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# **Welfare Conditionality and Activation in the UK: the mental health impacts of benefit sanctions**

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Philosophy

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## Abstract

Increasingly, social security systems in the UK and internationally stipulate work-related behavioural requirements for claimants of out-of-work benefits. These are accompanied by claimant monitoring as well as the threat and imposition of financial penalties, which are known as benefit sanctions. The growth in recent decades in the use of behavioural conditions and sanctions has generated significant debate and contestation, in terms of the ethical justification of such approaches and, relatedly, evidence regarding their overall effectiveness.

An important topic concerns the impacts of benefit sanctions on claimants. Policymakers typically assume that sanctions will improve labour market outcomes for the unemployed, which will then lead to a range of individual and societal benefits. A well-developed literature exists in relation to the labour market impacts of sanctions, though less is known in terms of their wider effects. A small but growing body of research, nevertheless, links benefit sanctions with outcomes such as financial hardship and foodbank usage, and there is increasing concern regarding adverse impacts on mental health.

This thesis investigates the relationship between benefit sanctions and mental health outcomes, and considers whether higher rates and/or longer durations of sanctions are associated with adverse mental health impacts. A quantitative study is undertaken that focuses on Jobseekers Allowance (JSA) sanctions during the period of Coalition government (2010-15). In this period, the frequency of sanctions varied significantly and their severity was increased following the Welfare Reform Act 2012. This exogenous variation is used to better estimate the independent effect of sanctions on mental health outcomes.

Given data availability, the empirical investigation carries out four analyses involving different data sources, outcomes and research designs at separate data levels. The first two studies carry out longitudinal ecological analyses using local authority-level data and fixed effects models. They find that, following the Welfare Reform Act 2012: every 10 additional sanctions applied per 100,000 population per quarter are associated with 4.57 additional antidepressant prescribing items; and that every 10 additional sanctions applied per 100,000 working age population per quarter are associated with 8.09 additional people suffering from anxiety and/or depression.

The third study carries out a multi-level analysis, which provides a robustness check on the aggregate-level analysis carried out in the second study. It finds that, in the post-reform period, increases in the area-level sanctions rate are associated with increases in the likelihood that JSA claimants suffer from anxiety and/or depression. Finally, the fourth study carries out a difference-in-differences analysis. It indicates that the harsher sanctioning environment brought about at the onset of the Coalition government is associated with an increase in JSA claimants newly experiencing anxiety and/or depression.

These results combine to provide a robust indication that JSA sanctions are associated with adverse mental health impacts, which is an important contribution to the existing empirical literature. They suggest that UK sanctions policy is overly harsh, and that steps need to be taken to reduce the adverse effects that it entails for claimants.

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## Author's declaration

I declare that, except where explicit reference is made to the contribution of others, that this thesis is the result of my own work and has not been submitted for any other degree at the University of Glasgow or any other institution.

Name: Evan Williams

A handwritten signature in black ink that reads "Evan Williams". The signature is written in a cursive style with a large, stylized 'E' and 'W'.

Signature:

## Abbreviations

ALMP	Active Labour Market Policy
BMA	British Medical Association
BPS	British Psychological Society
CEM	Coarsened Exact Matching
CPAG	Child Poverty Action Group
DCLG	Department for Communities and Local Government
Defra	Department for Environment Food and Rural Affairs
DfEE	Department for Education and Employment
DiD	Difference-in-differences
DSS	Department of Social Security
DWP	Department for Work and Pensions
ESA	Employment and Support Allowance
EU	European Union
FJF	Future Jobs Fund
GDP	Gross Domestic Product
GVA	Gross Value Added
HoC	House of Commons
HSCIC	Health and Social Care Information Centre
IB	Incapacity Benefit
ILO	International Labour Organization
IS	Income Support
JRF	Joseph Rowntree Foundation
JSA	Jobseeker's Allowance
LFS	Labour Force Survey
LPO	Lone Parent Obligation
NAO	National Audit Office
NDYP	New Deal for Young People
NHS	National Health Service
NICE	National Institute for Health and Care Excellence
OECD	Organisation for Economic Co-operation and Development
ONS	Office for National Statistics
PAC	Public Accounts Committee
RCT	Randomised Control Trial
RPS	Royal Pharmaceutical Society
UB	Unemployment Benefit
UBI	Universal Basic Income
UC	Universal Credit
UI	Unemployment Insurance
UKSA	UK Statistics Authority
WCA	Work Capability Assessment
WFI	Work Focused Interview
WHO	World Health Organisation
WPC	Work and Pensions Committee
WTC	Working Tax Credit

# Chapter 1. Introduction

## 1.1 Behavioural conditionality, activation and benefit sanctions

In recent decades, welfare states across developed economies have been transformed through the proliferation of conditional approaches to social provision (Watts and Fitzpatrick, 2018). Whilst the requirement that individuals satisfy certain conditions in order to access state support is not a uniquely contemporary phenomenon, the role of conditionality has nevertheless grown significantly in scope and importance. Arguably, it now represents a key constitutive element of social policy interventions in areas as diverse as health, social housing and homelessness, criminal justice and anti-social behaviour, education, migration and social security (Deacon, 2004). This thesis is concerned specifically with developments in social security, where behavioural conditionality, monitoring and sanctions are widely used policy tools in relation to unemployment and other out-of-work benefits (Immervoll and Knotz, 2018). This behavioural shift within social security is itself closely associated with the development of a so-called “activation paradigm” within labour market policy since the 1990s (Bonoli, 2010: 448). In terms of unemployment benefits, for example, a key area of overlap is represented by the growth in conditions relating to job search activity, suitable work criteria and involvement in employment-related programmes as requirements of continued benefit receipt. Increasingly, furthermore, such work-related conditions are being enforced through both the threat and imposition of benefit sanctions (Knotz, 2018).

The use of financial penalties within the social security system represents the central topic of this thesis. In particular, the investigation is concerned with sanctions policy in the UK, where financial penalties are used to enforce conditions that affect a wide variety of working age individuals, including the unemployed, long-term sick and disabled people, lone parents and those in low-paid employment (Dwyer and Wright, 2014). Successive UK governments have intensified the role and use of sanctions since the 1990s, increasing the overall number of behavioural requirements that are attached to benefit receipt, increasing the length for which sanctions apply and restricting access to

mitigating support in the form of hardship payments (Adler, 2016). A particularly important period in terms of UK sanctions policy occurred during the Coalition government (2010-15), which imposed an unprecedented overall number of sanctions on claimants and - following the implementation of the Welfare Reform Act 2012 - significantly increased the severity of the financial penalties that can be applied. Webster (2016: 2) describes this period in terms of a “great sanctions drive”, in which nearly a quarter (24%) of claimants of the main unemployment benefit, Jobseeker’s Allowance (JSA), received at least one sanction, and in which monthly sanction rates experienced substantial fluctuations (NAO, 2016a).

Sanctions policy, both in the UK and internationally, has generated significant debate. Within this discussion, an important area of contention relates to the impacts of sanctions on claimants. The explicit aim of sanctions policy is to improve employment outcomes, and as a result an extensive empirical literature has developed that examines the labour market impacts of sanctions in terms of employment re-entry, post-unemployment earnings, job stability and labour force attachment (McVicar, 2014). A growing area of research, furthermore, investigates wider impacts in terms of outcomes such as financial hardship, homelessness and food bank usage (Griggs and Evans, 2010; Dwyer, 2018).

A relatively underdeveloped area within this wider literature relates to the mental health impacts of benefit sanctions. The Department for Work and Pensions (DWP), which is the government department in charge of sanctions policy in the UK, assumes that sanctions will be associated with positive health outcomes for claimants (DWP, 2011a). For the DWP, this follows from their view that sanctions will be associated with increases in employment, and from the additional assumption that such employment will itself lead to beneficial impacts on health and well-being. A small qualitative literature, however, contests this assumption and indicates that sanctions are routinely accompanied by acute negative psychological impacts such as stress, anxiety and depression (Stewart and Wright, 2018; Dwyer et al., 2020). Indeed, in the public debate beyond academia, the potential for sanctions to lead to negative mental health impacts has attracted significant attention. Such impacts have been widely reported by the media, for example, and have been highlighted by various third

sector organisations (CAB, 2013; Hale, 2014; Mills, 2018). Importantly, these criticisms have also been reiterated by professional groups such as the British Psychological Society (BPS, 2017).

In light of this debate, this thesis aims to contribute to existing knowledge in the area of sanctions and mental health. Specifically, it investigates the relationship between sanctions and outcomes such as antidepressant prescribing, anxiety and depression. As indicated above, a limited amount of existing qualitative research provides valuable insight into claimant experiences with regard to sanctions and emotional and psychological outcomes. Building on this evidence base, this thesis adopts a quantitative approach and examines larger-scale longitudinal data on sanctions and mental health outcomes across separate but mutually reinforcing analyses. In so doing, it is able to pay particular attention to the issue of identifying causal effects using the data that are available, specifically in relation to the impact of sanctions on mental health.

In particular, the empirical investigation focuses on JSA sanctions during the period of Coalition government (2010-15). As indicated above, this period is significant in that it was characterised by large variations in the frequency of sanctions as well as an increase in their overall severity. These developments are also considered to be exogenously determined by the government's policy decisions, as opposed to by changes in the behaviour of claimants themselves (Loopstra et al., 2018). Consequently, this period provides the context within which it is possible to better estimate the independent effect of sanctions on claimant outcomes. Using available data from across the time period, the analyses in this thesis make use of separate research designs to investigate outcomes such as antidepressant prescribing, anxiety and depression. In addition to the time period selected, causal inferences are supported by a variety of additional sensitivity and robustness tests in the separate quantitative analyses. In so doing, the research is able to examine a largely underappreciated consequence of sanctions policy, and make an empirical contribution to ongoing debate in this area. Benefit sanctions are now common across international social security systems, and so the findings of this thesis have wider relevance beyond the UK context, contributing to the contested issue of the "efficacy and

ethical legitimacy” (Watts and Fitzpatrick, 2018: 15) of conditionality more broadly.

Within the UK itself, developments in sanctions policy during the Coalition government form the basis of conditionality as it currently operates within the social security system and the changes continue to attract significant public scrutiny. Following the implementation of the Welfare Reform Act 2012, for example, criticism regarding sanctions has prompted several official investigations, which include: an independent review by Oakley (2014); two inquiries by the House of Commons Work and Pensions Committee (HoC WPC, 2015; 2018); an investigation by the National Audit Office (NAO, 2016a); and an inquiry by the House of Commons Public Accounts Committee (HoC PAC, 2017). Each of these investigations has made a wide range of recommendations for the DWP. Most recently, for example, the House of Commons Work and Pensions Committee (HoC WPC, 2018: 19) argues that the DWP needs to “urgently evaluate the effectiveness of reforms to welfare conditionality and sanctions introduced since 2012”. This recommendation is a recurring one across the various reports, which repeatedly highlight the concern that the sanctions reforms implemented in 2012 were not sufficiently informed by a robust evidence-base, and that a subsequent evaluation of the changes has unfortunately not been forthcoming. The DWP has now accepted this criticism and committed to improving evidence in this regard, specifically in the areas of employment and health (HoC WPC, 2019).

## **1.2 Research objectives**

Informed by the above discussion, the main aim of this thesis is to investigate the relationship between benefit sanctions and mental health outcomes. Specifically, the thesis aims to investigate whether there is evidence that higher rates and/or longer durations of sanctions are associated with adverse mental health impacts. In this regard, the objectives of this study are as follows:

- i. To contribute to the existing literature on the wider impacts of benefit sanctions, by using quantitative methods, longitudinal data and causal modelling frameworks to establish robust claims about the causal impacts of sanctions on mental health outcomes.
- ii. To provide evidence that can be used to inform sanctions policy in the UK, specifically in relation to the reforms introduced by the Welfare Reform Act 2012.

### **1.3 Thesis outline**

In order to achieve the above objectives, this thesis is structured as follows. First, Chapters 2 to 4 review the existing literature. Chapter 2 sets the broader context for the use of benefit sanctions across contemporary social security systems, through an examination of the separate but related concepts of behavioural conditionality and activation. By highlighting the link between conditionality and activation in the area of unemployment benefits, the discussion then goes on to consider the available empirical evidence that documents the growth of work-related conditions and sanctions across developed economies in recent decades. Chapter 2 ends by providing an overview of the ongoing debate regarding the ethical legitimacy of behavioural conditionality, within which it is argued that empirical evidence relating to the impacts of benefits sanctions is of central importance.

Next, Chapter 3 details developments relating to behavioural conditionality and sanctions in UK social security policy in recent decades. Beginning with the introduction of JSA in 1996, it considers the proliferation of work-related behavioural requirements beyond the initial concern with unemployment, eventually encompassing lone parents, disabled people and - following the introduction of Universal Credit (UC) in 2013 - people in low-paid work. Chapter 3 ends by detailing developments in sanctions policy during the Coalition government (2010-15), with a specific focus on JSA sanctions. This is important as it provides the context for the empirical investigation that is ultimately carried out.



