job strain may be particularly problematic for the less employable.

30.5. Conclusion

One purpose of this section was to illustrate the occupational differences underlying the imputation technique. Another purpose was to show the distribution of demands and control across British occupations. Higher social classes tend to have higher demands and much higher control, with the net result that job strain shows no social gradient – but that active (high-demand high-control) jobs become replaced by passive (low-demand low-control) jobs as we go down the occupational hierarchy. (The same is likewise true as we move from people with high skills to low skills). Nevertheless, there is still substantial variation between occupations within social classes, which highlights the value of looking at the fine-grained occupational level rather than just at nine broad occupational groups.

This chapter has described how demands and control can be imputed into a longitudinal survey by linking separate surveys of work through detailed occupational codes. It then illustrated the pattern of demands and control across occupations that underlie this imputation. In the following two chapters, I now turn to applying this imputed data to the main research questions of this thesis: the impact of job strain on WLD and incapacity benefit receipt.

31. APPENDICES

This chapter has two Appendices (which appear at the end of the thesis) and four Web Appendices (available from www.benbaumberg.com/thesis.htm):

- **Appendix 5A: Construction of the demands-control scales** – how Item Response Theory models were used to create these scales.
- **Appendix 5B: Empirical Bayes estimates** – methodological details and examples.
- **Web Appendix 5a: bespoke occupational classification based on SOC 2000.**
- **Web Appendix 5b: bespoke occupational classification based on SOC 90.**
- **Web Appendix 5c: industry-occupation cross-classification based on SOC 90.**
- **Web Appendix 5d: bespoke occupational classification for converting SOC 2000 into SOC90.**

CHAPTER 6:
The effect of job strain on work-limiting disability in the British Household Panel Survey

32. INTRODUCTION

The previous chapter outlined a technique to impute data on demands-control into the British Household Panel Survey (BHPS). In this chapter I use this imputed data to examine the effect of job strain on WLD. I begin by describing other features of BHPS and methodological details, before presenting the descriptive statistics and main results. I conclude with a discussion of the limitations, and the key results in the context of the WII results and wider literature.

33. DATA AND METHODOLOGY

33.1. Variables

33.1.1. Work-limiting disability (WLD)

BHPS includes a single-item question of WLD in every wave bar 1999 and 2004, asking *"Does your health limit the type of work or the amount of work you can do?"* From wave 2 BHPS interviewers were also instructed to tell respondents that this "*includes both paid and unpaid work*."

Two other follow-up questions were also asked to those saying they had a WLD: *"Does your health keep you from doing some types of work?"* and *"For work you can do, how much does your health limit the amount of work you can do?"* These are referred to as WLD-type and WLD-amount respectively, and are used in sensitivity analyses.[97]

33.1.2. Other variables used

As for the WII analyses, the regression models here use an extensive range of baseline health and socio-demographic controls – with fewer health controls and more socio-demographic controls being available in BHPS. These variables have been split into five groups:

- *demographic/administrative* (wave, age, gender, ethnicity, marital status, number of children, region);

[97] WLD-amount is recoded into a binary variable, with 1= 'a lot'/'somewhat'.

- *socioeconomic status* (education, log net household income, perceived financial situation, tenure, Cambridge scale of social distance);
- *health* (GHQ caseness (minor psychiatric morbidity), four categories of activity limitations (climbing stairs, walking, dressing, housework), past-year hospitalisation and 13 categories of health problem);
- *work* (industry, sector, temporary job, size of workplace, hours of work, occupational pension); and
- *partner's characteristics* (has partner in household, partner's WLD, partner's employment, partner's incapacity benefit receipt).

I also include physical work demands in many specifications; this is imputed in the same way as demands-control (Appendix 6B). A number of other controls are also used below to explore different aspects of this association. The full list of variables are given in Appendix 6A, with the descriptive statistics available in Web Appendix 6a.

As described in Chapter 3, there are two main pathways from job strain to WLD: a job requirements pathway and a health pathway. I again investigate the extent to which the relationship between strain and later WLD is affected by potentially mediating/confounding variables, including general health, baseline WLD, and other working characteristics.

In BHPS, we potentially observe imputed job strain on 16 different occasions per person for each year 1991-2006 (although in four of these baseline/follow-up WLD is not available). To ensure that I adequately take into account the clustering of these observations, the BHPS analyses use Generalized Estimating Equations ('GEE'). GEEs are different to the cluster-robust models in Chapters 3 and 4, in that as well as estimating the regression coefficients they also estimate a working matrix of the average within-person correlation between waves. They have several advantages over the random effects models that are more commonly found in the sociological literature,[98] and are robust to mis-specification of the between-wave correlations (Ballinger 2004:146).[99] GEEs are particularly suited to situations where the between-wave correlations are a nuisance term (Cui 2007:209), which is the case here.

Aside from this difference, the models are estimated in identical fashion to Chapter 3. I look at how far job strain at a baseline wave affects WLD at the following wave, using a logit GEE model,[100] and taking into account whether a person had a WLD at baseline.

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[98] There are four main advantages of using GEE over random effects (RE) models. First, GEEs are usually estimated as marginal models, which makes it much easier to present the results as average marginal effects. Second, more flexible structures of between-wave correlations are possible in GEE, such as correlations that are greater between closer waves (Ballinger 2004) – whereas RE models assume equal correlations between different time gaps. Third, for inference on a population, random effects models depend on correctly specifying the random effects, which can lead to noticeable biases relative to GEE models in simulations, which causes Hubbard et al (2010:471-472) to argue that GEE models are a 'compelling alternative' to RE models (p474). Finally, GEE models have various practical advantages in Stata: they are faster to estimate, they allow the use of misspecification-robust standard errors, and they allow weights. The main disadvantage is that RE models have greater flexibility in e.g. modelling random slopes (Subramanian and O'Malley 2010) and cross-classified models, they are more robust to data that are missing at random conditional on other covariates, and there are some technical problems with logit GEE models (Sabo and Chaganty 2010). In practice, the results of GEE and RE models tend to be similar, and this is tested in section 35.1.6 along with robustness to missing data.

[99] The main analyses below use an exchangeable correlation structure, assuming a constant person-specific correlation between waves. The estimates are robust to mis-specification of this matrix and a recent guide suggested that 'intensive modelling' of this structure would provide 'negligible' gains (Ziegler and Vens 2010). Nevertheless, I check two other correlation structures in section 35.1.6: an autoregressive(1) model where correlations are only estimated between successive waves; and an unstructured model that estimates the between-wave correlations with no constraints.

[100] The models use the XTLOGIT command with the 'pa' option to use GEE (and also with the 'robust' option; (Cameron and Trivedi 2009)). As before, the probability of reporting a WLD is estimated as the logit function of a linear combination of covariates:

$$\Pr\left(WLD_{i,t+1} = 1 \middle| \mathbf{x_{it}}\right) = \frac{e^{\mathbf{x}'\boldsymbol{\beta}}}{(1 + e^{\mathbf{x}'\boldsymbol{\beta}})} = \text{logit}(\mathbf{x}'\boldsymbol{\beta})$$

...where y_i refers to the outcome variable (the binary measure of WLD), while the vector $\mathbf{x}$ refers to the observed covariates (e.g. job strain) and the vector $\boldsymbol{\beta}$ refers to the unknown parameters to be estimated.

(Footnote continues on following page).

The models include the controls mentioned in Section 33.1.2, measured at the baseline wave alongside job strain.[101] I check the goodness-of-fit of the main model using an adapted form of the tests used in Chapter 3.[102] Ideally I would also account for the clustering of demands-control within particular occupations/years, but the computational burdens made this impractical. However, sensitivity analyses suggest that this makes a minimal difference to the results (Appendix 6A).

Estimation of mediated effects uses the same method as described in Chapter 3. That is, I look at the change in the average marginal effect of job strain when potential mediators are introduced, and bootstrap this to obtain a confidence interval.

33.2.1. Sample selection, weighting, and missing data

The main analyses are a complete case analysis of British employees aged 20 to 59(f)/64(m) who personally provided the required data at both a baseline wave and a follow-up wave, one year later. To make these analyses nationally representative – and also to account for any attrition biases – I would ideally use the weights supplied with BHPS. However, weighting is problematic when looking at pairs of successive waves in panel studies; as Jenkins (2010:13) notes, *"in this case, it is unclear what population of interest the pooled transitions are intended to represent and hence how to calculate suitable longitudinal weights or to combine the weights typically supplied."* Some analyses therefore ignore weights entirely (Cappellari and Jenkins 2008), which will produce unbiased estimates if the weights would simply depend on the observed covariates (Winship and Radbill 1994).

While the regression coefficients in properly-specified unweighted analyses would be unbiased, this would still lead to biases in the average marginal effects, which depend on the prevalence of different types of people in the data. Here, I follow Jenkins' suggestion to complement the main (unweighted) analyses with various sensitivity

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(Footnote 100 continued) The difference to the previous models is that the variance V_i is then defined as (Hardin and Hilbe 2003:58):

$$V_i = \phi A_i^{1/2} R(\alpha) A_i^{1/2}$$

...where $R(\alpha)$ is the working correlation matrix, $A_i^{1/2}$ is a diagonal matrix of standard deviations at each wave, and ϕ is a scale term.

[101] The model is of the form:

$$\Pr\left(WLD_{i,t+1} = 1 \middle| \mathbf{x_{it}}\right) = \text{logit}(\boldsymbol{\beta_1}\mathbf{STR_{it}} + \boldsymbol{\beta_2}\mathbf{DEM_{it}} + \boldsymbol{\beta_3}\mathbf{SES_{it}} + \boldsymbol{\beta_4}\mathbf{H_{it}} + \boldsymbol{\beta_5}\mathbf{WRK_{it}} + \boldsymbol{\beta_6}\mathbf{PRTNR_{it}})$$

where $\mathbf{STR}$ refers to job strain, $\mathbf{DEM}$ refers to a vector of demographic characteristics, $\mathbf{SES}$ refers to a vector of socioeconomic status controls, $\mathbf{H}$ refers to a vector of health controls, $\mathbf{WRK}$ refers to a vector of other workplace controls, and $\mathbf{PRTNR}$ refers to a vector of partner characteristics.

[102] The Hosmer-Lemeshow test cannot be run after GEE models, but it is possible to create a similar version by creating dummies for each decile of predicted probabilities, and testing their joint significance in a model that contains a continuous linear version of the predicted score on the logit scale (Hardin and Hilbe 2003:167). Plots of the predicted probabilities were also checked (Hardin and Hilbe 2003:148; Hedeker and Gibbons 2006:145).

analyses using the supplied weights.[103] I also use multiple imputation on the unweighted data to look at the impact of short-run attrition between the baseline and follow-up waves.

34. DESCRIPTIVE RESULTS

Descriptive results for WLD and job strain are presented in Table 23 and Table 24 respectively, based on the 44,382 person-wave complete case sample.[104]

Table 23 shows that 7.5% of the sample have a WLD at the baseline wave. This is composed of 6.2% of the entire sample who report a limitation with the type of work they can do, and 5.5% who report a limitation with the amount they can do – although the more severe forms of limitation are much rarer. As in the WII study, WLD increases between baseline and follow-up, although this effect is less pronounced as the waves are closer together in BHPS than WII.

Table 23: Descriptive statistics for WLD

	Baseline	Follow-up
WLD (overall)	7.5%	8.2%
WLD: Limited in type of work	6.2%	7.0%
WLD: Limited in amount of work		
A lot	0.6%	1.0%
Somewhat	1.9%	2.2%
Just a little	2.9%	3.0%

n=44,382 for WLD (overall), and 44,362-44,377 for other WLD variables . See text for sample details.

Previous research on the older (50+) working population has shown a moderately large level of change in WLD over time; 29% of those initially reporting a WLD do not report a WLD two years later (Banks and Tetlow 2008:42). The levels of year-on-year change in WLD in the (younger) BHPS sample are even higher, as shown in Table 24. Of those reporting a WLD at baseline, 44.4% do *not* report a WLD one year later. This compares to 4.3% of the larger group of people without a WLD at baseline who develop a WLD between waves. We must remember that the question wording for WLD does not ask about 'longstanding' health conditions (see section 33.1.1), and will therefore capture acute as well as chronic WLDs.

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[103] I do this in two ways: (i) using the longitudinal weights from 2006 for all individuals that respond at every wave 1991-6, excluding the booster samples; and (ii) using the longitudinal weights that incorporate the Scottish and Welsh booster samples, that exclude responses 1991-1998.

[104] This sample includes all observations from British employees aged 20 to 59(f)/64(m), with complete data on the variables in model 7 of Table 26, plus data on perceived financial situation.

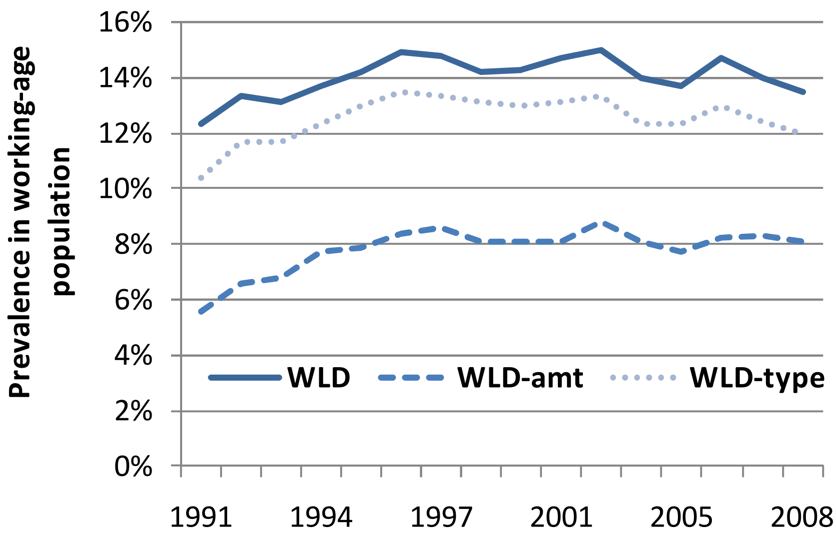
Table 24: Wave-to-wave transitions in WLD

Percentages are within categories of baseline WLD

	Follow-up WLD	
Baseline WLD	No WLD	WLD
No WLD	95.7%	4.3%
WLD	43.8%	56.2%

If we look at the trends in WLD (Figure 39), we can see that there was a rise in WLD 1991-1996, a plateau 1996-2006, and possibly a slight decline more recently. This rise is visible for all three measures of WLD, although it is not on the same scale as the rise from 11% to 16% reported in LFS.[105] There are two further differences from the nationally representative cross-sectional results in Chapter 1; these rises in ill-health are the same in other measures of health (LLSI, poor general health), and no rises in WLD are visible among workers. Interpretation of these trends is complicated by the panel nature of the data, which means that the sample will become less representative as time progresses (Lynn 2006).

Figure 39: Trends in WLD in the BHPS panel



See section 33.1.1 for details of the three measures. WLD-amt is here turned into a binary variable of 'a lot/somewhat' limited. Calculated on the working-age British population (men aged 20-65, women aged 20-60), excluding proxy interviews, and using cross-sectional weights. Total n for WLD is 99,229, consisting of between 5,066 (2008) and 7,138 (1991) observations per year.

Table 11 shows the tertiles of job demands-control and how these relate to the imputed continuous scales. The sample average for job demands and control are predictably close to zero (0.03 and 0.05 respectively), which was the population mean in the work surveys. The tertiles of job control are separated by around 0.4 standard deviations at

[105] WLD rose from 13.2% to 16.6% 1992-1996 in LFS, compared to a rise from 12.3% to 14.9% 1991-1996 in BHPS. The LFS trends are calculated in March, while the BHPS fieldwork period is primarily September-December, hence the years for comparison appear slightly different.

the individual level, while those for demands are separated by around 0.3 standard deviations.

Table 25: Descriptive statistics for job demands and control

		Continuous job demands-control scores			
	n	Mean	Std Dev	Min	Max
Job demands tertile					
Low demands	16162	-0.46	0.14	-1.11	-0.28
Moderate demands	14399	-0.14	0.07	-0.28	-0.01
High demands	13821	0.21	0.17	-0.01	0.65
Job control tertile					
Low control	13388	-0.39	0.22	-1.43	-0.14
Moderate control	14474	0.03	0.09	-0.13	0.18
High control	16520	0.43	0.18	0.18	1.00

Complete case sample, n=44,382.

The main demands-control imputation takes into account the trends 1992-2006. This means that the prevalence of the low-control tertile rises from 13% to 41% 1991-2006, and the high-demands category rises from 12% to 50%. In the sensitivity analyses, I test the robustness of the results to imputations based on occupation but not survey year, where the prevalence of each category of demands-control is more stable across years.

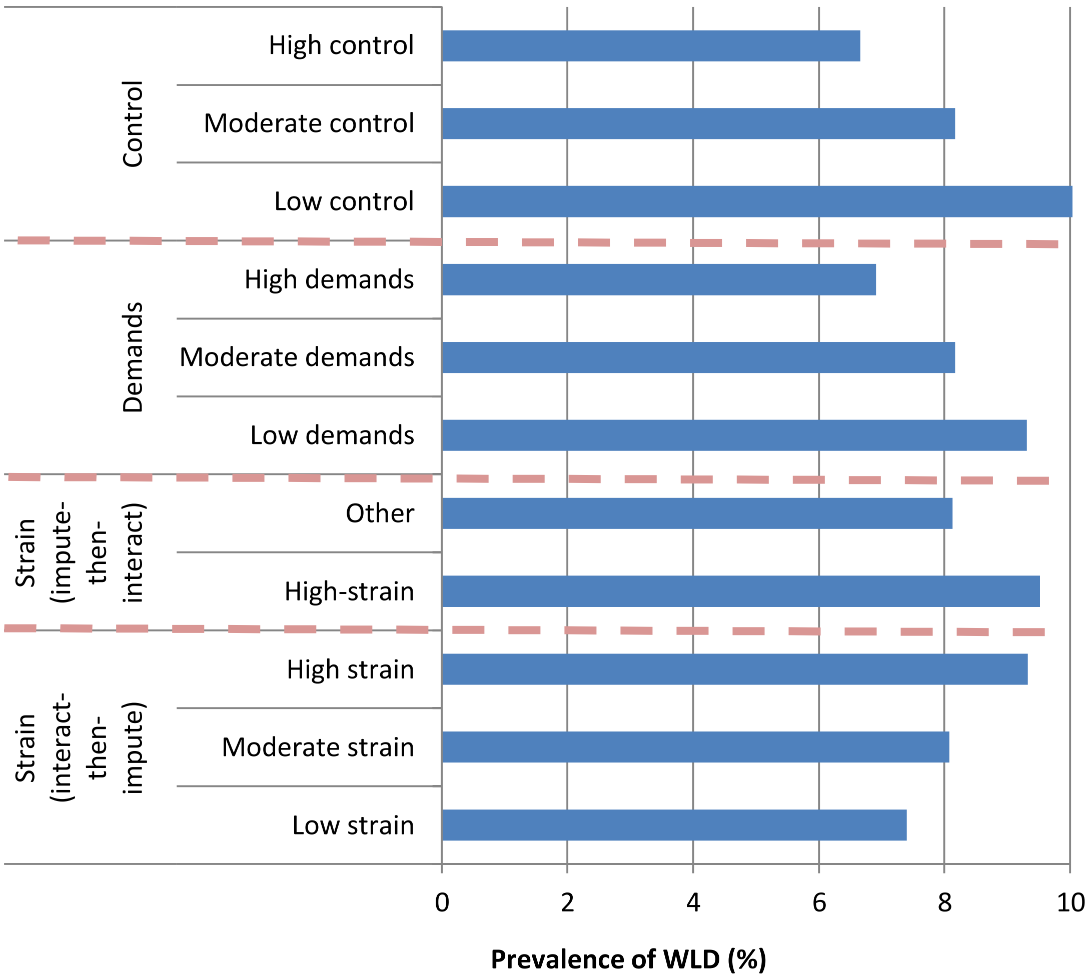
The prevalence of impute-then-interact job strain (highest tertile of demands*lowest tertile of control) is 5.0%, which is substantially lower than the 11.0% reported at the individual level in WII. This does not mean that job strain is higher in Whitehall civil servants than in the wider population, but rather reflects (i) the different scales that are used; (ii) the relative nature of tertile-based demands-control measures; and (iii) estimating strain on the occupational rather than the individual level (Chapter 5).

34.1.1. Bivariate associations of job strain and WLD

I hypothesised that WLD at follow-up would be higher for those in high-demands, low-control jobs. However, from Figure 40 we can see that this is not the case for the simple, unadjusted relationship. Low-control jobs lead to higher-WLD – but it is low demands rather than high demands that has a noticeably higher risk of WLD. Overall, high-strain jobs have slightly higher levels of WLD whether measured in the normal impute-then-interact way or with the revised interact-then-impute method.

Such relationships may be due to confounding – low demands and low control are both more common in more disadvantaged groups, and wider societal disadvantage is likely to influence WLD. We therefore need to investigate the relationship between job strain and WLD after taking into account these likely confounding factors.

Figure 40: Unadjusted association of baseline demands-control with WLD at follow-up



Complete case sample, n=44,382.

35. REGRESSION RESULTS

While a fuller comparison between the BHPS and WII results will be undertaken in the Discussion and Chapter 9, it is helpful to recap the main WII findings here:

- Job strain at baseline led to a significantly higher risk of reporting a WLD at follow-up.
- This relationship was primarily due to the effect of job demands and the interaction of demands and control, rather than control alone.
- The effect of job strain on WLD occurred primarily through the job requirements pathway.

Within BHPS, the effects of demands-control after controlling for different groups of confounders are shown in Table 26. As for the first tables in the previous chapters, these figures are odds ratios where coefficients <1 indicate a reduced risk and >1 an increased risk.

In the empty model with no covariates, we can see that there is a strong, statistically significantly reduced ($p<0.001$) risk of WLD for those with high control, but no effect of job demands. In models 2-6 the effect of job control is similar (if slightly attenuated) after taking account of a wide variety of possible confounders. This picture changes more for job demands, however, where there is an apparently protective effect after adjusting for age, gender and baseline WLD (model 2) that is maintained through models 3-6.

It is interesting that both demands and control appear to have nonlinear effects in all of these models – the contrast is between high vs. low+moderate demands, and low vs. high+moderate control, rather than showing a dose-response relationship across the tertiles. In both cases, though, there is then an abrupt change in model 7 where both demands and control have smaller effects that are no longer significant at the 5% level.

Table 26: Regression of WLD on baseline job strain [odds ratios]

	[1] Empty	[2] Initial	[3] + Waves	[4] + Health	[5] +Work	[6] + Partner	[7] + SES/ Phys.
Low control (base)							
Moderate control	0.83***	0.87**	0.86**	0.85**	0.84***	0.84**	0.90+
High control	0.73***	0.81***	0.80***	0.80***	0.79***	0.80***	0.91
Low demands (base)							
Moderate demands	1.04	0.94	0.96	0.96	0.98	0.99	1.03
High demands	0.94	0.78***	0.81***	0.79***	0.81**	0.81**	0.88+
N	*44382*	*44382*	*44382*	*44382*	*44382*	*44382*	*44382*
Num persons	*10459*	*10459*	*10459*	*10459*	*10459*	*10459*	*10459*

The control variables included in the models are:

Initial (model 2): baseline WLD, age group (five-year bands), gender, marital status (married/never married/other), number of children (0/1/2/3+), black and ethnic minority status, region (10 dummies)

Waves (model 3): model 2 + dummies for each wave of BHPS

Health (model 4): model 3 + 13 categories of longstanding illness, GHQ caseness, 4 categories of activity limitation, and past-year hospitalisation

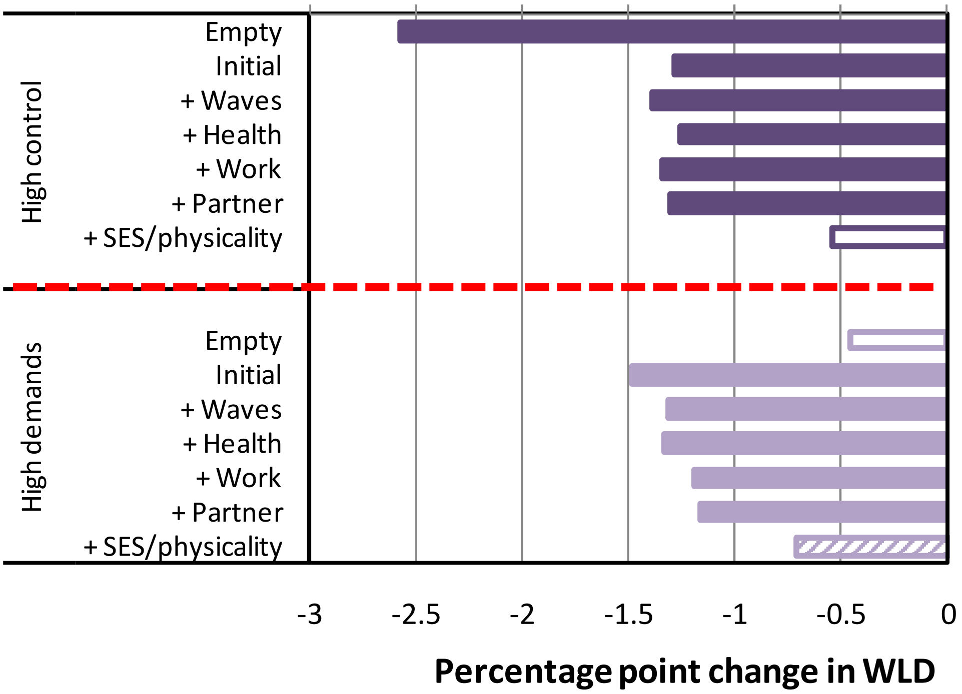
Work (model 5): model 4 + hours of work (four groups), member of occupational pension scheme, sector (public/private/nonprofit), temporary job, size of workplace (grouped), industry (1-digit SIC92, 15 groups)

Partner (model 6): as model 5, + whether cohabiting; partner's incapacity benefits receipt; partner's employment status; partner's WLD

SES/physicality (model 7): model 6 + qualifications (six groups); occupational status (Cambridge scale); income (log household net income); tenure (three groups); physical job demands (tertiles)

For reasons given in Chapter 3, it is better to look at these results as average marginal effects (AMEs) rather than odds ratios. This produces the results in Figure 41 – which illustrates that the effects of demands and particularly control are robust to a series of adjustments, but are sharply attenuated by controls for SES.

Figure 41: Estimated effect of high (vs. low) job demands-control on later WLD



Average incidence of WLD at follow-up is 8.0%. Solid bars are significant at the 5% level; bars shaded with hashes are significant at the 10% level; empty bars are not significant. Estimates are population-averaged marginal effects, based on the same sample and control variables as Table 26.

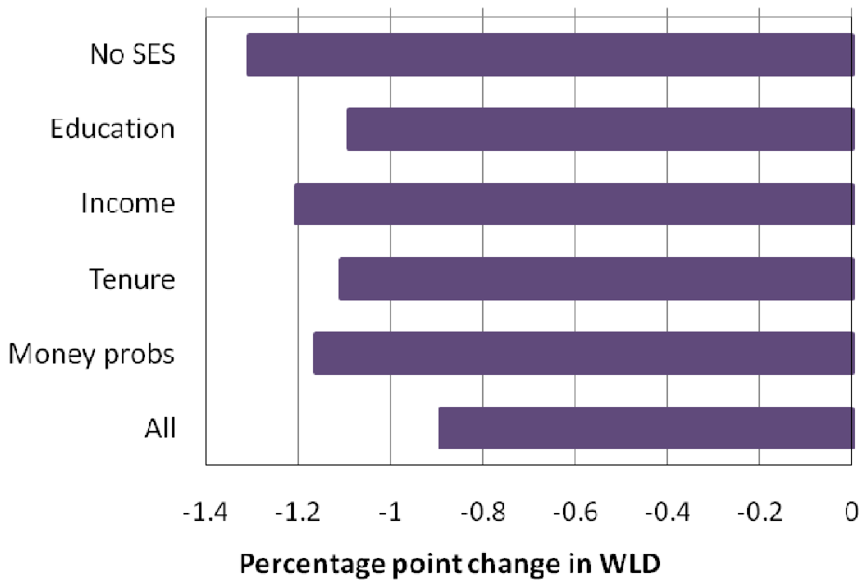
35.1.1. SES: methodological problem or substantive confounder?

This raises the question: what is it about SES that appears to confound the relationship between demands-control and WLD?

I initially split the SES measures controls by whether they were measured on the individual or occupational level. (For example, social status is on the occupational level as it is based on the respondent's job, while education is on the individual level).

Looking first at the individual-level SES controls (education, tenure, income, and financial concerns), the effect of demands and control are robust to all of these measures as shown in Figure 42. Even when included all four SES control simultaneously, the effect on WLD remains strong (attenuated from 1.3 to 0.9 percentage points) and highly significant (p=0.02). Virtually exactly the same pattern is found for job demands (the effect reducing from 1.2 to 0.9 percentage points of WLD, and still significant p=0.02). The remaining models in this chapter include individual-level controls for log income, tenure and education.

Figure 42: Effect of individual-level SES controls on the control-WLD relationship



Average incidence of WLD at follow-up is 8.0%. All effects are significant at the 5% level. Models are based on model 6 in Table 26, additionally including (in turn) education, log net household income, housing tenure, and financial worries.

When we control for seven different measures of occupation-level SES,[106] however, we find that high control becomes non-significant for each measure. All of these attenuate control by at least as much as status – the measure included in Figure 41 – and sometimes more, with the strongest attenuation being for one-digit occupational group (Web Table 6.SESB). Moderate control has a similarly protective effect to high control in all these models however, and is still significant at the 5% level in two cases (NS-SEC, one status measure) and the 10% level in three others (all status measures). This attenuating effect is not as strong for job demands, which generally remains significant at the 10% level.[107]

There is therefore a striking pattern, whereby the effects of demands-control are strongly robust to all individual-level SES controls, but attenuated below conventional levels of significance for all occupation-level SES controls. ***This strongly suggests a methodological rather than substantive explanation.*** In Chapter 5, I explained that the JEM measures of job control are based on the average level of job control in each occupation. While there were some differences between occupations within the same major occupational group (e.g. between taxi drivers and bus drivers), the greatest

[106] Five of these are different measures of occupational status – including the Cambridge scale (Prandy 1990), which is the status measure included in the main models in Figure 41 above. The two other measures are social class (NS-SEC) and one-digit occupational group (SOC90). See Appendix 6A for details.
[107] For one of the status measures it is significant at the 5% level, and for NS-SEC it becomes entirely non-significant.

differences were between major groups (e.g. between managers and machine operatives).

This means that when we control for occupation-level SES, we substantially reduce the range of variation in demands-control (because we are only looking within occupational groups) and increase the level of measurement error.[108] In effect, we are trying to look at the effect of one measure of job demands-control after controlling for another, more powerful measure of the same job characteristics (occupational groups were partly designed to capture job control; see Rose and Pevalin 2005)[109] – and it is therefore retrospectively unsurprising that we find null results.

One way of dealing with this is to exclude occupation-level measures of SES for the rest of the analyses. The crucial question is then: are the occupation-level SES controls capturing any other confounders that are not captured by the individual-level measures of SES?

Occupation-level job control may be a proxy for social status (Sacker et al 2001:764), and I specifically used social status as one of the occupation-level SES controls. But we can have some confidence that there is no confounding due to cultural/status factors in this case, as the attenuating effect of education – conventionally taken to be a strong marker of status – was minimal. Similarly, the attenuating effect of income and money worries was small.[110] Given that income and education cover arguably the two central aspects of class (Macleod and Davey Smith 2003), it could be argued that occupation-level SES is simply serving as a proxy for control and (to a lesser extent) demands.

Yet the assumption that occupation-level SES *only* captures job demands and job control (conditional on education, income and tenure) is a strong one. Davey Smith and colleagues have argued that *"it is probable that many, apparently plausible associations between psychosocial exposures and health, reported in the literature are..the product of confounding"* by SES (Macleod et al 2001), and many job characteristics are patterned by occupational grade (Kristensen et al 2002). A study of sickness absence in Whitehall II found effects of demands-control that were strongly

[108] These two points combine with one another: when we control for major occupational groups we are relying on within-major-group differences. These differences often rely on relatively small numbers of observations, so levels of measurement error will become more significant relative to the size of the differences in control between occupations.

[109] While the NS-SEC was theoretically linked to job autonomy, only control over working hours (flexitime) was included in the final version. A 'service relationship' was taken to be indicated by "salary payments, the presence of an incremental scale, a longer period of notice and a high degree of autonomy" (p47).

[110] Income is both variable and measured with a substantial degree of error, and occupation-based measures of SES may therefore partly capture income and/or wealth. However, the fact that controlling for income *and* financial worries makes such a little difference to the results suggests that this residual confounding is minor.

attenuated by grade but not by education, housing tenure or car access (North et al 1996:336).

I therefore investigated whether other occupation-level factors might be responsible for the apparent association of job control and WLD. First, I imputed physical work demands from the JEM using the same methods as for imputing demands-control (Appendix 6B). However, physical demands do not influence WLD, and the effects of demands-control are effectively unchanged when physicality is included in the model (Web Table 6.Main). Following Karasek's recommendations (1998:348), physicality is included in the rest of the models in this chapter.