Chapter 4 reviews the literature on the impacts of benefit sanctions, considering the available UK and international evidence in relation to both labour market and wider outcomes. It begins by discussing the dominant theoretical framework that is relied upon in the empirical literature on labour market impacts of benefit sanctions, known as job search theory. It then provides an overview of the empirical evidence on impacts such as employment re-entry, post-unemployment earnings, job stability and labour force attachment, distinguishing between what is known in relation to the threat and imposition of benefit sanctions. This overview focuses on the primarily quantitative literature on these impacts, and consideration is given to attempts to identify causal effects, given that individuals who receive sanctions may differ in important ways to those who do not in a manner that may influence labour market outcomes. Next, Chapter 4 provides an overview of the developing quantitative and qualitative literature on the wider impacts of benefit sanctions, where a range of negative outcomes have been observed. In the process of discussing this literature, the mental health impacts of sanctions are identified as the particular focus of this thesis. To support the empirical investigation, therefore, Chapter 4 ends by developing a theoretical framework that elucidates the assumed link between sanctions and adverse mental health impacts. In this regard, this framework distinguishes between the influence of separate material and psychosocial mechanisms.

Chapter 5 details the methodological approach that is taken in order to investigate the relationships identified. Given the reliance on a quantitative approach, the discussion first considers debates relating to causal inferences within quantitative social research, and then goes on to outline the various methods that are used in the empirical investigation itself. Data availability is a key issue that shapes the research designs that are ultimately adopted in the empirical chapters, and so considerable attention is given to how mental health outcomes and sanctions are measured and operationalised within the research. Specifically, the investigation considers antidepressant prescribing and self-reported anxiety / depression as outcomes, whilst a reliance is ultimately placed on local authority-level data on JSA sanctions using the DWP's Stat-Xplore database (DWP, 2018c).

Chapter 5 ends by providing an overview of the data and methods that are used in each empirical chapter, which investigate the relationships of concern at different levels (local authority-level; multi-level; individual-level) and using different techniques (fixed effects regression; random intercept regression; difference-in-differences regression) in order to respond to the limitations placed upon the research by the issue of data availability. Given the various differences between the analyses carried out, and the specific research questions that underpin them, more detailed information in terms of data and methods are provided in the relevant empirical chapters themselves, which aids the interpretation of the results obtained.

The empirical investigation itself is carried out in Chapters 6 to 9. Chapter 6 and Chapter 7 carry out longitudinal ecological analyses at the local authority-level using fixed effects regression analysis. The former considers impacts on antidepressant prescribing using data from NHS Digital (2018) whilst the latter considers rates of self-reported anxiety and/or depression using estimates produced from the Quarterly Labour Force Survey (QLFS) (ONS, 2018c). The analyses in these chapters are carried out using data between 2010 and the end of 2014, and are able to identify separately the impact of the harsher sanctions regime introduced by the Welfare Reform Act 2012. To consider the influence of compositional bias on the results in Chapter 7, Chapter 8 goes on to carry out a multi-level analysis. It uses local authority-level data on JSA sanctions and individual-level data on JSA claimants' self-reported anxiety and/or depression, again using the QLFS. Finally, Chapter 9 aims to improve upon the causal inferences made in the preceding empirical chapters by carrying out a difference-in-differences analysis, focusing specifically on developments in sanctions policy brought about at the onset of the Coalition government in 2010.

Chapter 10, finally, provides a concluding chapter. First, it summarises the findings of the empirical investigation, relating them back to the overarching research question motivating the separate empirical analyses and highlighting the overall contribution of the thesis. Important avenues for future research are then identified, particularly in terms of the need for individual-level research in this area, whilst discussion is then provided regarding what the findings imply for contemporary sanctions policy. The chapter ends with a reflection on what the

collective weight of evidence on the impacts of benefit sanctions, including the findings presented in this thesis, implies for sanctions policy. It highlights the need for policymakers to respond to the available evidence in this area by adopting a precautionary approach to policymaking that ensures that the harms associated with sanctions are minimised. Several options are outlined that would help achieve this aim, all of which are readily implementable.

## **Chapter 2. Conditionality and activation**

### **2.1 Introduction**

This chapter provides context to the current role of benefit sanctions within welfare states, by situating their development in relation to the wider concepts of conditionality and activation. First, this chapter considers the notion of behavioural conditionality, outlining the different types of conditional requirements that can be identified and their relationship to a so-called “behavioural turn” (Dwyer, 2016: 42) in social policy. Whilst the development of conditionality has been identified in terms of a “broad and far-reaching shift” (Deacon, 2004: 911) in the welfare state, affecting areas as different as education, health and housing, this thesis is specifically concerned with the impacts of benefit sanctions as applied within the social security system. Consequently, the discussion focuses on conditionality in relation to unemployment benefits, which is an area that has also been subject to a so-called “activation turn” (Bonoli, 2010: 435) since the 1990s. This chapter explicates the link between conditionality and activation in the area of unemployment, and then goes on to outline the available international evidence regarding the growth of conditionality and sanctions in recent decades. Finally, this chapter considers the ongoing ethical debate in relation to the acceptable role of conditionality across the welfare state. A central issue within this debate, it is argued, relates to benefit sanctions and their associated impacts.

### **2.2 Behavioural conditionality**

#### **2.2.1 Types of benefit conditions**

Clasen and Clegg (2007: 171) argue that entitlement and eligibility to socially-provided benefits have “always and everywhere” been conditional in certain respects. They contend, therefore, that such conditions are a fundamental part of the ways in which welfare states have, and continue, to regulate access to the support that they provide. Indeed, it is arguably difficult to conceive of a social benefit provided by a nation state that could be designed without any conditional requirements attached to it. The notion of a Universal Basic Income (UBI), for example, has recently attracted considerable policy discussion. UBI is

premised on the notion of the state providing a regular and unconditional cash payment (Standing, 2017). Whatever the merits of this proposal, even a truly unconditional UBI provided by a nation state would inevitably involve some restriction in eligibility to citizens or residents, which can be regarded as a categorical condition specifying who can and cannot receive the support in question. In actually existing social security systems, of course, conditional requirements apply much more broadly than in the example just given and often include demands relating to claimant behaviour. This section, therefore, focuses its attention on the different types of benefit conditions that exist, including their relationship to behavioural requirements and, ultimately, their enforcement through benefit sanctions.

Clasen and Clegg (2007) develop a framework for understanding the different types of benefit conditions that can be demanded, which distinguishes between three 'levels' of conditions: conditions of category; conditions of circumstance; and conditions of conduct. Conditions of category refer to conditions that restrict eligibility to particular benefits to members of specifically-defined groups. A basic requirement for an individual to be eligible for unemployment benefit, for example, is that they are considered to be unemployed. Once categorical definitions have been established, access to support can then be regulated through conditions of circumstance, which refer to eligibility and entitlement criteria based on the circumstances of claimants. Such conditions are numerous, but might include work history-based criteria in the case of contributory benefits, for example, or need-based criteria in the case of means-tested benefits. Finally, conditions of conduct, which place behavioural demands upon individuals, are applicable once both conditions of category and circumstance have been established. As Clasen and Clegg (2007: 174) emphasise, these conditions are conceptually distinct from the first two 'levels' in that they serve to regulate "*ongoing* benefit receipt" once initial access has been established. In the case of unemployment benefits, for example, typical requirements are often work-related, in that they specify availability requirements, job search criteria and involvement in training activities as a condition of continued benefit receipt (Immervoll and Knotz, 2018).

### 2.2.2 Unemployment benefit and behavioural demands

Clasen and Clegg's (2007) framework is useful in that it helps to identify the routes that are available to the state in terms of regulating access to social benefits. In particular, each conditional 'level' provides a 'lever' through which it is possible to expand or restrict access through category re-definition, changes to eligibility and entitlement criteria and the development of behavioural requirements. It is important to recognise, however, that explicit attempts to influence behaviour are not necessarily restricted to the 'level' of conditions of conduct. A good example is provided by the case of unemployment benefit, where retrospective behavioural requirements are widely imposed as a condition of benefit receipt (Langenbucher, 2015). In the UK, for example, access to unemployment benefit since it was introduced in 1911 has been restricted - in the form of a maximum six-week disqualification - for those who lost their previous job due to perceived misconduct or to those deemed to have left their previous job voluntarily (Adler, 2016). Clearly, such retrospective conditions aim to influence individuals who are employed by discouraging and promoting particular forms of behaviour. In terms of unemployment benefits at least, therefore, behavioural conditions can be specified that determine both initial access (conditions of circumstance in the form of retrospective requirements) as well as ongoing access (conditions of conduct).

Indeed, this argument can be further extended to apply to the very definition of unemployment itself (conditions of category). In the UK, for example, a new definition of unemployment was introduced in 1989, which required individuals to be available for work and to actively seek work (Price, 2000). This change brought the categorical identification of unemployment more in line with the internationally-accepted definition adopted by the International Labour Organisation (ILO, 1982), which had previously defined unemployment in similar terms. In this regard, Boland and Griffin (2015: 29) identify a broader shift that they describe as the "death of unemployment", which refers to the systematic replacement of the category of 'unemployment' with the term 'jobseeker' by policymakers. This can be seen clearly in the UK, which replaced the pre-existing unemployment benefit with Jobseeker's Allowance (JSA) in 1996. This shift has also occurred internationally, with organisations such as the OECD

increasingly framing unemployment in terms of seeking paid work (OECD, 2013; Immervoll and Knotz, 2018). Rhetorically, the emergence of the ‘jobseeker’ re-frames the notion of unemployment in direct relation to the labour market. Accordingly, unemployment is viewed as a transitional phase in which individuals must actively seek employment in order to constitute themselves as being in a state of unemployment.

### 2.2.3 Defining behavioural conditionality

Considered together, the behavioural requirements that have been developed and imposed in the case of unemployment benefits complicate the distinction between conditions of category, circumstance and conduct, by embedding behavioural demands at each ‘level’ of conditionality identified by Clasen and Clegg (2007). Indeed, as Chapter 3 will go on to detail, benefit sanctions in the UK social security system apply for behavioural reasons relating to all three ‘levels’ of conditionality. Whilst their framework is nevertheless useful in terms of delineating the types of conditions that can and do exist, and indeed in identifying the growth in conditions of conduct as a relatively new area of concern, this suggests that a narrow emphasis on conditions of conduct is insufficient in terms of providing an overall account of how behavioural conditionality operates in practice. In this regard, Watts and Fitzpatrick (2018) adopt a wider conceptualisation of behavioural forms of welfare provision, which recognises that behavioural requirements serve to regulate both initial as well as ongoing access to benefits, and are underpinned by three key characteristics:

“they specify *behavioural requirements* which determine initial access to and/or continued receipt of benefits, goods and services; they employ *monitoring and surveillance processes* that verify compliance with those requirements; and they impose *sanctions* in the event of non-compliance or, in some cases, offer *incentives* for compliance.” (Watts and Fitzpatrick, 2018: 31)

According to the above conceptualisation, behavioural conditionality is constituted by behavioural requirements that are enforced through claimant monitoring and sanctions. Importantly, the recent growth of such requirements across the social security system and other areas of social policy has been

criticised in terms of representing a “behavioural turn” (Dwyer, 2016: 42), in which the welfare state has been re-imagined primarily as a tool with which to influence individual behaviour. A notable example of this policy agenda is provided by the UK, which established a Behavioural Insights Team (BIT) in 2010 to apply the insights of so-called ‘nudge theory’ into policy design and service provision (Gandy et al., 2016). Partly, criticism of this shift is directed at the fact that, as a policy solution, it relies on the contested assumption that the social issues in question are “fundamentally *behavioural* in nature” (Watts et al., 2014: 16). This is a topic that will be further discussed in the next section in relation to unemployment. Indeed, Friedli and Stearn (2015: 40) argue that in this policy area, the ‘behavioural turn’ has extended in terms of “psychological conditionality”, in which interventions have sought to modify not only the behaviour of unemployed claimants but their beliefs and attitudes as well. On this account, the key to tackling unemployment is not only conceived of through claimant behaviour change, but also in terms of the acquisition of “work-appropriate attitudes and beliefs” (Friedli and Stearn, 2015: 40).