Second, I looked at respondents' reports of promotion prospects, overall job satisfaction, satisfaction with job security, and satisfaction with pay. None of these factors attenuated the relationships to any noticeable degree (Web Table 6.SEScontrols).

Third, I looked at respondents' satisfaction with their manager and use of initiative at work. I looked at these variables separately as they were only available at a subset of BHPS waves (21,735 rather than 44,382 observations). The attenuating effect of these two variables was non-existent, with the association of control and WLD even being slightly increased (Web Table 6.SEScontrols).

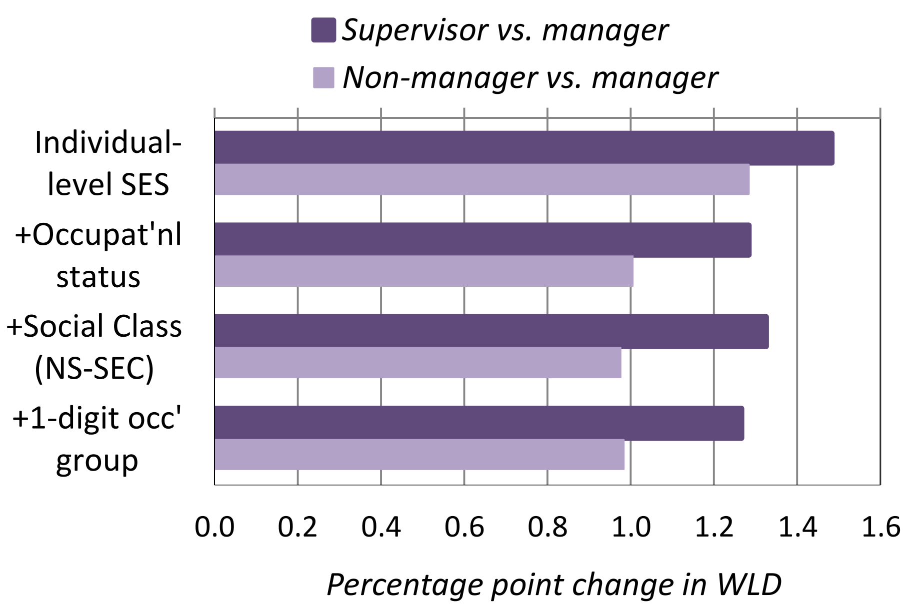
Fourth, I looked at two broader attitudinal measures that could be linked with occupation: whether individuals self-define as working-class or middle-class, and the extent to which they value fulfilling work. Again, these were only available at certain waves, and even excluding these variables the effect of job control is slightly different in these different subsamples. Nevertheless, as for every other individual-level measure of SES, these had at most small effects on the relationship between job control and WLD (Web Table 6.SEScontrols).[111]

Finally, the closest measure to job control available on the individual level in BHPS is whether the respondent is a manager, supervisor/foreman or non-supervisor. To the extent that this is a proxy for job control, we would expect to see a similar effect of supervisory status on WLD (past BHPS research suggesting that supervisory status has no impact on health itself; Jones et al 2010). Moreover, because this is an individual-level measure, we should be able to adjust for occupational-level measures of SES and still see a significant relationship, thereby testing empirically whether control is simply serving as a marker of occupational status (Macleod 2000).

[111] In the case of self-reported class there was effectively no change (both with and without class: AME=0.7%, p<0.25 for control, AME=0.3% p=0.59 for demands), while there was a slight attenuating effect if I additionally control for valuing work (for control, AME=0.83%, p=0.22 without controlling for valuing work, AME=0.76%, p=0.25 after controlling for it; for demands, AME=0.70% vs. 0.69%, p=0.28 vs. p=0.29).

These predictions are tested in Figure 43. We can first see that after controlling for individual-level SES (and the extensive array of other covariates), managerial status does indeed have a moderate effect on WLD, with non-managers having a 1.3 percentage point greater risk of WLD at follow-up. The difference between managers and supervisors is also significant; indeed, if anything it is slightly (and non-significantly) greater than the difference between managers and non-managers. It has been argued that supervisors are in a particularly difficult situation at work, *"subjected both to the demands of upper management to discipline the workforce and the antagonism of subordinate workers"* (Muntaner et al 1998, cited by Landsbergis 2010), which Olin Wright described as a 'contradictory class position' (Vanroelen et al 2010:961).

Figure 43: Effect of managerial status on WLD by follow-up



Average incidence of WLD at follow-up is 8.0%. All effects are significant at the 5% level. 'Individual-level SES' model is model 7 in Table 26 excluding occupation-level 'status'. The additional models include for (in turn) occupational status (Cambridge scale), NS-SEC (social class, measured by the National Statistics Socio-Economic Classification), and one-digit occupation (SOC90).

In these models, controlling for occupation-level SES only has a slight-to-moderate attenuating effect – despite the strong relationship between managerial status and occupational status, NS-SEC and major occupational group. Managerial status remains strongly significant ($p<0.05$ for non-managers, $p<0.01$ for supervisors) throughout. This provides further support that the attenuating effect of occupation-level SES for the imputed measures of demands-control is a methodological rather than substantive finding.

In conclusion, this section began by observing that there was an apparent effect of job control and job demands on WLD – with the effect of job control being as predicted, but high job demands leading to *less* WLD. However, these effects became smaller and non-significant at conventional levels when I controlled for occupation-level measures of SES like occupational class or status.

I have argued that controlling for such occupation-level measures of SES is inappropriate when using data imputed from occupations, as it leads to attenuation biases. I then presented a large variety of analyses that suggest that there is no confounding by any of the other possible measures that occupation-level SES is proxying for. The effect of control is robust to (i) physical job demands of that occupation; (ii) individual-level measures of SES such as income and education; (iii) other workplace characteristics, including opportunities for promotion and multiple aspects of job satisfaction; (iv) self-identified social class; and (v) the value the respondent places on fulfilling work. I also showed that managerial status – a proxy for job control – significantly affects WLD, even after controlling for occupational-level SES measures.

For the rest of this chapter, I therefore use the main three individual-level SES controls (education, tenure and income) and physicality, but do not use the generic occupation-level SES controls. This still goes far beyond the level of controlling for SES in most JEM studies, as I describe in the Discussion below when considering the evidence for causality.

35.1.2. Strain and other coefficients

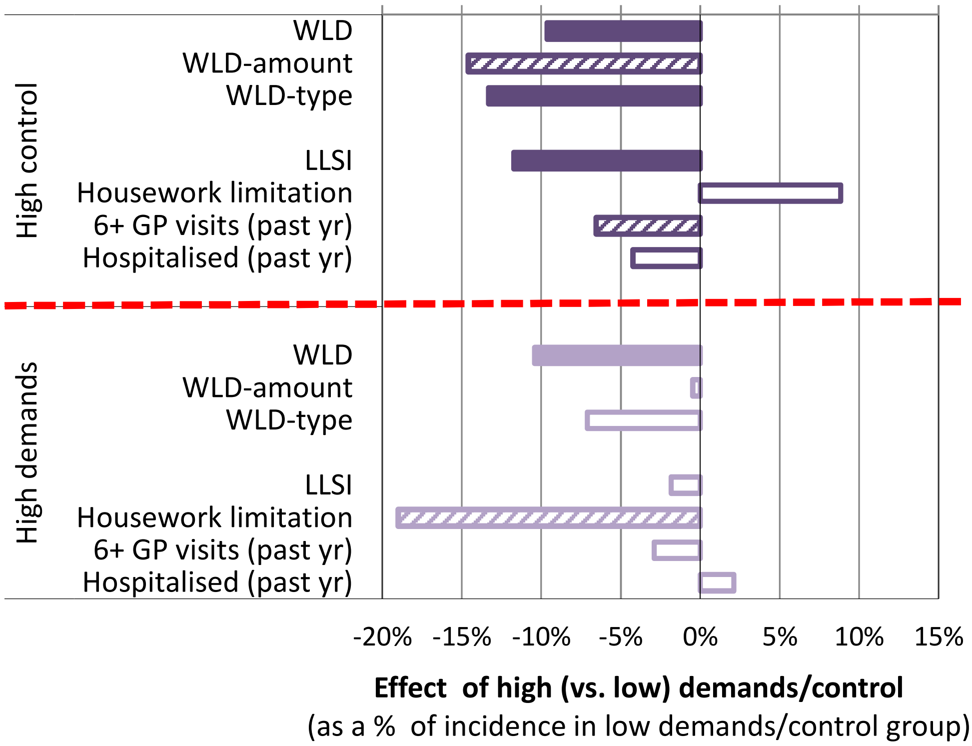
Unlike in Chapter 3, the models above do not include a term for the low control*high demands interaction. This is because the interaction term was not significant whether I use the conventional impute-then-interact measure (OR=1.13, 95% CI 0.89-1.43) or the interact-then-impute measure (highest vs. lowest tertile OR=1.01, 95% CI 0.85-1.20), nor were these significant when demands and control themselves were dropped (Web Table 6.CDlin). For the remainder of the results in this chapter, I therefore do not consider the interaction of demands and control.

The coefficients on the other covariates in the model with physicality, education, income and housing tenure are presented in Web Table 6.covariates. This shows that WLD is higher for those with a baseline WLD, who are older, who have no or 2-3 children (vs. one child), in certain regions (North-East, East Midlands, Wales vs. London), in some years (1991 vs. 1992/1997/2001-6), with lower income, in social housing, in some industries (electricity/gas/water supply having a higher risk than the reference category health/social work, with lower risks in manufacturing, construction and finance), with most types of health problems (all except alcohol/drug problems), with most types of activity limitations (all except getting dressed), who have been hospitalised in the past year, with minor psychiatric morbidity, and whose partner reports a WLD.

35.1.3. Specificity

A further test of whether the association of demands-control and WLD is due to unmeasured confounding is whether it is ‘specific’ to fitness-for-work-related outcomes. As in Chapter 3, I expected high demands and low control to possibly increase ill-health, but to have greater effects on WLD reflecting the additional job requirements pathway. I therefore conduct similar analyses for two other measures of WLD and for four other health outcomes, with the results shown in Figure 44.

Figure 44: Specificity of job demands-control effects to WLD outcomes



Average incidence of outcomes are 8.0% (WLD), 3.3% (WLD-amount), 6.8% (WLD-type), 6.8% (LLSI, 'limiting longstanding illness' – see Chapter 1), 1.6% (housework limitation), 12.4% (3+ GP visits), and 5.9% (hospitalized). Solid bars are significant at the 5% level; bars shaded with hashes are significant at the 10% level; empty bars are not significant. Estimates are based on the same sample and control variables as model 7 ('full') in Table 26 but excluding the occupation-level covariate 'status'. n=44,382 for WLD and 44,328-44,831 for other outcomes.

Job control has a significant effect on two of the three WLD outcomes at the 5% level, and the other outcome at the 10% level.[112] In contrast, it affects only one of the other health outcomes (and a second at the 10% significance level),[113] and the health variable that is affected is the very one that is closest to WLD (activity-limiting health problems; 55% of people with a WLD report a LLSI, compared to only 2% of those without a WLD). The effect of job control is therefore partly specific to WLD measures, providing suggestive evidence for a health-independent relationship of control and WLD.

The evidence for the protective effect of high demands on WLD, however, is weaker. While it has an effect on the main overall measure of WLD, it has weaker and non-significant effects on the two other measures of WLD.[114] Moreover, the largest protective effect of job demands is on housework-limiting health problems (although

[112] If I treat WLD-amount as an ordinal variable, job control is significant ($p<0.01$).

[113] If I treat frequency of GP visits as an ordinal variable, control is significant ($p<0.05$).

[114] If I treat WLD-amount as an ordinal variable, high demands marginally significantly ($p<0.10$) *raises* (sic) the level of WLD, for reasons that are unclear.

this is not significant), an outcome entirely unrelated to work. Although the effects of demands in general are stronger for WLD than for health-related measures, this raises questions as to whether demands has a causal effect, as I discuss below.

35.1.4. Pathways between job demands-control and WLD

As described in Chapter 3, there are two potential pathways between job demands-control and WLD: a health pathway, and a job requirements pathway. Unfortunately we have to assume that baseline health variables are either confounders or mediators of this relationship, when *a priori* there are variables that could be both. I therefore check whether the results are different when we make different assumptions about two possible confounders/mediators (Web Table 6.spec):

- If we remove the controls for baseline WLD, then the results are unchanged.
- If we add controls for baseline general health, then there are minor changes – the significance of high control was pushed fractionally above the 5% level – but the size of effect was similar.

This suggests that neither general health nor baseline WLD are important confounders *or* mediators of the relationship of demands-control with WLD at follow-up.

Using the mediation models described in section 33.2.1, I then investigated the relative strengths of the health and job requirements pathways, based on the assumption that we can identify the job requirements pathway through the residual effect of job demands-control on WLD after adjusting for health. The results are shown in Figure 45 and Figure 46.

These show that for both demands and control, the job requirements pathway is statistically significant (the 95% CI excludes zero), while the health pathway is much smaller and further from significance. However, due to the wide confidence intervals the difference between the two pathways is not statistically significant at conventional levels (p=0.3 for control, p=0.4 for demands) – in other words, the job requirements pathway seems to be larger than the health pathway, but there is a greater-than-usual possibility that this is due to chance.

Figure 45: Pathways between job control and WLD
Marker shows bias-corrected estimate; bar shows 95% confidence interval

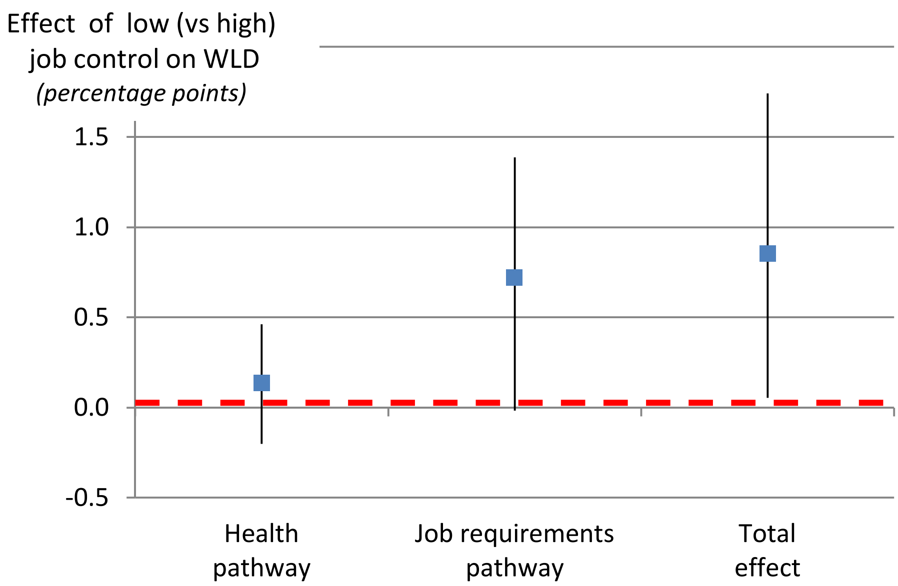
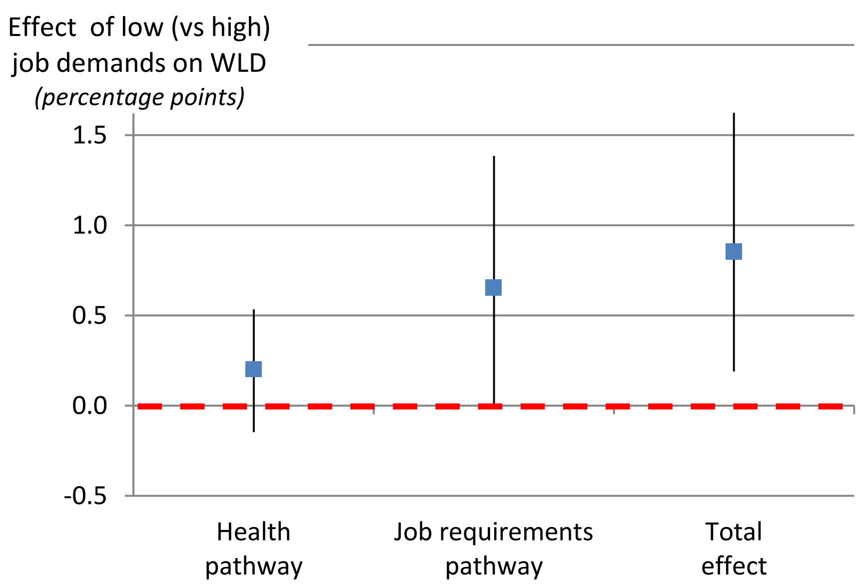


Figure 46: Pathways between job demands and WLD
Marker shows bias-corrected estimate; bar shows 95% confidence interval



Estimates are bias-corrected bootstrap estimates with 200 replications. All models include the controls as per model 7 ('full') in Table 26 but excluding the occupation-level covariate 'status'. Compared to Table 26, this model uses a slightly smaller sample due to missing data in the mediators (n(obs)=43846, n(persons)=10387) and uses bootstrap rather than normal-based estimates of the confidence interval. The mediators included in the health pathway are the 13 categories of longstanding illness, GHQ caseness, housework limitations, dressing limitations, walking limitations and past-year hospitalization at the follow-up wave. The job requirements pathway is the direct effect of job strain on WLD after controlling for the health pathway (see text).

35.1.5. Explaining the trend in WLD

Chapter 2 documented a rise in job strain over the 1992-2006 period. If we apply this to the models here, I estimate that WLD would be 0.3-0.4 percentage points lower in 2006 if job control was unchanged from the 1992 level.[115] This is a small but not negligible change in the actual level of WLD in 2006, which was 7.4%, although this effect will be an underestimate due to attenuation bias (Chapter 5).

However, if I also include the effect of job demands, then there is effectively no change in WLD due to the 1992-2006 trends (the change being less than one-hundredth of a percentage point). This is because the decline in control (which increases WLD) is matched by the increase in demands (which lowers WLD). It is therefore unclear as to whether changing job strain can explain any of the rise in WLD over this period. I return to this issue in the Discussion.

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[115] Model includes controls for physicality, income, education and tenure, and uses linear demands/control (rather than tertiles); see model 1 in Web Table 6.Lin. Estimates are for estimated WLD in the 2006 sample at the average control in 2006, minus the equivalent estimate at the average control in 1992.

35.1.6. Sensitivity analyses

A number of other sensitivity analyses were conducted to test the robustness of the findings to various methodological decisions, including different forms of demands and control; looking only among workers; excluding those who are 'temporary sick' at baseline; different estimation techniques (including random effects models); and accounting for the fact that demands-control is observed for occupations rather than individuals. The findings above were robust to all of these (although demands or control variously become significant at the 10% rather than 5% level in some checks); this is summarised in Appendix 6C.

In other sensitivity analyses, however, the results changed. First, when I looked at the impact of demands-control on WLD two years (rather than one year) later to mirror the lag in WII, control becomes strongly significant while there is no apparent effect of demands. Second, when using different imputation methods (Chapter 5), significant results were found for control in 6 of 7 methods but for demands in only 4 of 7. Third, when accounting for missing data, demands remained significant but control became non-significant.

To aid comparisons to the WII results, I conducted the analyses on a subsample close to WII – that is, national government employees under 60.[116] I here found a larger but only marginally significant effect of control, and a stronger and statistically significant protective effect of high demands (Web Table 6.CS). I return to this when comparing the WII and BHPS results in Chapter 9.

Finally, I tested whether the effect was also found for different subgroups (men and women, older and younger workers). No differences were found by age, but the covariates seemed to behave differently for men and women ($p<0.001$). Focusing specifically on demands-control, there was a decreased risk of WLD for high control in men but not women ($AME_{control}$=-1.3%, $p<0.05$ (m) vs. -0.1% (f)) and a decreased risk of WLD for *high* demands in women but not men ($AME_{demands}$= -2.0%, $p<0.001$ (f) vs. +0.2% (m)) – although only in the latter case was the gender *difference* statistically significant ($p=0.17$ for control, $p<0.01$ for demands).

I consider the implications of these many sensitivity analyses in the Discussion below.

116 This leaves a subsample of 1937 observations (632 people). More specific subsamples limited to London-based employees aged 40-60 were simply too small. Physical demands was excluded from these analyses, as in Chapter 3.

36. DISCUSSION

36.1. Limitations

There are several limitations of the analyses in this chapter that I discuss when interpreting the main findings below: the assumptions necessary to give these results a causal interpretation, the difficulty of dealing with attrition biases, and problems inherent in the imputation method (attenuation biases and low power). Furthermore, while covering major condition types and activity limitations, the health measures are cruder than in WII.

It is also worth reiterating that the WLD measure in BHPS is not ideal. A minor problem is that respondents (from wave 2 onwards) were instructed to consider both paid and unpaid work, and this could lead to inflated WLD-reporting among those out-of-work – although 'unpaid work' is more similar to paid work than the 'other regular daily activities' that were asked in the WII measure. A greater problem is the justification bias described in Chapter 1; as for WII though, the same results were found when the analysis was restricted to those still working at follow-up. Furthermore, the qualitative analyses (Chapter 8) reveal problems with this WLD measure, and I return to these results in Chapter 9.

At the same time, this study has a number of strengths. The BHPS data are nationally representative, rather than restricted to the civil service or any other particular occupational cohort. I impute demands-control for what I believe to be only the 2nd time on a British sample for any outcome (the first being for later-life employment in general in Blekesaune et al 2008), and test a variety of different forms of this imputation. I control for an extensive array of controls, including the employment situation of people's partners – a strength of the household-based data in BHPS. I use a more robust version of binary mediation analysis than is common in the literature. And finally, I run a large number of models that test various assumptions in order to produce more valid inferences below.

36.2. Main findings

There are four findings from this chapter that merit further discussion:

1. Low-demand (rather than the predicted high-demand) jobs raised the risk of WLD at follow-up.
2. Low-control jobs at baseline predicted WLD at a follow-up wave – as long as occupation-level SES measures were not included as covariates.

3. I estimate that WLD would be 0.3-0.4 percentage points lower in 2006 if job control was at the level it was in 1992. However, if I also take into account job demands, then the rise in job strain is not associated with a noticeable change in WLD.
4. The effects of demands-control on WLD occur mainly through a health-independent ‘job requirements’ pathway.

36.2.1. Finding 1: a protective *effect of high job demands?*

Against expectations, high job demands *reduced* the risk of WLD; when controlling for physicality and individual-level SES I estimate that people in the third most-demanding jobs had 0.9 percentage points lower WLD than those in the least-demanding jobs (7.3% vs. 8.2%). This was not only robust to a large number of controls, but was robust through the missing data analyses and was even marginally significant after controlling for many occupation-level measures of SES. That said, the specificity of the effect of job demands was low – its effect on other measures of WLD was smaller and non-significant, while its effect on housework limitations was even larger than its effect on WLD. There were also several sensitivity analyses where no effect of demands could be found (looking at WLD two years after the baseline wave; in 3 of the 7 imputation methods).

The possibly protective effect of high demands contrasts with the results from the WII data in Chapter 3, where demands had a strongly detrimental effect on WLD. In Chapter 9 I compare WII and BHPS systematically – but even ignoring WII, the absence of a harmful effect of job demands conflicts with much of the existing literature (Chapter 1), and my own hypothesis. Moreover, from the deeper understanding of the nature of fitness-for-work in Chapter 8, it seems highly unlikely that there is a harmful impact of high demands on WLD; there are no obvious processes through which this occur. There are therefore five ways of explaining this result.

First, it is commonly asserted that ***attenuation biases*** explain why imputation-based studies differ from self-report studies in the apparent effect of job demands. It is correct that imputation-based studies suffer attenuation biases, but this does *not* appear to explain the discrepancies, either in this study or more widely. Of the 19 JEM studies I reviewed that included job demands (see Chapter 5), demands was significant in six cases – but in five of these, high job demands was protective rather than harmful (as was ‘stress’ in Blekesaune and Solem 2005), and similar results are sometimes found for external assessments (Chapter 1) or work unit averages (Morrison et al 2003:216). If anything, attenuation biases seem to be concealing a weakly *beneficial* effect of high demands on health/fitness-for-work.

A second possibility is that ***self-report studies*** are subject to confounding. Currently there is a debate in the mental health field prompted by a team using aggregated job strain within work units of the Danish public service (Bonde 2008; Bonde et al 2009a; Bonde et al 2009b). Recently in the *American Journal of Epidemiology* (Kolstad et al 2011), this team argue that self-report studies are biased and should be discounted in favour of imputation-based methods and external assessments. While Kolstad et al do not actually compare self-reports and imputed measures very rigorously,[117] their argument has some strength – as I said above, attenuation biases are unlikely to explain the consistent absence of a detrimental impact of demands in JEM studies. Furthermore, there has long been a debate around 'common method bias' in psychological research based exclusively on self-reports (Conway and Lance 2010). Yet it is unclear exactly what could be confounding the relationship between demands and WLD given the long list of controls used in the Whitehall II analyses (Chapter 3), and it seems unlikely that the JEM models are correct in showing demands has a beneficial impact on WLD.

A third explanation is that it is ***imputation-based studies that are subject to confounding***. This is the argument that is commonly advanced when job demands shows protective effects, suggesting confounding by income/SES (e.g. Hemström 2005:644; Niedhammer et al 2008:94). However, I here control for an extensive array of SES controls and still find occasional suggestions of a protective effect for job demands.

There may be other confounders that are associated with occupational averages (Diez Roux 2004:104-6), particularly given that occupation-level correlations are often stronger than individual-level correlations (Ostroff 1993). In the literature on using neighbourhood SES as a proxy for individual SES, it has been argued that aggregate measures capture more confounders (Geronimus et al 1996:531), although the reverse argument has been used when looking at medical treatments at the hospital vs. individual level (Claiborne Johnston et al 2002). In the case here of occupations, the most likely occupation-level confounders are those relating to selection *into* jobs and socialization *within* jobs (North et al 1996; Daniels 2006:271), and there is empirical evidence of both of these effects (Kohn and Schooler 1982; Ostlin 1988; although see Karasek and Theorell 1992:115). Yet I control for baseline mental health and a number of other variables that would be expected to covary with selection into jobs (such as job satisfaction and education), and still find a protective effect. Unobserved confounding is possible and a priority for future research, then, but superficially it does not appear especially likely to fully explain the results here.

[117] Given attenuation biases, we would expect the apparent effect of job strain to be lower for imputed demands-control than for self-reports. A better test would be to correct the imputation-based method for measurement error and to then see if it is statistically significantly different from the self-reported effects (using procedures I describe in the research recommendations in Chapter 9).

Fourth, the ***question on WLD may be interpreted differently*** by those in high-demands occupations. Respondents were asked, *"Does your health limit the type of work or the amount of work you can do?"* In Chapter 1 I noted that some people interpret this question as referring to their current job, while other people interpret the question more broadly. Potentially people in high-demand occupations are less likely to consider physical jobs as 'work you can do', even taking into account their own physical demands – and they therefore report less WLD. (This may explain why physicality has no effect on WLD, as found in section 35.1.1). Further evidence for this interpretation is briefly given in Chapter 8, and this seems a plausible explanation of these findings, as I argue in Chapter 9.

This does not explain possibly protective effects of demands in the wider literature, so a final option seems worth considering: that there are ***fundamental differences*** between the nature of self-reported and aggregated demands (Bliese and Jex 1999:4) that mean that the conclusions from one level cannot be assumed to apply at the other (Geronimus et al 1996:536; Morrison et al 2003). The most common use of aggregate measures as proxies for individual measures is for SES, where it has been argued that neighbourhood-level SES captures a different construct to individual-level SES (Diez Roux 2004:105) due to area effects. Along with confounding, this explains why neighbourhood SES can have similar or even stronger effects on health outcomes than individual-level SES (Geronimus et al 1996; Geronimus and Bound 1998:484; Mustard et al 1999), despite attenuation biases (Chapter 5).

There is some evidence that occupation-level demands-control are different constructs to individual-level measures. First, there is some – if inconsistent – evidence that group-level averages of demands-control have an *independent* association with outcomes even after taking individual-level demands-control into account (Bosma et al 1997; De Jonge et al 1999; Van Yperen and Snijders 2000; Van Veldhoven et al 2002; Morrison et al 2003; Tummers et al 2003; Liu et al 2005; Tucker et al 2005:286). Second, correlations between working characteristics are different on the occupational level – for example, *individuals* in more physical jobs have slightly higher demands (reflecting the overlap in the concepts; Choi et al 2011), but more physical *occupations* have lower demands.[118] In the Whitehall II study, externally-assessed psychosocial characteristics were much more strongly patterned by job grade than individual self-reports were (North et al 1996).

A difference is certainly possible, then, but why would high-demand occupations have an effect on WLD above-and-beyond the impact of high-demand individual jobs? Three possibilities seem plausible:

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[118] My analysis of Skills Surveys 2001-2006 (see also Karasek et al 1998; Choi et al 2008:25-6).

- *Contextual effect*: Working hard in a low-demands occupation (which may be unexpected/resented) may be a more stressful experience than working hard in a high-demands occupation (Bliese and Jex 2002:274) – an effect that has been shown empirically for morale (Tucker et al 2005:286).
- *Work organisation*: High-demands occupations may be organised in an inherently different way from low-demands occupations, in ways that make it easier to cope with health problems at work. Suggestive evidence for this is given in Chapter 8.
- *Good and bad demands*: In Chapter 1, I described how job demands has both good and bad aspects, and that a number of studies find evidence for an optimal level of demands somewhere between the extremes. It is possible that between-occupation variations in demands (the imputed data) may mainly capture 'good demands' – the rewards of intellectually stimulating work. Conversely, within-occupation variations (which dominate self-reports and the instrumental variables analysis of Kivimaki et al (2010b)) may mainly capture 'bad demands' – overwork at a particular job. Or similarly, between-occupation non-linearities in demands may poorly approximate the nonlinearities in demands seen at the individual level (Greenland 2001:1347).

All this is speculation – but it is at least as plausible an explanation of the protective effects of high demands reported in the wider literature as any of the other competing explanations. Future research could provide greater certainty: the imputation methodology could be developed to (i) provide more accurate estimates of demands and control, to tease apart their effects more clearly; (ii) include a wider variety of controls for other characteristics of work, particularly around intellectual stimulation; (iii) attempt to impute nonlinearities in demands; (iv) control for unobserved characteristics of individuals that do not vary over time; and (v) estimate the selection of individuals into high-demands occupations (see Chapter 9).

For the time being, however, the effect of job demands on WLD remains uncertain.

36.2.2. Finding 2: Job control and WLD

Job control has a statistically significant effect on WLD in nearly all analyses; when controlling for physicality and individual-level SES I estimate that people in the third highest-control jobs had 0.8 percentage points lower WLD than those in the lowest-control jobs (7.7% vs. 8.5%). This was true even after controlling for an extensive list of covariates and using a variety of imputation methods – until I control for occupation-level SES (class, status or major occupational group), where the effect becomes small and non-significant. I have argued here that this is likely to be for methodological reasons, and that it is reasonable to exclude occupation-level SES from the models. Occupation-level measures are collinear with occupation-level job control, and if we control for both of these in the same model then the results will be biased towards zero (as the imputed measures of control have very high proportions of measurement error compared to true information).

While this problem is rarely explicitly acknowledged in previous imputation-based studies of job demands-control, it does perhaps explain why almost no JEM studies control for occupation-level SES. Indeed, of the 28 studies in the JEM review, only five control for occupational class – and four of these only do so through a crude 'blue-collar vs. white-collar' measure.[119] Even the controls for *individual*-level SES are often poor; most JEM studies either use a single measure of SES or none at all (Reed et al 1989; Klonoff-Cohen et al 1996; Andersen et al 2004; Niedhammer et al 2008). One study even explicitly omits an *individual*-level measure of SES on the grounds that it is too strongly correlated with job control (education in Amick III et al 2002). Strangely, I have been unable to find an extended discussion in the JEM literature on this issue,[120] and residual confounding by SES in these studies seems likely (Davey Smith and Phillips 1992; Fewell et al 2007).

In this chapter I go beyond previous studies in looking at multiple occupation-level SES measures and focusing on the possibility of confounding if we exclude them. First, I control for a greater number of individual-level measures of SES than previous studies: income, education, tenure and financial problems. Second, I adjust as far as possible for any other factor that may be being captured by occupation-level SES: overall job satisfaction; satisfaction with each of job security, pay, use of initiative and the respondent's manager; promotion prospects; self-defined class; and valuing work. In all of these, the effect of job control was barely attenuated (or even increased).

Beyond this, the most convincing evidence for a genuine effect of job control is from using managerial/supervisory status as a proxy for control. This has a moderate and significant effect on WLD, even after adjusting for occupation-based SES measures

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[119] Many JEM studies control for education (Karasek et al 1988; Pieper et al 1989; Steenland et al 1997; Jansson et al 2009) or income (Muntaner et al 1998; Marcoux et al 1999; Seidler et al 2004). A few studies in the JEM review did however go beyond this. These fall into three groups:

- Studies that only adjust for individual-level SES, but use multiple measures (generally education and income/poverty, but sometimes including past unemployment or age of labour market entry): (Homer et al 1990; Amick III et al 2002; Blekesaune and Solem 2005; Wieclaw et al 2008), or even parental social class (Hemmingsson and Lundberg 1998; 2006; Blekesaune et al 2008);
- Studies that adjust for a very crude blue-collar vs. white-collar measure of occupational class (Theorell et al 1998), sometimes alongside an individual-level measure of SES (Alterman et al 1994; Johnson et al 1996);
- Two studies that adjust for a detailed measure of occupational class. In one case this was not a deliberate decision, but rather a side-effect of using a JEM to explore the role of control in explaining inequalities between the various classes (Gisselmann and Hemström 2008); while in another a five-banded class measure was used in a sensitivity analysis and mentioned in passing (Hammar et al 1998:552).

[120] Karsek and Theorell (1992:77) simply note that (i) they control for a (binary) class measure; and (ii) demands-control predict psychological strain at the occupational level much more strongly than occupational status does.

such as class and status, as well as the extensive array of other covariates. It is still possible that the apparent effect of job control is instead due to some unmeasured confounder – particularly if the results for demands are interpreted as suggesting confounding (although in the previous section I noted several other persuasive interpretations). However, the biggest uncertainty comes from the reduction in the effect of job control when missing data are taken into account, which suggests that the apparent effect may be partly explained by the attrition of people in low-control jobs. There is therefore some evidence for a causal role of job control in explaining WLD, but doubts remain.

A final question is whether the beneficial effect of high demands suggests confounding that also affects control. This appears less likely than in the case of demands: findings for job control are more consistent and specific, and in general self-reported, aggregated and externally-assessed control all seem to show similar results (see Chapters 1 and 5). Partly this may be because job control is a more homogeneous concept where nonlinearities and ambiguous effects do not apply. Still, if the WLD question is interpreted as applying to *potential* jobs, then people in low-control occupations may be considering different types of jobs to those in high-control occupations.

In conclusion, my interpretation is that there is some evidence that low control has a harmful effect on WLD, but that considerable uncertainty remains.

*36.2.3. **Finding 3: explaining trends in WLD***

The implication of my results here is that WLD would be 0.3-0.4 percentage points lower in 2006, if job control had not declined over the 1990s. This is a relatively small although not negligible effect – a reduction of WLD in 2006 from 7.4% to 7.0% – although this will be an underestimate due to the attenuation biases discussed in Chapter 5. At the same time, I found that high job demands had a large protective effect on WLD. If this is genuine, then the rise in job strain is estimated to have little effect on WLD: the increased WLD from declining control is matched by the declining WLD from rising demands.

It seems unlikely that demands genuinely does have a protective effect on WLD – but from the results in this chapter, we simply do not know whether rising job strain has contributed to rising WLD over the 1990s.

*36.2.4. **Finding 4: there is a job requirements pathway between control and WLD***

Finally, I investigated the relative strength of the 'health' and 'job requirements' pathways between job control and WLD – that is, the extent to which low control (or high demands) seem to lead to worse health, or whether control affects people's ability to do their job at any given level of health. These mediation analyses depend on a number of additional assumptions reviewed in Chapters 3 and 4, which introduce an additional level of caution.

The results suggest that the effect of control on WLD partly works through a health-independent job requirements pathway. This effect was significant using the main mediation method, and for the most robust of the alternate methods in the sensitivity analyses. The relative size of these pathways is imprecisely estimated, but the job requirements pathway seems to be larger than the health pathway.

Another sign of a health-independent pathway comes from the specificity analyses, where I looked at outcomes that are more strictly focused on health than WLD (housework limitations, GP visits and hospitalisation). In nearly all cases the effect of demands-control was smaller and less statistically significant than for WLD (other than the effect of demands on housework limitations, which may be capturing the association of demands and the allocation of domestic responsibilities). This provides some support for the thesis' hypothesis that people with the same level of health are more likely to see limitations in their fitness-for-work if they are in low control jobs.

36.3. Conclusion

This chapter has shown that those in low-control jobs are more likely to report a work-limiting disability a year later, an effect that works partly independently of any effect of job control on health. While the imputation method does not allow us to control for occupational status or class, this result is strongly robust to the inclusion of a wide range of demographic, SES and work-related controls – although there are some signs that it may be over-estimated by the greater drop-out among those in low-control jobs.

However, interpretation of this result is complicated by a possible *protective* effect of high demands on WLD, and the divergence between the WII and BHPS results. In the next chapter, I see whether we see more consistent results when we look at an outcome that is further downstream, the effect of demands-control on incapacity benefit receipt.

37. APPENDICES

This chapter has three Appendices (which appear at the end of the thesis), one Web Appendix, and several Web Tables (available from www.benbaumberg.com/thesis.htm):

- **Appendix 6A: Control variables used in BHPS** – details of the covariates used (including categories and how the variables were derived from the publicly available BHPS dataset).
- **Appendix 6B: Physical demands** – construction of Item Response Theory model.
- **Appendix 6C: Results of sensitivity analyses.**
- **Web Appendix 6a: Descriptive statistics for control variables.**

CHAPTER 7:
The effect of job strain on incapacity benefit receipt in the British Household Panel Survey

38. INTRODUCTION

In the previous chapter I used imputed demands-control in BHPS to relate demands-control to WLD reporting one year later. In this chapter, I develop this analysis to investigate whether control and demands influence the chances of incapacity benefit receipt – and the extent to which this occurs through WLD. This parallels the analyses using WII data in Chapter 4, except that rather than looking at health-related job loss in general I am able to focus specifically on transitions to incapacity benefits. I begin by describing the measures of benefits receipt in BHPS and methodological details, before presenting the descriptive statistics, main results, and pathway analyses in turn. I conclude with a discussion of the limitations and key results.

39. DATA

39.1.1. Incapacity benefits receipt

Each wave in BHPS asks respondents whether they – individually, or jointly with others – have received each of a number of individual benefits in the past year. The measure of incapacity benefits receipt has previously been derived (e.g. Whittaker et al, in press) by combining three benefits: 'Invalidity Pension, Benefit or Allowance', 'NI Incapacity benefit (not employer's sick pay' (both prior to 1995), and 'Incapacity Benefit' (since 1995), to which I also added claims of Severe Disablement Allowance.

While these questions appear straightforward, I expect considerable levels of measurement error. A validation survey that linked BHPS responses to DWP records found that nearly all of those people reporting incapacity benefit claims in the survey were genuinely claiming this benefit (Lynn et al 2004). However, around 50% of those claiming the benefit did not report this to the survey interviewers – with the most likely explanation being that individuals report receipt of a different benefit (possibly Disability Living Allowance), given a substantial amount of confusion among recipients as to exactly which benefit they receive (Hedges and Sykes 2001:22-25).

This measurement error should not bias the estimated effect of job strain, but it will reduce the power of these analyses yet further.

39.1.2. Other outcomes

To see if the effect of demands-control was specific to fitness-for-work-related outcomes, I investigated several other outcomes in sensitivity analyses. First, I used a broader measure of disability benefits receipt that included a number of other benefits that could conceivably have included mis-reported incapacity benefit receipt. This includes Disability Living Allowance (1992-), Attendance Allowance, Mobility Allowance (1991-7), and Industrial Injuries Disablement Benefit. Income Support was not included, as this would primarily include non-disabled lone parents.

Second, individuals were asked to described their employment situation and could give the response '***long-term sick***/disabled'. This had a similar prevalence in the sample as incapacity benefits receipt, but this conceals considerable divergence between the two measures: only around half of people defining themselves as 'long-term sick' also reported claiming incapacity benefits (presumably partly because they were not claiming these benefits, and partly because of the under-reporting of benefits claims).

Third, I looked at ***non-employment*** per se, which I expected to be less health-focused and less closely related to job strain. This used the standard BHPS measure of employment, based on (i) whether the respondent reported working in the past week; and (ii) for respondents not working, whether they said they had a job that they were away from last week.

Finally, at every wave respondents reported if they had lost a job in the previous year, and if so, the ***reasons that they lost the job***. From this annual employment history, I created a dummy variable for whether respondents said 'I gave up work for health reasons' (hereafter 'health-related job loss'), 'I was made redundant' (redundancy), or 'I was dismissed/sacked' (sacked). Health-related job losses did not necessarily lead to a spell of long-term sickness; respondents would often report moving to other jobs or being unemployed.

40. METHODOLOGY

40.1. Statistical modelling

The theoretical model for the relationship between job strain and incapacity claims is the same as in Chapter 4; there is a WLD pathway from demands-control to benefit receipt, and a residual 'job effects' pathway that includes everything outside of fitness-for-work. The statistical models used are similar to those in the previous chapter. That is, I look at the impact of baseline job strain on incapacity benefit receipt on year later, using Generalized Estimating Equations ('GEE') to account for the fact that I observe most individual respondents several times.

The models include the controls mentioned in the previous chapter, which are all measured at the baseline wave alongside job strain.[121] Ideally I would also account for the clustering of demands-control within particular occupations/years, but the computational burdens made this impractical. However, sensitivity analyses suggest that this makes a minimal difference to the results (see Appendix 7A).

Estimation of mediated effects similarly looks at the average marginal effect of job strain when potential mediators are introduced, and bootstrap this to obtain a confidence interval. To account for sampling design and attrition I conduct a sensitivity analysis using weights, and also conduct a multiple imputation analysis.

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[121] The model is of the form:

$$\Pr\left(Incapacity_{i,t+1} = 1 \middle| \mathbf{x_{it}}\right) = \mathrm{logit}(\boldsymbol{\beta_1}\mathbf{STR_{it}} + \boldsymbol{\beta}_2\mathbf{DEM_{it}} + \boldsymbol{\beta_3}\mathbf{SES_{it}} + \boldsymbol{\beta_4}\mathbf{H_{it}} + \boldsymbol{\beta_5}\mathbf{WRK_{it}} + \boldsymbol{\beta_6}\mathbf{PRTNR_{it}})$$

where $\mathbf{STR}$ refers to job strain, $\mathbf{DEM}$ refers to a vector of demographic characteristics, $\mathbf{SES}$ refers to a vector of socioeconomic status controls, $\mathbf{H}$ refers to a vector of health controls, $\mathbf{WRK}$ refers to a vector of other workplace controls, and $\mathbf{PRTNR}$ refers to a vector of partner characteristics.

41. DESCRIPTIVE RESULTS

Job strain by definition is only observed by people who are working, so there is no baseline incapacity benefits receipt in the sample. By the follow-up wave (around a year later) and using the complete case sample of 52,608 person-wave observations, the incidence of incapacity benefits receipt is 1.1%, while that of long-term sickness is 0.6% and other non-employment is 4.3%.[122] (Descriptive information on job strain, WLD and the control variables are available in Chapter 6).

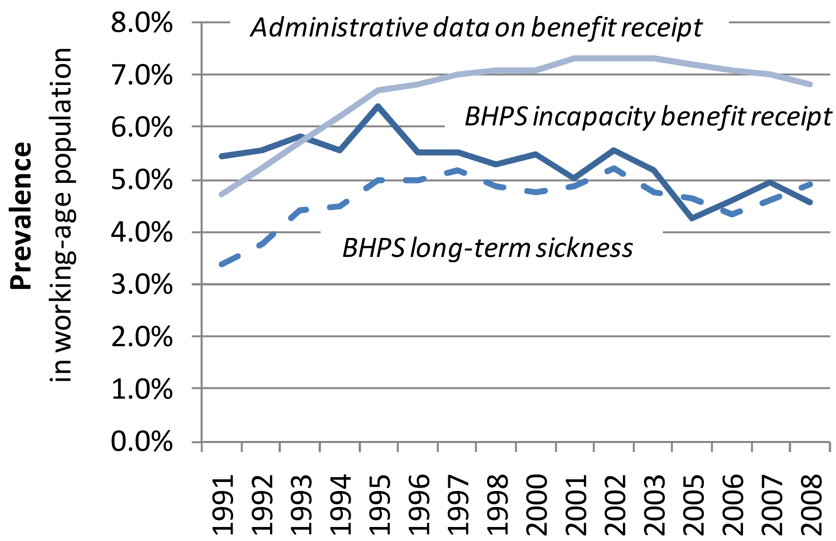
These proportions have changed over time. If we look at the full sample (not just those working at baseline, Figure 47), we can see that there was a slight rise in incapacity benefit receipt 1991-1995 (4.3% to 5.1%) followed by a larger decline to 3.7% in 2008. This differs from the more accurate population-wide trend in DWP administrative data (at the top of the graph), which shows a much sharper rise to 1995 and followed by a plateau more recently – with the gap between the two series increasing until about 2000. This also differs from the BHPS trend in long-term sickness (rather than benefits), which is much closer to the DWP administrative data (a sharper rise to 1995 and a plateau more recently).

As described in section 39.1.1, there also appears to be a substantial amount of under-reporting of incapacity benefit receipt in BHPS compared to official data – and this may have increased over time given the rise in 'credits only' claimants.[123] This is particularly striking when we consider that the BHPS series should be *higher* than the official series, as it includes all past-year claims rather than the number of claims at a particular moment in time.

[122] 465 people working at the baseline wave also claimed incapacity benefits in the past year, of which 207 are excluded from the models as they report being absent from work due to sickness in the previous week.

[123] 'Credits only' claimants are those that pass the disability criteria for incapacity benefits but do not have sufficient contributions to qualify for the benefit so instead primarily receive income support (see Introduction, footnote 3). The share of credits-only claimants not actually receiving 'incapacity benefit' rose from 30% in 1999 to 40% in 2008.

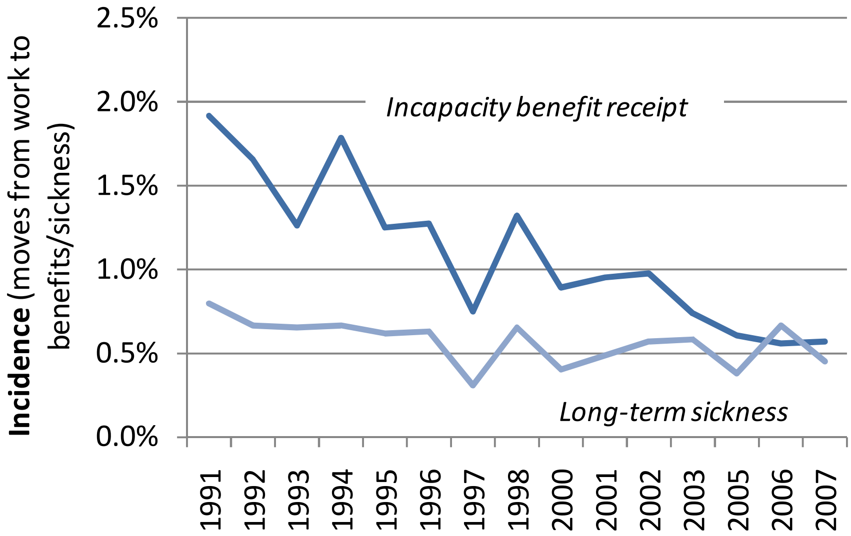
Figure 47: Trends in prevalence of work incapacity in the BHPS panel



BHPS trends calculated on the working-age British population (men aged 20-65, women aged 20-60), excluding proxy interviews, and using cross-sectional weights. For data sources for official trends, see Introduction.

It is also useful to look at trends in *transitions* from work to incapacity in BHPS in Figure 48. These are not necessarily direct transitions; around half of claimants do not move directly from work to incapacity benefits (Kemp and Davidson 2008a), and some people who have left work but not claimed incapacity benefits in the past year may nevertheless move onto incapacity benefits in future. Bearing this in mind, the year-on-year transitions show a sharp decline in transitions from work to incapacity, from 1.3% in 1991 to 0.3% in 2007. In principle this could be due to non-random attrition in the BHPS sample and other design features that make the sample less representative over time (Lynn 2006) – something that is also suggested by the increasing gap between the BHPS and official series in Figure 47. Yet we see a much smaller decline in the transitions from work to *long-term sickness* over the same period, suggesting that the decline is very specific to incapacity benefits receipt.

Figure 48: Trends in transitions from work to incapacity in the BHPS panel



Incidence based on working-age British employees (men aged 20-65, women aged 20-60), with incapacity measured at the following wave (one year later). This excludes proxy interviews and non-respondents at the following wave, and uses cross-sectional weights.

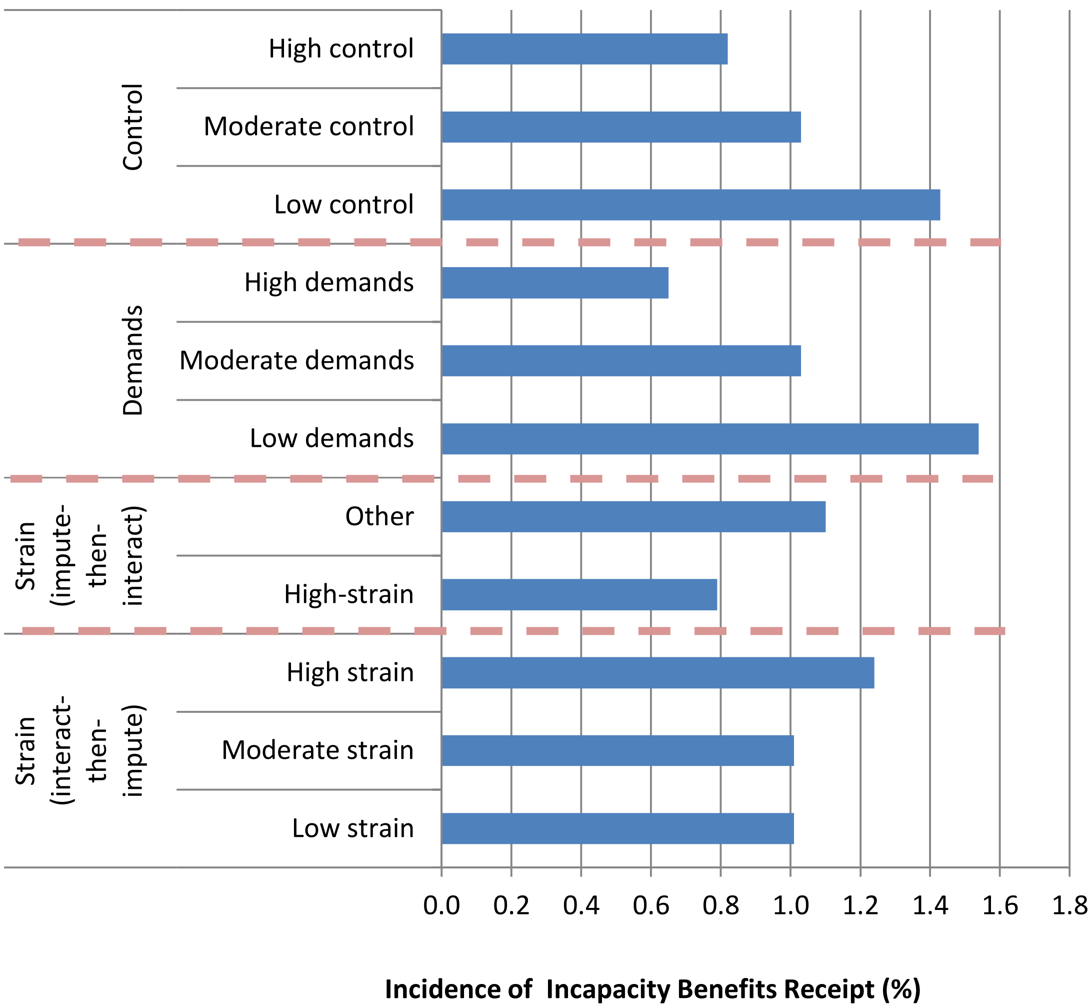
The most likely explanation for this trend is that the BHPS asks about incapacity benefit claims in the past year, and *not* whether the respondent is claiming at the specific time of the interview. Given the evidence that both on-flows and off-flows have been decreasing (Chapter 1), we would expect there to be fewer people that claim incapacity benefits at some point during any given year. The extent of the observed decline in BHPS is still surprising though, and may reflect increasing measurement error over time, and/or the changes in the proportion of incapacity benefit claimants that are 'credits-only'. Trends in BHPS cannot therefore be taken to reflect trends in the population as a whole.

41.1.1. Bivariate associations of job strain and incapacity benefits receipt

While the WII results and literature review suggested that we would expect greater levels of incapacity benefit receipt among those in high-demands, low-control jobs, the preceding chapter suggested that high-demand jobs might see a lower risk. The protective effect of high-demand jobs is borne out in Figure 49, which presents the unadjusted relationship between job strain and incapacity benefits receipt. Low-control jobs lead to higher levels of incapacity benefit receipt, but this is mirrored in low-demands (rather than high-demands) jobs, where the relationship is even stronger than that for control.

Using the conventional impute-then-interact method, this results in high-strain jobs having a *lower* risk of incapacity claims than other jobs. However, using the interact-then-impute method, high-strain jobs have a *higher* risk of claims. I return to the difference between these methods below.

Figure 49: Unadjusted association of incapacity claims with demands-control



However, the key question is whether these associations are maintained after adjusting for likely confounders.

42. REGRESSION RESULTS

While a fuller comparison between the BHPS and WII results will be undertaken in the Discussion and Chapter 9, it is helpful to recap the two main WII findings:

1. There is a significant effect of job strain on health retirement, but this is strongly attenuated and non-significant after controlling for other job characteristics (satisfaction, support, variety).
2. If we focus only on the effect of job strain on health-related job loss via WLD, then job strain has a significant effect even after controlling for other job characteristics.

In BHPS, the effects of job strain after controlling for different groups of confounders are shown in Table 27. As for the first tables in the previous chapters, these figures are odds ratios where coefficients <1 indicate a reduced risk and >1 an increased risk.

In the empty model with no covariates, we can see that there is a strong, statistically significantly reduced ($p<0.001$) risk of moving to incapacity benefits for those with high demands, and a smaller but still highly significant ($p<0.01$) effect of job control. When controls for survey wave are added, however, then job demands becomes non-significant while the effect of control becomes larger and highly significant, a situation that remains when an extensive list of other controls is added in models 4-6. As in the previous chapter, the inclusion of the SES controls makes job control non-significant, a finding I explore in more detail shortly. (For reasons given in Chapter 3, it is better to look at these results as average marginal effects rather than odds ratios, and these results are shown in Figure 50).

Table 27: Relationship between job strain and incapacity benefits receipt [odds ratios]

	[1] Empty	[2] Initial	[3] + Waves	[4] + Health	[5] +Work	[6] + Partner	[7] + SES/ Phys.
Low control (base)							
Moderate control	0.83+	0.86	0.70**	0.68***	0.68***	0.68***	0.85
High control	0.73**	0.83	0.52***	0.52***	0.54***	0.54***	0.90
Low demands (base)							
Moderate demands	0.72**	0.75**	0.99	1.01	1.00	1.01	1.18
High demands	0.49***	0.50***	0.81	0.85	0.83	0.84	1.06
N	*52608*	*52608*	*52608*	*52608*	*52608*	*52608*	*52608*
Num persons	*10742*	*10742*	*10742*	*10742*	*10742*	*10742*	*10742*

The sets of confounders are as follows:

Initial (model 2): age group (five-year bands), gender, marital status (married/never married/other), number of children (0/1/2/3+), black and ethnic minority status, region (10 dummies)

Waves (model 3): model 2 + dummies for each wave of BHPS

Health (model 4): model 3 + 13 categories of longstanding illness, GHQ caseness, 4 categories of activity limitation, and past-year hospitalisation

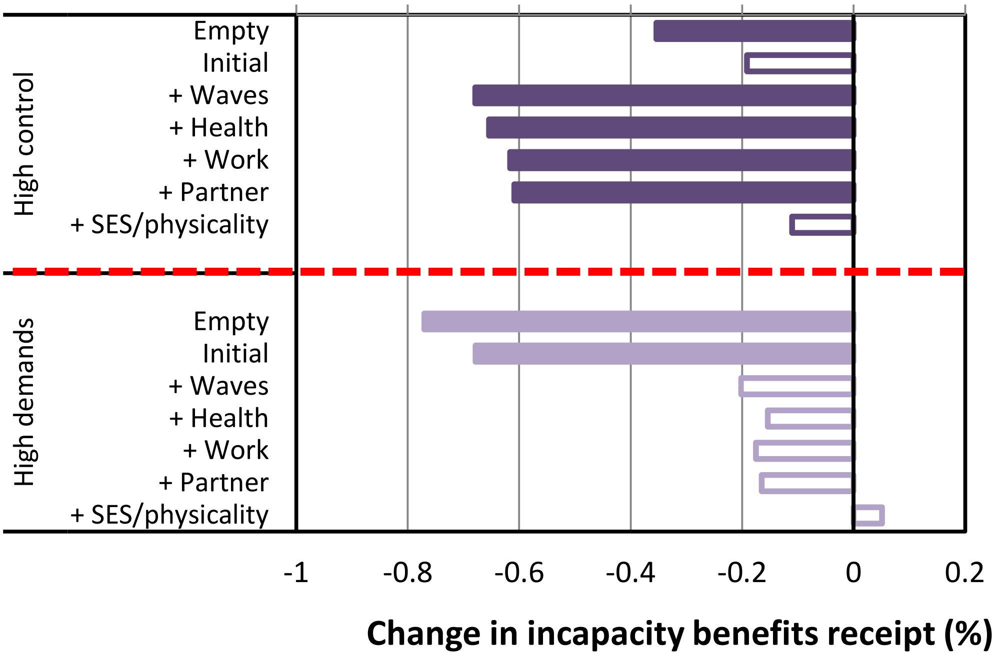
Work (model 5): model 4 + hours of work (four groups), member of occupational pension scheme, sector (public/private/nonprofit), temporary job, size of workplace (grouped), industry (15 groups of 1-digit SIC92)

Partner (model 6): model 5 + whether cohabiting; partner's incapacity benefits receipt

SES (model 7): model 6 + qualifications (six groups); status (Cambridge scale); income (log household net income); tenure (three groups)

This again raises the question: what is it about SES that appears to confound the relationship between control and WLD – and are we once more justified in omitting occupation-level SES from the models?

Figure 50: Effect of baseline job demands-control on moving to incapacity benefits



Average incidence of incapacity benefits receipt by follow-up is 1.1%. Solid bars are significant at the 5% level; bars shaded with hashes are significant at the 10% level; empty bars are not significant. Estimates are population-averaged marginal effects, based on the same sample and control variables as Table 27.

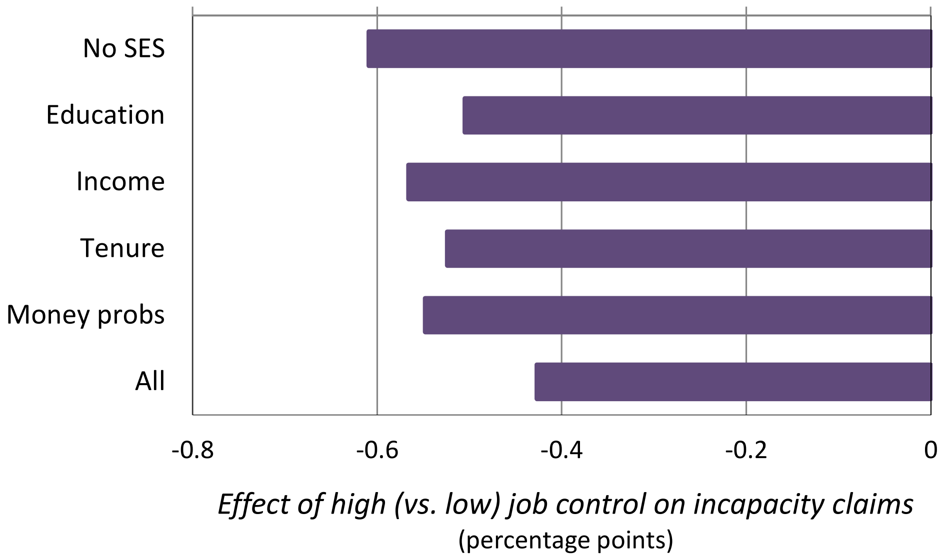
42.1.1. SES: methodological problem or substantive confounder?

In the last chapter, I illustrated the problems of controlling for occupation-level SES when imputing demands-control through occupation. On the basis of a number of additional analyses, I argued that it was possible to exclude occupation-level SES from the model without leading to unmeasured confounding.

Here we begin with exactly the same issue: job control is highly significant after controlling for an extensive barrage of potential confounders, but becomes much smaller and non-significant when controlling for the *a priori* measures of SES (income, education, tenure and occupational status). When I tease these different SES measures apart, control is not significant when I control separately for any of the seven measures of occupation-level SES that are available (see Web Table 7.SESB).[124] However, when I adjust for individual-level measures of SES, the attenuation is much less pronounced and I still see a strong and statistically significant effect of job control, as shown in Figure 51.

[124] However, moderate control does remain significantly different from low control in some models (2 at the 5% level and 2 at the 10% level, out of 7 occupation-level measures of SES).

Figure 51: Effect of individual-level SES controls on the control-incapacity relationship



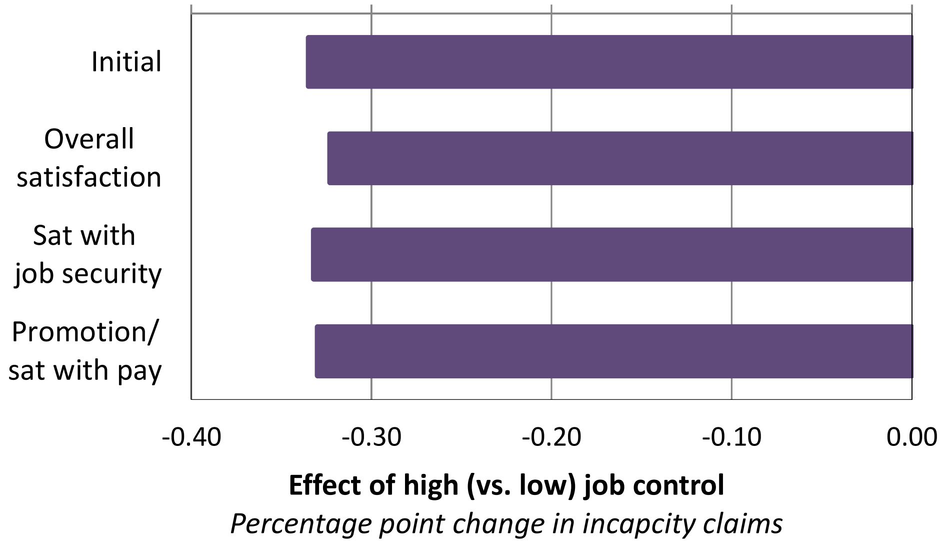
Average incidence of incapacity benefits receipt by follow-up is 1.1%. All effects are significant at the 1% level. Estimates are based on model 6 ('partner') in Table 27. The additional controls are education, log net household income, housing tenure, and financial worries.

I can therefore repeat the step in the previous chapter of excluding occupation-level measures of SES (class, status) from the remaining models in this chapter. But again I have to ask: are the occupation-level SES controls capturing any other confounders that are not captured by the individual-level measures of SES?

I therefore investigated whether other occupation-level factors might be responsible for the apparent association of job control and incapacity claims. First, I checked whether the apparent effect of job control was really a proxy for the physical demands of work. After including an imputed measure of physicality, the effect of control was slightly attenuated ($AME_{control}$ =0.34% vs. 0.44%) but remained significant (p=0.03). Physicality was also a strong predictor of incapacity benefits receipt (70% greater odds for the highest tertile of physicality; p<0.01), and the rest of the models in this chapter therefore include it as a control.

Second, I created models that adjust for a variety of other characteristics of people's work – including job satisfaction, which may be a mediator between control and benefits receipt. These are shown in Figure 52, and clearly show that these variables are orthogonal to the control-incapacity benefit relationship. (This is despite overall job satisfaction being a significant predictor of incapacity claims).

Figure 52: Effect of other job characteristics on the control-incapacity benefit relationship



Average incidence of incapacity benefits at follow-up is 1.1%. All effects are significant at the 5% level. Estimates are based on model 6 in Table 27, plus individual-level SES controls (log household income, education, tenure). The additional controls in this model are: overall job satisfaction, satisfaction with job security, and (simultaneously) opportunities for promotion and satisfaction with pay.

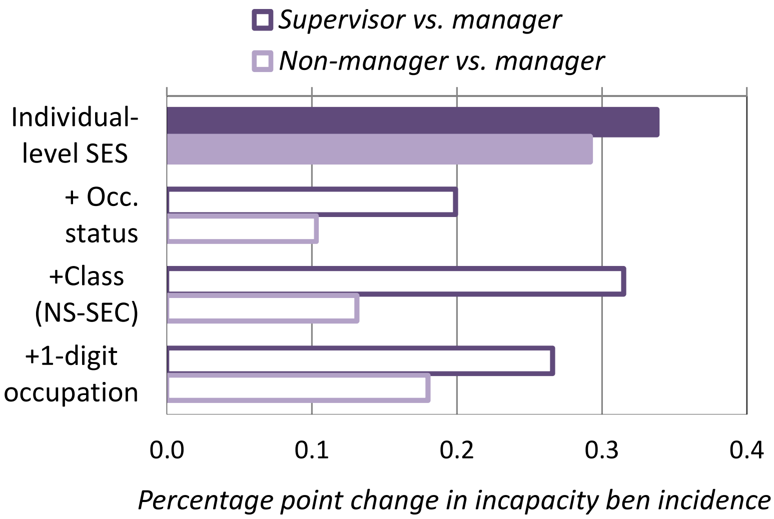
Third, I looked at the respondents' satisfaction with their manager and with their use of initiative at work, using the subsample of waves at which these measures were available. Again, the attenuating effect of these variables was small (AME=0.62% vs. 0.59%, p<0.05 in both cases), and neither variable significantly affected incapacity claims. Third, I looked at two class-associated attitudinal measures (self-defined class and valuing work), again in different subsamples where these variables were available. While the effect of control was smaller and non-significant in one of these subsamples before the controls were added (reflecting random variation and a smaller sample size), neither variable was significant, nor did they attenuate the effect of control.[125]

Finally, I used respondent's managerial status as a proxy for job control, shown in Figure 53. The 'individual-level SES' model shows that like imputed job control, managerial status is strongly associated with incapacity benefits receipt after controlling for physicality, income, education, tenure and financial problems. In the following three models, however, we see that managerial status becomes attenuated and non-significant after adjusting for occupation-level SES.