As previously highlighted, conditional requirements have always provided states with a means through which to regulate access to the publicly available support that they provide, and claimant behaviour has consequently long been of concern. With regard to unemployment benefits, this concern is invariably work-related, as shown by the establishment of unemployment insurance in the UK in 1911 that was “explicitly presented as a measure ... that would help to encourage good work habits” (Clasen and Clegg, 2011: 4). Indeed, Watts and Fitzpatrick (2018: 2) argue more broadly that welfare state interventions per se are “intrinsically *socially controlling*”, given that such support inevitably “shapes, moulds and constrains the choices people make”. Whilst recognising this broader context, it is nevertheless important to be cognisant of the fact that it is possible for the nature and extent of behavioural conditionality to vary significantly through time. Chapter 3 of this thesis provides more depth regarding the growth of behavioural conditionality in the UK social security system in recent decades, paying particular attention to the increased role of benefit sanctions. In this chapter, furthermore, Section 2.4 provides a broader account of the growth of conditionality and sanctions internationally. First,



however, the next section considers the concept of activation, which is closely associated with behavioural conditionality in the area of unemployment.

## 2.3 Activation

### 2.3.1 Defining activation

Bonoli (2010: 435) identifies a so-called “activation turn” that has occurred in labour market policymaking across developed economies since the 1990s. Whilst conceptually distinct, the relationship between work-related behavioural conditionality and activation is a close one. Indeed, the behavioural demands placed on unemployed claimants that were identified in the previous section - particularly in terms of conditions of conduct - have been referred to in the literature as “activation requirements” (Lødemel and Moreira, 2014: 1) as well as in terms of “benefit activation” (Clasen and Clegg, 2011: 9). Clasen and Clegg (2006), furthermore, specifically recognise work-related behavioural conditionality in their definition of activation, which understands the concepts in terms of the interaction between social security and labour market policy by distinguishing between a narrow and a broad form:

“Most narrowly, [activation] involves developing tighter links between unemployment protection policies and active labour market policies. More broadly, activation is about increasing labour market entry and participation, and phasing out temporary labour market exit options for working age claimants (early retirement, disability and long-term sickness benefits). In its narrow and sometimes also its broad meaning, activation implies making established welfare rights more conditional on job seeking efforts.” (Clasen and Clegg, 2006: 527-528).

Barbier (2001: 5) refers to an “intrinsic fuzziness” that pervades usage of the term activation, and as the above definition makes clear part of this imprecision relates to the fact that the concept combines several closely related but nonetheless distinct elements. In addition to work-related behavioural conditionality, for example, activation is associated with a wider set of interventions described as active labour market policies (ALMPs), which Sage (2015a: 320) defines broadly as “targeted schemes that enrol (and often

mandate) unemployed people onto programmes intended to promote and speed up labour market reattachment.” Indeed, in debates surrounding activation in the 1990s the term was more narrowly associated with a focus on promoting ALMPs (OECD, 1994). Over time, however, the concept of activation developed to encompass the broader set of concerns articulated in the definition above, notably the interaction between (un)employment, the social security system, ALMPs and behavioural conditionality itself (OECD, 2006). A clear example of this interaction is provided by the fact that failure to comply with mandated involvement in ALMPs represents grounds for a sanction across developed economies (Immervoll and Knotz, 2018).

### **2.3.2 Developments in activation**

As is the case for behavioural conditionality, activation is not a uniquely contemporary concern. Bonoli (2010), for example, categorises its development since the 1950s into three separate phases. Up until the 1970s, countries focused on the need to up-skill their workforces in response to the skilled-labour shortages that developed in the context of rapid post-war economic growth. ALMPs in this phase were pioneered in Sweden, where the Rehn-Meidner strategy combined attempts to modernise industry with both full employment and income equality (Sihto, 2001). The second phase, in contrast, refers to the late 1970s and 1980s, in which policies were developed in response to the persistently high levels of unemployment affecting most developed economies. Labour market policies during this period were concerned with what Bonoli (2010: 443) refers to as “occupation”. Given the context of mass unemployment, even extremely active interventions could not expect to have much influence on employment outcomes, and so ALMPs merely aimed to attenuate the potential for skill-loss. The third phase, lastly, describes developments from the 1990s onwards within a generally improving economic context, whereby falling unemployment rates shifted the attention of policymakers towards groups such as the long-term unemployed. In the view of policymakers, the low levels of demand and wages available for low-skilled labour meant that unemployed individuals faced greater disincentives to work than had existed previously.

Bonoli (2010: 435) describes this latter stage in terms of an “activation turn”, in which policies have had a dual focus on the provision of greater work incentives as well as employment assistance. This qualitative shift in activation has also been understood in terms of both “punitive and enabling mechanisms” (Raffass, 2017: 350), which can be thought to be present in both of the dimensions identified. With regard to work incentives, for example, this stage of activation is associated with the more widespread development of minimum wage policies and working tax credits, though it has also entailed cuts in the real value of unemployment benefits and greater use of benefit sanctions (Serrano Pascual and Magnusson, 2007; Immervoll and Scarpetta, 2012). With regard to employment assistance, furthermore, Lindsay et al. (2007) distinguish between human capital development and work first approaches. Human capital development approaches emphasise investment in skills, education and training as a route out of unemployment. In theory, a supply-side focus on up-skilling should be beneficial for the short- and long-term labour market prospects of unemployed individuals, in terms of finding paid work, potential job quality and future in-work progression. Work first approaches, in contrast, focus on job search and more basic skills training as a route to getting unemployed individuals into work as quickly as possible. The emphasis in this approach, therefore, is on short-term job outcomes as opposed to job quality or suitability.

### **2.3.3 Important drivers of activation**

Arguably, the development of work-related behavioural conditionality and sanctions represents a key constitutive element of the punitive dimension of the ‘activation turn’, specifically within its focus on work incentives. In an attempt to identify important determinants of this shift, Knotz (2019: 616) argues that there is a “political logic” to the timing of sanctions reforms across the OECD, in that governments have tended to introduce tougher sanctions rules during economic downturns in response to the pressure of reduced tax revenue and increased expenditure on unemployment benefits. Clasen and Clegg (2011) take a wider view, however, situating behavioural conditionality primarily in terms of the state’s adaptation to the fundamental realities of post-industrial labour markets. Given the central importance of social security to the functioning of labour markets, the authors argue that unemployment benefit systems have

always reflected the broader economic context in which they operate. In the post-war era, for example, contributory unemployment insurance was favoured in order to support the stable employment relationships upon which manufacturing-based economies functioned, primarily required to protect against the risk of cyclical and frictional forms of unemployment. In the post-industrial era, in contrast, unemployment benefit systems have been redesigned to support the flexible employment relationships and structural forms of unemployment that are characteristic of the service-dominated economy.

In particular, this transformation has seen a hollowing-out of labour markets across developed economies, including the proliferation of precarious low-wage work, higher youth and long-term unemployment (Goos and Manning, 2007; Bosch et al., 2009; Eichhorst and Marx, 2015). According to Clasen and Clegg (2011: 1), this shift has led to a so-called “triple integration” in unemployment protection across Europe. First, unemployment benefits have become more homogenised, representing a move away from contributory benefits and their emphasis on claimants’ labour market history as economies have become less able to provide stable employment attachments. Second, a risk re-categorisation has seen the traditional policy focus on the unemployed broadened to include groups in the working-age population that were previously exempted from work-search requirements, such as single parents and disabled people. This has occurred, for example, by reducing the differences in the conditions of circumstance and conduct for various out-of-work benefits, given the apparent need to integrate as much of the working-age population into the labour market as possible. Third, a process of benefit activation has occurred, primarily in terms of a growth in work-related behavioural conditionality, such as requirements surrounding job search, a widening in the types of jobs that claimants are expected to accept as well as a greater emphasis on work first forms of ALMPs and sanctions.

Whilst Clasen and Clegg’s (2011) analysis prioritises adaptation to post-industrial labour markets as a key determinant of policy development in this area, it might also be argued that social security systems themselves play an important role in influencing how labour markets function. McDaniel and Berry (2017: 24), for example, argue that social security arrangements and labour market imperatives

do not constitute separate “spheres of activity”. Rather, they interact with one another in a two-way process to ultimately sustain underlying economic dynamics. Indeed, the UK’s work first model of activation arguably serves to reinforce and perpetuate Britain’s “low-pay, no-pay” (Shildrick et al., 2012: 2) labour market by ensuring that demand for such employment from employers is met by an available supply of labour (Bosch, 2009; Collins and Murphy, 2016). Adler (2016) makes a similar point in relation to benefit sanctions specifically, viewing them as a disciplinary tool deployed to pressurise claimants into accepting low-paid and insecure employment (see also Wacquant, 2010; Fletcher and Wright, 2018). Considered within this punitive dimension, activation has been viewed as a form of “re-commodification” (Greer, 2016: 162), through its erosion of the ability of unemployed individuals to effectively withhold their labour power from the labour market (Grover, 2012; Wiggan, 2015).

### **2.3.4 The passive versus active distinction**

The ‘activation turn’, in both its work incentives and employment assistance dimensions, has been associated with a shift away from a policymaking focus on the demand-side of labour towards one of labour supply, a development that is at least partly underpinned by behavioural explanations of unemployment (Aurich, 2011). Indeed, proponents of activation have sought to label pre-existing policy responses to unemployment as ones that encouraged claimants to be overly ‘passive’ (Wright, 2012). This has been the view, for example, of those bodies that have promoted activation such as the EU, the OECD and the World Bank, which contrasted ‘active’ approaches to unemployment with so-called ‘passive’ income maintenance (Sinfield, 2001). Critics of this view argue that activation, particularly in its work first variants, ignores the structural determinants of unemployment and consequently mistakenly re-frames lack of work in terms of a “deficient work ethic among individuals” (Watson, 2015: 251) and as the individual responsibility of the unemployed (Wiggan, 2012). In a partial counter, furthermore, Clasen and Clegg (2011: 3) note a number of the “productive functions” of the compensatory approach in the industrial era welfare state, in terms of improving job matching and stabilising aggregate demand during economic downturns. Indeed, such unemployment benefit systems existed within a context of full employment - albeit one based on a

male-breadwinner model - that was actively managed by an interventionist macroeconomic policy.

Wright (2016: 236), furthermore, argues that the widespread use of the passive/active dichotomy relies on an influential “deficit model” regarding the agency of benefit claimants, who are viewed by policymakers as “inherently deficient ... naturally inactive and in need of activation - either because of their perceived incompetency or immorality”. As Wright (2016) outlines, Murray’s (1984; 1990) notion of an emerging underclass and Mead’s (1992; 1997) concept of welfare dependency are two influential accounts that deploy the deficit model, albeit through adoption of two diverging perspectives on human motivation and agency. Murray (1990), for example, draws on the theory of the ‘economic man’, viewing individuals as rational maximisers who act rationally when they maximise their own self-interest. Unemployment benefits, on this account, merely serve to sustain poverty through the creation of a so-called welfare trap, which can only be counteracted through the re-arrangement of both monetary and non-financial incentives and disincentives. Mead (1992: 133) adopts a different view, in which unemployed individuals are “dutiful but defeated” and are no longer able to act rationally in their own self-interest. According to this perspective, work is valorised as the overriding moral obligation in society, and given the failure of the unemployed to meet this moral duty the state itself has a right to coerce individuals into paid employment through the use of activation measures.

### **2.3.5 Cross-country differences**

Thus far, this section has provided an overview of activation, detailing its relationship to behavioural conditionality and sanctions as well as the important material and ideational factors that have contributed to its most recent articulation. Whilst an overall turn towards activation across developed economies since the 1990s has been identified, a large literature exists that seeks to detail country-specific trends, timings and developments as well as the extent of convergence in this area (Barbier and Ludwig-Mayerhofer, 2004; Serrano Pascual and Magnusson, 2007; Betzelt and Bothfeld, 2011; Immervoll and Scarpetta, 2012; Weishaupt, 2013; Lødemel and Moreira, 2014). Providing an

account of cross-country developments in activation is difficult, not least due to the broad nature of the concept itself and the wide array of policies that it encompasses. Bonoli (2010), nevertheless, identifies Denmark and the UK as two of the earliest adopters of activation in Europe in the 1990s, albeit contrasting Denmark's human capital focus with the UK's broadly work first approach. The UK reforms themselves were heavily influenced by similar developments that occurred in the US (Daguerre and Etherington, 2014). Germany, in contrast, is viewed as a relatively late adopter, via implementation of the Hartz IV reform in 2005 that merged unemployment benefit and social assistance as well as reduced benefit generosity. Lødemel and Gubrium (2014), nevertheless, identify a move towards work first forms of activation across most European countries in the 2000s, including in those countries that had previously pursued more human capital-oriented approaches.

Given the broad nature of activation as a concept, it is arguably more useful to focus on specific components when seeking to provide supporting evidence of developments that have occurred across countries and through time. Indeed, given the specific concerns of this thesis, the next section considers in more depth the available international evidence in the area of conditionality and sanctions. Until relatively recently, comparative accounts in this area have been limited by data availability, which has restricted accounts of conditionality to descriptive overviews based on a limited number of countries (see, for example, Clasen and Clegg, 2011). Whilst these accounts are extremely useful, recent work has improved the scope of the available evidence in terms of both longitudinal (Knotz, 2018; 2019) as well as in cross-sectional data (Venn, 2012; Langenbucher, 2015; Immervoll and Knotz, 2018). These are discussed in the next section.