[125] In the case of self-reported class, AME=0.72% both before and after controlling for class, p<0.01 in both cases. Even before controlling for valuing work, the effect of control in this subsample was smaller and non-significant (AME=0.09%, p=0.65) – but additionally controlling for valuing work had no impact on this relationship (AME=0.10%, p=0.64). See Web Table 7.SEScont.

This provides less support for the effect of control than the equivalent analyses for WLD (which were still significant after adjusting for occupation-level SES), possibly due to the greater power of the analysis for the more common outcome of WLD, and the strong correlation between managerial status and occupation-level SES.

Figure 53: Effect of managerial status on incapacity benefits receipt by follow-up



Shaded bars are significant at the 5% level; unshaded bars are not significant at the 10% level. 'Individual-level SES' model is the same as model 7 in Table 26 but excluding occupation-level status. The additional models include for (in turn) occupational status (Cambridge scale), NS-SEC (social class, measured by the National Statistics Socio-Economic Classification), and one-digit occupation (SOC90).

Overall, this section has again shown that the non-significant results for job control in Figure 50 are likely to be due to the use occupation-level measures of SES, which cause particular problems when using imputed data. The effect of control is robust to individual-level SES and a number of additional controls (other workplace characteristics, including physicality, opportunities for promotion and multiple aspects of job satisfaction; self-identified social class; and the value the respondent places on fulfilling work). Managerial status also shows similar effects as imputed job control, although this is attenuated and becomes non-significant after controlling for occupation-level SES. In the remaining models in this chapter I exclude controls for occupation-level SES, before returning to the issue of causal inference in the Discussion.

42.1.2. Strain and other coefficients

In Chapter 5 I described two different methods of looking at the interaction of demands*control. If I replace demands-control with the conventional JEM measure of job strain – using the impute-then-interact method – then strain is far from significance (and in fact shows that the combination of high demands and low control is *less* harmful than we would expect). Yet if I use the interact-then-impute method – which as I argued previously, better-captures the level of strain in each occupation – then greater levels of strain increase the chances of incapacity claims, such that those with moderate strain have 27% greater odds ($p<0.10$) of claiming incapacity benefits compared to those with low strain, and those with high strain have 51% greater odds ($p<0.01$).

This suggests that the interact-then-impute method is better able to capture job strain than the conventional method. That said, it is not easy to separate out the interaction of demands*control from the effect of control alone – when including the interaction term *alongside demands-control* in the same model, the coefficients become non-significant (Web Table 7.CDlin). For this reason, the interaction terms are not included in any of the models in this chapter. Still, there are weak signs that it is the *combination* of low control and high demands that is important,[126] a finding that is potentially important when we compare the BHPS and WII results in Chapter 9.

The coefficients on the other covariates in models including physicality, education, income and housing tenure are presented in Web Table 7.covariates. This shows that incapacity claims are more common in those who have claimed incapacity benefits in the past year, in older age groups, with zero or three children (with the lowest claims among those with two children), in certain regions (North-West, North-East, East Midlands, Wales, Scotland vs. London), before 1995, with certain health problems (musculoskeletal, breathing, heart, anxiety/depression, other) or functional limitations (doing housework, walking >10mins), who have been to hospital in the past year, with minor psychiatric morbidity, in education (vs. health and social work), with a partner claiming incapacity benefits ($p<0.10$), who do not have a degree, with lower income ($p<0.10$), in social housing, and in more physically demanding jobs.

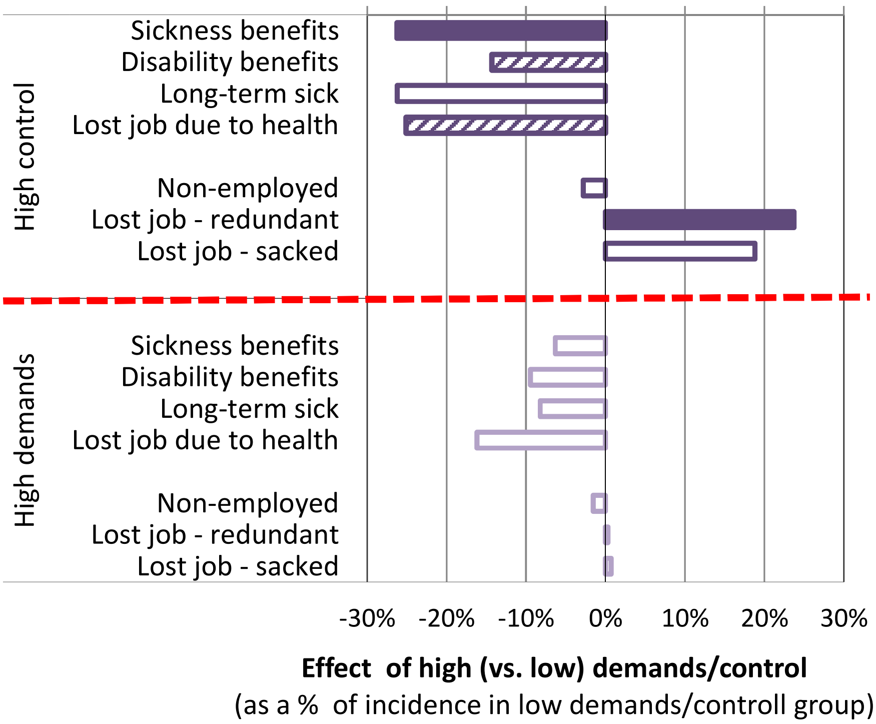
42.1.3. Specificity

Aside from showing an association between job control and incapacity benefits receipt after controlling for plausible confounders, further evidence of causality can be gleaned from the specificity of the effect – that is, whether job control appears to affect health-

[126] When tertiles of imputed demands, control and strain are included in the same model, none of the coefficients on demands or control individually are significant, but the term for high strain is marginally significant at the 10% level (see Web Table 7.CDlin).

related labour market outcomes more than other outcomes (as argued in Chapter 4). In this light, the effect of demands and control on a variety of outcomes is shown in Figure 54.

Figure 54: Specificity of job demands-control effects on incapacity benefits



Average incidence of outcomes are 1.1% (incapacity benefits receipt), 2.2% (disability benefits), 0.6% (long-term sickness), 0.9% (lost job due to ill-health), 4.4% (non-employment exc. Incapacity benefits), 3.1% (made redundant) and 0.4% (sacked). Solid bars are significant at the 5% level; bars shaded with hashes are significant at the 10% level; empty bars are not significant. Estimates are based on the same sample and control variables as model 7 in Table 26 but excluding occupation-level status.

For job control, the results are highly specific to the four health-related outcomes. Only the result for incapacity benefits is significant at the 5% level (disability benefits and health-related job loss being significant at the 10% level, and the rarest and therefore lowest-powered outcome of long-term sickness being p=0.11) – but the effect size is consistent across three of the four health-related outcomes, and all four contrast markedly to the non-health-related outcomes. (It is slightly surprising that high job control appears to make redundancy *more* likely, although this perhaps reflects that another explanation of job loss was 'it was a temporary contract which ended', which is as common as redundancy but more likely among those with low control). This offers additional support to interpret the association of control as a causal effect.

Demands differs markedly from control here; it is not remotely close to significance for any of the seven outcomes (even for the relatively large effect on health-related job loss, $p>0.3$).

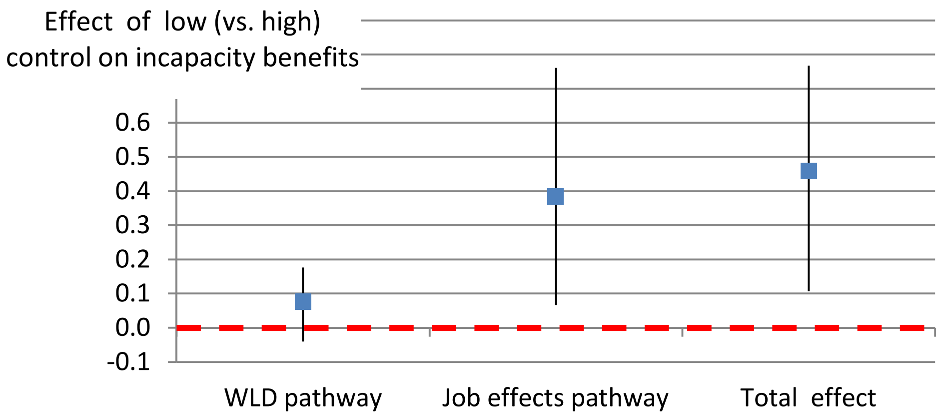
42.1.4. Pathways between job control and incapacity benefits receipt

There are two potential pathways between job control and incapacity benefit receipt: a WLD pathway and a residual job effects pathway. Unfortunately we have to assume that baseline health variables are either confounders or mediators of this relationship, when *a priori* there are variables that could be both. It is therefore worthwhile to check if the results are different when we make different assumptions about two possible confounders/mediators, by adding controls for baseline WLD and self-reported general health. Baseline WLD and self-reported general health had no noticeable impact on the results (Web Table 7.CDspec), which suggests that neither are important as *either* confounders or mediators of the effect of job control on incapacity benefits.

Using the mediation models described in section 40.1 and the previous chapter, I then investigated the relative strength of the WLD and job effects pathways – bearing in mind the caveats about mediation models (Chapter 4) and the effect of demands-control on WLD (Chapter 6). The results in Figure 55 show that the WLD (including health) pathway is non-significant, but that the job effects pathway is both statistically significant and marginally significantly ($p<0.10$) larger than the WLD pathway. The non-significant WLD pathway is relatively small, at around 1/15th of the incidence of incapacity benefits receipt (0.07% vs. 1.05%). If we unpack the WLD pathway into the different temporal sequences involved (Chapter 4), then none of these individual pathways are significant, and the WLD pathway is even smaller and less significant when looking at the effect of job control on non-employment in general.

These results are not surprising – we saw in the previous chapter that the effect of job control on WLD was small and sometimes non-significant, and the pathway from job control to WLD to incapacity benefits receipt is therefore also small. This analysis does however draw attention to the large, unexplained job effects pathway from job control to incapacity benefits, an issue I return to in the Discussion.

Figure 55: Pathways between job control and incapacity benefit receipt
Marker shows bias-corrected estimate; bar shows 95% confidence interval



Estimates are bias-corrected bootstrap estimates with 200 replications. All models include the controls as per model 7 in Table 26 but excluding occupation-level status. The total effect is equivalent to this model on a slightly smaller sample (n=44388, n(persons)=10452) and using bootstrap rather than normal-based estimates of the confidence interval. The WLD pathway controls for WLD measured at both baseline and follow-up waves; the job effects pathway is the direct effect of job control on incapacity benefits receipt after controlling for the WLD pathway (see text).

42.1.5. Explaining trends

Chapter 2 documented a rise in job strain over the 1992-2006 period. From the models here, I estimate that without the decline in job control, the transitions to incapacity benefits would be around one-sixth lower than they are today. (That is, 0.39% of the sample would move from work to incapacity benefits during the following year, as opposed to the actual 2006 figure of 0.46%).

This effect is statistically significant, and we see roughly the same effects even if we take rising job demands into account.[127] It is also a moderately large effect – and may actually be larger still, given the significant attenuation bias inherent in the imputation method. We have already seen that there was a decline in transitions to incapacity benefits over the 1990s in BHPS (section 41). The models suggest that the decline in transitions to incapacity benefits would have been even greater had it not been for the decline in job control.

[127] This difference is significantly greater than zero (p<0.001), with a 95% CI of 0.02%-0.13%. The results are similar if trends in job demands are also taken into account (a relative decline of 15% and an absolute decline of 0.07%), although the 1992 to 2006 change is no longer statistically significant.

42.1.6. Methodological sensitivity analyses

A number of other sensitivity analyses were conducted to test the robustness of the findings to various methodological decisions, including different forms of demands and control; looking only among workers; excluding those who are 'temporary sick' at baseline and looking at incapacity claims two waves later; different estimation techniques (including random effects models); different imputation techniques; accounting for the fact that demands-control is observed for occupations rather than individuals; and accounting for missing data. The findings above were robust to all of these, and are summarised in Appendix 7C.

Finally, I tested whether the effect was also found for different subgroups (men and women, older (45+) and younger workers). The full set of covariates interacted with both gender ($p<0.01$) and age ($p=0.03$) were significant. For gender, the effect of control was stronger among men than women, but this difference was not significant ($p=0.22$). By age, control only had an effect in younger workers (AME=0.6% $p<0.01$, vs. AME=0.01% ns in older workers), a difference that was marginally significant ($p<0.10$).

I consider the implications of the subgroup analyses in the Discussion below.

43. Discussion

43.1. Limitations

This chapter has the same limitations as the previous chapter: the assumptions necessary to interpret these results causally, justification biases in the measure of WLD, the possibility of non-random attrition, and attenuation bias and low power that is inherent in most imputation-based methods. Beyond this, it has the additional problem that leaving work to claim incapacity benefits receipt is not that common – 1.1% of the sample, or around 570 cases – and which declines sharply 1991-2006, even though transitions to long-term sickness are constant.

It also has the same strengths as the previous chapter: a nationally representative sample, an extensive array of controls including partner characteristics, improved forms of binary mediation analyses compared to much of the current literature, and a large number of analyses to test different assumptions embedded in the main models.

43.2. Main findings

There are four findings from this chapter that merit further discussion:

1. People in low-control jobs at baseline were more likely to claim incapacity benefits by the follow-up wave – as long as occupation-level SES measures were not included as covariates.
2. Job demands and conventional measures of job strain had no effect on incapacity benefit receipt, although an alternate measure of job strain raised the risk of incapacity claims.
3. Based on these models, I estimate that transitions to incapacity benefit receipt would be one-sixth lower in 2006 if job control was at the same level it was in 1992.
4. The effect of low control on incapacity benefit receipt occurs primarily through a WLD-independent 'job effects' pathway.
5. Control *only* had an effect among younger workers (p<0.10).

43.2.1. Finding 1: Job control and incapacity benefits receipt

In this chapter, I found a strong effect of job control on incapacity benefits receipt even after including a variety of controls and across a wide range of sensitivity analyses.

As for the WLD analyses, this effect disappeared if I controlled for any occupation-level measure of SES, an effect that is likely to be due to the collinearity of job control and occupation-level SES. I again tested whether excluding occupation-level controls seemed to lead to other forms of confounding of the relationship between job control and incapacity benefit receipt. This did not seem to be the case – the effect of control was robust to additional controls for the physical demands of work, promotion prospects; satisfaction with five aspects of work; self-defined class; and valuing work in general. Controlling for SES did appear to be important – controls for qualifications in particular seemed to attenuate the effect – but the effect of job control was still strong and highly significant even after controlling for multiple individual-level measures of SES.

Again, though, across the wide range of sensitivity analyses there are some inconsistencies. When I looked at managerial status as a proxy for job control, I found similar results to imputed job control – but this was became non-significant after controlling for occupation-level SES, unlike in the previous chapter. Some attenuation would be expected due to the close relationship of occupational status and managerial status, and it is debatable how far managerial status proxies for the aspects of job control that are most important (see Chapter 8), but this leaves the possibility that there are other occupation-level confounders of the control-incapacity relationship. Nevertheless, the robustness of the effect of control to an extensive array of controls and sensitivity analyses does provide some evidence in favour of a causal effect of job control.

43.2.2. Finding 2: Job demands, job strain and incapacity benefits receipt

In this chapter there was no evidence of any harmful impact of high job demands in any of the main or subsidiary analyses, in contrast to my hypothesis – but in keeping with most of the previous literature on health-related labour market outcomes (see Chapter 1).

While I can therefore be relatively confident that there is little effect of job demands alone on incapacity benefit receipt, the results in this chapter should not be taken as evidence against the existence of an interaction of high demands*low control. The imputation technique struggles to capture the interaction of high demands*low control, and it was this interaction that seemed to be particularly important in WII. If I used the conventional impute-then-interact measure of strain, then people in high-strain jobs are *not* more likely to claim incapacity benefits than those in other jobs. Yet if I use the (preferable) interact-then-impute measure I described in Chapter 5, then I find that people in high-strain jobs are considerably more likely to move onto incapacity benefits – although when included in models alongside demands and control, the coefficients are imprecisely estimated and non-significant. Essentially there is insufficient information in the JEM technique to test such interaction effects with any degree of precision, but there are some signs that it is this interaction that leads to incapacity claims, which I take into account when synthesising the results of the thesis as a whole in Chapter 9.

43.2.3. ***Finding 3: explaining trends in incapacity benefits receipt***

The implication of my results here is that transitions to incapacity benefit receipt would be one-sixth lower in 2006 if job control had not declined over the 1990s (0.46% vs. 0.39%). This is a moderately-sized effect, particularly compared to the smaller effect predicted for WLD and that both sets of results are likely to be underestimates due to attenuation biases. The rise in job demands appears irrelevant to changing incapacity benefits, but this may be because of the aspects of job demands that are captured by the imputation method (as I discussed in the preceding Chapter).

In the context of the overall research question, this suggests that declining job control could be a factor in the increase in incapacity benefits in the UK. This possibility must be tempered with a considerable amount of uncertainty – over the robustness of the effect of job control, over the robustness of the trends themselves (see Chapter 2), and over the link between *transitions* to incapacity benefit receipt and the total *stock* of those claiming the benefit. I draw together the policy implications of this in Chapter 9.

43.2.4. ***Finding 4: pathways between control and incapacity benefits receipt***

I hypothesised that job strain would be a stronger risk factor for health-related outcomes than job loss in general, due to the effects of a 'WLD pathway' as well as a residual 'job effects' pathway. On the one hand, I find no evidence for a WLD pathway between job control and incapacity benefits, with the estimates being small and non-significant. On the other hand, a fitness-for-work focused pathway is suggested by the specificity analyses, where low job control raised the risk of negative labour market outcomes related to health but had no effect on non-employment in general and even raised the risk of redundancy.

The mediation analyses are subject to additional uncertainties here – both the additional assumptions reviewed in Chapters 3 and 4 on mediation analysis, and the difficulties interpreting the results for WLD in BHPS (Chapter 6). The stronger evidence is therefore from the specificity analysis that suggest a fitness-for-work effect, as I argue in Chapter 9.

43.2.5. ***Finding 5: an effect only at younger ages***

Finally, while not a substantive focus of this chapter, a sensitivity analysis suggested that control only had an effect on incapacity claims at younger ages (under-45). This was only marginally significant (p<0.10) and there is a strong risk of spurious effects in subgroup analyses – yet it is worth drawing attention to this finding as it mirrored a similar (if non-significant) pattern in WII, and can also be found in some wider JEM studies (Alfredsson et al 1982; Hammar et al 1998). Future research should therefore consider the ways that people are selected into occupations occurs over the lifecourse, and the extent to which working conditions at older ages are a *response* to earlier ill-health/difficulties as well as a risk factor (Chapter 8).

43.3. Conclusion

This chapter has shown that those in low-control jobs are more likely to claim incapacity benefits a year later. This is a moderately strong effect; if job control had not declined over the 1990s, then I estimate that transitions to incapacity benefits in 2006 would have been one-sixth lower. This result is strongly robust to the inclusion of a wide range of demographic, SES and work-related controls, and numerous sensitivity analyses. In contrast, there was no evidence for any effect of job demands on incapacity benefit receipt or any related outcome.

There are nonetheless some uncertainties around the effect of job control. The imputation method does not allow me to control for occupational status or class, and although the effect is robust to many measures that capture one or other aspects of status/class, it is possible that some confounding remains. And while job control affected health-related outcomes more strongly than other outcomes, the pathways analysis found that these effects were not mediated through measured WLD (with all the problems described in Chapter 6), making it hard to be certain as to the processes underlying these results.

In the concluding chapter, I bring these results together with the WII data and the wider literature, suggest future avenues for research, and discuss what this means for policy. Before this, though, there are a variety of unanswered questions from the quantitative analyses of the previous five chapters. Terms such as ‘fitness-for-work’, ‘job demands’ and ‘job control’ have been used without going into the detail of what this means to people. Different analyses have alternately suggested that it is demands or control or their interaction that particularly matter, or even that demands can have beneficial impacts. And the *processes* through which demands-control lead to incapacity benefit receipts have not yet been explored, as I instead focused on establishing whether there was an aggregate association between them. In the next chapter I begin to correct some of these omissions by qualitatively analysing the accounts of people suffering from health problems themselves.

44. APPENDICES

This chapter has one Appendix (which appears at the end of the thesis) and several Web Tables (available from www.benbaumberg.com/thesis.htm):

- **Appendix 7A: Results of sensitivity analyses.**

CHAPTER 8:
Pathways from demands-control to fitness-for-work and to incapacity – a qualitative analysis

45. INTRODUCTION

In this chapter I present the results of qualitative research that is designed to take us further in understanding the role of changing working conditions in patterns of incapacity benefit receipt, focusing on three questions.

- First, the concept of self-reported 'fitness-for-work' is central to this thesis, but in the quantitative analysis is proxied by relatively crude survey questions. In this chapter I ask: *what does it mean for people to say their health limits their ability to work, and how is this reflected in answers to questions on WLD?*
- Second, I have found that *either* job control *or* job strain predict undesirable outcomes – but the exact pattern of this has been inconsistent. In this chapter, I ask: *how do demands and control influence fitness-for-work-related outcomes?*
- Third, the previous analyses have been concerned with average effects, that is, whether people in high-strain jobs more likely *on average* to suffer a health-related job loss, holding other factors constant. Yet as Chapter 1 made clear, incapacity benefit receipt is socially patterned, being primarily a phenomenon of low-skilled people in areas of low labour demand. Moreover, the quantitative analyses have variously been concerned with WLD, health-related job loss and incapacity claims, but without explicitly dealing with the paths between them. In this chapter I ask: *how – and for which people – does poor fitness-for-work lead to health-related job loss and incapacity benefit receipt?*

All these questions are suited to a qualitative research design, given that they focus on meaning or causality based on complex interactions between factors. The justification for the research design is developed in the next two sections, which explain the sample and research method respectively. The results are then presented in four sections focusing on different themes in the data, before the fifth section relates the resulting conceptual model to the earlier quantitative analyses. Finally in the conclusion, I summarise these results and place them within the wider literature.

46. Research Design

46.1. Sample

Unlike for quantitative research, qualitative samples do not aim to be *statistically* representative of a wider population. Instead, I aimed to achieve a sample which covered the *range of variation* within the wider population – and by understanding the processes operating for these varied individuals, I can then generalise to a *theory* which can explain what is happening across the whole population (Bauer and Aarts 2000; Gaskell and Bauer 2000; Ritchie et al 2003).

To select a sample, then, I needed to purposively select a sample according to relevant criteria that capture the full relevant variation (Ritchie et al 2003; Flick 2007). While it is not possible to know this precisely in advance (Bauer and Aarts 2000), the literature review and previous chapters suggest a number of axes of variation:

- Type of health condition (especially 'physical health' vs 'mental health')
- Whether the person left work
- Gender
- Education level
- Local labour demand
- Local incapacity benefit receipt

It is not simple to obtain a varied sample with a diverse range of health problems. I therefore decided to purposively recruit through GP practices, which contain people with multiple types of health problems, and serve the full spectrum of the population in a local area. I selected participants on two sets of criteria.

1. I chose GP practices in relatively advantaged and disadvantaged areas, which captures variation in both local incapacity benefit receipt and individuals' education level. I was however constrained to research in London for practical reasons, which means that local labour demand does not vary within the sample – a fact that I return to in the Limitations.
2. I selected working-age individuals in these practices who had recently had a health problem that interfered with their work, and among this group, selected participants to get a balance across gender, type of health condition, and whether the person left work or stayed in work. To do this, I approached patients in the waiting area of each GP practice. Those that agreed to take part then provided a phone number, through which I conducted a screening interview. I then arranged face-to-face interviews with those that matched the

> sampling criteria. A target of around 35-45 interviews was expected to be sufficient to obtain sufficient variation or 'saturation' (Manderbacka 1998:320).

In practice I conducted 30 interviews using this method (plus one interview obtained via snowballing) between October 2009 and June 2010. (I approached 341 people across five GP practices, of whom 184 initially agreed to participate, 139 were successfully screened by telephone, and 44 passed the screen). While the sample was varied in most respects, it became clear that I was obtaining relatively few incapacity claimants. I therefore supplemented this sample through three London offices of a Welfare-to-Work provider (as used in e.g. Wilton 2008), who asked clients claiming Employment and Support Allowance claimants if they were willing to take part. A further 11 participants were recruited in this way.

In total I spoke to 39 people who met the sampling criteria,[128] but these were not all included in the analysis. All interviews were recorded, transcribed and read, but it became apparent after analysing the first 24 interviews that theoretical saturation had already been reached for some types of participant; I therefore purposively selected 8 of the 15 remaining interviews for analysis.[129] The final 32 interviews came primarily from a GP practice in a disadvantaged area (12) and the Welfare-to-Work provider (9 participants), but also from another GP practice in a disadvantaged area (3), three GP practices in more advantaged areas (7), and one participant recruited by snowball sampling from an existing participant.

As Table 28 shows, this resulted in a sample that was varied in terms of health condition, education, gender, incapacity benefits claims, leaving work, and age – and also in type of work (Table 29). I discuss whether this was sufficient to achieve capture the full range of variation in relevant characteristics in the Limitations section below.

Table 28: Properties of the sample

Male	16	Aged 20s/30s	12
Female	16	Age 40s	13
No qualifications	6	Age 50+	7
NVQ Level 1 qualifications	3	Mental health condition	13
NVQ Level 2 qualifications	8	Physical health condition	19
NVQ Level 3 qualifications	6	Has claimed ESA	14
Degree-level qualifications	8	Not claimed ESA	18

[128] Three interviewees turned out not to have health problems that interfered with their work.

[129] The interviews not-analysed include four people with physical health conditions (one claiming ESA) and three with mental health conditions (one who had claimed ESA until failing a medical, one who was above working-age, and one whose situation was dominated by family/visa issues).

Table 29: Anonymised list of participants

Name	***Health conditions*** *(secondary conditions)*	***Type of job***	***Other details*** *(current work status, age, qualification level)*
Michael	Early-stage Parkinsons, (depression)	Freelance TV producer	Not working, late 40s, degree
Melanie	Diabetes, (hand operation)	Supermarket assistant	Working, early 30s, no quals
Tom	Hip problem	Catering manager	Not working, early 40s, level 2 quals
Isiah	Appendix removed	Delivery man	Working, mid-30s, level 3 quals
Liam	Alcoholism, depression, epilepsy	Delivery man	ESA, early 30s, level 3 quals
Carl	Burst appendix	Repair centre manager	Working, late 40s, level 3 quals
Khaled	Bad back, (finger operation)	Bus driver	Working, early 50s, level 2 quals
Maureen	Back problems	Administrator for parking firm	Working, early 50s, level 3 quals
Adrianna	Depression/Post-Traumatic Stress Disorder [PTSD]	Airline manager	Not working, early 60s, degree
Nick	Neck pain, (anger)	Property finance	ESA, mid 30s, degree
Ricardo	Broken pelvis in work accident	Bike courier -> caretaker	Working, late 30s, level 1 quals
Tessa	Epilepsy, abscesses	Manager in the council	Working, late 30s, level 4 quals
Yvette	Back problems	Auditor	Working, 40s, degree
Lindsey	*Depression*	*Charity shop assistant manager*	*ESA, early 50s, level 2 quals*
Sarah	*Depression*	*Supermarket supervisor*	*ESA claimant, late 20s, level 2 quals*
Erica	*Bowel problems, (depression)*	*Strip club manager -> Retail supervisor*	*Working, early 40s, level 2 quals*
Cheryl	Depression, back pain	Childcare->accounts manager	Not working, early 30s, level 3 quals
Petra	Depression	Airline steward	Working, mid 40s, level 3 quals
Ali	*Heart, diabetes, leg ulcer*	*Indian chef*	*ESA, early 50s, no quals*
Steve	*Depression/anxiety*	*Warehouse manager*	*ESA, mid 40s, level 1 quals*
Naveed	Arm/shoulder operation, (depression)	Self-employed martial arts instructor	Working, early 40s, degree
Elizabeth	Stress	Senior cancer nurse	Not working, late 40s, degree
Rachel	ME, depression	Special needs assistant in secondary school	ESA, late 40s, degree
Maryah	Stress	Marketing director	Not working, 40s, degree
Ashley	Back/chest pain	Building->Blind fitting	Working, mid 30s, no quals

Damian	Depression, (back injury)	Events manager -> Electrician	ESA, late 30s, level 2 quals
Jemimah	Fibromyalgia [chronic widespread pain]	Self-employed chef	Working, late 50s, no quals
Helen	Back/leg problems, depression	Policeman	Working, late 40s, no quals
Dele	*Schizophrenia, (diabetes)*	*Kitchen porter*	*JSA/DLA, early 50s, level 2 quals*
Scott	*Pneumonia, anxiety*	*Maintenance planner*	*ESA, early 40s, level 1 quals*
Yusuf	*Post-Traumatic Stress Disorder [PTSD]*	*Retail assistant ->Cleaner*	*ESA, mid 20s, level 2 quals*
Marjorie	*Back problems, broken shoulder, (depression)*	*Cleaner, dinner lady*	*ESA, late 40s, no quals*

All names are pseudonyms; some detail has been removed to ensure anonymity. Participants in italics are those that were recruited via the welfare-to-work provider.

46.2. Methodology

I here decided to use depth interviews, as focus groups would not allow me to investigate individual (sensitive) narratives in detail, and are susceptible to social influence effects (Barnes et al 2008:661). The interviews followed a semi-structured format, with the topic guide being developed through two pilot interviews, and the final version available in Web Appendix 8a. Participants were given a choice of interview location; most were at their homes but some were conducted in cafes/pubs, and several of the interviews obtained from the Welfare-to-Work provider were conducted in a private room at one of their offices.

The 32 interviews were digitally recorded and lasted between 35 and 135 minutes. The resulting 38 hours of recordings were transcribed in full and analysed using the Framework method (Ritchie and Lewis 2003). This meant that a coding frame was developed based on previous literature and an initial subsample of five interviews. Each transcript was then read 2-3 times (once while playing the recording) and coded in NVivo. After being coded, each interview was summarised in a charting framework that covered key themes (e.g. 'relationship with manager'). The final analysis used both the framework (to compare cases) and the codes (to compare themes and extract quotes).

The research received ethical approval from the LSE Research Ethics Committee and the NHS Research Ethics Service.[130] I attempted to involve people with health problems themselves by asking for volunteers from the Royal Association for Disability and Rehabilitation. However, given repeated requests from other researchers and the

[130] Approval was granted by the Joint UCL/UCLH Committees on the Ethics of Human Research (Committee A), reference 08/H0714/110; and the Research Management team within each PCT.

low-profile nature of the research, only one person volunteered. They took part in a pilot interview and provided guidance on the topic guide, and this was complemented by debriefing sessions after each interview. A plain-English version of these results will be given to all participants, who will be encouraged to provide additional feedback.

A critical issue here is how far we treat these accounts as accurate descriptions of people's lives. I have already discussed how people's beliefs on the link between health and work may be influenced by cultural factors (Chapter 2), and it is a substantial challenge to tell genuine limitations apart from 'malingering' (Halligan et al 2003). Validity was strengthened by assurances of anonymity and repeated, specific probing – but I return to these concerns in the Limitations section.

Finally, there are many possible reasons for using quotations (Corden and Sainsbury 2006); I here use them to convey a sense of the lived experience behind the otherwise abstract categories in the analysis. As such, they have been selected to vividly exemplify the analysis, but are not substitutes for the analysis itself.

47. THE NATURE OF 'FITNESS-FOR-WORK'

For certain people at certain times, their health precluded *any* type of work. This could happen when they were in hospital, or in bed continuously (such as Ricardo after he broke his pelvis), or for those like Sarah who just 'broke' and was crying non-stop. Most of the time, though, people were capable of some – limited – work; that is, they had a ***partial*** rather than ***absolute*** limitation. In this section I set out the four main kinds of partial limitations as a foundation for the further analyses.

47.1. Types of partial limitations

47.1.1. Task-specific limitations

Among those with physical health problems, the primary fitness-for-work issue was an inability to do *specific* activities within the job. The most common was lifting heavy objects, which was impossible for those with certain musculoskeletal or heart complaints. Posture at work also emerged as a problem, though only if people were required to sustain a certain posture for lengths of time (a specific *demands-dependent* limitation; see below). Other specific physical limitations were mentioned by one or two respondents, including running (for both Helen's role as a police officer and Erica's role as manager of the girls in a strip club), bending to clean under primary school tables, having sufficient back strength to stop a motorbike or wear a police vest, and sufficient arm mobility to run martial arts classes.

Physical tasks were not the only form of specific limitation. Certain jobs involved potentially dangerous situations – most obviously driving, but also caring for children – and some health conditions made people unable to carry these out safely, arising from either physical (epilepsy, diabetic comas) or mental health complaints ('fuzziness' from medication, alcoholism, poor concentration). Two people who had experienced a traumatic event at work also said they were unable to go back into the industries they had worked in for many years (airlines and textiles for Adrianna and Steve respectively) because of the bad memories it brought back.

47.1.2. Demands-dependent limitations

High demands had beneficial aspects – many people found challenging work was more rewarding (section 50.1) – but it could turn *individual tasks* that were possible into a *job* that was impossible. The resulting limitations were both physical and mental.

Physically, working faster and for longer with a health problem could lead to pain or exhaustion, which was both problematic in itself and harmful to respondents' wider lives (section 50.1). Demands-dependent limitations could also be task-specific, in that there was one particular aspect of the job that was difficult to sustain. Most commonly this concerned posture: people could manage to sit or stand in certain positions, but only for a limited length of time before the pain became overwhelming (section 51.4.2).

Mentally, the main problem of higher demands was ‘stress’. While stress is a multifaceted phenomenon (Kinman and Jones 2005) sometimes used by participants to describe workplace conflicts or responsibilities, I here refer to the feeling of being unable to cope with the quantity or intensity of work, which was a critical issue for several participants. Sometimes stress would arise among people without prior health problems; Maryah described breaking into tears and having dreams of being burnt alive as a result of her ‘un-doable’ workload as a marketing director. For others, it was a pre-existing health problem that made it particularly difficult to cope with their job’s demands – *“I could either do the job or handle the depression, but I couldn’t do both”,* as Lindsey put it. I return to the role of stress in section 49.1.1.

47.1.3. Interpersonal limitations

Dealing with clients or customers was a major part of many jobs, from bus drivers to retail assistants to cancer nurses. Given that ‘emotional demands’ has been linked to ill-health and sickness absence (Chapter 1), I expected interpersonal limitations to be important in fitness-for-work judgements – but what the data showed was more nuanced. Certainly there were people who struggled in dealings with customers; for example, Dele was trying to avoid people-focused jobs given his schizophrenia. But more commonly I heard people with mental health problems speaking about how rewarding they found interacting with people at work, and how they could cope relatively easily with negative interactions. While Sarah ultimately left her supermarket job due to her depression, she said that *“even when I’m down, I can stand in front of a customer like a robot”* when they were angry.

Rather than with clients/customers, it was instead the relationships with *colleagues* that commonly posed problems. In some instances relationships were cordial but problems were caused by isolated behaviours or occasions, such as for Lindsey who got on well with her manager, but handed in her notice after her manager had a bad day and took it out on her. For others there was a complete relationship breakdown – sometimes described as ‘bullying’ – and this was a major contributing factor to their mental health problems and departure from work. Speaking about systematic theft among his subordinates, Steve explained *“that’s what put me in hospital* [when he was sectioned]*, Ben, was the thought that I’d been let down by my so-called friends.”* While I expected interpersonal limitations to only be relevant for people-centred jobs, it instead seemed that they can cause problems in most settings, particularly those involving intensive team-based work.

47.1.4. Performance limitations

Finally, people could be able to perform tasks at work but struggle to achieve the expected level of performance. This could be because they physically could not do the work as quickly as needed, but at least in this sample, the limitations were primarily mental:

> *"When I first started back at my job, I ended up in tears a couple of times because I just couldn't do the job… I went back to a job I'd done previously for three months and all of a sudden it was (...) finding the information that I needed, that I knew was in my brain that I couldn't find at the time and I think more frustration than anything else. And fear."* – Erica, retail job

I explore this in more detail in section 49.1.1 below.

48. CONTROL , ACCOMMODATIONS, AND THE LOGIC OF WORK

In the previous section, I described how fitness-for-work limitations are often *partial* rather than *absolute*, and that there are various types of partial limitations (task-specific, demands-dependent, interpersonal, and performance-related). The next two sections develop this by looking at whether people can *change* their original working conditions to better fit their limitations, beginning with whether participants could adjust their existing job.

48.1. Making accommodations

The simplest way of fitting an existing job around a task-specific limitation was to stop or change problematic tasks. For example, several participants' employers were happy to remove heavy lifting from their roles, those with musculoskeletal pain often received an improved physical environment (e.g. a headset for speaking on the phone), and Melanie was temporarily allowed to miss out her turn on the tills given her recent hand operation. This could also include allowing regular breaks for those who struggled to maintain a fixed posture (see section 51.4.2).

Some mental health problems also led to task-specific limitations that could be accommodated. This was relatively rare, but Sarah was – eventually – freed from working on the tills (which would send her into panic attacks), while Yusuf was sent to clean stairwells on rainy days (when he could not deal with the resulting chaos of the shopping centre).

A different form of accommodation was to reduce demands on a person's return-to-work after absence. When starting a new job some months after a breakdown, Cheryl was eased into the role gradually – starting with no responsibility and only working until lunchtime, and gradually taking on more demands. As she said, *"I'm thankful for that cos I don't know how I would get back into employment otherwise."*

Finally, in selected cases employers could radically change working conditions by redeploying workers – a response that I saw twice among this sample. Redeployment for Liam meant avoiding interpersonal problems ('mickey-taking') at the bookies he worked at; while Helen was moved from being a police officer to desk work on the instructions of the occupational health department. Redeployment was an ideal solution for people with a health problem – but for practical reasons these were only available in larger employers with suitable vacancies.

48.2. Job control

Aside from formal adjustments, workers' level of control also influenced their ability to work despite ill-health. A simple form of this was the ability to take regular breaks for those with problems in maintaining a fixed posture, but the impact of control went deeper than this for those with fluctuating conditions (50% of recent incapacity claimants, according to Sainsbury et al 2008:151). Johansson et al (2004:1859) use the term 'adjustment latitude' to refer to as *"the opportunity people have to reduce or in other ways alter their work effort when e.g. feeling ill."* This allows people to reallocate their demands to work around episodic problems on days where it was impossible to do certain tasks (task-specific limitations), work at a fast pace (demands-dependent or productivity-related limitations) or indeed do any work at all (absolute limitations).

"'Do I work as quickly when I'm in a lot of pain', then I think well hand on heart I've got to say no," Yvette explained, but "*the amount of work I do at the end of the day is the same, but it takes more effort. And you know if I feel like one day I've had a crap day, then maybe the next day I'll try and work harder."* She therefore said that she did not have a work-limiting disability, despite her health often affecting her work. In contrast, those without adjustment latitude could not conceal their limitations (see also Irvine 2011a), and their off-days were much more visible to their employer. Sarah described how she worked 'incredibly hard' at the supermarket on her good days, but just had other days where her depression made her unmotivated and unable to do anything. While her good manager was understanding, other managers would apply pressure to her that she experienced as 'bullying' – with consequences I explore in section 49.1.

48.3. The limits of accommodations and control

These were the ways in which fitness-for-work limitations could be fitted around work, then, but what of the people for whom they could not?

Accommodations were constrained in two ways. First, some people would simply not tell their employer about their health problem, because they wanted to cope themselves or they expected their employer to be unsympathetic (section 51.4.2). Second, employers were only willing to make 'reasonable adjustments' (in the words of the Disability Discrimination Act). Other adjustments were seen as impossible, particularly where they clashed with the core task in a job. These could be for concentration lapses (Damian simply could not continue as an electrician), core physical demands (such as in catering), and a variety of other issues.

For some people, then, control and accommodations could be substitutes for one another – Helen requested formal accommodations to take breaks to stretch her back in her police desk job, but this was a regular feature of Cheryl's office job. More commonly, though, control and accommodations were not substitutable:

- Those with high-control jobs could take breaks without disclosing their condition to their employer, but this was not available for others.
- Jobs were either structured by the employer to allow adjustment latitude or they were not; there were no suggestions that a particular manager could change this for individual employees. Instead, adjustment latitude was a feature of high autonomy jobs where managerial control was exercised through targets, rather than serving clients/customers or working directly in teams:

  "If you want to take longer for lunch, take longer for lunch. If you want to turn up late, turn up late. But you're going to have to do the work somewhere along the way, it's up to you when you do it" – Nick, property development
- Conversely, control over the timing and organisation of work only rarely enabled people to avoid specific problematic tasks. Marjorie would have been able to continue as a primary school teacher if she had greater control over her work pace – technically it would have been possible to work around her back problem, but "*you don't have time to do that on a daily basis because you've got three hours to get all this work done."* The only other examples were Naveed and Jemimah, whose self-employment gave them exceptional levels of control that enabled them to work around substantial limitations. In general, though, accommodations were necessary to overcome task-specific limitations.

48.4. The difficulty in reducing job demands

For demands-dependent limitations, adjustment latitude could aid coping with fluctuating limitations, while accommodations could reduce demands temporarily after illness. Yet there seemed to participants almost no scope whatsoever for permanently reducing demands.[131] Some felt there was no point in even contemplating such a change; as Scott put it, *"there is no room for manoeuvre. I know as far as I was concerned, the pressure was always going to be there."* Others tried to persuade their employers to reduce the demands without success; Lindsey's charity shop *"saw it as making excuses not to do your job."*

Given that this was not a study of employers, I can say only a limited amount about the reasons behind this (although see the Discussion). But it seems plausible that this was due to the pressure to produce a profit and/or meet targets as some participants

[131] Note however that demands is made up of two components: mandatory and discretionary demands (Green 2006). Workers could choose to reduce their discretionary effort; Yvette had 'definitely pulled back' in response to feeling under-valued at work.

argued; Lindsey explained her area manager's resistance to cutting her workload through a cascading series of bureaucratic pressures. This also is implied by the exceptional circumstances of cases where demands *were* lowered. Two self-employed people managed to cut their demands by buying-in additional staff – but as a result they were struggling to make ends meet. In contrast, Ricardo was working in a low-demands job as a church caretaker after his motorbike accident, a job he had got through a friend. The church was under no financial pressure, and Ricardo tellingly described his job as 'a gesture', an act of charity outside the normal logic of work.

48.4.1. __Coping with demands-dependent limitations__

The only remaining choice for those with demands-dependent limitations was to reduce the hours of work – and this was a choice they frequently took. This enabled them to finish work before their body and mind were pushed to breaking point, and for them to recover from the physical and mental exhaustion of coping with a health problem in work. Erica's struggles are typical here:

> *"It's mental* (laughs)*… Absolute madness… It's like the Third World. [The clothes retailer] work you really, really hard."*
>
> [Later in the interview]: "*I'm not physically capable of this 20 hours a week, running this house - which isn't a lot because you can see the state of it (laughs) - and a private life. I'm not capable of it all and I just literally don't have a private life. No friends, no boyfriend – no nothing."*

This tactic was particularly successful for jobs where the workload was determined by the hours of work. Where the job was defined by completing a specific set of tasks, it could instead be preferable to work *more* hours with a manageable level of intensity – although this could be a symptom of an inability to meet the demands of the job. Where workers were struggling without any ability to reduce demands, this could lead to short-run performance limitations and problematic long-run results (section 49.1.1).

49. The Role of Employers

If changing the nature of the job fails, then a person with specific fitness-for-work limitations had three options. They could try and struggle on in their current job. They could try and change job, either with the same employer or with another organisation. Or they could stop work. These choices heavily were constrained by the behaviour of employers, and it is this employer role that is the focus of the current section.

49.1. The current employer

Among those in the sample that had left work, there was a variety of ways of parting from their employer. Participants had variously (i) lost their job for reasons unrelated to their health; (ii) handed in their notice, either at a particular point of crisis or relatively early-on during their absence; (iii) lost their job for health reasons, sometimes after a period of sickness absence; or (iv) taken sickness absence but were still formally contracted to their employer. For the latter two groups – and also for those who remained in work – the decisions of their employer could therefore play an important role.

49.1.1. Dealing with impaired performance

Despite impaired performance (section 47.1.4), interpersonal conflicts and task-specific limitations, in no case was an employee in this sample actually sacked for poor performance.[132] Sometimes this was because the decrease in performance was sufficiently small that the employer was not troubled, particularly if the manager was understanding (as in Yusuf's cleaning job) and the worker got better at coping (as happened to Erica following the quote in section 47.1.4). Alternatively, for workers with adjustment latitude, it could be possible to conceal intermittent poor performance on other days by working harder or putting in extra hours. Nick took this to extremes when his neck pain was particularly bad: *"I said I was going looking for properties… and I'd come home and I'd just lie down all day, and not get a sick note."*

Yet a small number of cases suggested a further possibility that I had not expected: that impaired performance was transformed into other fitness-for-work limitations, and it is these *transformed* limitations that led people to lose their jobs. Sometimes struggling workers did not come to work or were sent home on the basis of being unfit-for-work, with the employer dealing with ill-health as an issue of absence rather than performance (as discussed below). But the more concerning cases were those where performance limitations were changed into demands-dependent limitations – in other

[132] This does occasionally happen though, particularly for non-disclosed mental health problems (Sainsbury et al 2008).

words, employers applied pressure until the main limitation was a more serious problem of stress.

Scott's experience as a maintenance planner shows this vividly. He had suffered severe pneumonia, and his brain was only gradually recovering after being deprived of oxygen. He returned as quickly as possible from sickness absence only to find that he had an enormous backlog of work, combined with the constant pressure inherent in his job and an inability to concentrate for long enough to catch up. He was getting pulled into meetings and asked to explain himself, which combined with the natural anxiety he had previously overcome, led him to panic in meetings ('they were hell'). In the end he broke down on the way to work, and after a period of absence his contract was terminated through mutual agreement.

Scott had not told his employers that he was struggling, but Sarah suspected that pressure – which she experienced as 'bullying' – was being deliberately applied to her in order to get her to leave, a tactic that was ultimately successful. Bullying here was not an isolated interpersonal clash, but a systematic result of performance limitations; other supermarkets would be the same, she said, *'it's the way they're trained'*:

> *"Blatantly they're told, "Don't let them get away with that. I want them sacked. They're on the sickness record. They're costing us a lot of money. I want them sacked." You know what I mean? You can't prove it….I've sat there in the office and I've heard them talking about other people… I'd hear them saying, "Right, what we want to do is if we get him – if we provoke him and get him worked up blah, blah, blah and then we can sack him on those ground. But we need to do that first, or else we can't sack him."*

In these cases the original fitness-for-work limitation was *not* stress. Rather, there was initially a *different* physical or mental performance limitation ('distress' rather than 'stress'; Dobson and Schnall 2009) (see also Sainsbury et al 2008), which was then transformed into an acute problem of stress and sickness absence, and culminated in leaving that job.

In summary, it was not the case in this sample that employees lost their jobs directly due to impaired performance. Instead, employers kept workers without reducing demands (see sections 48.3 and 48.4). Some people struggled on in this situation, but others ended up taking sickness absence (see below), and in the worst cases this built up into an acute problem of stress that ultimately led to mental health problems and leaving that job.

49.1.2. Dealing with sickness absence

An alternative to struggling on was to take sickness absence – a decision influenced by a number of factors I discuss in section 50, and sometimes made by the employer themselves. However, workers were much more likely to lose their jobs due to sickness absence than impaired performance. A few people formally had their employment contract terminated after several months of sickness absence – but sometimes this was by mutual consent when it was clear to both parties that there was no prospect of returning to work in the near future. Ricardo's relatively informal working relationship delivering pizzas meant that his employment was effectively terminated as soon as his motorbike accident had occurred.

Where people had a single period of absence for an acute health episode (such as for an operation) and then returned to work, managers could apply pressure to return quickly – but generally employers were sympathetic, with occupational health departments (where they existed) playing a major role. Instead, problems occurred for repeated short-term absences. Melanie was generally performing well in her job, but she described how her badly managed diabetes – she'd had three diabetic comas in the *week* before the interview – conflicted with her supermarket's absence policy:

> *"You're just forever getting warnings whenever you're sick... When an ambulance is called to work [after going hypo], I won't let them take me away because that will by my percentage [that monitors absence] gone up again. That is why we all go back to work, you know. …So it's not a good place to be"*

Several other participants described similar pressures to minimise absence or else face the sack, and it was through repeated absences due to depression that Liam lost his job. Notably for Liam, his manager had also suffered depression and with *"another manager [I] would have lost my job. Probably about a year previous"*. But ultimately his manager had to sack him or else face the sack himself for contravening the formal sickness management policy of the delivery company. Individual managers could be more or less tolerant, but were constrained by the wider framework they operated in.

Overall, there are two ways in which employers decided that a worker should leave. Long absences could eventually culminate in the employer terminating the contract. Alternatively, employers would ultimately lose patience after repeated short absences – an issue for those debilitated by regular acute episodes (epilepsy, panic attacks, diabetic comas) or fluctuating chronic conditions (musculoskeletal pain, depression) who did not have adjustment latitude.

49.2. Potential employers

If staying in their current job became impossible – or even undesirable – but they wanted to work, participants' thoughts turned to getting a suitable job where their fitness-for-work limitations were less of a problem. Yet whether such thoughts were realistic varied considerably from person to person.

For a start, individuals had different levels of employability. Those with skills and experience in short supply in the labour market were confident that they could find a job. Others were more worried, citing a lack of qualifications, their age, or poor language skills. And nearly everyone felt that it was a difficult time to be looking for work (late 2009/early 2010, during the recession).

But beyond this, health was a further barrier to work. Participant after participant felt that telling an employer about health problems at interview would harm their chances of getting a job, whether due to stigma (Dele's fear of being branded a 'mad man') or future sickness absence (Nick saying that he would not take someone on in his condition). Hiding health problems was one response, used by both Nick and Dele and reportedly recommended by some agency/Jobcentre staff. Others were more open, either because they instinctively felt obliged to reveal their conditions, or because lies on occupational health questionnaires[133] may only be credible for a short amount of time. As Sarah bluntly put it, *"if I try and go to a new job, within two weeks they're gonna notice I'm fucked up. They're going to sack me."*

49.2.1. Beyond 'hidden unemployment'

So far, this picture fits the theory of Beatty et al (2000) – the labour market is a 'queue for jobs', and those with health problems are pushed to the back of it and become the 'hidden unemployed'. However, there are several subtleties when we consider that individuals have to find a *suitable* job.

First, we might expect that the greater the requirements of jobs in general, the harder it will be to find a suitable job – although investigating this requires different methods from those used here. Second, and more visibly, several participants knew that they had skills that were valued by employers. They also knew that there were jobs that were compatible with their health. The trouble was these two sets of jobs simply did not match up:

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[133] The 2010 Equality Act made it unlawful to ask detailed health/sickness absence questions before offering a person a job. However, the practical effect of this is limited as (i) employers can still ask if candidates would 'be able to carry out a function intrinsic to the work concerned'; and (ii) only the EHRC can bring a claim against an employer (see http://www.time-to-change.org.uk/blog/equality-act-2010-answering-employer%E2%80%99s-questions-about-health, accessed 27/4/2011).

> "[On catering work] *If they want to give me £50, I don't give him £50 job, I would give him about £22... If I got a good education, then I got a choice, I can do it or not, take it or leave it. Because I haven't got any education. I have to do it"* – Ali

This Catch-22 situation explained why Ali saw no other prospect than staying on incapacity benefits until retirement. Others were similarly trying to change from a sector in which they had worked but was no longer suitable, to another sector for which they had no relevant skills or experience. As several said: other than the job they have been doing*, 'what other jobs can I do?'*

The situation was entirely different where people were not looking to change career entirely, and for those that were more highly-skilled. Scott was looking for a less stressful job that minimised travel, but was able to build on his previous ICT experience and was just at the point of going for an interview for a suitable job near his home. At the other extreme, those with no qualifications and stigmatising health problems were sometimes worried about getting another job even in exactly the same area they had previously worked. The Catch-22 was by no-means typical, then; but where it existed it made the prospect of work seem remote.

50. Other Influences

There is only a limited space for individuals to make their own decisions, then, between the decisions of actual and potential employers. But this space does exist, and this leaves individuals with a certain amount of agency. As well as fitness-for-work, a number of other considerations therefore become important. While these are not the major focus here, I briefly review the major factors that emerged from the interviews as a stepping stone to a broader conceptual model in section 51.

50.1. Wanting to work

Nearly everyone said that they would ideally be working, particularly when compared to being on benefits; those unable to avoid claiming ESA variously describing this as 'hell to go through', 'abhorrent to me', feeling 'ashamed', and not wanting to be a 'dole bludger'. As in other research, work was generally seen as a positive force for mental health and wellbeing (Loretto and White 2006; Vickerstaff 2006; Sainsbury et al 2008). Other views were occasionally heard – from two women under no pressure to return-to-work after their caring responsibilities shrank, Ashley's ambivalence about insecure low-pay work vs. claiming incapacity benefits, and Adrianna saying she could not bring herself to *'just go work in Tesco's or whatever'* after she recovered from PTSD – but these were exceptions in the midst of a general enthusiasm for work.

Alongside this, there was much greater variety around people's satisfaction with *their particular job*. Some were highly committed, whether 'feeling bad' for colleagues/clients when absent, or just 'loving' the job – which occasionally involved the positive sides to high demands:

> *"Stress was fun. Stress was enjoyable. Stress was something interesting for the night, you know?... It was fun and the worse it got, the more fun it was"*
>
> – Erica, on her job as dance manager in a strip club

Others however disliked their job – particularly those who continued working through health problems, for one of three reasons. First, while respondents may have been *able* to work, doing so may actively have been damaging their health. Respondents variously felt (or were told) that work was causing musculoskeletal problems, a slowed pace of post-operation recovery, and even the deterioration of a longstanding heart complaint. Where work was causing mental ill-health this usually led to a breakdown or change of job, but only after people had continued to work through this for a while – in the intervening period, Steve would *'come home and cry'* after work, while Sarah desperately wanted to work but felt that her job was *'seventy-to-eighty percent of my depression'*.

Second, work may not be causing long-term damage to a person's health but may cause physical pain in the short-run. There were several examples of this, from feeling *'very uncomfortable constantly'* to *'gritting my teeth all the way through it... Any rational person would not have done it'*. Finally, coping with the mental and physical stresses of work could exhaust people and make them unable to function in their wider lives. Those with physical health problems sometimes said that it took several days to recover from work, while Lindsey isolated herself socially as she could not deal with people at night after dealing with them all day.

These were all reasons why certain individuals left particular jobs – but others continued to work through these. We may speculate that this reflects attitudinal differences (although this is necessarily speculative as these are difficult to measure objectively; Halligan et al 2003), yet *most* respondents were understandably unhappy if they were working through considerable pain, risks to their health or an inability to live beyond their work. What instead seemed to determine work outcomes was whether people felt they had any choice but to continue working, and it is to these other constraints I now turn.

50.2. Other factors influencing the decision to work

For several participants, there was no 'choice' in staying at work as they felt compelled to work – or to keep up their particular job or hours of work – because of their financial needs. Those on benefits sometimes spoke about the urgency of returning to work given the difficulty of surviving on this income, although benefits did provide a way for people to get by without working, and it could be a severe problem then this was unavailable (for non-EU nationals and those failing ESA medicals). In contrast, those with more resources could survive temporarily by drawing down savings rather than claiming benefits, and maintain this indefinitely if they had a partner who was working.

Another consideration was where caring responsibilities needed to be met, primarily to young children, but also to elderly parents. Three women with mental health problems had stopped work to care for their children, with the household surviving from their husband's income. Family crises such as a child being excluded from school or the death of a partner could even lead people to leave work in order to help their family. In one case this was medicalised; her daughter's threat of suicide led Elizabeth to be signed off sick with depression, but it was primarily the need to care for her daughter while she refused to go to school that caused her to take a career break.

Finally, much has been made of the assumed role of doctors in individuals' fitness-for-work judgements (see Discussion). It was therefore striking that while doctors' role in providing treatment was valued, their role in fitness-for-work judgements was marginal. Many doctors had no idea whether their patient was working or what their job was, instead simply signing sick notes on request. Where doctors did speak about work, this was primarily to discourage people from working – Naveed's report of his doctor's reaction being an extreme example:

> *"I'm telling you that you can't work. Do you understand me? You're telling me that you've been working and I'm shocked, I don't want you to work"*

Doctors were generally heeded when they spoke about invisible health risks or recovery from novel treatments, but otherwise such discouragement was overwhelmed by other forces. Continuing the example above, Naveed ignored his doctor because he loved his job, had a mortgage to pay, was strongly against claiming benefits, and had substantial control over his job that enabled him to continue working (albeit with pain).

Moreover, participants had strong views of their own as to whether they were fit-for-work that doctors were often powerless to change. Yusuf felt completely unable to work, and when his GP gave him 'little lectures' about work he simply threatened to change GP. On only one occasion did a GP seem to nudge a participant towards work, when Steve was teetering on the brink of responding to a disability-focused welfare-to-work provider.

Medical professionals were generally unimportant, then, but attitudes, finances, and caring responsibilities all played a part in determining whether a person was working.

51. INTEGRATING MIXED METHODS

Many nominally 'mixed methods' studies struggle to integrate their various components (Bryman 2007), so in this section I explicitly develop the analysis to address the earlier quantitative analyses. I focus on four implications: (i) the nature of WLD; (ii) the role of demands; (iii) the role of control; and (iv) pathways from WLD to incapacity benefits.

51.1. The nature of WLD

In section 47 I described the nature of fitness-for-work limitations – but for the quantitative analyses, the crucial question is how these limitations relate to survey questions on WLD. At the end of the interviews, I therefore asked participants to answer a short series of WLD questions as if they were in a survey interview (cf. Gooberman-Hill et al 2003; Adamson et al 2004), using cognitive testing techniques.[134] While there is not the space to report the results in detail, there are two points that have important implications.

First, the WII WLD questions ask about 'work or other regular daily activities'. I noted in Chapter 3 that these questions may be interpreted as *activity* limitations as well as work limitations, and this was borne out here; while several participants focused on work, many spoke about daily activities such as shopping. This was particularly true for those who were not working and therefore could not think about work activities in the 'past four weeks'. The questions were interpreted as more work-focused among those working, and I re-interpret the WII results & mediation analyses in this light in Chapter 9.

Second, respondents differed in the types of jobs they were thinking about when answering the BHPS/LFS questions. Some said they had focused only on jobs they had done in the past or were doing at the time, others thought about all jobs in the economy – but most said they were thinking about jobs that they would *consider* doing. In one case, this meant that a back problem was not interpreted as a WLD because they simply would not consider doing physical work:

> *"I wouldn't say go and dredge canals or, probably go and work in a supermarket where I'm lifting boxes onto shelves or things like that. But because I wouldn't want to do that anyway it's not affecting it"* – Maureen

This focus on *potential jobs* contrasts markedly from the WII questions where people were thinking about their *present job* (as prompted by a focus on the past four weeks).

[134] Half were given written forms of the LFS WLD question and WII scales, and half were given verbal forms of the BHPS WLD questions.

Not only may this explain the difference between the WII and BHPS results, but it may also explain why higher demands seems to *reduce* the risk of WLD in BHPS (as discussed in Chapter 6). I return to this in some detail in Chapter 9.

51.2. The role of demands

It is difficult in qualitative research to pick out specific cases where demands 'made a difference' – people's lives are complex with multiple interacting factors that produce particular outcomes. Nevertheless, we can identify those respondents for whom demands in their particular job seemed to play some role in their fitness-for-work, alongside other factors.

Job demands is mentioned in nearly every qualitative study on health and work (e.g. Kennedy et al 2007; Gewurtz and Kirs 2009), and here I focus on what I termed 'demands-dependent limitations', where the problem was mental or physical strain. (I do not focus on the role of demands in providing challenges and rewards at work, but this should be borne in mind (Karasek and Theorell 1992)).

The *physical* strain of high demands made Tom plan to change jobs, while Ricardo was only able to work due to his exceptionally light role. Physical strain also led to exhaustion, with several respondents (Erica, Naveed, Jemimah) limiting their hours but still finding knock-on impacts on their non-work lives.

High demands also led to *mental* strain (the 'stress' that caused Lindsey, Sarah, Scott, Yusuf and Maryah to leave their jobs and contributed to Steve's problems, and whose temporary reduction enabled Cheryl to return to work). Given the debate about the 'subjective' vs. 'objective' nature of demands, it is worth emphasising how stress was a particular problem *for those with pre-existing physical or mental conditions*, where no allowance was made for performance limitations. Cheryl, Lindsey, Sarah and Yusuf all spoke about how their mental health made it difficult to deal with demands that other people could cope with:

> *"I'm not [my manager]. I'm me. It's what I can reasonably do. What my strength is suited for. But they don't see it like that"* – Lindsey

In line with the literature on 'job crafting' (Chapter 6), Sarah argued that her health made it impossible for her to negotiate her workload effectively – other staff would reply *'just don't pressure me'* to the managers, but "*I can't do that, I'd end up bursting into tears.*"

Still, if we accept that demands are only a problem for those with insufficient resources to cope (Chapter 1), then – holding resources constant – a rise in demands would be expected to increase demands-dependent limitations. It is difficult to test this using BHPS/WII as we have no ability to hold resources constant, but from the resulting picture of fitness-for-work processes, we can infer that demands is *ceteris paribus* likely to influence fitness-for-work.

Finally, there were suggestions that poor interpersonal relations partly resulted from high demands, which meant that people had 'no time' or 'can't be bothered' to develop friendships. While others here described high demands alongside supportive social environments, wider research suggests that that such a link may be genuine (Salin 2003:1222-5; Aquino and Thau 2009:726-7; McCann 2009:246; Stoetzer et al 2009).

51.3. The role of control

Control did not just determine whether those with a limitation continued in work, but also whether they felt they had a limitation to begin with, in two ways. First, if it was necessary to take regular breaks from a fixed posture, some had sufficient control to take these easily (Cheryl), others could adjust their jobs to accommodate this (Jemimah, Helen), but those with low control and no possibility of accommodations had substantial problems (Ali, Khaled). Second, adjustment latitude – the ability to fit work around bad days – could make the difference between normal and impaired performance. Several people managed to work around pain (Nick, Tessa, Yvette) but Sarah's bad days were visible to her managers in the supermarket and contributed to what she perceived as their bullying.

In the light of the trends identified in Chapter 2, it seems likely that many survey questions are proxies for adjustment latitude in some way (control over work pace, how work is done and task choice), and that we have therefore seen declining adjustment latitude. Yet home working is related to adjustment latitude too (Kelliher and Anderson 2008), and this has risen in recent decades (Chapter 1). While I would expect home working only to be possible for those who *already* have adjustment latitude – and therefore declining control in the population as a whole can coexist with rising home working for the minority with adjustment latitude – further research would be valuable in testing this explicitly.

51.4. Pathways from WLD to incapacity benefits

In the quantitative analyses, I hypothesised a link between demands-control and fitness-for-work, and that this would culminate in health-related job loss and ultimately incapacity claims. However, the data here allow me to develop a fuller conceptual model of the link between demands-control and incapacity, and this is shown in Figure

56. This does not deal with how such processes unfold over time (which is well covered in Sainsbury and Davidson 2006; Tjulin et al 2010), the critical role of changes in health (Kemp and Davidson 2010:216), or the role of working conditions in job satisfaction, but rather addresses the factors that influence whether initial fitness-for-work limitations result in job loss and incapacity benefits receipt.

This model draws attention to the critical role of *potential* jobs as well as actual working conditions. When people are faced with a fitness-for-work limitation that cannot be accommodated, then they can change to a different job that suits their limitation – but this is only possible if such jobs exist and they are able to get them. When interpreting the quantitative analyses, this means that (i) there is likely to be 'reverse causality' such that workers with health problems move over time into more suitable jobs; (ii) I have only captured part of the impact of demands-control on incapacity receipt, looking at the effect of people's current job but not the changing nature of potential jobs; (iii) we would expect the role of demands-control in a particular job to be greater for people who are less able to change jobs. In Chapter 9, I recommend extending the quantitative analyses to interact demands-control with skills and local labour demand.