## **2.4 Trends in behavioural conditionality**

### **2.4.1 Measuring overall conditionality**

Knotz (2018) analyses data on unemployment benefit conditionality and sanctions across 21 OECD countries between 1980 and 2012, based on a dataset compiled by Knotz and Nelson (2015). Specifically, the data available allow conditionality to be measured on the basis of three separate dimensions:

availability requirements; job-search and reporting requirements; and sanction rules. First, availability requirements relate to the definition of suitable employment in each country, and refer to the type of work and wages that claimants are required to accept, as well as identifying the range of circumstances in which claimants are permitted to refuse offers of employment. Second, job-search and reporting requirements refer to the frequency with which the job-seeking activity of claimants is checked, as well as whether claimants are required to sign agreements that detail the conditions. Third, sanction rules refer to the length and severity of sanctions. These can be imposed for reasons such as becoming unemployed voluntarily, refusing job offers, non-attendance at meetings and failing to sufficiently evidence job-search activity. Importantly, Knotz (2018) highlights two key limitations to the data. First, the dataset measures conditionality and sanctions in relation to unemployment insurance schemes in each country, with the exception of Australia and New Zealand. This is important, as the stringency of conditions and sanctions is considered by Knotz (2018) to be stricter in means-tested or social assistance schemes. Second, the data take into account how stringent conditionality rules are intended, as opposed to how they are actually applied in practice.

These limitations aside, the data nevertheless provide unique insight into the development of conditionality and sanctions across international social security systems in recent decades. As part of the analysis, Knotz (2018) constructs an overall measure to capture the conditionality of unemployment benefits across the OECD, which is determined by combining the average strictness of both conditions and sanctions together. Importantly, this measure confirms that conditionality has increased overall between 1980 and 2012, which supports the substance of the discussion detailed in Section 2.2. Indeed, consistent with the notion of an ‘activation turn’, the topic of Section 2.3, the increase is particularly observed from around 1990 onwards. This is driven by a “significant increase” (Knotz, 2018: 101) in the strictness of both conditions and sanctions at that point in time.

It is worth noting, however, specific developments for the individual components that form this overall measure. In terms of availability requirements, claimants



have come under increasing pressure to accept employment in an occupation other than their previous one as the degree of occupational protection fell. In terms of wage conditions, a trend is observed in countries increasingly defining suitable wages in precise terms, such as in relation to the claimant's previous wage or the current benefit level. There is also some evidence, nevertheless, of what Knotz (2018: 97) describes as a "recalibration" in availability requirements, as there was an increase in the number of countries explicitly recognising reasons for refusing work based on grounds such as caring responsibilities, lack of appropriate skills and ethical, moral or religious concerns. With regard to job-search requirements, furthermore, there was a clear trend towards reporting requirements becoming more clearly defined. In 1980, for example, 70% of countries had no systematic checks of claimants' job-search activity, a figure that had fallen to below 20% in 2012. The use of both voluntary and compulsory jobseeker agreements, in addition, rose rapidly from the early 1990s onwards, instruments that specify the behavioural demands on claimants and any support they will receive from their caseworker.

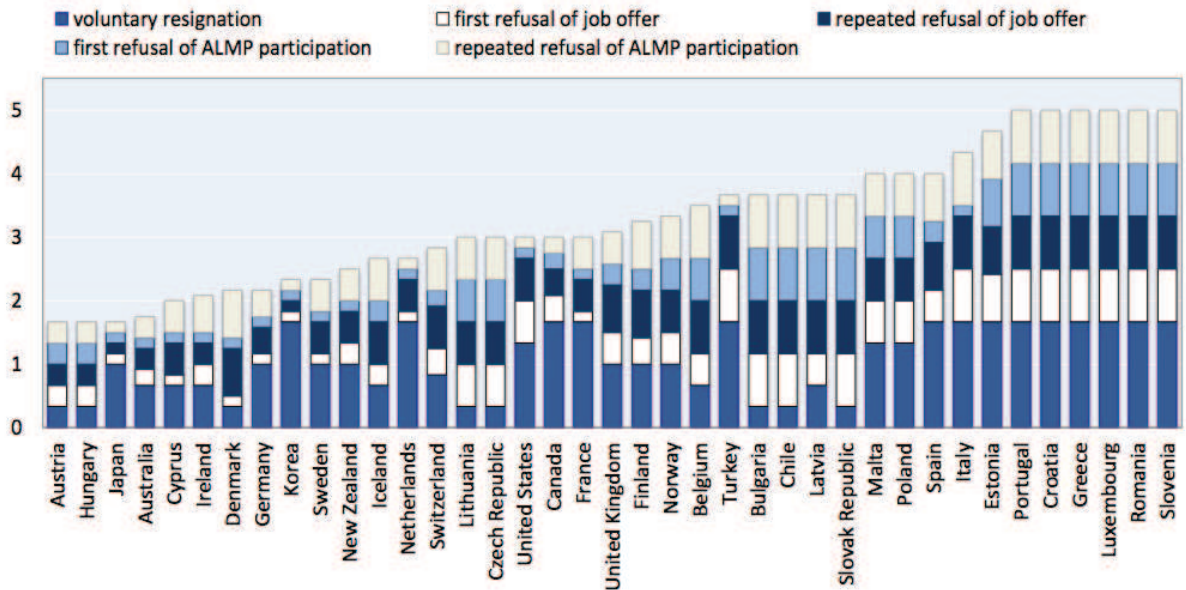
#### **2.4.2 Strictness of benefit sanctions**

Regarding benefit sanctions specifically, Knotz (2018) finds that the rules surrounding sanctions also became more clearly specified in the period analysed. This occurred, for example, through the development of additional sanctions for second and third refusals of employment. In the 1980s, under 20% of countries had sanctions for a second refusal, whilst none imposed sanctions for a third refusal. By the end of the period, however, these proportions had increased to 50% for second refusals and 40% for third refusals. Such sanctions are escalating, furthermore, in that they increase in severity for repeat refusals. The average length of a sanction increases from 10 weeks for an initial refusal of work to 15 and 20 weeks for second and third refusals respectively. Importantly, the average 'effective' duration of sanctions for initial refusals of work, which is measured by combining the length of benefit sanctions imposed with the benefit share withdrawn as a result of a sanction, increased across the period. Interestingly, nevertheless, the average duration of sanctions for initial refusals of work fell where sanctions for second and third refusals were introduced. This dynamic means that the average duration of sanctions for initial refusals of work

peaked at approximately 13 weeks in the late 1990s, and then gradually reduced to 10 weeks by the end of the period.

Using the same dataset, Knotz (2019) provides greater detail on individual countries with respect to the trend in the overall strictness of their sanctioning rules. In particular, Austria, Finland, Ireland, Italy, Sweden and the UK saw notable increases in the strictness of sanctions across the period. Belgium, Denmark and Japan, in contrast, introduced fewer changes, albeit ones which increased the strictness of sanctions, whilst no reforms were observed in Greece since 1980. Although rare, instances of relaxations in sanctions were also observed in certain countries, such as Australia, Germany and New Zealand. Since such instances of relaxations are rare compared with increases in strictness, nevertheless, it is clear from the evidence provided by Knotz (2018; 2019) that an overall trend towards more severe penalties is observed.

Building on the work of Venn (2012) and Langenbucher (2015), Immervoll and Knotz (2018) provide the most recent cross-sectional data on conditionality and sanctions from across 39 OECD and EU countries in 2017. Their analysis provides a useful addition to that provided by Knotz (2018; 2019) in that they are able to rank countries in terms of the strictness of their requirements and sanctions, though again the focus remains on sanctions rules in unemployment insurance schemes as opposed to means-tested or social assistance schemes. In terms of sanctions specifically, Immervoll and Knotz (2018) construct an overall strictness indicator based on the severity of sanctions for different reasons, which include: voluntary resignation; first and repeated refusals of job offers; and first and repeated refusals of ALMP participation. Based on this measure, the UK is mid-ranked in terms of the harshness of its sanctions regime, placed 20<sup>th</sup> most severe out of the 39 countries. This is shown in Figure 2.1. As the authors note, sanctions rules are generally strictest in Southern and Eastern Europe and less severe in Central and Northern Europe as well as in non-European countries such as Japan and South Korea. Interestingly, nevertheless, the UK ranks comparatively worse on the overall conditionality indicator, which includes sanctions rules as well as availability, suitable work, job-search and monitoring requirements, where it is placed 8<sup>th</sup> in terms of strictness.

**Figure 2.1:** strictness of benefit sanction rules across the OECD and EU, 2017

Source: Immervoll and Knotz (2018)  
Used with permission of the OECD

Whilst the UK is placed in the middle of the sanctions ranking detailed in Figure 2.1, Immervoll and Knotz (2018) nevertheless specifically highlight the unusually steep and escalating nature of the sanction rules in the UK. This refers to the fact that the length of sanctions increases rapidly for second and third infringements. Chapter 3 will provide more detail on the precise nature of the UK system, including information on the severity of sanctions, the reasons for which they apply and the frequency with which they have been applied. The sanctions that Immervoll and Knotz (2018) refer to are termed ‘high level’ in the UK system, for reasons such as not participating in mandatory workfare, not accepting a job offer, and losing a job voluntarily or due to misconduct. They lead to complete benefit loss for periods of 13, 26 or 156 weeks for first, second and third infringements respectively. Chapter 4 of this thesis reviews the available evidence on the impacts of benefit sanctions, discussing studies that use data from the US, the UK and other European countries such as Denmark, Finland, the Netherlands, Sweden and Switzerland. It is useful to note here that out of these countries the UK is ranked second in the Immervoll and Knotz (2018) ranking, similar to but slightly below Finland in terms of strictness. Differences in the length of sanctions can be large. In Sweden, for example, the first refusal of a job offer leads to a one week sanction, compared with 13 weeks in the UK.

Before progressing to the UK policy chapter, the final section in this chapter considers the ongoing ethical debate regarding the acceptable role of behavioural conditionality within the social security system and across the welfare state more broadly. Arguably, evidence relating to the impacts of benefit sanctions forms a central issue within this debate, which has important implications for the empirical focus of this thesis.

## **2.5 Ethical legitimacy of behavioural conditionality**

### **2.5.1 Competing normative perspectives**

The ethical justification of behavioural conditionality is contested by a variety of normative perspectives, including - though not limited to - contractualism, communitarianism, rights-based approaches, paternalism and utilitarianism (Deacon, 2004; Paz-Fuchs, 2008; Watts and Fitzpatrick, 2018). The discussion in this section provides a broad overview of each of these perspectives, without seeking to defend any one view in particular. Rather, the main rationales within this debate will be outlined, and the relevance of ethical concerns ultimately situated within the aims and contribution of this thesis. Watts and Fitzpatrick (2018: 15) defend a form of value pluralism in their development of a framework for assessing behavioural conditionality in terms of both its “efficacy and ethical legitimacy”. In contrast to a monist pursuit of a single normative perspective, their pluralism is based on the view that legitimate yet incommensurable values inevitably conflict as part of social policy analyses, and that there is therefore a need to consider trade-offs between competing societal objectives using multiple criteria. Whatever the relative validity of this perspective in terms of normative theory, an identifiable merit of the direct engagement with trade-offs inherent within the pluralist approach is its emphasis on the “interdependence between ethical reasoning and empirical evidence” (Watts and Fitzpatrick, 2018: 139) when attempting to provide overall assessments of particular policy agendas.

Appeals to contractualist arguments are prominent within attempts to justify behavioural conditionality, and can be most succinctly summarised as the view that there can be “no rights without responsibilities” (Giddens, 1998: 65). Appeals to contractualism have a long history, motivating, for example, the

demands placed on individuals from the Elizabethan Poor Laws onwards as well as part of the more recent 'behavioural turn' (Paz-Fuchs, 2008). According to contractualism, reciprocal obligations emerge from a social pact between state and citizen, whereby an individual's access to rights must be matched by their own fulfilment of particular responsibilities (Deacon, 2004). Viewed through the lens of "reciprocal responsibility" (Sage, 2012: 359), behavioural conditionality can be considered as justified if it is accepted that the various behavioural demands that are established, as well as the concomitant need for monitoring and sanctions, represent a fair quid pro quo for the provision of state support. At a basic level, contractualist arguments simply adhere to the view that a "something for nothing" (Paz-Fuchs, 2008: 89) exchange is itself morally unjust. Additional assumptions, however, are required to motivate particular demands made in specific instances. In terms of unemployment benefits, for example, the fundamental expectation placed upon claimants is that they do what they can to secure employment. The contractual logic in this scenario therefore also assumes that this obligation would not be met without the imposition of behavioural conditions.