More broadly, Figure 56 draws attention to the reasons why some people with fitness-for-work limitations leave work and claim incapacity benefits, while others – sometimes with greater limitations – continue working. In the quantitative analyses, this draws attention to the factors that should be controlled for in the regression models, and suggests that we should expect considerable heterogeneity in the effects of demands-control. Leaving work seemed more likely for those who:

...had employers who were less likely to adjust the job;

…had employers who were less tolerant of poor performance / absence;

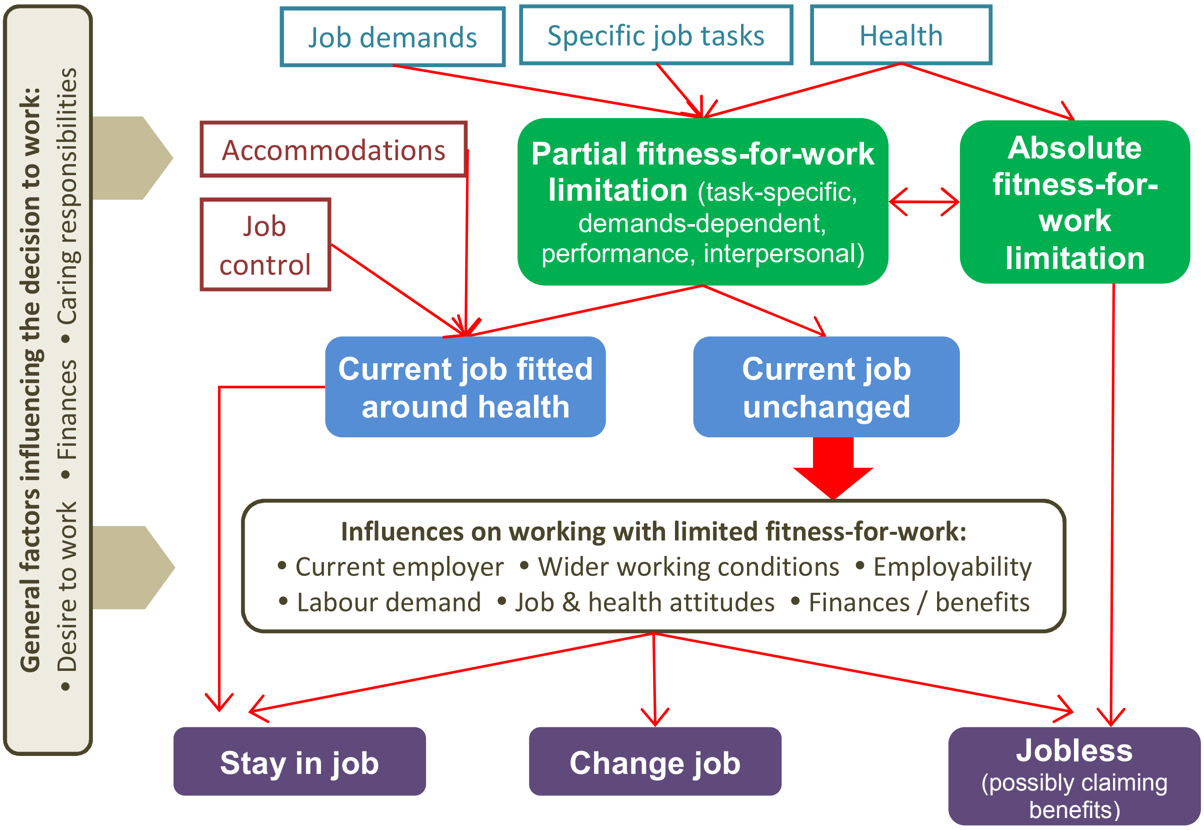
...were less able to get another job;

...were less committed to their jobs and (in rare cases) to work in general;

...had greater caring commitments;

…could get by financially out-of-work (either through other sources of income or incapacity benefits)

Figure 56: Simplified model of working conditions and fitness-for-work



While the analysis so far has been presented in terms of particular themes (as in the 'holistic work ability model' of Ilmarinen et al 2005), in practice these factors combine with one another in the complex web implied by Figure 56. It is clearest to illustrate these inter-relationships in two case studies.

51.4.1. Case study 1 (Lindsey)

Lindsey had managed her depression for many years while working in charity shops, but had been finding it increasingly difficult to cope. She said that everyone thought her assistant manager job 'was a doddle', but it's actually 'very pressurised' given the weekly targets that management now placed on staff – and trying to speak to the employer about the workload was simply seen as 'making excuses'. This left a 'vicious circle' of pressure and depression, where she was isolating herself from friends and family. When her otherwise understanding manager took out a bad day on her, she thought *"I just thought ' no, I can't deal with this anymore', and I just decided, I don't know, put my notice in and go because it just seemed =Rather than sort of like argue it out with her"* – although if it had not been for this argument, she felt she could not have carried on much longer anyway.

This was during the recession, and Lindsey soon realised it was a bad time to be looking for work – she had only ever worked in retail, and the 'dole queues will be flooded with people' looking for these jobs. Yet applying for benefits would simply have been swapping the pressure of her old job for the pressure of the Jobcentre, so she instead got by on the little savings she had. Ten months after leaving her job her savings had run out and she 'couldn't see any hope', so she took an overdose. Her sister came as soon as she received the suicide note, took her to hospital, and later filled in the ESA application forms. By the time I spoke to her she was somewhat better, and – heavily reliant on her private Welfare-to-Work provider, who she described as 'wonderful' – she was thinking about returning-to-work.

Lindsey's story illustrates the complex relationship between working conditions and other factors. She was struggling in work as her depression made it hard to cope with the job demands, but it was a minor argument at work that precipitated her exit. It was then her limited employability (due to age and low skills) and generally low levels of labour demand that made her attempts to find work unsuccessful, which in turn led to a further deterioration of her health. In a complex path lasting many months, she was able to delay claiming benefits due to her financial resources, but ultimately found herself on ESA when the money ran out and her health deteriorated.

*51.4.2. **Case study 2** (Khlaled)*

Khaled's situation was quite different. He was forced to sit down for nine hours in his job as a bus driver (who have the lowest control of any occupation; Chapter 5). This led to back pain he described as *'like having a knife in your back, cutting across'*; sometimes he could not feel his feet by the end of a shift, and he was too exhausted when he got home to socialise. The main option available to him was to stop the Saturday overtime he used to do; nevertheless he still said he had to *'push myself to the limit'* just to get through the working week. He was not willing to tell his employer about his back problem for fear of losing his job, which was also the reason he tried to minimise sickness absence (including when returning-to-work after a hand operation).

Khaled was in no doubt that he could claim incapacity benefits if he needed to. He had briefly done this before due to an injury, and repeatedly stressed he had 'evidence, evidence' of his back problems on X-rays and MRI scans that he could show to benefits assessors. However, he did not want to deny his kids the right *'to have that just* little bit *extra*', and therefore felt he could not afford to stop working or to reduce his hours further. Furthermore, the only jobs he could get that would be less painful would have led to an unacceptable drop in salary. At one point he was even offered a retail job by a cousin, but this had half the income of his present job and was, he felt, a *'sinking man's job'*. While this may sound like a 'choice', it certainly wasn't experienced as such – *“It's recommended [by doctors] to do something else, but (...) there's not much that can be done.”*

Job demands (in terms of sustained shifts), a lack of control, low levels of employability, and financial pressures all conspired to leave Khaled arguably not fit-for-work, yet still working.

52. DISCUSSION

52.1. Drawing conclusions from qualitative data

There are two main issues we have to face in interpreting the results in this chapter. First, the most common form of generalisation in survey research is 'representational generalisation' to a wider population, which follows from the idealised random sample. This is *not* the form of generalisation I am claiming. Instead, I am aiming for 'theoretical generalisation' (Ritchie and Lewis 2003); that is, to generalise from the *processes* operating in a sample to the *processes* operating in the population more generally, by creating a theory that can account for the varied cases within the sample. *"The only very important rule for selecting the sample"*, according to Gobo, (2004:444), is that there should be sufficient data on the full range of variation of the phenomenon under study (the 'theoretical saturation' of Strauss and Corbin 1990).

The achieved sample was largely successful, with two caveats: I assume that the same issues around fitness-for-work operate for rare conditions that were not represented in the sample (e.g. bipolar disorder), and I only conducted interviews in one labour market (London). Otherwise the sample varied according to a number of key dimensions as shown in section 46.2. It therefore seems reasonable to generalise the theoretical account to the wider British population – bearing in mind that theories are not context-free, and must be revised when extended across time and space (Ritchie and Lewis 2003). The discussion that follows therefore changes tense, from the past tense of what happened in the sample, to the present tense of the nature of contemporary Britain.

Second, we need to consider how far these accounts represent the 'real world' rather than a particular narrative (section 46.2). Previous research has shown how the social situation of the interview and interviewer matter (Cornwell 1984), and the views of workers with health problems may be very different from those that work with them (Tjulin et al 2010). This is particularly important in the light of earlier discussion on internal vs. external assessments of working conditions (section 51.2), while section 50.1 noted the stigma associated with worklessness and this may have encouraged respondents to give 'public accounts' that validate their identity (Cornwell 1984).

While I take this into account in the Discussion below, I nevertheless largely take the participants' accounts as evidence – albeit imperfect – of 'the way things really are'. Partly this is because I did not just depend on respondents making causal links themselves, but instead analysed the pattern of responses to specific questions on work and health, and the extent which the data support a plausible theoretical account. And partly this is because the detailed questioning on working conditions, health and work trajectories provides a thick description that is less susceptible to simple social influence effects. For example, I have already described how some people felt the job demands were excessive for *them* even though they believed that others could have coped. The process of inference should nevertheless be sufficiently transparent for more sceptical readers to form their own conclusions.

52.2. Main findings

There are four main findings from this chapter:

1. *Types of fitness-for-work limitation:* some people have absolute fitness-for-work limitations that (often temporarily) prevent them doing any job. Mostly, however, fitness-for-work limitations are partial, and these limitations can be task-specific, demands-dependent, interpersonal or performance-related.
2. *Fitting work around health*: both job control and accommodating employers could make it easier to fit a job around health problems, with control and accommodations sometimes substituting for one another and at other times addressing different limitations. But there were limits to how far control or accommodations could go, particularly in reducing demands.
3. *Dealing with limitations at work*: if people could not fit their work around their health, then they could either struggle on in that job, change job, or leave work entirely. Their ability to struggle on depended on their current employer, while their ability to change job depending on their employability and wider labour demand.
4. *Individual variation*: a range of other factors were important in whether people carried on in work (e.g. caring responsibilities, finances, attachment to work). Some people therefore continued in work even though they were in pain or damaging their health, while others with less severe limitations left the labour force.

In this section I summarise each finding in turn, setting them within the wider literature on work and health.

52.2.1. Finding 1 – Fitness-for-work and job characteristics

Innumerable qualitative studies have found that working characteristics affect whether people continue working despite health problems. Yet typically these studies do not focus analytically on the inter-relationship of work and health, simply listing the different working conditions that mattered within that particular sample (e.g. Liedberg and Henriksson 2002; Kennedy et al 2007). Instead, I here started by dividing between different forms of fitness-for-work limitations. *Absolute* fitness-for-work limitations are those where people were very unwell and unable to do any work at all (see also Farrell et al 2006:18; Irvine 2011a).

Most fitness-for-work limitations, however, are partial, and these come in four types. *Task-specific* limitations are when there are particular tasks the person cannot do, including physical tasks like heavy lifting, or tasks that might be impossible for health and safety reasons like driving (see also Bailey et al 2007:137). *Performance-related* limitations are where people cannot satisfy the demands that are expected of them in a job (Sainsbury et al 2008:58). *Demands-dependent* limitations revolve around physical or mental strain, where the intensity and workload in a job turns individual tasks that are possible into a job that is impossible (Sainsbury et al 2008:34) – limitations that incapacity claimants commonly argue are not sufficiently taken into account in DWP medical assessments (e.g. Hedges and Sykes 2001:134). This type of limitation also includes the physical strain of maintaining a fixed posture for long periods (reported by one-in-five Pathways to Work participants with a musculoskeletal WLD; Bailey et al 2007:137).

Finally, partial limitations can be *interpersonal*. I had expected the main difficulties to arise from emotional demands from clients/customers, but it was actually much more common for people with mental health problems to cope with emotional demands while struggling with other relationships at work (as also suggested by Davidson 2006; Wilton 2008). This does not contradict existing research; interpersonal relationships are known to be important (Mackay et al 2004:98-99; Lutgen-Sandvik et al 2007; Carder et al 2009) and in one large study work-related mental health problems were seven times more likely to relate to managers than to clients/customers (Ballard et al 2008). Interpersonal limitations therefore do not seem to be limited to people-focused occupations.

52.2.2. Finding 2 – Job control and the limits of accommodations

While both formal accommodations and job control help workers with health problems to stay in work, they address different limitations:

- Control can allow workers to work around days their health is particularly bad – a widely replicated finding that goes by a variety of names,[135] but which I here follow Johansson et al (2004) in terming 'adjustment latitude'. This also allows people to take regular breaks, which helps those who struggle to stay in fixed postures for long periods.
- Workplace accommodations tackle task-specific limitations by removing/changing problematic tasks (Sainsbury et al 2008:157), and also

[135] This includes 'flexibility' (Sainsbury et al 2008:134; Irvine 2011a), 'margin of manoeuvre' (Durand et al 2009), or the lack of 'working on demand' (Gewurtz and Kirs 2009:40).

overcome demands-dependent or performance limitations through phased returns-to-work (Farrell et al 2006:63; Kennedy et al 2007; Sainsbury et al 2008:64; Gewurtz and Kirs 2009). There is demonstrable evidence that such accommodations are effective (Franche et al 2005; Tompa et al 2008).

Control and accommodations can substitute for one another, particularly in allowing employees to take regular breaks. Yet against my expectations, this substitutability seems uncommon. Control is rarely so great that it allows workers to remove particular tasks from their role; an accommodation is instead required.[136] In turn, accommodations are limited as they require workers to disclose their health condition and request special treatment, something that many prefer to avoid (Sainsbury et al 2008:54, 155; Wilton 2008:369-370; Gewurtz and Kirs 2009; Irvine 2011b).

More fundamentally, though, there are three limits to the adjustments that employers are willing to make.[137] First, adjustment latitude is an outcome of work organised around periodic targets, rather than subject to the regular demands of customers/clients, colleagues or an assembly line – and hence is difficult to create adjustment latitude for individual workers where it does not already exist. Second, jobs often have core tasks where specific limitations cannot be accommodated, and it has been argued that functional multiskilling (multi-role team-based work) extends the number of core tasks in each role (Foster and Wass, submitted).[138] Workers can instead be redeployed to a different role (Farrell et al 2006:63), but this option is often unavailable (Sainsbury et al 2008:70). Employers may also make little effort to accommodate those who (if accommodated) would still have performance limitations (Sainsbury et al 2008:75, 91-2)

Yet the greatest difficulty was for reducing demands. While demands are often temporarily lowered or flexible working provided (Simm et al 2008:64-67), employers almost never offer a *permanent* reduction in demands as this conflicts with public/private sector pressures to maximise productivity (Taylor et al 2003:452; Magnussen et al 2007:194; Sainsbury et al 2008:91-2; Gewurtz and Kirs 2009) – despite employer self-reports to the contrary (Health Work and Wellbeing Strategy Unit 2010:17). This was explicitly the justification used by managers in the two employment tribunal appeals in Foster & Wass (submitted:25), where managers insisted they could only make accommodations if they themselves were allowed some leeway in their

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[136] Workers can however make informal adjustments between themselves in supportive environments (Eakin and MacEachen 1998:910; Farrell et al 2006:67).

[137] Employers are only obliged to make 'reasonable adjustments' under the Disability Discrimination Act (Howard and Cox 2007; Foster and Wass, submitted).

[138] Foster & Wass' specific claims are however difficult to reconcile with the consistent finding (e.g. Christensen et al 2008) that more varied work in the form of 'skill discretion' is associated with *lower* risks of disability pension (see Chapter 1), with employers explaining that it was easier to accommodate workers within roles containing multiple tasks (Sainsbury et al 2008:65).

targets by higher levels of management. Workers can instead deal with demands-dependent limitations by reducing their hours (Corden and Nice 2006:55), but while this can be an accommodation of sorts (Sainsbury et al 2008:157), it is the worker rather than the employer that has to bear the cost.

These decisions about the organisation of work are taken prior to the existence of individual workers – indeed, they assume an 'ideal worker' (Foster and Wass, submitted) – and there is only a limited ability of individual managers to make exceptions in individual cases (as implied by Farrell et al 2006:62; Gewurtz and Kirs 2009:40), despite the Government's emphasis on the role of managers (Black 2008:11). Any attempt to make exceptions will introduce additional costs, and can introduce resentment among co-workers who may not be aware of their colleagues' condition (Magnussen et al 2007:193; Sainsbury et al 2008:67). Indeed, the 'engaged employers' in Sainsbury et al (2008:89) said that this was the biggest barrier they faced in making adjustments, as they were wary of 'setting a precedent' for other employees.

Rather than challenging this, workers tend to accept that the structure of work cannot be changed to fit around them (Sainsbury et al 2008:75), and the vast majority of recent incapacity claimants say that no realistic accommodations would have helped (Sainsbury et al 2008:157-9). Workplace accommodations are therefore valuable but subject to considerable constraints, limited by prior decisions about the organisation of work that have already determined demands, control, and core tasks.

52.2.3. <u>Finding 3 – Employers and employability</u>

If fitness-for-work limitations are not fully accommodated, workers can try to 'struggle on' (Sainsbury and Davidson 2006) – but they are then less productive at work or take periods of sickness absence. In some situations employers are patient, accepting performance limitations or helping people return-to-work after a period of absence, yet at other times these lead to job loss in various ways. Where workers are struggling to meet the demands of the job, this can build up into an acute problem of stress, ill-health and sickness absence. Workers absent for long periods of time may have their contracts terminated, particularly for those in insecure jobs on short-term contracts (Sainsbury and Davidson 2006:38-9). And repeated short-term absences can be grounds for being sacked (Sainsbury and Davidson 2006:51). This is a particular problem for fluctuating conditions, which are reported by half of recent incapacity claimants (Sainsbury et al 2008:151) and are sometimes felt by people to make them unemployable (Magnussen et al 2007:193-4).

People who disclose a health problem may therefore be worried about losing their job (Sainsbury et al 2008:58) – a worry that seems to be based in reality, with some employers resistant to take back employees who develop health problems (Nice 2008;

Sainsbury et al 2008:58). While not covered in detail here, the transition from absence to job loss depends on a number of features of the employment relationship, not least the Disability Discrimination Act and the contractual status of the employee (Irvine 2010). The primary point here, though, is that the possibility of struggling on can be taken out of the hands of the individual worker.

Rather than struggling on, people with health problems can change to a more suitable job where their fitness-for-work is less limited (Sainsbury et al 2008:108). Getting such a job, though, can be a problem. Older workers with low skills are not always attractive to employers, particularly when looking to change jobs to a sector for which they have no experience (where people return-to-work after claiming incapacity benefits, they often move to jobs that do not use their existing skills; Bailey et al 2007:6). This can be compounded by health problems that make people even less desirable to potential employers – over 60% of employers say they were *not* likely to recruit someone with a mental health problem (Bunt et al 2001:34) – and employers are even less willing to make accommodations for new recruits (Sainsbury et al 2008:66). People with health problems can be caught in a Catch-22 situation: they are not fit enough to do the jobs they are qualified for, and not qualified enough for the jobs they could actually do.

The role of employability and labour demand in incapacity benefit receipt is well-known and was reviewed extensively in Chapter 1 – even if it is often excluded from accounts that deal with working conditions (e.g. Hansson et al 2006). In recent versions of the theory (Beatty et al 2009; Houston and Lindsay 2010), labour demand is important not just because ill-health reduces the chances of redundant workers getting re-employed (they are pushed to the back of the 'queue for jobs'), but also because low labour demand makes employers more health-selective in retaining workers.

This theory has been justifiably influential in the current academic understanding of incapacity benefits, but the analysis here suggest three refinements. First, low labour demand is important not just because it makes it harder to get *any* job, but because it makes it harder to get a *suitable* job (Davidson 2006:195; Gewurtz and Kirs 2009:41). This is one explanation for why the employment rate for those with a WLD *and* with low skills is so low. Second, it is not just workers with observable health problems that are pushed to the back of the queue for jobs; even workers who do not disclose their health problems will struggle to find suitable work in areas of low labour demand.

Third, if working conditions deteriorate, this can be seen as *constituting* a decline in labour demand for people with health problems. If job demands increases and control at work declines, then some people will suffer fitness-for-work limitations when they previously could perform to the expected level in their job. And when they come to look for suitable work, they will find fewer suitable jobs exist. *Ceteris paribus*, rising job strain is therefore likely to reduce labour demand for people with health problems even if labour demand for people without health problems is unchanged. Working conditions should not be considered a competing explanation to labour demand, but rather as a complementary part of the same story of why certain people in certain places are much more likely to claim incapacity benefits.

52.2.4. <u>Findings 4 – Other influences on work and benefit receipt</u>

While the focus here has been on working conditions and fitness-for-work, there are a number of other factors that influence whether people with health problems are working or claiming benefits. Finances were predictably important; some people felt compelled to continue working, while others had greater financial resources that allowed them to stop work but maintain their lifestyle (at least for at time). Benefits were felt to be low and claimants usually wanted to work to get more money, but for those who were eligible they at least provided a way to survive when they were unable to work, and the risk of not being able to easily return to benefits is a disincentive to work (Purdon et al 2006:23; Magnussen et al 2007:194). Caring responsibilities also influenced work decisions, with women in particular leaving work to raise children or to deal with family crises (Irvine 2011a).

People's desire to work also played a role, although perhaps not as great a role as that assumed by policymakers or suggested in other research (Corden and Nice 2006:50; Sainsbury and Davidson 2006; Sainsbury et al 2008:102-4). Nearly everyone was positive about work *in general*, either to avoid the stigma of being on benefits or because of the benefits of work itself for mental health and wider wellbeing – although a few people were unenthusiastic about low-end low-paid work (Lindsay and McQuaid 2004; Kemp and Davidson 2010). Attitudes to their *particular* job were more mixed; people were more likely to continue working if they loved their job, were worried about losing it, or were worried about the impact of absence on colleagues/clients (Johansson and Lundberg 2004; Irvine 2011a).

Rather than being an enduring personality trait though, commitment to work was strongly affected by health. While only a minor theme here, it is emotionally demanding to manage health problems at work (Wilton 2008), and emotional support at work is therefore important for the return-to-work process (Lysaght and Larmour-Trode 2008; Tjulin 2010). Pain and exhaustion could make the job itself less enjoyable and could linger on into people's home life (Magnussen et al 2007:193); indeed, the boundary is unclear between negative emotional responses and medicalised mental health problems (Irvine 2011a). And people were understandably more reluctant to work where they felt it was damaging their health (Sainsbury et al 2008:58), although this could be outweighed by other pressures and desires to work (Barnes et al 2008). It is therefore difficult to come to conclusions about how far 'work motivation' causes people to stay in work, or is simply a marker of lower barriers to working.

Among these influences on people with health problems, the role of medical professionals was limited. Incapacity claimants commonly consult their GPs (Sainsbury et al 2008:159), but GPs often knew little about their patients' jobs and concentrated instead on dealing with their health problem (Kennedy et al 2007). Many patients had strong views on both their ability to do the job and its effect on their health, and expected their GP to rubber-stamp their own view (Farrell et al 2006:59-60). Where GPs did express a view, this was often to recommend *against* work that they saw as a health risk (Farrell et al 2006:70; Sainsbury and Davidson 2006:28; Nice 2008:6; Sainsbury et al 2008; Conolly and Hales 2009:2) – but even when patients agreed with their doctor, there was a multiplicity of other reasons why people would continue working against their GP's advice (Farrell et al 2006:57; Sainsbury and Davidson 2006:28; Sainsbury et al 2008:81). It is therefore unsurprising that different GPs do not seem to affect their patients' chances of claiming incapacity benefits, at least for those with mental health conditions (Whittaker et al, in press).

The implication of all this is that people can view themselves as not (fully) fit-for-work yet continue working nonetheless, while others with less severe limitations may leave work. Where decisions were not taken out of people's hands by actual and potential employers (section 49), individual differences were explained by a mixture of financial resources, caring responsibilities, and commitment to a particular job (given possible effects of the job on people's health and wider lives).

53. CONCLUSION

In this chapter I developed a conceptual model of the relationship between working conditions, fitness-for-work and incapacity claims – a model that fits previous research, but combines it into an integrated account that makes clear how and when working conditions matter. I began by setting out the difference between absolute and partial fitness-for-work limitations (and the four types of partial limitations), and the ways in which job control and workplace accommodations can help reduce different types of limitations. Where jobs are not fitted around health, workers can struggle on, change to a more suitable job or leave work. These decisions are affected by a number of factors such as finances, caring responsibilities and commitment to their job – but any agency is sharply limited by the decisions of current or potential employers.

This model links to the quantitative analyses in five ways that I set out in section 51. First, I show how survey questions on WLD are interpreted. Second, I explained how job demands lead to demands-dependent fitness-for-work limitations. Third, I drew attention to the role of job control in enabling workers with health problems to stay working (particularly given the limitations of workplace accommodations). Fourth, the qualitative data explain why poor working conditions are more likely to lead to incapacity claims among the low-skilled in areas of low labour demand, who find it harder to change to a more suitable job. Finally, the conceptual model draws attention to other factors that influence the path from fitness-for-work limitations to incapacity claims.

In the concluding chapter, I summarise evidence from the entire thesis in evaluating whether working conditions contributed to the rises in work-limiting disability and incapacity benefit receipt – and go on to draw together the implications of the resulting conclusions for research and policy.

54. APPENDICES

This chapter has one Web Appendix (available from www.benbaumberg.com/thesis.htm):

- **Web Appendix 8a: Topic guide** used for the interviews.

CHAPTER 9:
Conclusions and Implications

55. INTRODUCTION

As set out in Chapter 1, there are three overarching research questions of this thesis:

1. Do high job demands and low job control make people more likely to report a WLD?
2. Do high demands and low control make people more likely to suffer a health-related job loss and/or claim incapacity benefits?
3. If so, has rising job strain contributed to rising WLD and incapacity benefit receipt since the 1980s?

The previous seven empirical chapters have provided evidence on these questions. In this final chapter, I draw together the findings from across the thesis for each question in turn, discuss the extent to which these findings represent an original contribution, and finally draw out the implications for both research and policy.

56. QUESTION 1: DEMANDS-CONTROL AND FITNESS-FOR-WORK

The first research question asked if demands-control influences WLD. Evidence on this was presented in chapters 3, 6 and 8. I first summarise each finding in turn, before synthesising these into a single account of how far the evidence supports a causal effect of demands-control on WLD.

56.1. Findings

56.1.1. Self-reported demands-control

In Chapter 3, I looked at the effect of demands-control using the Whitehall II (WII) cohort of middle-aged civil servants, which is one of the few large publicly available longitudinal surveys to ask about both working conditions and health. With these data, I tested whether people who reported they were in a high-strain job at baseline were more likely to report a WLD at the following wave. I controlled for an unusually large number of possible confounders including multiple measures of socio-demographics, mental and physical health – and if we assume that there are no other relevant differences between people in low- and high-strain jobs (see below), this can be interpreted as a causal effect.

The results confirmed the original hypothesis, with 19.9% of those in high-strain jobs predicted to have a later WLD compared to only 14.4% of those in low-strain jobs, an effect that was highly statistically significant. The main analyses use a composite measure of WLD, but similar effects were found across seven of eight measures of physical and mental WLD. The finding was robust to a large range of sensitivity analyses, taking into account everything from other psychosocial work characteristics to missing data to alternate variable parameterization.

'Job strain' here includes the combined effects of high demands, low control, and the high demands*low control interaction. Taking this apart, the effects were generally due to job demands or the interaction of demands*control; only when looking cross-sectionally was there any for an effect of control in the absence of high demands. There were also signs that the effect of strain was greater among 39-50 year olds than older age groups ($p<0.10$).

There are two ways in which this effect could occur: job strain could affect health itself (the 'health pathway'), or it could make those at a given level of health more likely to report a WLD (the 'job requirements pathway'). In WII it seemed that it was the job requirements pathway that explained the effect of strain on WLD, rather than job strain leading to greater levels of measured ill-health.

56.1.2. Imputed demands-control

In Chapter 6, I repeated this analysis using the nationally representative British Household Panel Survey (BHPS). Rather than using self-reported demands-control, I imputed demands-control into BHPS from average levels in each of 135 occupations, using occupational averages and time trends from the Employment in Britain/Skills Survey series 1992-2006. While this cannot capture the variations in demands-control *within* occupations, it arguably produces a more objective measure of demands-control *between* occupations, as we are no longer looking at outcomes in the same person that describes their working conditions.

As before, I tested whether people who reported they were in a high-strain job at baseline were more likely to report a WLD at the following wave. I control for an extensive list of possible confounders, including sociodemographics, other features of work, health and partner characteristics. Again, if we assume that there are no other relevant differences between people in low- and high-strain occupations, this can be interpreted as a causal effect.

A crucial issue in imputation-based analyses – not discussed in detail in the previous literature – was the difficulty in controlling for occupation-based measures of SES such as occupational class. Any occupation-based SES covariate strongly attenuated demands-control to non-significance at conventional levels, but this is likely to be a methodological rather than substantive finding (see below). I therefore excluded occupation-based SES but conducted a range of sensitivity analyses to check whether apparent effects of demands-control were due to occupation-related confounding.

Using these methods, I found some evidence that low control raised the risk of WLD, but also evidence for a similar effect of *low* demands (rather than high demands) – the highest tertile of both occupational demands and control was associated with about one-tenth less WLD (0.8-0.9 percentage points). Neither result seemed to result from occupation-related confounding, as they were robust to physical job demands and multiple aspects of satisfaction with work; and using managerial status as a proxy for control, the results are even robust to occupation-level SES. However, the apparent effect of control was reduced to non-significance when I took into account missing data, while the apparent effect of demands was larger for *housework*-limiting disability, yet smaller and non-significant for other measures of WLD.

Both of these effects on WLD were found to operate primarily through the job requirements pathway – that is, the associations were not explained by any increased risk of ill-health in those in low-control, low-demands jobs. However, we must bear in mind that the measures of health in BHPS are less detailed than in WII, hence the health pathway is likely to be underestimated.

56.1.3. ***Qualitative analysis***

In Chapter 8, I analysed the results of 32 qualitative interviews among people with health problems. These people were selected to reflect as far as possible the full range of fitness-for-work situations: they had a variety of health conditions; they were previously in a wide variety of jobs; some people had stayed in their jobs while others had left work and claimed benefits; and they were diverse in terms of characteristics such as age, education and gender – although for practical reasons they were all from the London area. The interviews were recorded, transcribed, and then analysed using thematic coding and the ‘Framework’ approach.

The analysis produced three key findings for question 1. First, during the interviews I asked people various different survey questions on WLD:

- The WII questions ask about ‘work or other regular daily activities’ in ‘the past four weeks’, so respondents related them to their recent experiences – which for workers generally included limitations in their current job, but for non-workers mainly related to activity limitations such as shopping or housework.
- The BHPS questions ask about ‘work you can do’ or ‘work you might do’ – which respondents generally took to be work they have done in the past or

would consider doing. In at least one case, this meant that a person who would not consider physical work nevertheless said they were not limited in the type of work they could do, because 'I wouldn't do that work anyway'.

In the following section I re-interpret the quantitative analyses in the light of these results.

Second, high demands could be positive in that it led to greater challenges, stimulation and job satisfaction. However, for fitness-for-work it had a primarily detrimental effect through 'demands-dependent' fitness-for-work limitations [139] – that is, where the duration or intensity of work led to physical strain (pain/exhaustion) or mental strain (stress). In these cases, high demands turned individual tasks that were possible into a job that was impossible.

Third, high job control could enable people to work around their fitness-for-work limitations. Those who struggled in fixed postures could take regular breaks, while those with fluctuating conditions could use their 'adjustment latitude' to catch-up work on good days that they were unable to manage on bad days. Workplace accommodations were also important, particularly in overcoming task-specific limitations. However, accommodations could only partly substitute for high control, partly because the worker is required to disclose their health problem to their employer, and partly because employers were only willing to make certain adjustments. In the light of the wider literature, I argued that individual managers had a limited ability to make exceptions for individual workers given prior decisions about the organisation of work.

However, neither control nor accommodations allowed job demands to be permanently lowered outside of exceptional circumstances. The main way of dealing with demands-dependent limitations was therefore for workers to reduce their hours – although while this is sometimes regarded as an 'accommodation', it is the workers rather than the employers that have to bear the financial penalty.

56.2. Synthesis

The WII and BHPS results appear to conflict as to whether high demands *raises* (WII) or *lowers* (BHPS) the risk of WLD. There are a number of methodological differences between BHPS and WII, but while many of these cannot be ruled out, none of the following seem likely to explain the divergent results:

[139] It is also possible that high demands contributes to interpersonal limitations (by creating a poorer social climate) and performance limitations (to the extent that they require greater intensity/duration than individuals can supply), but the evidence here is weaker.

- Sample composition (WII includes middle-aged Whitehall civil servants; BHPS is nationally representative). However, Chapter 5 suggested that demands and control are only slightly higher in civil servants than others, and a sensitivity analysis in Chapter 6 suggests that even among civil servants in BHPS there is a protective effect of high imputed demands on WLD.
- The measure of job demands[140] (both WII and the JEM cover working hard/intensively, but WII additionally asks 'Do you have enough time to do everything?' and 'Do different groups at work demand things from you that you think are hard to combine?');
- The control variables available (greater health controls in WII; greater workplace and SES controls in BHPS);
- The time period under study (1991-2001 in WII, 1991-2006 in BHPS);
- The way the survey data were collected (self-completion mailed questionnaires in WII, primarily face-to-face interviews in BHPS);
- And many other practical differences that are less transparent (such as the respondents' perception of the purpose of the study, with Whitehall II being presented as a 'Health Survey' done jointly with the Civil Service Occupational Health Service).