Grover (2012) critiques such defences of behavioural conditionality by highlighting the unequal power relations that in reality underpin the so-called reciprocal state-citizen contract. Somewhat problematically, the state is able to act unilaterally in setting the terms of the agreement with claimants, who have minimal power to influence the process and are compelled to conform to the result. Communitarian arguments avoid this criticism by emphasising the commitments that individuals have towards one another independent of any state-citizen contract, which are seen to "arise merely from their membership of a community" (Watts and Fitzpatrick, 2018: 130). In particular, these obligations are based upon principles of mutual respect, recognition of the needs of others and a commitment to the common good. Behavioural conditionality can be justified on this view if it is seen to play an important role in articulating the collectively established common good, as well as in ensuring that the civic responsibilities associated with it are carried out. Such arguments have been brought to bear in debates surrounding anti-social behaviour (Deacon, 2004), though they might equally be applied in relation to social security. In this policy domain, the communitarian approach would directly affirm a commitment that

is more implicit within the contractualist perspective, arguing that employment should be afforded an overriding moral status as the primary activity through which individuals are able to contribute to the common good.

Rights-based approaches, in contrast, have been advanced to contest behavioural conditionality, based on the view that social citizenship should be accompanied by unconditional entitlements when it comes to ensuring that basic needs are met (Watts and Fitzpatrick, 2018). For these critics, needs claims come prior to and are privileged above any behavioural obligations. Behavioural conditionality is therefore unjustified since it makes such entitlements conditional upon individuals' performance as opposed to being available based upon their inalienable rights. Such views are often advanced on the basis of citizenship rights, though Dean (2013) goes as far as to defend a vision of social rights in which a degree of unconditional support should be made available to all individuals, citizens or otherwise, on the basis of shared and interdependent human needs. Indeed, the impact on both citizenship and migrant rights of conditionality is an important and arguably overlooked area of concern (Shutes, 2016; Edmiston, 2017). Weaker forms of rights-focused approaches have made the more limited claim that the extent of behavioural conditionality, as currently enforced, over-emphasises duty at the expense of any substantive commitment to social rights (Deacon, 2004). On this view, a reciprocal balance between rights and responsibilities is not currently being realised in practice. Whilst behavioural demands on the unemployed have significantly increased in recent decades, this has arguably not been accompanied by any meaningful expansion in employment support (Daguerre and Etherington, 2014; Fitzpatrick et al., 2019).

Paternalist perspectives of conditionality circumvent discussion of rights and responsibilities, arguing instead that behavioural requirements are justifiable as they ultimately serve the best interests of those who claim state support. Mead (1989: 165) is a prominent and influential proponent of this view, defending behavioural conditionality as a legitimate "exercise in authority" that should be used to compel claimants to behave in ways that are thought to be good for both them and society as a whole. As discussed in Section 2.3, this perspective is underpinned by a particular conception of human agency, which views the

unemployed as welfare dependent and unable to act in their own best interests. Behavioural conditionality is arguably required, therefore, to force individuals to pursue activities that would improve their own lives but that they would not otherwise carry out. As is the case with contractualism and communitarianism, the paternalist defence of conditionality affords paid work a normative primacy above other forms of social activity, leading it to favour forms of conditionality based upon work-related behavioural requirements designed to promote entry into paid employment. Any short-term negative implications of this approach for individuals, such as the imposition of benefit sanctions, are justified by the argument that claimants will benefit in the long-term through the development of work-related behaviour and subsequent opportunities to gain employment and increase their income (Dunn, 2014).

Despite its specific normative commitments, therefore, the paternalist defence of behavioural conditionality is heavily reliant on its empirical claim relating to the overall long-term benefits of such approaches. The same is true of utilitarian arguments that have appealed to the deterrence effects of conditionality and its role in minimising state expenditure through the 'efficient' use of public resources (Paz-Fuchs, 2008). It can be reasonably assumed that an expansion of behavioural conditions, and the increased monitoring and sanctioning of claimants that they imply, will serve as an effective deterrent for eligible individuals. Indeed, deterrence effects can be observed empirically as a key driver of the widening gap between official levels of unemployment and the claimant count in recent decades (Phillips, 2017). The link between behavioural conditionality and state expenditure is not straightforward, however, not least because of the administrative cost associated with maintaining a monitoring and enforcement bureaucracy. In their assessment of the UK sanctions regime, for example, the National Audit Office (NAO, 2016a) found that the overall cost implications of conditionality are unknown, given the fact that many of the wider costs for individuals and the state have not been measured. Indeed, a growing empirical literature contests the link between sanctions and positive labour market outcomes, and identifies a range of negative wider impacts on claimants themselves and third parties such as children (Watts et al., 2014).

## 2.5.2 The role of empirical evidence

Whilst empirical evidence is clearly important to the validity of paternalist and utilitarian arguments, Watts and Fitzpatrick (2018: 152) argue that evidence regarding the “effectiveness in practice” of conditional approaches should be of concern to all the normative perspectives discussed in this section. This argument follows from the fact that, in the area of social security, the perspectives that seek to justify specific forms of behavioural conditionality make a common set of assumptions that themselves are open to empirical scrutiny. First, they assume that without behavioural conditions claimants would not carry out the activities necessary to find paid employment. Second, by emphasising paid work as the desired end goal that is achieved using conditionality, they make implicit assumptions regarding the benefits of paid work to individuals and society as a whole. As previously highlighted, furthermore, Watts and Fitzpatrick (2018) defend a value pluralist framework that recognises that competing normative commitments will inevitably collide in social policy debates, often without there being a clear means of resolution. It therefore becomes necessary to consider potential trade-offs between normative commitments and evidence regarding the actual impacts of behavioural conditionality, as investigated through empirical research.

Whilst the differences between these perspectives are not necessarily fully resolvable through empirical investigation alone, evidence relating to the impact of behavioural conditionality on claimants is clearly relevant for each viewpoint when attempting to provide an overall assessment of particular policy designs. In this regard, Chapter 4 discusses the UK and international research that investigates the impacts of benefit sanctions on claimants, focusing both on labour market as well as wider effects. This discussion provides important background for the empirical chapters in this thesis, which investigate the relationship between sanctions and mental health outcomes for claimants of unemployment benefits. Prior to this, however, Chapter 3 provides an overview of developments in behavioural conditionality in the UK social security system in recent decades, paying particular attention to the increased role of benefit sanctions.



## 2.6 Conclusion

This chapter has situated the use of benefit sanctions in relation to the broader notions of behavioural conditionality and activation. In terms of conditionality, behavioural requirements have been developed for each of the three conditional ‘levels’ identified by Clasen and Clegg (2007). Thus, whilst the recent growth in conditions of conduct is a core feature of the behavioural shift in welfare state provision, behavioural conditionality itself can be understood in terms of behavioural requirements that regulate both initial and ongoing benefit access. These demands, furthermore, are enforced through both monitoring and sanctions for non-compliance. For unemployment benefits, these behavioural requirements are invariably work-related, specifying acceptable reasons for leaving work as well as availability requirements, job search criteria and involvement in training as conditions of continued benefit receipt. As such, behavioural conditionality within the social security system is viewed as a key constitutive element of the so-called ‘activation turn’ that has occurred across developed economies since the 1990s.

Whilst both behavioural conditionality and activation are not uniquely contemporary phenomena, a qualitative shift in their emphasis has arguably occurred in recent decades. This is clearly demonstrated by the available cross-country data that relate to conditions and sanctions in the OECD and the EU. The ethical legitimacy of behavioural requirements and sanctions is viewed differently by competing normative perspectives. An important issue within this debate relates to the impacts of behavioural conditionality on claimants, which will be further investigated from Chapter 4 onwards. First, however, Chapter 3 will provide an overview of the growth in behavioural conditionality and benefit sanctions in the UK, which provides the policy context for the empirical investigation conducted in this thesis.

## Chapter 3. Conditionality in UK social security policy

### 3.1 Introduction

This chapter details developments in UK social security policy since the 1980s. Specifically, it tracks the growth of behavioural conditionality and benefit sanctions imposed on unemployed claimants, as well as on additional groups such as lone parents and disabled people. As Chapter 2 highlighted, nevertheless, it is important to remain cognisant of the fact that the receipt of unemployment benefits in the UK has always been conditional in certain respects. Since the introduction of an unemployment insurance benefit in 1911, for example, a maximum six-week disqualification could be applied to claimants who lost their previous job due to perceived misconduct, to those deemed to have left their previous job without just cause, and for not being available for work (Adler, 2016). This chapter, however, does not seek to provide a comprehensive account of behavioural conditionality since 1911, and is instead divided into three sections according to more recent government administrations: Conservative governments (1979-1997); New Labour governments (1997-2010); and the Coalition government (2010-2015). The initial Conservative period provides a good starting point as it ended with the introduction of Jobseeker's Allowance (JSA), the unemployment benefit that forms the basis of the empirical investigation in this thesis.

An important distinction to highlight before discussing developments in behavioural conditionality throughout this period is that between disqualifications, sanctions and disentitlements. In the pre-JSA period, six-week disqualifications applied for the three main reasons listed above, which relate mainly to conditions of circumstance in the form of retrospective requirements. Following the introduction of JSA, however, disqualifications were replaced as an official term by sanctions, which applied to an expanded list of requirements encompassing both retrospective requirements as well as ongoing conditions of conduct. In terms of disentitlement, furthermore, the introduction of JSA in 1996 established greater provisions for the enforcement of the new definition of unemployment, introduced in 1989, which required individuals to actively seek

work. Failure to demonstrate compliance with this demand within JSA leads to disentitlement, as opposed to a sanction. In practice, this meant that the affected individual would stop receiving JSA but would be able to re-claim and receive the full JSA amount straight away if they could show that they were now meeting the necessary requirements (DWP, 2011a). In order to provide sufficient context for the empirical investigation in this thesis, the final section of this chapter focuses specifically on JSA benefit sanctions in the context of the Coalition government, which oversaw various developments as well as a change in the rules relating to both sanctions and disentitlements.

## **3.2 Conservative governments (1979-1997)**

### **3.2.1 The ‘Stricter Benefit Regime’**

Since at least the 1980s, successive governments have sought to develop stricter social security regimes that tighten eligibility and increase conditional requirements. The first Conservative government of the 1979-1997 period oversaw dramatic rises in unemployment, and responded initially by developing training programmes for the young and the long-term unemployed (Lindsay and Mailand, 2004). Given the scale of unemployment at the time, however, and combined with attempts to reduce the number of civil servants, the administrative capacity of the state to monitor job search was limited up until 1986 (Price, 2000). This situation changed following the implementation of the Restart Programme, which increased the maximum period of disqualification from six to 13 weeks in 1986 and then to 26 weeks in 1988, and expanded the institutional resources available to enforce new requirements (Finn, 2003). Consequently, those unemployed for over six months were required to undertake mandatory job search reviews whilst those unemployed for over a year were offered opportunities designed to help them back into employment, with sanctions applying for non-participation. Legislation passed in 1989, furthermore, restricted the reasons for which claimants could refuse job offers and required them to actively seek work. Price (2000: 267) refers to developments in this period as the formation of a “Stricter Benefit Regime”, which aimed to increase institutional pressure on claimants to seek paid employment.

### 3.2.2 Introduction of Jobseeker's Allowance

The Jobseekers Act 1995 is considered a “watershed moment” (Dwyer, 2016: 45) in terms of behavioural conditionality. As set out in the White Paper *Jobseeker's Allowance* (DSS, 1994), Jobseeker's Allowance (JSA) established a single unemployment benefit consisting of a contributory component and an income-based component. Eventually introduced in October 1996, JSA saw a reduction in the length of entitlement to contributory benefit from twelve months to six months, a reduction in benefit for under-25s and the restriction of entitlement for 16 and 17 year olds (Strickland, 1996). Significantly, JSA was accompanied by the creation of the Jobseeker's Agreement, a document that claimants had to sign and abide by and which aimed to enforce the active jobseeking requirement introduced in 1989. Accordingly, unemployed individuals had to develop back-to-work action plans, record job search activity in jobsearch diaries and specify the minimum wages that they would be willing to accept. Claimants were also required to visit the Jobcentre every two weeks and search for work outside of their own occupation after three months. In addition to the new requirements, JSA advisers were given discretionary powers to issue a Jobseeker's Direction, which could compel an individual to look for work in a particular way, dress in a certain manner to improve their employability, or attend a course to improve jobseeking skills and motivation.

Importantly, the new JSA framework was enforced through an increased emphasis on benefit sanctions (Strickland, 1996). Similar to the pre-JSA regime, claimants could be sanctioned for a discretionary period of up to 26 weeks if they lost their jobs through misconduct, left work voluntarily or refused to apply for a notified vacancy. In addition, however, JSA introduced new pre-defined sanctions of two weeks, or four weeks for repeat non-compliance within the same 12 months, for failing to carry out a Jobseeker's Direction. Those deemed not to be available for and actively seeking work, or who refused to sign the Jobseeker's Agreement, would be disentitled from JSA. Taken together, these changes saw the extension of the sanctions regime from largely retrospective conditions of circumstance to include ongoing conditions of conduct, and reenforced the categorisation of unemployment in behavioural terms. An additional and important change in terms of the material circumstances of

unemployed individuals, furthermore, saw sanctioned JSA claimants lose automatic entitlement to hardship payments, payable at 60% or 80% of the JSA rate. Under the new regime, individuals would not get any support unless they could demonstrate that they would suffer hardship as a result of their sanction, and even then would get no financial support for the first two weeks unless they were in a pre-defined 'vulnerable' group.

### **3.3 New Labour governments (1997-2010)**

#### **3.3.1 Welfare-to-work and the New Deal**

The New Labour government elected in 1997 inherited both a comparatively low unemployment rate and benign economic conditions (MacDonald, 1997), but nonetheless went on to advance and entrench behavioural conditionality both for the unemployed and for previously exempt groups such as lone parents and disabled people. Reform was in part motivated by the view that the social security system had "become part of the problem itself" (DfEE, 2001: 1), blamed for the fact that, in 1997, the claimant count was 60% higher than in 1979, approximately one fifth of households had nobody in work and a third of children lived in poverty. In response, New Labour promised to rebuild the welfare state according to an approach that emphasised paid work as the best way to combat poverty, in what Finn (2003: 709) describes as an "employment-first" welfare state. As outlined in the Green Paper *A New Contract for Welfare* (DSS, 1998), policy interventions under the new paradigm would combine a carrot and stick approach in order to incentivise paid work. Accordingly, the provision of stronger work incentives through the establishment of a national minimum wage, tax credits and subsidised childcare would be accompanied by benefit sanctions in case of non-compliance with the requirements of benefit receipt. Reform began almost immediately in 1997, though the new era of welfare-to-work programmes that promised support tailored to the needs of each individual was formalised in the Welfare Reform and Pensions Act 1999 (Hasluck, 2001).

A core element of the New Labour reform agenda was the development of New Deal programmes for various groups of benefit claimants, with primary importance placed on tackling youth unemployment (Jarvis, 1997). Introduced in 1998, the New Deal for Young People (NDYP) was a compulsory programme

directed at individuals aged 18-24 who had been claiming JSA for over six months (DfEE, 1997). The NDYP involved a 'Gateway' period of up to four months of intensive job-search support, with claimants who were still unemployed at the end of this period moving on to a second stage that offered one of five compulsory options: full-time education and training for twelve months; work experience in the voluntary sector for six months; a community placement with the Environment Task Force for six months; a private-sector job supported by wage subsidy for six months, with training for at least one day per week; or an additional self-employment route with support towards starting and running a business. Importantly, refusal of the NDYP options in the second stage resulted in the claimant facing a benefit sanction. Initially, the NDYP operated within the JSA sanctions regime of pre-defined sanctions of two and four weeks for first and second 'failures', but from March 2000 a third repeat 'failure' resulted in an individual having their benefit suspended for up to 26 weeks (Finn, 2003).

### **3.3.2 Broadening the scope of conditionality**

The NDYP was accompanied by other New Deals for groups such as lone parents and disabled people (van Reenen, 2004). As the Green Paper *Towards full employment* (DfEE, 2001) describes, each New Deal was based on principles similar to those outlined for the NDYP, though participation for lone parents and disabled people was initially voluntary. An early emphasis on incentives and support, however, gradually shifted towards more conditional approaches that sought to promote employment outcomes (Dwyer, 2016). Throughout the first decade of the New Labour era, for example, two distinct conditionality regimes developed that distinguished between unemployed people through the JSA regime and lone parents and disabled people through the Work-focused Interviews (WFIs) regime (Gregg, 2008). WFIs were intended to provide personalised support for individuals to move towards employment by encouraging them to engage with a mix of support services and employment programmes. Mandatory WFIs were introduced for lone parents in 2001, affecting those claiming Income Support (IS) with a youngest child aged 5 or older (Johnsen, 2014). WFIs were extended to all lone parents claiming IS from 2004, and incorporated mandatory Action Plans in 2005, with failure to comply

resulting in an open-ended 20% sanction until the conditions were met. For disabled people, mandatory WFIs were introduced in pilot form in 2003 and rolled out thereafter (Toerien et al., 2013).

Subsequently, a Green Paper (DWP, 2006) and a report commissioned by the DWP recommended the further extension of work-related conditionality to lone parents and disabled people (Freud, 2007), with the various changes incorporated in the Welfare Reform Act 2007. Lone Parent Obligations (LPOs) were introduced in 2008, initially requiring claimants of IS with children over the age of 12 to be available for and to actively seek work (Johnsen, 2014). For disabled people, Employment and Support Allowance (ESA) was introduced for new claimants in 2008, replacing Incapacity Benefit (IB), disability related Income Support (IS) and Severe Disablement Allowance (SDA) (Patrick, 2011). The new regime distinguished between the severely disabled and those temporarily unfit to work and required new claimants to undergo a Work Capability Assessment (WCA), from which three outcomes follow. First, those with severe conditions enter the Support Group, which entitles them to unconditional support. Second, those with less severe conditions are placed in the Work Related Activity Group (WRAG) and required to participate in WFIs, produce action plans and engage in work-related activities. Failure to comply would result in an open-ended sanction of 50% of the Work Related Activity Component (WRAC) of ESA for the first four weeks and 100% thereafter until the conditions are met. Finally, those found fit for work are disqualified from ESA and must instead claim JSA.

Following the Welfare Reform Act 2007, the DWP commissioned an additional report, *Realising Potential* (Gregg, 2008), which set out aims to establish personalised conditionality and support in line with the government's previous Green Paper (DWP, 2008a) and subsequent White Paper (DWP, 2008b). The Gregg report (2008: 27) describes behavioural conditionality as a "central tenet" of the social security system, and recommended that it should be extended to the vast majority of claimants so that all but the most in need would be required, under threat of sanction, to take steps towards finding work. The report argued, furthermore, that the existing sanctions regime was too complex and difficult to understand, and should align the imposition of a sanction more

closely with specific types of non-compliant behaviour. The recommendations were incorporated in the Welfare Reform Act 2009, enacted in the aftermath of the recession that followed the 2007-08 financial crisis (Daguerre and Etherington, 2014). In particular, the Act set out plans to abolish IS and IB and to move recipients onto JSA or ESA, as well as changes to make the sanctions system more “consistent, automatic and escalating” (Barker and Lamble, 2009: 324). The new rules, for example, introduced a new regime for non-attendance at mandatory Jobcentre appointments for JSA claimants, which would result in a benefit sanction of at least a week for a first ‘failure’ and two weeks for subsequent non-compliance.

## **3.4 Coalition government (2010-2015)**

### **3.4.1 Continuity and change**

The Conservatives and Liberal Democrats formed a Coalition government in 2010, forged together around an austerity programme that - amongst other rationales - renewed the emphasis on behavioural conditionality as a means for achieving reductions in public expenditure (McEnhill and Taylor-Gooby, 2018). Daguerre and Etherington (2014) highlight the features of the Coalition’s reforms that arguably represent a continuation of the New Labour agenda. First, the Welfare Reform Act 2009 had introduced a mandatory Work for Your Benefit scheme for long-term JSA claimants, introduced in pilot form pre-2010 and then fully rolled out by the Coalition. Subsequently, this was replaced by the Mandatory Work Activity scheme in 2011, which enabled advisers to instruct claimants to attend up to 30 hours per week unpaid compulsory work placements for a maximum of four weeks. Second, the Coalition continued to reduce the child age thresholds at which eligibility to IS for lone parents became conditional on active job search. Age thresholds were gradually reduced from age 10 or above in 2010 to 3 or above in 2014, with lone parents of children aged 5 and above expected to meet the full work-related requirements of JSA. Third, the Coalition continued to target the number of existing IB claimants. Between 2011 and 2014, all existing claimants of incapacity benefits were reassessed for ESA under the Work Capability Assessment (WCA).



Several aspects of the post-2010 period, nevertheless, distinguish it from the New Labour era in what Fletcher and Wright (2018: 324) describe as the Coalition's "punitive turn". Prior to 2010, the UK's work first approach combined both disciplinary and assistive measures as previously described, in the form of work-related conditionality, sanctions and a variety of employment-related support initiatives, a national minimum wage, support for childcare and working tax credits. Indeed, a significant initiative towards the end of the New Labour era saw the creation of the Future Jobs Fund (FJF) in 2009, which provided a temporary guaranteed job to young long-term unemployed people (Fishwick et al., 2011). The FJF, however, was abandoned by the Coalition in favour of its centrepiece welfare-to-work scheme, the Work Programme (WP) (DWP, 2012). The Work Programme was aimed at assisting the long-term unemployed into work, and was mandatory for JSA claimants and ESA WRAG claimants, on the basis of a "payment-for-results" (DWP, 2012: 2) model in which a range of sub-contracted private and third sector providers were allowed significant freedom in determining what support to provide. Critics of the scheme have argued that the payment structure incentivised providers to offer minimal support and poor quality services for long-term unemployed individuals, focusing on those closest to the labour market and ignoring harder to help groups in a process described as "creaming and parking" (Carter and Whitworth, 2015: 277).

A diminished emphasis on employment support was accompanied by a renewed emphasis on benefit sanctions, with the Coalition period characterised by what Webster (2016: 2) describes as a "great sanctions drive". Section 3.5 provides more depth on sanctions policy during this period, which provides the context to the empirical investigation carried out in this thesis. Here, however, it is important to briefly highlight two important features of this sanctions drive. First, the Coalition government oversaw an unprecedented level in the frequency with which sanctions were imposed. Between 2010 and 2015, for example, nearly a quarter (24%) of JSA claimants received at least one sanction (NAO, 2016a), whilst monthly rates of JSA sanctions were consistently higher compared with their historic level (Webster, 2016). ESA WRAG sanctions also experienced an increase, albeit to a smaller degree and from a lower base level. Second, the Coalition marked a step change by dramatically increasing the severity of the sanctions that could be applied, as enacted by the Welfare

Reform Act 2012. For JSA claimants, the minimum length of a sanction was increased from one to four weeks, whilst the maximum length was increased from 26 to 156 weeks, in an escalating system in which claimants could be sanctioned for fixed lengths of 4, 13, 26 and 156 weeks (DWP, 2013a). For ESA WRAG claimants, furthermore, the new regime replaced the previous system with a 100% open-ended reduction of the central ESA component, followed by a sanction of one, two or four weeks following re-compliance.

### **3.4.2 Universal Credit**

The Welfare Reform Act 2012 is significant, furthermore, for its introduction of Universal Credit (UC), as initially outlined in an earlier Green Paper (DWP, 2010a) and White Paper (DWP, 2010b). Universal Credit began a phased introduction in 2013 for new claimants, amalgamating several existing means-tested benefits and tax credits into a single working-age benefit, which include: JSA; ESA; IS; Housing Benefit; Child Tax Credit; and Working Tax Credit (Dwyer and Wright, 2014). Existing contributory benefits, however, such as contributory JSA and contributory ESA remain. Universal Credit is delivered as a single payment that is intended to incentivise entry into work for the unemployed, as well as progression to more or higher paid work for those on low wages. An important feature of the new benefit is the Claimant Commitment, also introduced for JSA and ESA WRAG claimants. Similar to the Jobseeker's Agreement, the commitment sets out the job seeking and work-related conditions of benefit receipt and may include, for example, the requirement to attend the local Jobcentre more frequently, to spend 35-hours a week looking for work or to apply for a minimum number of jobs per week (Gillies et al., 2013). Importantly, the monitoring of the Claimant Commitment intensifies the threat of sanction for claimants, who can have their benefit removed for non-compliance with any item that it includes (Fletcher and Wright, 2018).

Arguably, the most significant feature of Universal Credit is its extension of conditionality to those in employment, re-categorising the previously 'deserving' status of low-paid workers and constructing 'dependency' as a failure to be fully financially independent of the state (Dwyer and Wright, 2014; Millar and Bennett, 2017). In-work conditionality operates through the creation of a

“conditionality threshold” (DWP, 2010b: 31), which specifies that recipients in low-paid or part-time employment with earnings that fall below the minimum will be subject to conditionality until they find better paid work or work more hours. Thus, Universal Credit aims to achieve “personalised conditionality” (DWP, 2010b: 27), whereby the intensity of welfare conditionality applied varies along a spectrum. In addition to the in-work group, this includes: ‘no conditionality’ for those in the ESA Support Group; ‘keeping in touch’ with the labour market via mandatory WFIs for lone parents with a child aged one to five; ‘work preparation’ for ESA WRAG claimants; and ‘active job search’ for JSA claimants and lone parents with a child aged 5 or above. Sanctions operate in a similar fashion to the JSA regime, with penalties of between one and 156 weeks depending on the number and type of ‘failures’ committed (DWP, 2019). Whilst three-year sanctions have recently been abolished by the DWP, the harshness of the system continues, including consecutive as opposed to concurrent sanctions combined with repayable hardship payments.