This leaves five remaining possibilities in explaining the different results for job demands that cannot be ruled out, which I present in increasing order of plausibility:

1. **Studies using self-reported demands may be biased**; people who say they are in a high-demand job are more likely to report a WLD, but this may reflect the types of people who report high-demand jobs rather than the actual effects of demands (Chapter 6). This is the argument by Kolstad et al (2011) on the basis of null findings of studies using occupation or work-unit averages. Still, in Chapter 3 the self-report results are robust to an extensive array of confounders, and it is unclear what unobserved factors could have such a strong effect (health-independent coping abilities seeming the most likely).
2. **Studies using occupational averages of demands may be biased**; people in high-demands occupations may be different from those in low-demands occupations even after accounting for a large number of covariates (Chapter 6). Again, though, I control for a large range of covariates and the protective effect of high demands usually remains. While the analyses in Chapter 6 exclude

140 The measure of job control is also different. Many of the WII and the JEM questions seem to capture similar concepts, but WII additionally has the questions 'My working time can be flexible', 'I can decide when to take a break', 'I have a say in choosing with whom I work', and 'I have a great deal of say in planning my work environment'. None of these are available in the JEM, but this also includes the question 'And how much influence do you personally have on deciding the quality standards to which you work?')

occupation-level SES measures, the apparent beneficial effect of demands is maintained even when I include a variety of controls that are likely to be associated with occupational class/status.

3. A more speculative possibility is that **there are fundamental differences in what imputed and self-reported demands measure** (Chapter 6). Not only do job demands have both good and bad aspects, but some research suggests the effects of demands are nonlinear. It is possible that average job demands in an occupation captures ‘good demands’, while within-occupation differences (the major part of self-reports) capture ‘bad demands’. There is no direct evidence for this in either direction, but it remains superficially plausible.
4. **Job demands may reduce the risk of WLD in the short-term and raise it in the longer-term**. The harmful effect of high demands in Chapter 3 was looking at WLD 2-3 years later, while the protective effect in Chapter 6 was looking at WLD 1 year later. The importance of lags was shown in Chapter 6 where I repeated the BHPS analyses with a two-year lag: I then found no protective effect of high demands (together with a strongly protective effect of high control). Previous research suggests that the pressures to attend work from high demands lead to lower short-term absence and higher long-term absence – but ‘short-term’ here often means <7 days, and it is unclear what genuine causal processes could lead demands to have such different effects looking one vs. two years into the future.
5. **The measures of WLD in WII and BHPS mean different things.** In WII I measured WLD using an eight-item scale asking primarily about ‘work or other regular daily activities’ in ‘the past four weeks’. From testing these questions in the interviews in Chapter 8, I found that working people related this to both everyday activities and their current job, while non-working people focused on everyday activities rather than work. In contrast, respondents interpreted the BHPS measure as referring to jobs they would consider doing.

   One consequence of this is that the effect of demands-control on WLD in BHPS is likely to be smaller than on WLD in WII, because WLD in WII is focused on the particular job (for which demands-control is measured) rather than the potential jobs in BHPS (which may differ from the actual job). More importantly, reported WLD in BHPS will be affected by the types of jobs that people would consider doing, and this is likely to be influenced by demands *independently of any actual effect of demands on fitness-for-work.* People in high-demand jobs may well be less likely to consider doing heavy physical work even controlling for the physicality of their current job, and this could explain the apparent protective effect of high demands on WLD.

These possibilities have very different implications[141] – but there are four reasons to believe that it is the biases in the WLD BHPS measure that are responsible. First, there is the evidence from Chapter 8 on how the WLD question is actually interpreted. Second, it simply seems implausible in the qualitative analyses that high demands increases fitness-for-work (even though it may well increase job satisfaction); there are no processes through which demands could have a beneficial effect on fitness-for-work. Third, the BHPS models of work outcomes in section 52.2.2 find no effect of demands in either direction; whatever biases exist seem only to apply to the WLD models. Fourth, the BHPS analysis found no effect of physicality on WLD (see section 35.1.1) but a strong effect on incapacity receipt; it would be surprising for physicality to have no effect on WLD, but this becomes more easily interpretable if *potential* physicality is being picked up measures of low demands. It therefore seems reasonable to reject the WLD results (but not the work outcome results) in BHPS on the grounds of biases in how respondents interpret the WLD question– although in doing this, I must also reject the BHPS finding that low control raises the risk of reporting a WLD.

In conclusion, then, how strongly do we have evidence that high job strain has a causal effect in raising WLD? The evidence is suggestive, with strong and robust effects of strain on WLD when using self-reported demands-control in WII, which takes place through a health-independent 'job requirements' pathway – which fits a theoretical account of the role of strain in Chapter 1 that was borne out in the qualitative analyses of Chapter 8. Yet this must be tempered by the imperfect nature of the WLD measure in WII (combining activity limitations with work limitations), and the necessary assumption that – conditional on the extensive observed characteristics – people in high-strain and low-strain jobs otherwise have no relevant differences. Given the longstanding debate on whether self-reported demands can be trusted, future research is necessary to replicate this finding in other samples and using other methods (as I recommend below).

Beyond this, a link of job strain to fitness-for-work is implied by the results of section 52.2.2 on labour market outcomes (e.g. where job control in BHPS affects health-related *but not other* outcomes). Ultimately, I interpret this a likely causal role for job strain in WLD, but given the problems with the BHPS WLD analyses, considerable uncertainty remains around the size of this effect.

[141] If the discrepancy is because self-reports are subject to confounding, then the BHPS findings should be prioritised above the WII findings. Yet if occupation-level confounding or biases in the BHPS WLD measure are responsible, then we should prioritise the WII results over the BHPS results. And if demands can be both positive or negative depending on which aspects are being captured with what delays, then *both* the WII and BHPS findings can be correct, and we have to think about more complex effects of demands on WLD.

57. QUESTION 2: DEMANDS-CONTROL AND JOB LOSS/INCAPACITY

The second research question asked if demands-control influences health-related job loss and incapacity. Evidence on this was presented in chapters 4, 7 and 8.

57.1. Findings

57.1.1. Self-reported demands-control

In Chapter 4, I looked at the effect of demands-control using WII in similar fashion to the analyses of WLD. That is, I tested whether people who reported they were in a high-strain job at baseline were more likely at the following wave to (i) say they left the Civil Service for health reasons ('health retirement'), or (ii) describe themselves as 'long-term sick'. I again controlled for an unusually large number of possible confounders, and the resulting estimates can be given a causal interpretation on the assumption that there are no other relevant differences between people in low- and high-strain jobs (see below).

The main analyses using baseline measures of job strain at waves 3 and 5 were low-powered to detect an effect due to the rarity of these two outcomes, and the models suggested large but non-significant effects. For health retirement it was possible to supplement this with wave 1/2 baseline data to increase statistical power, and in these analyses I found a statistically significant effect: 1.3% of those in low-strain jobs were estimated to take a health retirement by the following wave, compared to 2.1% of those in high-strain jobs. This effect could only be observed among those aged <50, although differences by age were not significant and may simply represent chance fluctuations.

When probed further, this overall effect seemed to represent two distinct pathways. First, there seemed to be a pathway that was not related to fitness-for-work – similar effects of job strain could also be seen on *non*-health related outcomes (early retirement, non-employment), and job demands had faintly protective effects as long as it was combined with at least moderately high levels of control. This non-fit-for-work pathway was associated with job satisfaction and variety: when included in the models these were strong predictors of health retirement in their own right and left the strain effect halved in size and non-significant. It is unclear whether this reflects a genuine but WLD-independent effect of job strain that is mediated by other job characteristics, or simply reflects confounding by other aspects of the work environment.

Second, there was a significant pathway from job strain to both forms of health-related job loss *via WLD*, which was not found for non-health-related outcomes like early retirement. Some caution is needed in interpreting mediation analyses as they make further assumptions compared to conventional regression models: for example, they assume that the people for whom job strain affects WLD are the same people for who WLD affects health-related job loss. Further research is needed to test these assumptions in more detail, particularly given suggestive but inconsistent findings that the effects of job strain on health retirement differ by age and gender.

57.1.2. Imputed demands-control

In Chapter 7, I repeated this analysis by imputing demands-control into BHPS using occupational averages, looking primarily at the outcome of incapacity benefit receipt itself. Again, imputed data has the advantage of being more objective, at the cost of being subject to downward attenuation biases. Following the template from the previous analyses, I tested whether people who reported they were in a high-strain job at baseline were more likely to claim incapacity benefits in the following year, controlling for an extensive list of possible confounders (including physical job demands).

For the same reasons as in the WLD analyses, it was necessary to exclude occupation-level SES covariates, and I therefore carefully tested the sensitivity of the results to other work characteristics (including *inter alia* job satisfaction, job security, relationships with managers, and class identity) as well as including several complementary individual-level measures of SES. To treat the resulting associations as causal effects, we once more have to assume that there are no other relevant differences between people in low- and high-strain occupations that are not captured by the observed covariates.

I found no signs of any effect of job demands on health-related job loss, nor of the interaction of demands*control beyond their separate effects. However, people in low control jobs were significantly less likely to claim incapacity benefits by the follow-up wave (0.9% of the tertile of people with the highest-control jobs, vs. 1.3% in low-control jobs). This effect was only observable at younger ages, although again this may reflect chance fluctuations as differences by age were only significant at the 10% level.

The effect of control was strongly robust to most sensitivity analyses. Using individual managerial status as a proxy for job control, though, I find significant effects in the main models that are halved in size and rendered non-significant by controls for occupation-level SES. This may simply reflect the close relationship of managerial status and occupation, but occupation-level confounding by unobserved factors (i.e. beyond the measures of satisfaction, security et al that I control for) cannot be entirely ruled out.

The effect of job control on incapacity benefits occurred barely at all through WLD, but as I describe in section 56.2, this may well reflect problems in the BHPS WLD question.

In contrast, a fitness-for-work related pathway is suggested by the strong and consistent effect of control on health-related labour market outcomes, and the null or even reverse effects of control on other types of job loss.

57.1.3. Qualitative analysis

The 32 qualitative interviews in Chapter 8 also provided three important findings about the pathways from fitness-for-work limitations to health-related job loss, and from there onto incapacity benefits. First, workers who were 'struggling on' despite fitness-for-work limitations could lose their job if their employer was unsympathetic about performance limitations or (more commonly) sickness absence. Some people lost their job after a long period of absence, but for those with fluctuating conditions the major problem was instead the disciplinary procedures put in place to tackle regular short-term absences. Even if people wanted to stay in work with a fitness-for-work limitation, this decision could be taken out of their hands.

Second, where control or workplace accommodations could not reduce fitness-for-work limitations, people could look for more suitable jobs in which their health interfered less with their work. However, this was a much easier option for well-qualified people in areas of high labour demand than for those who were less attractive to employers – particularly if they could not continue in jobs where they had prior experience. This could create a Catch-22 situation where workers were not healthy enough to do the jobs they could get, but not employable enough to get the jobs they could do.

Third, in the space for decision-making that was constrained by the actions of actual or potential employers, people had some agency over whether they continued working. Their decisions were constrained by financial resources and caring responsibilities – both of which were not always experienced as a 'choice' – and their commitment to their job, which could be lessened if their job was damaging their health and leaving them so exhausted that it was severely limiting their non-work life. Some people therefore claimed benefits despite less severe fitness-for-work limitations than others, who I described as 'not fit-for-work yet still working'.

57.2. Synthesis

The results on health-related labour market outcomes are more consistent than the results for WLD: job strain increases the risk of health-related job loss and incapacity, whether we measure job strain through self-reports or through occupational averages. This fits the hypotheses outlined in Chapter 1 and the conceptual model built up from empirical data in Chapter 8. The effect of job strain seemed to be primarily because of the risks of low control rather than high demands, which fits the wider literature on sickness absence and the smaller (nearly entirely Nordic) literature on disability pensions reviewed in Chapter 1. In both analyses it appeared that effects were only visible at younger ages (although in neither case were differences by age significant at conventional levels), and the qualitative analyses suggest that these effects will be greater for less employable people in areas with fewer jobs.

That said, there are some subtle variations between the analyses. In WII, it was primarily the interaction of demands*control that affected health-related job loss (high demands with moderate/high control was associated with *reduced* risks of health-related job loss in Chapter 4, occasionally significantly). In contrast, in BHPS it was only control that mattered; the interaction of high-demands occupations with low-control occupations had no effect. The most likely explanation for this is that the imputation method is poor at capturing this interaction – occupations that are (on average) high-demands and low-control are not necessarily those where the *combination* of high demands and low control among individuals is most common. This is a particular problem for the conventional 'impute-then-interact' method used in demands-control imputation studies.

I therefore also looked at the preferable 'interact-then-impute' method (as far as I am aware, this is the second study to use this method (after Wieclaw et al 2008), and the first to compare the two methods). Unlike the conventional method, strain measured by interact-then-impute had a strong and significant effect on incapacity claims. While this became only marginally significant when demands and control were also included in the model, this was primarily because of large standard errors (due to collinearity), and alongside the WII results this provides weakly suggestive evidence that it is the *interaction* of high demands and low control that is particularly likely to lead to health-related job loss and incapacity. If control is at least moderately high, it is even possible that more 'good demands' lower the risk of health-related job loss, although the evidence for this is relatively weak.

Does this mean we can conclude that there is a causal effect of job strain on health-related job loss and incapacity claims? I interpret the data as suggesting the answer is 'yes', but with two caveats. First, I only find a significant effect in WII if I make one of two assumptions. On the one hand, I find a significant effect if I assume that job satisfaction, support and skill variety are mediators of the effect of job strain rather than confounding causal agents in their own right. On the other hand, I find a significant effect if I assume that the mediation analysis is valid; that is, that the aspects of WLD that are affected by job strain are the same aspects of WLD that influence health-related job loss. While the mediation analyses in Chapter 4 separate out three different temporal sequences that show the effects are robust to one possible violation of this,[142] in section 60.1 I recommend further research that explicitly tests this assumption.

Second, conditional on the observed covariates, I assume there are no other relevant differences between those in high- and low-strain jobs, nor between those in high-control and low-control occupations. While it is reassuring that both self-reports and occupational averages show similar results, it is still possible that there is residual confounding (as discussed in section 56.2) and that this in the same direction in both analyses. In section 60.1 I therefore recommend further research that tests in various ways for unobserved confounding.

Nevertheless, the weight of evidence – regressions based on self-reports and occupational averages, qualitative data, theoretical plausibility and the prior literature – suggests that there is likely to be a causal impact of job strain on health-related job loss and incapacity.

[142] Chapter 8 suggests that the WLD questions in WII are interpreted differently by those in-work and out-of-work. If we look at the mediation analyses in Chapter 4, we can see that two of the pathways explicitly deal with this: I find some significant effects of job strain on health retirement and long-term sickness via WLD even after controlling for job satisfaction/support/variety and *when WLD is measured only among workers*.

58. QUESTION 3: EXPLAINING TRENDS

The third research question asked how far trends in job strain contributed to trends in WLD and incapacity benefits. Evidence on this was presented in chapters 2, 5, 6, 7 and 8.

58.1. Findings

58.1.1. Trends in demands-control

In Chapter 2, I tried to establish whether there genuinely has been a rise in job demands and a decline in job control, in the face of seemingly unresolved conflicts between major surveys. To do this, I systematically reviewed all of the publicly available trend data on each aspect of demands-control – covering 44 surveys in total and 99 pairwise comparisons between comparable questions in different years. For each survey I extracted information on question wording and methodological details (including *inter alia* sampling methodology, survey mode, weighting, and question routing), and I assessed the degree of comparability for each pairwise comparison.

Ideally this would have produced a clear, unambiguous picture of trends in demands-control since the 1980s, but in practice the picture was messier; seemingly similar questions in different surveys show different trends that cannot be explained through any obvious methodological changes. Like many other survey questions (e.g. on self-reported health or longstanding illness; Chapter 1), individuals are likely to interpret the questions differently based on subtle changes in the questionnaire context and social environment, and this introduces a substantial element of random noise.

Nevertheless, despite some additional caveats, the overall conclusion is that strain did rise in the 1990s, with different aspects of demands-control deteriorating to different degrees. 'Working hard/fast' and 'control over pace/effort' showed the sharpest changes, with smaller or more inconsistent changes in 'having enough time to do the job', 'control over how work is done' and 'control over task order'. However, this deterioration does not extend to all measures or all periods: 'say over decisions at work' increased in the 1990s, demands-control was generally stable post-2000, and the limited evidence from the 1980s suggests increasing control over task order/choice (although other evidence has suggested intensification in the 1980s).

The picture of rising strain in the 1990s is similar to the interpretation of Green and colleagues – unsurprisingly, given that the highest-quality trends are taken from the Skills Survey series that Green et al have collected – and the main contribution is therefore to provide a systematic, transparent basis for prioritising these data over others.

I also considered whether rising *subjective* job strain can be taken as evidence of rising *objective* job strain. While it is impossible to rule out a culturally-driven 'work stress epidemic', there is no direct evidence for this proposition, and a variety of suggestive evidence against it. For example, a rise in the willingness to talk about 'stress' offers no explanation for why reported control has declined, and there are a number of case studies showing processes of intensification. Moreover, in contrast to claims that 'this can't be so', there are a variety of plausible explanations as to why these shifts may have taken place. I concluded that there was a likely – although not entirely certain – rise in job strain since the early 1990s.

58.1.2. ***Trends in demands-control by qualification level***

In Chapter 5, I looked at trends in demands-control by skill level, using the Skills Survey series. I found that the position of the low-skilled *did not* deteriorate over the 1990s relative to the high-skilled. Job control is lower among the less-educated, but so are job demands, making the overall share of high-strain jobs similar across education levels (although there is a strong gradient in passive versus active jobs). Given that incapacity benefits are more common among the low-skilled in areas of low labour demand, there need to be other explanations as to how a population-wide rise in job strain led to the concentrated phenomenon of incapacity benefit receipt.

58.1.3. ***Impact on WLD***

In Chapter 6, I used the models described in section 56.1.2 to estimate the level of WLD if demands-control were still at their 1992 level. The models suggest that WLD would have been 7.0% (not 7.4%) if control had not declined – but it would be unchanged (at 7.4%) if we take into account both falling control and rising demands, as high job demands are estimated to lower the risk of WLD (section 56).

58.1.4. ***Impact on incapacity claims***

In Chapter 7, I used the models described in section 57.1.2 to estimate transitions to incapacity benefits if demands-control were still at their 1992 level. While demands was estimated to have no impact, the models suggest that one-sixth fewer people (0.39% of employees rather than 0.46%) would move from work to incapacity benefits in the following year if control had not declined.

58.1.5. ***Types of control***

While the interviews in Chapter 8 do not directly deal with trends, they do raise the question of whether the *aspects of control* that have declined are the ones that are causally important for fitness-for-work. While control over the way that work is performed could occasionally reduce fitness-for-work limitations (by enabling people to work around specific problems), it was rare that control was powerful enough to overcome task-specific limitations. Instead, it was control in the form of 'adjustment latitude' that mattered most, partly in allowing workers to take breaks from fixed postures that caused them difficulty, but primarily in allowing workers with fluctuating conditions to work around their bad days without displaying reduced performance.

As described in section 57.1.3, the interviews also showed how low control and high demands were a particular problem for people with low employability. Not only are high-skilled people better-able to fit their health around their work in their current job, but they generally find it easier to change to a more suitable job if they cannot reconcile their current job with their fitness-for-work limitations. This addresses the concerns raised in section 58.1.2.

58.2. Synthesis

It is difficult on the evidence here to estimate how far changing demands-control explains the rise in WLD described in the Introduction and Chapter 1 – we can only quantitatively estimate this link in the BHPS analyses, and in section 56.2 I argued that the BHPS WLD analyses cannot be trusted. Nevertheless, there is evidence that job strain has risen, and I argued that it is likely that strain raises the risk of a WLD, even if the extent of this is uncertain. Moreover, it is suggestive that the inexplicable rise in WLD among workers was 1991-1998, the very period that job strain seems to have been rising. There is therefore a plausible link between strain and WLD, but it is impossible to determine the size of this role, or indeed whether the apparent rise in WLD reflects changes in objective levels of fitness-for-work or simply people's willingness to report a WLD (Chapter 1).

There is greater evidence that rising job strain has increased the level of incapacity benefit receipt, given the consistent evidence reported in section 57.2. Moreover, the qualitative analyses explain how population-wide declining control can lead to concentrated incapacity claims among the low-skilled in areas of low labour demand. It is worth stressing that this does not appear to have been counterbalanced by declining physical demands: while greater physicality does raise the risk of incapacity (Chapter 7), the limited existing evidence does not show that this has declined in recent years (Chapter 1).

Using imputed demands-control in BHPS, I estimate that one-sixth fewer people would transition from work to incapacity in the following year if control had not declined 1992-2006.[143] Yet it is unlikely that this is a reliable estimate of the contribution of control:

[143] The fact that incapacity claims plateau in the late 1990s is not evidence against a relationship, given that policies deliberately made it less desirable and more difficult to claim incapacity benefits from 1995. The earlier 1980s rise in incapacity claims is likely to be unrelated; the limited available data suggests that

- In Chapter 5 I described how imputed data will lead to underestimates of causal effects – occupational averages are an inaccurate measure of working conditions in a specific job, and this leads to attenuation biases.
- It is unclear if the types of control that have been declining are the same types of control that lead to incapacity claims. For example, the composite measure of control that I use includes 'control over the quality standards to which you work', which is only vaguely related to adjustment latitude. And some aspects of control – home-working, flexitime and say over decisions at work – rose at the same time that my measures of control were declining (Chapter 1). That said, adjustment latitude seems closer to 'control over work pace' and 'control over task order' than it does to any of the forms of control that have risen, and there have been sharp rises in several sources of effort pressure (e.g. colleagues, customers) that make adjustment latitude difficult (Chapter 2).
- The regression models focus on levels of demands-control in people's job at the baseline wave, but do not capture demands-control in the jobs they could *potentially* do. While these characteristics will be associated with one another, the estimates here are still likely to only be capturing part of the causal effect of job strain.
- Similarly, the regression models here focus on transitions from work to incapacity. However, the greatest changes over time seem to have been in the reverse transitions from incapacity back to work (Chapter 1). I therefore am only estimating the impact of demands-control on one direction of *flow*, and cannot make any estimates about the resulting impact on the *stock* of claimants.

In conclusion, it seems that job strain rose in the 1990s in the UK, and I estimate that transitions from work to incapacity would be one-sixth lower if control had not declined. However, this should be treated as a suggestion that deteriorating control may be a practically-important determinant of contemporary levels of incapacity, rather than as a quantitatively accurate assessment of the size of effect. I therefore argue that rising job strain is a complementary explanation for rising incapacity, alongside existing accounts based around labour demand and benefit system changes.

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control may have risen in this period, although there are no data on demands, and others have suggested rising strain in the 1980s (Chapter 2).

58.3. One-paragraph summary

This thesis has come to three main conclusions. First, job strain is likely to raise work-limiting disability – but the size and practical significance of this is unclear. Second, there is reasonable evidence that job strain noticeably raises the risk of health-related job loss and incapacity claims, which may be a particular problem for less-skilled people in areas with fewer jobs. Third, job strain is likely to have risen in Britain in the 1990s which in turn is likely to have contributed to rising WLD and incapacity claims. While the degree to which job strain explains rising WLD is unclear, the most plausible interpretation is that rising strain played a noticeable role in both the early-90s rise in incapacity and the late-90s policy failure to reduce these historically high levels.

59. ORIGINALITY

As a thesis in the interdisciplinary field of Social Policy, I have built on a large body of existing research across a wide variety of disciplines, and tried to bring these together to provide insights into a policy-relevant debate. My hope is that its additional contributions are part-methodological and part-substantive:

Methodological

1. While my analyses here are based on relatively simple regression models, I do not simply take the resulting regression coefficients to be causal effects. Instead, I carefully subject the results to an unusually large number of tests – an exhaustive list of control variables, looking for specificity across multiple related outcomes, and sensitivity analyses that look at possible mediators/confounders, missing data, and the functional form of demands-control and covariates.
2. I use an improved form of binary mediation analysis compared to the usual Baron & Kenny approach, and also draw attention to the often-ignored additional assumptions of mediation analyses.
3. As well as comparing the effect of self-reported demands-control to imputed measures, I improve on existing methods of imputing demands-control. This is partly through a much more careful consideration of potential confounding through SES, which (surprisingly) is rarely discussed in the previous literature. It is partly through imputing data in a number of different ways (e.g. using Empirical Bayes estimation to improve accuracy, and taking different covariates into account) and checking the robustness of the results to these alternate methods. And partly it is by looking at the effect of strain through the interact-then-impute method rather than the conventional impute-then-interact

measures, which in the case of Chapter 7 made the difference between a null effect and a consistent, strongly significant effect of strain.

4. Each research question is answered through a combination of qualitative and quantitative methods, including the use of cognitive interviewing to see how people interpret questions on WLD – which helped identify likely biases in the BHPS WLD analyses.

Substantive

5. To my knowledge this is the first attempt to systematically review trend data in quite this way. While it was not able to fully explain inter-survey differences, it does provide a systematic and transparent basis for judging comparability, and a complete picture that takes into account all of the available data without arbitrary exclusions.
6. I provide additional research on the effects of demands-control on two rarely-investigated outcomes: work-limiting disability (for which few longitudinal analyses exist) and incapacity claims (for which the few previous studies had been almost entirely conducted in the Nordic countries).
7. To my knowledge, this is the first study to explicitly test the non-health ('job requirements') pathway between demands-control and WLD. Similarly, few studies test the extent to which WLD mediates the effect of demands-control on health-related employment outcomes.
8. While Chapter 8 follows on from many previous qualitative studies of health and working conditions, I extend existing models of hidden unemployment to develop a new conceptual model that integrates health, working conditions, and employability.
9. Finally, the most important contribution of this thesis is that it addresses a hitherto unanswered question: to what extent does rising job strain contribute to rising WLD and incapacity benefits receipt? This is not to suggest that different parts of this have not previously been investigated, but rather than these have never been drawn together in the UK to ask this specific question (as I argued in Chapter 1).

60. Implications for Research

60.1. Strengthening causal inference

While I have provided evidence the job strain is likely to have causal effects on WLD and particularly health-related employment outcomes, substantial uncertainty remains. There are numerous further studies that could increase the strength of causal inference, which I have grouped into five categories: understanding WLD, objective vs. subjective demands, using panel data more effectively, testing the assumptions underpinning causal inference, and changing jobs.

First, neither measure of ***WLD*** used here is ideal; the BHPS WLD measure in particular is likely to produce biased results, while the incorporation of BHPS within the 'Understanding Society' survey only replaces this with the WII WLD measures. Future research should be based on more specific measures of fitness-for-work limitations, which ask more focused questions on types of work or work tasks – perhaps using validated scales like the Work Limitation Questionnaire (Munir 2008), or beginning with cross-sectional analyses of imputed demands-control and fitness-for-work in BSA 1995 or the Family and Working Lives Survey 1994/5. Greater understanding of how people respond to WLD questions – and how this relates to demands-control – could be garnered from the ELSA data, which not only includes conventional measures of WLD and demands-control, but also fitness-for-work expectancies and WLD vignettes.

Second, while my results for health-related employment outcomes were similar when using ***self-reported or imputed demands-control***, there is a need to explain the divergence between methods in the wider literature. Given that imputed data are expected to produce attenuated effects, it would make more sense to compare self-reports to an instrumental variables analysis using imputed data e.g. in ELSA. (The same could be done for the personnel manager assessments in WII). If this shows significant differences between methods then it would be worth exploring whether this is explained by the relative share of good and bad demands. While this is a difficult issue to investigate, suggestive evidence could come from looking at individual- and occupation-level correlations of demands with proxies for different types of demands (e.g. job satisfaction and intellectual stimulation vs. stress and overwork), or an in-depth case study of high-demands and low-demands jobs in high-demand and low-demand occupations.

Third, it would be possible to make better use of the ***panel nature of the data***. Unobserved time-invariant factors could be investigated, in more simple forms by looking at *changes* in demands-control over time (Head et al 2006), or in more complex forms using fixed effects or econometric binary panel models.[144] Different effects over different temporal lags could be considered – including looking at cumulative effects (Elovainio et al 2009), chronic (two-wave) WLD (commonly used in US research; Bound and Burkhauser 1999) and return-to-work vs. absence and health-related job loss (Holmes and Lynch 1990; Erens and Ghate 1993). Finally, Chapter 3 described how the effects of strain on fitness-for-work will be underestimated as people who have not been able to struggle on will have already left that job. It would therefore be sensible to restrict the analysis to those who have had health shocks between waves (Disney et al 2006).

Fourth, there are a number of ***assumptions underpinning causal inferences*** that need to be explicitly tested:

- Many working conditions are closely inter-linked, and policy implications often depend on exactly which aspects of the working environment have causal effects. In Chapter 4 the effect of strain was sharply attenuated by job satisfaction and skill discretion, while in Chapter 7 the effect of control was sharply attenuated by occupation-level SES. Theoretical accounts need to be developed as to which working conditions are confounders or mediators (or moderators, cf. the 'iso-strain model'; see Chapter 1) of the effects of demands-control on fitness-for-work, and these should then be tested empirically. Ideally these would use exogenous influences on only one aspect of the work environment, but given that these are rare, it would be useful to develop more precise forms of the imputation technique that would be more robust to occupation-level controls.[145]
- The mediation models in Chapter 4 assume that the people for whom strain affects WLD are the same people for whom WLD affects health-related job loss. This should be tested through stratified analyses, and for similar reasons, mediation should be tested using the individual components of the composite measure of WLD (e.g. mental WLD vs. physical WLD).
- Even after accounting for unobserved time-invariant factors (as above), we need to assume that there is no time-varying confounding. Stronger causal inference could be obtained using exogenous influences on demands and control as instruments, such as interventions, privatisation and reorganisation (Ferrie et al 1995), manager assessments (Green 2008a:11), and other

[144] For example, I could follow the standard practice of Mundlak models that account for initial conditions using Woolridge's method (Cappellari and Jenkins 2008; Oguzoglu 2009), or control for unobserved characteristics by using the residual term from a wage model (Longhi and Brynin 2010).

[145] The problem in Chapters 6/7 was that there are relatively few occupations *and* these are measured with error (due to small sample sizes), hence it is difficult to tease apart highly-correlated covariates. If the level of error in the imputed data is reduced – such as 'borrowing information' from other surveys (as in Tighe et al 2010) and administrative data using the family of techniques known as 'Small Area Estimation' – then it should be easier to include multiple occupation-level covariates.

occupation- (Cottini and Lucifora 2010:21-2) or workgroup-level (Kivimaki et al 2010b) influences on demands-control.

Fifth, the models here have focused on people's current job, but Chapter 8 makes clear that people may ***change jobs*** if they can – and this may explain why the effects of demands-control seem to be different at different ages. While investigating potential jobs is challenging, I would expect job strain to lead to incapacity more often for those with low qualifications in areas of low labour demand (cf. the evidence in Berthoud 2006:48-49,71 that labour demand has greater effects on more severely disabled people) – a hypothesis that would be straightforward to test in BHPS and which should be a high priority for future research. The reverse influence of health on job strain should also be modelled (taking into account employability and alongside the effects considered here), using Structural Equation Models or bivariate probit models (Oguzoglu 2010).

This is therefore a fertile area for future research, and the picture presented here will doubtlessly be subject to revision as the body of knowledge develops.

60.2. Wider questions

There are a variety of other related questions raised by this research. I have here not modelled the situation of the self-employed, and given their importance – accounting for around 14% of total employment – and the particular issues raised (suggested by Chapter 8), it is important to draw them into this account. Governments in both the UK and elsewhere are concerned with the rising proportion of incapacity benefit claims that are due to mental health problems. One contribution to this could be to investigate the effect of job strain on people with different types of health problems, testing the hypothesis that contemporary jobs are more affected by mental than physical health. Given the importance of interpersonal conflicts with colleagues in Chapter 8 and the suggestions that this is worsened by intensification, it would be interesting to investigate a possible role for this in mental health incapacity claims.

More broadly, Chapter 8 suggested that the possibility of making accommodations in the workplace may be bound up with the very structure of work. Foster and Wass' (Submitted) analysis of two employment appeal tribunals suggested that multiskilling and teamwork require workers to perform a greater variety of tasks, and that this makes it increasingly hard to accommodate those with health problems. The Skills Surveys show that teamwork – particularly in *non*-self-directing teams – has risen sharply in the UK (Gallie et al In Press). Furthermore, practitioners, policymakers and researchers alike have suggested that the 'light jobs' that older/disabled workers used to perform have been squeezed out of the economy (Feldman 1983:439; Hedges and Sykes 2001:144; Tomkins 2008). A major analysis – combining qualitative (including with employers, as in Sainsbury et al 2008), quantitative and historical components – would be valuable in testing whether the modern workplace is unable to accommodate people with certain fitness-for-work limitations, and the extent to which this is a departure from the past.

Finally, this study draws attention to the value of tracking trends in fitness-for-work over time (despite the biases in such questions), given diverging trends between WLD and other disability measures. It is therefore concerning that the WLD question in the LFS looks likely to be withdrawn as part of the harmonisation of disability measures across official surveys (White 2009a:46-7). While there are good reasons to change the specific question used, the tracking of fitness-for-work limitations over time should be a sufficient priority to ensure the place of a similar measure in one of the major national surveys such as the LFS.