### **3.4.3 Key developments in sanctions**

Thus far, this chapter has provided an overview of developments in conditionality and sanctions in UK social security since the 1980s, documenting an agenda that has been pursued by a variety of governments and in varying economic circumstances (Wright, 2012). Dwyer (2004: 265) describes developments in the earlier period in terms of “creeping conditionality”, in a process that has arguably reached a state of “ubiquitous conditionality” (Dwyer and Wright, 2014: 27) today following the introduction of Universal Credit. Whilst work-related conditionality has long played a role within the UK social security system, its influence is no longer restricted to the unemployed, now also applying to lone parents, disabled people and people in low-paid employment. With regard to sanctions specifically, significant developments have seen changes in the “nature, scope, and scale” (Adler, 2016: 199) of their application. Table 3.1 summarises these developments, adapting a table initially constructed by Adler (2018: 47). It contrasts the era prior to the introduction of JSA in 1996 with subsequent developments.

**Table 3.1:** developments in UK sanctions policy

Pre-JSA (1996)	Post-JSA and Welfare Reform Act 2012
Apply mainly for retrospective conditions of circumstance, e.g. for leaving work voluntarily, being dismissed for misconduct or not being available for work.	Also apply for ongoing conditions of conduct, e.g. for not actively seeking work, failing to attend a training or employment scheme, or missing an interview.
Apply to unemployed individuals - specifically applicants of unemployment insurance benefits.	Apply to unemployed individuals, lone parents, long-term sick and disabled people and those in low-paid unemployment - applicants and recipients of JSA, ESA, IS and UC.
Apply for discretionary periods of up to six weeks (1911-1986), 13 weeks (1986-1988) or 26 weeks (1988 onwards).	Apply for fixed periods ranging from four weeks to 156 weeks (though 156 week sanction dropped in 2019).
Sanctioned claimants had a right to claim means-tested social assistance (at a reduced rate) immediately.	Sanctioned claimants have to apply for discretionary hardship payments (also at a reduced rate) but, in most cases, only after a two-week delay. For UC claimants, these must be repaid.

*Source:* adapted from Adler (2018)

The following section considers JSA sanctions policy throughout the Coalition government in more depth, which is the period that provides the specific context for the empirical investigation carried out in this thesis. Sanctions policy between 2010 and 2015 is considered to represent a major economic and social policy experiment, which as previously alluded to involved an increase in the overall number of sanctions applied as well as a significant increase in their potential length of application (Webster, 2016). Whilst the next section focuses solely on JSA sanctions, it is important to re-iterate that ESA sanctions also saw important developments during this period. There were variations in the frequency of ESA sanctions, albeit at a much lower level than for JSA sanctions, whilst the severity of sanctions were increased from December 2012 onwards, as previously described.

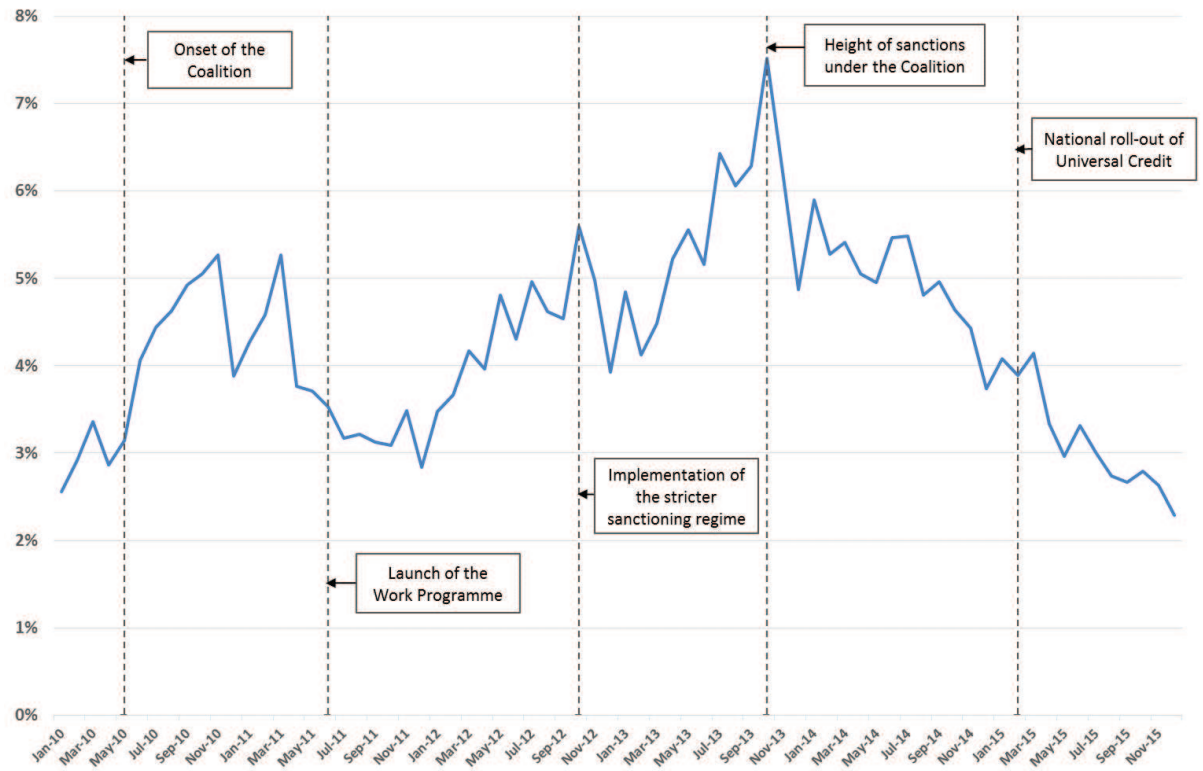
## 3.5 JSA sanctions policy during the Coalition

### 3.5.1 Variations in the frequency of sanctions

All JSA claimants are subject to the threat of sanctions for not complying with the rules of JSA receipt. In terms of the frequency of sanctions actually applied, however, the DWP has repeatedly claimed that such financial penalties

ultimately affect relatively few JSA claimants, arguing for example that sanctions are “only used as a last resort in a small percentage of cases” (DWP, 2015a: 2). Between 2010 and 2015, however, nearly a quarter (24%) of JSA claimants received at least one sanction, of which nearly three-fifths (58%) received one sanction, a fifth (20%) received two sanctions and just over a fifth (22%) received three or more sanctions (NAO, 2016a). These figures suggest that whilst sanction impositions do not affect a majority of JSA claimants, they certainly affect a sizeable minority that represents many more than the small percentage of cases claimed by the DWP. Importantly, furthermore, there was significant variation in sanction rates throughout this period, which saw notable rises and falls as the Coalition’s social security policies developed. This variation is demonstrated in Figure 3.1, which depicts the monthly rate of JSA sanctions imposed as a proportion of JSA claimants between 2010 and 2015.

As Figure 3.1 makes clear, there was considerable monthly variation in the rate of JSA sanctions throughout the 2010-2015 period. Following the onset of the Coalition government, for example, there was an immediate and marked rise in sanctions rates, which rose from 3.1% in May 2010 to 5.3% in March 2011 in what Webster (2016: 2) describes as an “unannounced change of policy”. Non-transparent changes in policy are, by their very definition, hard to verify. The available evidence, however, suggests that such departmental decision-making is likely to have played an important role throughout the period. In April 2011, for example, the previous “system of targets through benchmarks” (Couling, 2013: 3) for sanctions referrals was removed, which the DWP itself identified as a factor in the subsequent fall in the sanctions rate to December 2011 (NAO, 2016a). Whilst the DWP denies any subsequent use of sanctions targets from April 2011 onwards, the House of Commons Work and Pensions Committee (HoC WPC, 2014) notes that in the same month new performance indicators for Jobcentre Plus offices and staff were introduced, in which sanction referral rates formed part of an advisor’s overall performance assessment. In a review of sanctions policy under the Coalition, furthermore, the National Audit Office (NAO, 2016a) concludes that management focus was influential, citing evidence from Jobcentre staff relating to an increased emphasis on sanctions in the period leading up to the height of sanctions in October 2013, followed by a relaxation in management pressure in the following period.

**Figure 3.1:** monthly rate of JSA sanctions (per cent of JSA claimants), 2010-15

Source: author's calculations using Stat-Xplore data (DWP, 2018c)

Another important determinant of sanctions during the period relates to the Work Programme, introduced in June 2011 and considered a key driver in the increase in sanctions from the beginning of 2012 to its peak of 7.5% in October 2013, over twice the monthly rate at the onset of the Coalition (NAO, 2016a; Webster, 2016). According to the Oakley Review (2014), for example, the Work Programme led to many more sanctions referrals than would otherwise have been the case, partly due to the fact that providers were obliged by the DWP to make sanction referrals according to a very strict interpretation of the sanctioning rules, irrespective of claimants' actual willingness to comply with such rules. Indeed, the principle reason for sanctions being imposed during this period related to 'failures' to participate in training or employment schemes, which in this context can be interpreted as the Work Programme (Webster, 2016). The number of sanctions relating to the Work Programme were followed in frequency by sanctions for not actively seeking work as well as not attending an interview. Importantly, Figure 3.1 depicts that following October 2013 the sanctions rate gradually returned to its pre-Coalition level. In addition to the change in management focus identified above, this reduction is partly explained by falling numbers of people involved in the Work Programme from the end of

2013 onwards, as opposed to any change in claimant behaviour, leading to the 3% sanction rate reached by the end of the Coalition in May 2015.

Whilst there is a clear fall in the sanctions rate from its height in October 2013, however, it is important to consider this change within the wider context of sanctioning policy. The JSA sanctions rate displayed in Figure 3.1, for example, does not include sanctions associated with unemployed claimants of Universal Credit (UC), which began its pathfinder phase in April 2013 followed by a national rollout in February 2015. Consequently, the observed sanctions rate underestimates the true level of sanctions from April 2013 onwards. Despite this underestimate, however, the entire Coalition period is characterised by an abnormally high imposition of benefit sanctions. Indeed, prior to May 2010 and going back to the introduction of JSA in 1996, the rate of sanctions was consistently below 3%, only rising to slightly below 4% during the 2007-08 financial crisis (NAO, 2016a). Webster (2016) calculates that during the Coalition period there were over a million more JSA sanctions than there would have been if the rate inherited from the previous government had continued. As the previous section described, furthermore, the Welfare Reform Act 2012 marked a step change by dramatically increasing the severity of the sanctions that could be applied. Whatever the overall rate of sanctions, therefore, the individual experience of a sanction was made significantly worse by the Coalition's social security reforms.

**Table 3.2:** list of the reasons for and length of JSA sanctions introduced by the Welfare Reform Act 2012

Type of sanction	Reason for sanction	1 <sup>st</sup> 'failure'	2 <sup>nd</sup> 'failure'	3 <sup>rd</sup> 'failure'	Previous regime
High level	<ul style="list-style-type: none"> <li>• Losing a job because of misconduct</li> <li>• Leaving a job voluntarily</li> <li>• Failing to apply for or accept a job</li> <li>• 'Neglecting to avail' yourself of a job opportunity</li> <li>• Failing to participate in Mandatory Work Activity (MWA)</li> </ul>	13 weeks	26 weeks: <i>if within 52 weeks but not within two weeks of previous failure</i>	156 weeks: <i>if within 52 weeks - but not within two weeks - of previous failure that resulted in 26 or 156 week sanction</i>	Variable 1 to 26 weeks except MWA (13 weeks)
	<ul style="list-style-type: none"> <li>• Failing to be available for and actively seeking work</li> </ul>	Disentitlement then up to 4 weeks loss of benefit	Disentitlement then up to 13 weeks loss of benefit: <i>if within 52 weeks - but not two weeks -of previous entitlement ceasing</i>		Disentitlement but no sanction
Intermediate level	<ul style="list-style-type: none"> <li>• Failing to participate in interviews</li> <li>• Failing to participate in a specified scheme for assisting people to obtain employment</li> <li>• Failing to carry out a jobseeker's direction that is part of a jobseeker's agreement</li> <li>• Other training scheme or employment programme sanctions e.g. losing a place on a training scheme through misconduct</li> </ul>	4 weeks	13 weeks: <i>if within 52 weeks - but not two weeks - of previous failure which resulted in a 4 or 13 week sanction</i>		Fixed 1, 2, 4 or 26 weeks
	Low level				

Source: DWP (2013a) and CPAG (2016)



### **3.5.2 Increase in the severity of sanctions**

Table 3.2 details the specific aspects of the new JSA sanctions regime that were introduced in October 2012, contrasting the new rules with the system that was previously in operation. As Table 3.2 indicates, the Welfare Reform Act 2012 introduced a tiered system with three levels of sanctions, according to which unemployed individuals are now at threat of losing their benefits for between four and 156 weeks depending on the type and number of rules not complied with. As previously indicated, the three most common reasons for a sanction during the Coalition period relate to failing to participate in training or employment schemes, not actively seeking work and not attending an interview. Perhaps most significantly, the minimum sanction length increased four-fold from 1 to 4 weeks whilst the maximum sanction length increased six-fold from 26 to 156 weeks. As Table 3.2 indicates, furthermore, the requirement to be available for and actively seeking work was previously accompanied solely by a disqualification, which in practice permitted almost immediate returns to JSA if the individual could show that they were now meeting requirements (DWP, 2011a). In the new regime, however, penalties for similar infringements are met with both a disqualification and a minimum four-week sanction, meaning that upon re-claiming JSA claimants will not begin receiving JSA again for at least a month. Publicly available statistics elide the distinction between disqualifications and sanctions, an issue that will be further discussed in Chapter 5.