61. IMPLICATIONS FOR POLICY

This is not the place for a long, philosophical digression on the nature of the relationship between evidence and policy (for which see Baumberg 2008; Baumberg 2009) – but a few words are helpful to understand what I am trying to achieve in this final section. Having summarised my own results in sections 56-58, I here turn to the implications of this for policy based on the cumulative state of evidence in the field. To this evidence I add (necessarily value-laden) judgements as to where uncertain knowledge is certain *enough* for action (cf. Karasek and Theorell 1992:156), and consider how to balance competing values when moving from research findings to policy recommendations. I also suggest new ways of thinking – variously called frames, narratives, ideas or stories – which are based upon the evidence, but inevitably go beyond it.

61.1. Reducing incapacity claims

The best way of dealing with a problem is not necessarily the reverse of its cause, and it is hard to argue with those who stress the importance of economic regeneration in high-claim areas of low labour demand (Houston and Lindsay 2010) – particularly given that low labour demand makes high-strain jobs a greater problem. It nevertheless seems reasonable that if rising job strain contributed to the high levels of incapacity benefit receipt in the 1990s, Government should at least consider tackling the causes of job strain (cf. Chapter 2) as part of a comprehensive strategy. Yet this has not so far been the case. Working conditions are not explicitly mentioned in the Prime Minister's Strategy Unit's report on *'Improving the Life Chances of Disabled People'* (PMSU 2005), nor in the series of Government Green Papers and White Papers reforming the incapacity benefits system (DWP 2006; 2008a; b; 2010a), nor by the current Coalition Government (DWP 2010b; Freud 2010). In the case of the influential Freud Report for the DWP (Freud 2007:5), Waddell & Burton's (2006) conclusion that work is good for you *provided that it is 'good work'* is explicitly quoted – but while the report emphasises the health benefits of work, the quality of work is not mentioned again.

This may be surprising given that phrases such as 'healthy workplaces' (DWP 2006:30) regularly reoccur throughout these documents. However, what the Labour Government meant by this was a workplace that prevents ill-health (e.g. through promoting healthy eating), and which has an occupational health function that can intervene early in the process of potential incapacity (DWP 2008a; b). It did *not* mean to imply that the nature of work itself should be reorganised to make it easier for people with health problems to stay in work, as I return to below. Still, even without drawing attention to working conditions, it can be argued that what I term the 'person-centred' and 'problem-centred' components of these approaches are in themselves responses to the problem of rising job strain.[146]

Person-centred policies are those that try and change the behaviour and/or attitudes of individual workers so that they can deal better with the working conditions that they face.[147] Interventions such as stress management (Semmer 2008) are generally regarded as evidence-based options, in that they have moderate if varied effects on psychological and psycho-somatic complaints (Lamontagne et al 2007; Richardson and Rothstein 2008; Semmer 2008; Martin et al 2009). Another aspect of resilience to poor working conditions is whether workers believe that 'work is good for them' and that non-work is a greater health risk than working through limitations (Waddell and Burton 2006), where the DWP have made a concerted attempt to change public attitudes (Aylward and Sawney 2007; Black 2008:64-5). Aside from high-profile public pronouncements, the major action has been attempts to change attitudes among medical professionals through e.g. changing the training of health professionals, and replacing the sick note certification system via GPs with new 'Fit Notes' (DWP and DH 2008).

Problem-focused policies are policies that attempt to encourage employers to develop their occupational health function and accommodate existing workers. The OECD have pointed out that *"employers...are uniquely well placed to help prevent... labour market detachment, because they are among the first to see the early signs and, knowing the worker's abilities and strengths, better able to respond adequately"* (OECD 2010:126). The DDA is the most high-profile example of this, compelling employers to make 'reasonable adjustments' for disabled employees (Howard and Cox 2007). Other active elements of this strand of DWP policy (Black 2008; DWP and DH 2008; Foresight 2008:197) include the Access to Work scheme that subsidises workplace accommodations, attempts to encourage greater investment in occupational health, and targeted support for smaller employers for whom it is more difficult to have developed in-house services. Under the same heading we can also bracket non-employer-based vocational rehabilitation, such as the 'Fit for Work Service' pilots currently being run in seven locations, the evidence-based Individual Placement and Support model for people with mental health problems (Perkins et al 2009:60; Heffernan and Pilkington In Press), and now-discontinued schemes such as the Condition Management Programme under Pathways (Whitehead et al 2009: 57) and

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[146] These are similar but slightly different to the more common terms *primary*, *secondary* and *tertiary* interventions developed by the US National Institute for Occupational Safety and Health (as made clear in Lamontagne et al 2007:271; Semmer 2008:7; Landsbergis 2009). The terms *person-centred* and *occupation/job-centred* are relatively common in the literature (Semmer 2008).

[147] Despite their importance in current DWP policy, I do not consider workplace health promotion activities as these are not specifically related to working conditions, nor do I deal with initiatives to improve the employability of disabled workers or to help them with jobsearch assistance (DWP and DH 2008; Whitehead et al 2009:26; DWP 2010a).

the Job Retention and Rehabilitation Pilots.[148] As for person-centred policies, the evidence suggest that problem-focused policies are probably effective in themselves (Whitehead et al 2009:47,50).[149]

While these policies are therefore evidence-based, there are nevertheless problems with this two-pronged approach. In Chapter 8 I suggested that the space in which individual attitudes mattered (let alone the attitudes of GPs) was constrained by the ability of workers to continue doing their job *per se*. Dame Carol Black in her review of the health of the working-age population called for *"a fundamental change in the widespread perception around fitness for work; namely, that it is inappropriate to be at work unless 100% fit and that being at work normally impedes recovery"* (Black 2008:11). Yet the main problem I found in my small-scale qualitative research was *not* that people felt that work was damaging their health and non-work would be better for them, but rather that they – or their employers – felt they were limited in the work they could do. Similarly, stress management and related interventions are rarely assessed against fit-for-work outcomes like absenteeism, and when they do, they seem to have negligible effects (Lamontagne et al 2007, Conclusion 3), suggesting that they have little effect on the determinants of fitness-for-work limitations.[150]

Another problem is that these approaches partly depend on the willingness of employers to invest in stress management and occupational health, to make accommodations for workers with limitations, and to recruit disabled workers in the first place. The Labour Government recognised this (PMSU 2005:14; DWP 2008a:79), but nearly always presented this as a problem of 'information' – Carol Black (2008:16) even claims that *"lack of appropriate information and advice is the most common barrier to employers investing in the health and well-being of their employees,"* and the main Government actions to encourage employer investment are an 'employer-led campaign' (DWP 2008b:98) and a 'Business HealthCheck tool' to highlight the costs of absence (DWP and DH 2008:11). There are however occasional – implicit – recognitions that that it does not always pay to recruit and retain workers with health problems, such as Carol Black (2008:53) invoking corporate social responsibility (rather than profitability) as an incentive to act, and the reliance on voluntary promises by organisations to employ more disabled people (DWP 2008a:79). In practice, person-

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[148] JRRP was an intervention of workplace (for adaptations) and healthcare (for treatment/counselling) support for people on long-term sickness absence but before claiming benefits, which ran 2003-5 in 6 sites. It was evaluated based on random allocation to treatment and control groups, but perhaps surprisingly showed no significant impacts on return-to-work – possibly due to low sample sizes combined with weak effects (Purdon et al 2006; Nice 2008).

[149] However, many small initiatives are undertaken and few have published evaluations. As Palmer et al note (In Press), the fact that effects are smaller in better-quality and larger studies is strongly suggestive of publication bias.

[150] This is despite suggestions that they affect the perceived quality of work (the meta-analysis of van der Klink et al 2001), although van der Klink et al provide few details of this claim.

centred interventions may only produce bottom-line benefits for the high-qualified workers that they are usually offered to (van der Klink et al 2001; Semmer 2008:5).

Workplace accommodations highlight this tension. The DDA obliges employers to make 'reasonable adjustments' for disabled employers, but beyond this the Government provides a small level of funds through the Access to Work scheme to help meet some of the extra additional costs – costs that by definition are 'unreasonable', otherwise they would be covered by the DDA (as explicitly acknowledged in DWP 2008a:65; 2010a:12). While never directly acknowledging that fitness-for-work limitations would lead to reduced performance or extra costs for employers, the Labour Government did repeatedly cite Access to Work as their major strategy for ensuring that disabled workers can access and retain work (PMSU 2005:146; DWP 2008a; HM Government 2009:6; DWP 2010a:51), and the current Coalition Government has recently done the same (Sayce 2011). Yet aside from the limited nature of these funds and the small number of workers affected,[151] the scheme is hamstrung both by its exclusive focus on task-specific limitations,[152] and its complete lack of recognition that the criterion of 'reasonableness' in making adjustments is dependent on prior decisions about the organisation of work (Chapter 8). Moreover, the DDA incentivises employers to *avoid* recruiting disabled workers, leading to unclear results in practice (Burkhauser and Stapleton 2003; Bell and Heitmueller 2009).

61.1.1. Job-centred policies

On a fundamental level, though, these are policies that tackle only the *symptoms* of rising job strain, rather than job strain itself. For job-centred policies that deal with working conditions directly, we have to look outside of the DWP and incapacity benefits policy to two other policy arenas:

1. The 'work as a risk for ill-health' field includes a large number of academic contributions (Chapter 1), and a policy agenda that in the UK is associated with the Health and Safety Executive, who in the early 2000s created the management standards for stress (Mackay et al 2004). Other recent

[151] The Labour Government pledged to double the Access to Work scheme from its previous levels of £70m spent helping 20,000 people per year (DWP 2008a:77). By way of comparison, 20,000 is roughly the same level of additional annual transitions from work to incapacity due to the 1992-2006 decline in control (according to the BHPS models, using the observed BHPS transition rate in section 42.1.5, an employment rate of 71% and the working-age population size of 38m). Employers (excluding smaller employers) now have to pay the first £1,000 of any adjustment under Access to Work, then 20% of the costs up to the maximum level of £10,000 (DWP 2010a:54-5).

[152] Access to Work primarily subsidises special equipment, support workers/communicator support, and fares to work for those who cannot use public transport. Hence in the recent extension to people with fluctuating conditions, the money is not being used to subsidise the costs of sickness absence but rather to fund a *"Support Worker [who] equips the employer to provide the support that the individual needs. This might just include a willingness to consider more flexible working arrangements"* (my emphasis; DWP 2008b:105). The current tender suggests the provision will primarily involve personal counselling and 'employer education' http://www.dwp.gov.uk/supplying-dwp/what-we-buy/access-to-work/mental-health.shtml

contributions include the Wellbeing and Work strand of the Government-sponsored Foresight 'Mental Capital and Wellbeing Project' (Dewe and Kompier 2008), and the Marmot Review (2010).

2. The 'quality of work' field sees positive working characteristics as constitutive of a better, more contented society – as well as sometimes linking these to the ability of older people to continue working (Cahalin 2009; Siegrist et al 2009:32), and work-life balance and/or child outcomes (Lexmond et al 2011). There has been interest internationally in the quality of work, with actions being undertaken in recent years by the EU, OECD and International Labour Office (Green 2006:xv). While policy in the UK has been more muted (see below), The Work Foundation set up a 'Good Work Commission' that considered the rise in job strain (Overell et al 2010), and have continued to push quality-of-work into public debate wherever possible.

In Britain there have predominantly been two types of job-centred policies within these fields. First, there have been calls dating back at least thirty years (Payne and Keep 2003) to ***improve skills*** in Britain, partly in order to supply the high-skilled workers who could attract high-wage, high-quality work to Britain (or encourage existing employers to change the way they organise their work). This is currently a major focus for policymakers. For example, the UK Commission on Employment and Skills – which since 2008 has provided 'strategic leadership'[153] for the skills agenda in the UK – has a strategic priority of 'increasing employer ambition' around skills,[154] and multiple actors are now focused on delivering the 'Ambition 2020' agenda (UKCES 2009).

This focus on the *supply* of skills has however been challenged by those who argue that it will lead to little change unless we also tackle the *demand* for skills at work (Payne and Keep 2003:206), particularly given rising levels of (formal) overqualification (Green and Zhu 2010, Table 2) and the global competition for high-skilled work (Brown et al 2010). One response is ***'exhortation'***; that is, the Government's attempt to persuade private (and indeed public) sector employers that they are, in fact, not acting in their own interest – their instinctive tendency to intensify work while limiting autonomy is actually harming rather than helping productivity. In the 1960s and 70s the calls for such major reorganisations often went under the heading of 'job redesign' or quality of working life initiatives; more recently the talk has been of 'high performance work organisations' (Karasek and Theorell 1992:180-189; Coffield 2004:738; Lloyd and Payne 2006a:152; Overell et al 2010, Chapter 4). To the very limited extent that any recent DWP reports touch upon working conditions, it is in this context of exhorting employers to improve the quality of line management (Black 2008:11; DWP and DH 2009:28-39).

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[153] http://www.ukces.org.uk/about-ukces/about-the-uk-commission/, accessed 14/3/2011

[154] http://www.ukces.org.uk/our-work/strategy-and-performance/, accessed 14/3/2011

Such initiatives include a plethora of changes, some of which may in fact lead to intensification and *declining* autonomy (Chapter 2), and on other occasions are so poorly implemented that there is little change in practice (Graveling et al 2008:15-18). Empirical research is therefore crucial, yet existing research is often poor quality, and subject to large selection biases. While some reviews show that health-focused comprehensive interventions have positive effects on organization-level outcomes including autonomy and absenteeism (Lamontagne et al 2007, Conclusion 4; see also Chapter 1; van Oostrom et al 2009), others suggest effects are more likely in the short-term than long-term and will typically not be seen for all outcomes (Semmer 2008:7) or even that effects are small and non-significant (Richardson and Rothstein 2008:83; Joyce et al 2010). The slightly separate reviews of quality of work life initiatives are again based on poor-quality research suffering selection biases, but to the extent that useful conclusions can be gleaned, these show positive effects on both productivity and wellbeing (Karasek and Theorell 1992:180-199).

Yet the trouble with relying on skills supply and exhortation is that they take no account of the wider institutional incentives for employers to organise work in particular ways. While management quality is obviously variable (Bloom and Van Reenen 2010) and exhortation-based approaches therefore have a role, many managers may actually be acting quite rationally in creating high-demands, low-control work (Wilson et al 2003). Even with Nordic-style exhortation, it has been difficult to convincingly demonstrate meaningful impacts on job autonomy or demands (Payne and Keep 2003:217-8), and the chances of this being successful in the UK are even lower given its 'shallow and fragile roots' (ibid, p208): it is a minor priority for Government with few associated funds and no 'culture of cooperation' with social partners. Substantive change seems inherently unlikely here (Karasek and Theorell 1992:232).

On a deeper level, it has long been argued that Britain is in a 'low-skill equilibrium' (Wilson et al 2003; Lloyd and Payne 2006b), where employers are incentivised to take the 'low road' of competing on the basis of intensified low-autonomy low-skill work, not the 'high road' of competing through autonomously organised skilled workers. Indeed, in Chapter 2 I argued that such features unique to Britain were the likely distal causes of rising job strain, whereas many other countries have seen intensification without declining autonomy. These incentives stem from a focus on short-term shareholder value, a deregulated 'flexible' labour market with few incentives for employers to invest in their workers' skills, weak trade unions, and increasingly ungenerous out-of-work benefits (Payne and Keep 2003:209). While a simplistic version of the low-skills equilibrium may be inaccurate (Belt and Giles 2009:40), the demand for skills and the limits of exhortation are increasingly being accepted, even by the UK Commission on Employment and Skills itself (Belt and Giles 2009:4; Giles et al 2010:Summary p4).

To address this we would have to make changes to almost every aspect of British society and the economy, and to spell this out coherently would be a 'massive intellectual undertaking' (Lloyd and Payne 2006b:474-5). Nevertheless, specific steps could include raising the National Minimum Wage and labour market re-regulation to make the 'low road' less appealing, and increasing the power (and desire) of workers to demand working conditions through e.g. greater 'decommodification' by the welfare state and raising the prospects and powers of trade unions (Lloyd and Payne 2002; Payne and Keep 2003:220; Landsbergis 2009:197-199; Schnall et al 2009b:338-347).

61.1.2. ***Macro-level evidence-based policy***

Taken to extremes, this seems to point to the wholesale reform of British capitalism as an evidence-based recommendation for reducing job strain – but in practice, policy debates that present themselves as 'evidence-based' have marginalised such structural determinants. For example, the National Institute for Health and Clinical Evidence's recent review of reducing long-term sickness and incapacity was framed as 'guidance for primary care and employers' (NICE 2009), while structural factors are omitted by both Dame Carol Black's review (2008) and a recent WHO review of workplace psychosocial factors (Leka and Jain 2010). Most tellingly of all, a British Academy-commissioned review by Chandola (2010) on workplace stress provides an excellent overview of the field and explicitly draws attention to Green's account of rising job strain – but then studiously ignores macro-level factors in its recommendations.

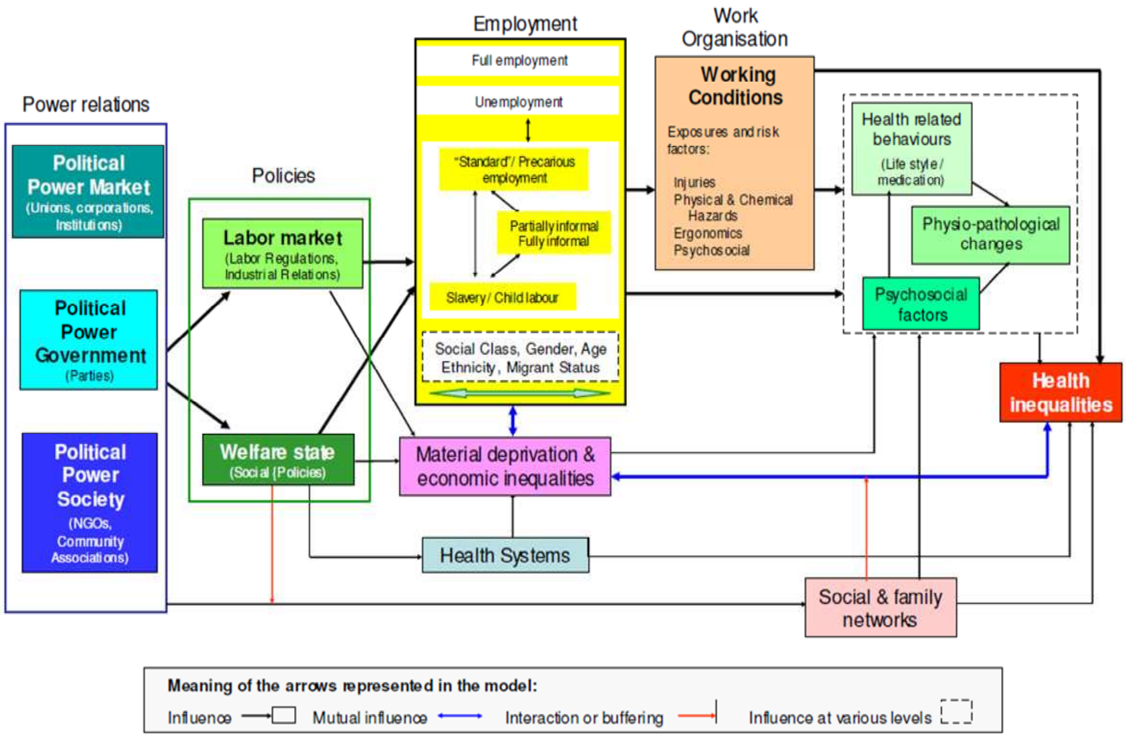
A practical reason for this marginalisation of structural factors is the existence of disciplinary boundaries between social policy, occupational epidemiology and the sociology of work. As Taylor et al put it (2003:454), *"occupational ill-health is still perceived, even by sociologists of work, as a rather narrowly focused issue, of little relevance to the more dramatic sweep of labour processes and production paradigms"*. Occupational health researchers have a reciprocal lack of interest and expertise in the structural determinants of occupational health risks – a tendency reinforced as employers are the main funders of occupational health practice and research, which makes it sensible to adopt a depoliticized focus on tackling symptoms rather than causes (Wainwright and Calnan 2002:43; Schnall et al 2009a:294). Moreover, the types of evidence available on the macro-level do not easily fit into the meta-analysis of randomized controlled trials that are the gold standard in this literature.[155] As such, fundamental constraints in the structure of work are reduced to a 'lack of management support' in implementing evidence-based interventions (Semmer 2008:7-8), for which

[155] When I publicly questioned Tarani Chandola at the launch event for his British Academy report, his explanation for omitting structural factors was that he was restricted to 'evidence-based' recommendations, and that the workplace stress literature provided no foundation on which to venture into the structural level.

the solution is more evidence on economic benefits (a call repeated endlessly in these debates, e.g. Giles et al 2010).

Yet alongside this is a second factor, which is highlighted by the smaller critical strand within occupational health and epidemiology. We have already seen that many researchers have explicitly linked trends in the nature of work to ‘stress’ and ill-health (see Chapter 1), and some researchers speak about ‘upstream factors’ (Lamontagne et al 2007:269,277) or the ‘root causes of disease’ (Landsbergis 2009:202) rather than blaming the individual (Karasek et al 1998:350; Dobson and Schnall 2009:126). On rare occasions there is a brief (Levi 2000:86) or even extended (Karasek and Theorell 1992; Schnall et al 2009a; Whitehead et al 2009:6) discussion of the role of macro-level factors in generating workplace psychosocial risk factors – of which the highest-profile recent attempt is the employment working group within the Marmot Review (Siegrist et al 2009), as shown in Figure 57 below, and reflecting Marmot’s focus on the ‘causes of the causes’ (Marmot Review 2010).

Figure 57: The Employment Working Group of the Marmot Review’s conceptual approach on the link of working conditions to social inequalities in health



Taken from Figure 1 in Siegrist et al (2009:7).

What limits these calls for action, as I see it, it that they take place from a critical perspective – that is to say, they are *openly* ideological and *deliberately* take no account of political realities. They are not claiming to be dispassionately evidence-based, but are rather trying to oppose ‘neoliberalism’ (Schnall et al 2009b), and often culminate in vague and utopian wish-lists with no detail of how such goals can realistically be achieved (Karasek and Theorell 1992:326; Schnall et al 2009b:345-346). The same is true for attempts to tie the study of health inequalities to the role of power in generating adverse working conditions; Scrambler's (2009:124) 'greedy bastards hypothesis' hardly presents itself in the clothing of evidence-based policy.

If we try and fit macro-level factors into a more dispassionate guise, then we come face-to-face with this problem of political realities. For example, it is difficult to see how the long-term decline in British trade unionism can be reversed (Overell et al 2010), or the entrenched suspicion of market intervention can be easily overcome. It has therefore been argued that any proposals to change this are *"a counsel of perfection and not within the bounds of practical, democratic politics"* (Coffield 2004:736) – and even though the Nordic countries are a demonstration that there is a niche for 'high-road' strategies in the global economy (Lloyd and Payne 2006b:474), it is unclear whether there is a path from contemporary Britain to a Scandinavian utopia. Instead, there are likely to be tensions between pursuing 'good jobs' and 'any jobs' (Esping-Andersen 2002:21; Gallie 2002) that evidence alone cannot overcome.

The Marmot Review demonstrates these tensions. Its employment subgroup bemoaned the lack of evidence linking structural factors directly to health (Siegrist et al 2009:8), but nonetheless made recommendations aimed at 'reducing the gap between the available science and policy at different levels' (p41-44), including at the structural level. Even here, the recommendations are vague – 'establish regulations to prevent the adverse effects of downsizing', and 'improve employees' control and autonomy at work by enabling decision making in task design, work control and related measures of work organisation'. Moreover, they explicitly say that these have not been 'evaluated with respect to their feasibility' (p44), instead restricting themselves to listing a number of 'constraints on action that are not due to insufficient evidence', by which they mean macro-level factors such as 'poor motivational and financial incentives' by employers and 'liberalisation of capital, trade and [the] labour market' (p40).

By the final Marmot Review report (2010:114) – for which Dame Carol Black was a commissioner – such tensions had been minimised by simply recommending more stress management guidance, and suggesting that better work would be profitable in the medium-to-long term. Only scant mention is made of the possibility that good work will not pay, and this through an even vaguer recommendation to 'develop greater security and flexibility in employment by encouraging/incentivising employers to create or adapt jobs that are suitable for lone parents, carers and people with mental and physical health problems'. It is perhaps understandable that the DWP and Dame Carol Black have deliberately refused to acknowledge such unresolvable tensions, and instead focused on 'win-win' changes to business practice and micro-level interventions.

61.1.3. Recommendations

Nevertheless, I believe that the structural level should not be excluded from evidence-based recommendations just because of disciplinary boundaries and political tensions. There are trade-offs and difficulties to be taken into account, and to pretend otherwise is to take this discussion into the realm of wishful thinking – but this is equally true if we pretend that micro- and meso-level policies can revolutionize the nature of British work. A recent report from the UK Commission of Employment and Skills puts this delicately:

> *"While there is a strong case for voluntarism and employers willingly buying in to the need to act rather than being forced to, there are questions about whether this sufficiently targets certain types of businesses and sufficiently supports the 'hard to reach' employers...Therefore, a key question...is whether there is the room, albeit in carefully targeted and exceptional circumstances, to use stronger policy instruments"* (Giles et al 2010, Summary p12-13).

The DDA is one example of this kind of 'stronger policy instrument'. In the context of likely links between job strain and incapacity claims (if of uncertain size), several short-term steps would be welcome:

- First, it would be a major step forward for the DWP to *mention* the role of working conditions as a cause of poor fitness-for-work, and make clear – unlike the incredibly brief mentions of 'stress' in existing documents (DWP and DH 2008:48; 2009:27) – that exhortation alone cannot overcome these tensions. This would officially recognise the links between them, and help keep this within the frame of debate.
- Second, the DWP's existing person-centred and problem-centred approaches can only be welcomed in themselves. However, the funding of the Fit-for-Work agenda has been a casualty of the deficit-reduction agenda; the pilots will not be rolled out nationally (and four of the eleven pilots were closed).[156] A better-funded, more integrated occupational rehabilitation system would help many people with health problems, even if this is ultimately constrained by wider factors.
- Third, the DWP should try and encourage workplace audits of psychosocial job characteristics (Gallie 2002), perhaps through a 'Workplace Commission' (Foresight 2008:192). The DWP and ESRC should also and fund further research on the relationship between the organisation of work (including but not limited to demands-control) and incapacity benefits receipt. While research is partly the outcome of societal priorities, in the Nordic countries such research has contributed to the job redesign agenda, and its absence can contribute to

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[156] http://www.dwp.gov.uk/health-work-and-well-being/our-work/fit-for-work-services/ accessed 30/5/2011.

'collective myopia' (Karasek and Theorell 1992:16-17). It is therefore concerning that the proposed National Centre for Working-Age Health and Wellbeing (DWP and DH 2008) is a further casualty of public spending cuts.

- Fourth, while there are limits to the exhortation-based approach to improving the quality of work, it would certainly be possible to push this further in the UK, such as by following the recommendations in Payne & Keep (2003:219) or by the UK Commission on Employment and Skills (Giles et al 2010). Increasing training and awareness for managers may also be useful (Foresight 2008:192). Researchers can even play a role in this, through participatory research, links with unions and employers, and in general being exhorters of business to change (Schnall et al 2009a); and unions themselves can be champions of job redesign (Karasek and Theorell 1992:235). The role of pension funds is also under-explored, given that improvements in the quality of work may reduce shareholder value but be balanced by declining costs of early retirement.
- Fifth, the Government employs about one-quarter of the British workforce,[157] whose working conditions are therefore directly manipulable by policymakers, and who are additionally subject to the public sector Disability Equality Duty (DWP 2008b:97). It is difficult to see how rising job strain in the public sector could be anything other than a direct consequence of policy decisions, in particular the rise of New Public Management techniques that explicitly attempt to increase the demands and reduce the discretion of public sector workers (Chapter 2).

  It does not have to be this way (Green 2009b:30). For example, Lloyd & Payne (2010) have shown how vocational teachers in FE in Norway have much higher levels of autonomy and discretion that people in the same occupation in the UK, while Karasek & Theorell (1992:315) show similar findings when comparing air traffic controllers in Sweden and the US. The current Government's taskforce on the mutualisation of public services (led by Prof Julian Le Grand) is promising here, as part of the 'Red Tory' strand within the Conservative Party (Blond 2009). Research is however needed to demonstrate that this achieves the predicted improvements in autonomy, and does so without unduly raising interpersonal conflict. Even if this is achieved – and at the present time, the fate of employee mutuals is unclear – public sector job cuts will mean a further intensification of work.
- Finally, in cross-departmental discussions on economic strategy, the DWP should draw attention to the potential external costs (i.e. incapacity benefit receipt) of poor working conditions (as in Foresight 2008:194). These

[157] It may also be possible to specify conditions on public sector purchasing contracts (Payne and Keep 2003:220), although this has not always proved successful (Geurts et al 2000).

discussions necessarily have a wider remit than simply incapacity benefits, but this would be a worthwhile contribution – particularly in showing how some employer strategies impose additional costs on the benefits bill, rather than in the conventional 'win-win' arguments about improving working conditions.

61.2. The narrative of rising incapacity benefits

While the adoption of such recommendations may ultimately reduce the level of incapacity claims (if by an uncertain amount), the potential impact of the evidence in this chapter is less about 'what works', and more about 'what is incapacity' and 'what happened' – our understanding of the nature and rise in incapacity benefit receipt. Yet it is no less important for policy for this. Politics is not a technocratic exercise in applying empirical findings, but a place where different narratives clash (among many other sources, see Lakoff 2002).

Presently, the understanding of ***'what is incapacity'*** is based on an assessment of fitness-for-work, the Work Capability Assessment, which is entirely based on functional limitations (Technical Working Group 2007; Citizens Advice Bureau 2010). That is, people are assigned points for particular limitations: for example, if you are unable to 'repeatedly mobilise 200 metres within a reasonable timescale because of significant discomfort or exhaustion', then you are given 9 points; and if your limitations total 15 points or more, you are entitled to ESA (DWP 2011b).

However, unlike the old Invalidity Benefit test (Burchardt 1999:6), incapacity benefits do not take into account age, qualifications, or any other factors that influence whether such functional limitations translate into *incapacity*. In the conceptual model developed in Chapter 8, it was clear that incapacity is not just a function of health, but also relates to the type of a job a person is doing when their health deteriorates, and particularly whether they would be able to get the types of work they can do. Recently the independent review of the incapacity assessment (Harrington 2010:70) has raised the possibility of including 'real-world' considerations around employability, and from the research here it is difficult to see how a coherent definition of incapacity can avoid this.

Perhaps more importantly, our understanding of ***'what happened'*** is affected by the research here. It is widely argued that incapacity claims cannot reflect 'real' limitations in fitness-for-work, given that the number of claimants has risen, while work has become less physically demanding and the population has become healthier (see Introduction). This framing cites research on the non-health causes of sickness absence and incapacity benefit receipt, and implies these are not primarily health phenomena at all (Chapter 1). This framing even claims (as common-sense, rather than citing evidence) that *"in general, work has become safer and much less physically* *demanding, and less rigid, allowing more people with physical conditions to work through their illness rather than take time off work"* (DWP 2009:1). This has resulted in an energetic UK agenda that has tried to get incapacity benefit claimants back to work, trying to change claimants' attitudes and making the medical assessment more stringent (Technical Working Group 2007; Citizens Advice Bureau 2010), alongside a stigmatisation of those that claim (Chapters 1 and 8).

A genuine rise in job strain and work-limiting disability is a challenge to this narrative. To the extent that rising incapacity benefits are caused by job strain – remembering that while some of my results suggest a substantial role for job control, this cannot be considered definitive – then this lends support to another story, where people with health problems are excluded from the labour market by jobs that have become ever-more difficult for them. This explanation is rooted in factors outside of the control of the individual, and therefore suggests they are more 'deserving' than they are currently seen, and therefore, more entitled to public support (van Oorschot 2000).

To be clear: this is not to suggest that the conventional narrative is 'wrong', in the sense that the fundamental assumptions underlying it can be shown to be factually incorrect. There *is* substantial evidence that absence and benefit receipt are influenced by non-health factors (as reviewed in Chapter 1 and more vividly shown in Chapter 8), and the evidence that incapacity benefit receipt is in some sense 'hidden unemployment' is overwhelming. The role of job strain in incapacity benefits receipt is also uncertain, particularly in the scale of its effects.

Yet the research presented here does destabilise this narrative. It suggests that job strain rose over the 1990s, and that this is likely to have made it harder for people with health problems to stay in work – particularly those with low skills in areas with few jobs. Future research will hopefully reduce the uncertainty in this area, either destabilising the existing narrative yet further, or showing a relatively minor role for job strain. In the meantime, we must consider the possibility that the rise in incapacity benefit claimants is not just because of declining labour demand and incentives in the benefits system, nor due to a rise in 'dependency culture', but is instead because people with health problems have found it increasingly hard to find jobs that they can do.

Put simply, this opens up a space for a new narrative around incapacity benefits, which does not focus on the failures of the individual or the benefits system, but instead casts its eye on the world of work. In Annie Irvine's wonderful phrase (2011a:766):

> *"Perhaps the key question should not be whether an individual is fit for work, but whether the work is fit for the individual."*

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