### **3.5.3 Public controversy**

Sanctions policy during the Coalition government generated significant controversy, caused both by the frequency with which sanctions were applied as well as by the new harsher sanctions regime introduced by the Welfare Reform Act 2012. Adler (2016: 195), for example, argues that sanctions policy during this period came to represent a “new leviathan”, on the basis that at their height, the frequency of sanctions exceeded the number of fines originating in the criminal courts. Along with many of the Coalition’s benefit reforms in this period, sanctions attracted significant media attention, as well as criticism from various third sector groups and think tanks (CAB, 2013; Cowburn, 2015; Tinson, 2015). Criticism focused on the large number of sanctions made in error, narrow and inflexible adherence to sanctioning rules by Jobcentre staff,

disproportionate rates of sanctions imposed on particular groups and - as highlighted in Chapter 2 - adverse impacts on claimants (de Vries et al., 2017; Garthwaite, 2016; NAO, 2016a; Geiger, 2017).

Indeed, public controversy in this area motivated a number of official investigations into sanctions policy, including a review by Oakley (2014), a House of Commons Work and Pensions Committee (HoC WPC, 2015) inquiry and an investigation by the National Audit Office (NAO, 2016a). Subsequent to the Coalition period, furthermore, two additional inquiries were carried out by the House of Commons Public Accounts Committee (HoC PAC, 2017) and the House of Commons Work and Pensions Committee (HoC WPC, 2018), both of which considered the ongoing legacy of the reforms that were implemented in 2012.

Each of these official investigations made several recommendations to the DWP, and covered a wide range of issues and concerns. A recurring theme relates to the evidence that the DWP used to inform the 2012 reforms, as well as the evidence-base relating to the impacts of sanctions themselves. The National Audit Office (2016a: 10) report, for example, notes that the DWP provided “little evidence for its design choices” when introducing the escalating sanctions regime outlined in Table 3.2 and didn’t subsequently use its own data to evaluate the impacts of sanctions. In addition, the report argues that the DWP lacks a “strong evidence base about the effects of sanctions and the trade-offs involved” (NAO, 2016a: 38), in terms of the labour market and wider effects of sanctions, and the overall net cost or benefit of sanctions policy. These concerns were forcefully reiterated in the most recent inquiry, which recommended the DWP to “urgently evaluate the effectiveness of reforms to welfare conditionality and sanctions introduced since 2012” (HoC WPC, 2018: 19). In its response, the DWP accepted this criticism and stated that it intends to carry out an evaluation, specifically in relation to labour market outcomes and impacts on health and well-being (HoC WPC, 2019).

Partly informed by this pressing need, the empirical chapters in this thesis consider the mental health impacts of sanctions policy using data pertaining to the Coalition period. In order to inform this investigation, the following chapter

focuses specifically on the labour market and wider impacts of benefit sanctions, reviewing both the UK and international evidence in this regard.

### **3.6 Conclusion**

This chapter has provided an overview of behavioural conditionality in the UK since the 1980s, documenting the growth of work-related requirements and sanctions in the social security system to a state of “ubiquitous conditionality” (Dwyer and Wright, 2014: 27) for claimants today. Whilst work-related requirements played a role in the UK social security system prior to the period of analysis of focus here, this chapter has nevertheless demonstrated important qualitative changes along all the three ‘levels’ of conditionality that provide support for the ‘triple integration’ in unemployment protection previously discussed in Chapter 2 (Clasen and Clegg, 2007; 2011). An important shift was signified by the introduction of JSA in 1996, which brought together changes designed to better enforce the notion of unemployment as active jobseeking, more restrictive eligibility criteria and an expansion in the scope of conditionality and sanctions, which subsequently applied to both retrospective rules and ongoing work-related behavioural demands. Since then, behavioural conditionality and sanctions have been extended to previously exempt groups, such as lone parents, disabled people and people in low-paid employment. In addition, this chapter has outlined the various developments in sanctions policy that occurred during the period of Coalition government (2010-15). Specific attention was given to JSA sanctions policy during this time, which saw unprecedented levels in the frequency of sanctions applied and a significant increase in their severity. Sanctions policy in general, and these changes specifically, has generated debate regarding impacts on claimants, which is the focus of the following chapter.

## Chapter 4. Impacts of benefit sanctions

### 4.1 Introduction

This chapter discusses the literature that investigates the impacts of benefit sanctions on claimants, distinguishing between what are described as labour market and wider impacts. The stated primary aim of sanctions policy, both in the UK and internationally, is to increase rates of re-entry into employment for unemployed individuals (DWP, 2011a; Immervoll and Knotz, 2018). In addition, it is argued that these employment effects will lead to various wider benefits. The DWP (2011a: 10) argues, for example, that sanction-related employment effects will be accompanied by a range of “fiscal, as well as wider economic and social benefits”. These include: reduced public expenditure on a variety of benefits and increased tax receipts; benefits to the economy as a whole; increased income and improvements in health as a result of being in work; as well as reductions in child poverty from increases in parental employment. Based on this distinction between the separate impacts of benefit sanctions, this chapter first discusses the theory and evidence regarding labour market impacts before going on to discuss the evidence relating to wider impacts. Given the empirical focus on mental health impacts in this thesis, particular attention is paid to the existing literature in this area, as well as to considering the mechanisms that might link sanctions and impacts on mental health outcomes.

Before discussing the literature, it is important to highlight that there are at least three points in time at which benefit sanctions might have an effect. These are referred to as take-up effects, threat effects and imposition effects (Griggs and Evans, 2010). Take-up effects are influential before a benefit claim is made, since the very prospect of sanctions may deter eligible individuals from claiming in the first place. Threat effects, in contrast, occur during a benefit claim itself when the general threat of sanctions or an actual warning may impact claimants in some way. Imposition effects, lastly, occur once sanctions have actually been applied. Whilst take-up effects represent an important element of sanctions and conditionality more broadly, the focus here is on threat and imposition effects, given the empirical focus of this thesis. Regarding these latter two effects, furthermore, the literature on sanction impacts is more developed in the area of

imposition effects, which are easier to measure and investigate within empirical research. With the possible exception of formal sanction warnings, there is an evident difficulty in separating the threat effect of sanctions from the broader effect that the pressure of conditionality and monitoring might have on individuals. Indeed, since conditional requirements are premised on the threat of sanctions to ensure compliance, there are conceptual reasons against trying to delineate between their separate effects.

## **4.2 Labour market impacts**

### **4.2.1 Job search theory**

The economic literature on benefit sanctions relies solely on job search theory as formalised by Mortensen (1977), which has three key constitutive elements: job search intensity and effectiveness; job matching; and an individual's reservation wage. As is standard in economic modelling, job search theory adopts a rational choice theory of human agency, as described in Chapter 2 in relation to Murray's (1990) appeal to the behaviour of the 'economic man'. According to this framework, utility maximising individuals in the job search model compare the expected utility from unemployment benefits versus the expected utility associated with job search and possible employment. Job search intensity determines the rate of job offers that an individual receives, and job acceptance depends on the individual's reservation wage, the lowest wage rate that they are willing to accept. Unemployment benefits, it is therefore assumed, disincentivise work by lowering the cost of unemployment for individuals, reducing their job search intensity and increasing their reservation wage. Work-related conditionality, monitoring and sanctions, on the other hand, are assumed to increase the likelihood of an individual finding employment by increasing the relative costs of unemployment (van den Berg et al., 2004). Individuals are expected to respond to the threat of sanctions, for example, by increasing their job search intensity, lowering their reservation wage and accepting more job offers, or else risk having a sanction imposed.

Basic job search theory therefore appears to offer the unequivocal prediction that sanctions will result in positive employment outcomes, both in terms of threat and imposition effects. Developments of the basic model, however,

complicate this unambiguous view. Van den Berg and van der Klaauw (2006), for example, distinguish between formal and informal job search, in which formal routes refer to job search methods that are recognised by the social security system compared with unrecognised informal routes such as job referrals by friends, relatives or other contacts. Within this model, the threat of sanctions is potentially ineffective, since such a threat simply leads unemployed individuals to substitute formal job search for the informal job search methods that they were already doing. The overall level of job search, therefore, may not increase, implying no employment impacts whatsoever. Indeed, since the possibility exists that informal job search is in fact more effective than formal job search, monitoring and the threat of sanctions could even have a perverse effect on employment re-entry, though the relative balance of the two methods is assumed by van den Berg and van der Klaauw (2006) to ultimately be undetermined.

In addition, it is unclear what the job search model implies in relation to sanctions and labour market outcomes other than job re-entry. Arni et al. (2013), for example, argue that the theory's predictions regarding post-unemployment earnings and job stability are indeterminate. Job search theory posits that if an unemployed individual searches more intensely for a job as a result of threatened or imposed sanctions, they will then spend less time in unemployment. Such an outcome could have positive consequences, since less time spent out of work minimises the risk of skill depreciation and serves as a positive signal to employers. Consequently, it might be expected that sanctions will be associated with individuals finding jobs that are similar to the one that they had prior to unemployment, with potentially beneficial implications for earnings and job stability. On the other hand, an explicit feature of the job search model is that benefit sanctions function by influencing individuals to lower their reservation wages. This implies that the model should also expect that unemployed individuals will accept lower-quality jobs than they would otherwise do, at lower wage levels or job duration. Theoretical predictions, therefore, are inconclusive on the matter of post-unemployment effects, since it is unclear what the balance of factors is expected to be.

In addition, Arni et al. (2013) recognise that benefit sanctions may be associated with negative impacts on labour force attachment for some individuals. To explain this potential outcome within the job search framework, they posit - in an arguably ad hoc fashion - that a certain subpopulation of unemployed individuals gain only slightly more utility from being in registered unemployment than being in unregistered unemployment. For these individuals, both the threat and imposition effects of benefit sanctions reduces the utility of registered unemployment such that they would prefer to be unemployed but without registering to claim unemployment benefit. It is not clear, however, whether such individuals would eventually become economically inactive, as opposed to remaining unemployed but unregistered, and it is unclear on what basis job search theory would provide a means for making a prediction in this area. Once again, therefore, appeals to job search theory provide indeterminate predictions in the area of labour market outcomes. Informed by the present discussion, Table 4.1 summarises the labour market outcomes of benefit sanctions predicted by job search theory, distinguishing between threat and imposition effects.

**Table 4.1:** job search theory predictions of the labour market impacts of benefit sanctions

	Job re-entry	Post-unemployment earnings	Job stability	Labour force attachment
<i>Threat effect:</i>	Uncertain	Uncertain	Uncertain	Uncertain
<i>Imposition effect:</i>	Increase	Uncertain	Uncertain	Uncertain

Table 4.1 demonstrates that job search theory, despite its predominant application within the quantitative literature, has arguably limited potential in terms of informing research in this area. Indeed, out of the various potential labour market impacts that are identified, the job search framework provides only one clear prediction in relation to imposition effects and job re-entry. Based on a usefulness criterion alone, therefore, there is a clear need for a separate framework to be developed in order to inform both policymaking and research into the labour market impacts of benefit sanctions, though this potentially expansive task will not be carried out here.

Indeed, there are a number of criticisms of the application of job search theory in relation to benefit sanctions that go beyond its poor predictive insight. A central criticism relates to its reliance on rational choice theory, a highly

reductive account that might enable economic modelling but has much less relevance in terms of informing successful policy design, which needs to take into account the realities and complexities of human agency and motivation (Wright, 2012; 2016). Additional concerns, for example, highlight the evident need for claimants to have sufficient resources for successful job search and re-entry to take place, as well as whether there is sufficient job availability for sanctioned claimants to have realistic employment prospects (Taulbut et al., 2018). Clearly, the possibility exists that policymaker assumptions regarding sanctions and labour market effects might not correspond with the reality, and it is to the empirical literature that this section now turns.

#### **4.2.2 Empirical evidence**

The available quantitative research from the UK provides mixed evidence regarding labour market outcomes. The most methodologically robust study is carried out by the National Audit Office (NAO, 2016b), which analyses individual-level data on claimants who were part of the Work Programme. Chapter 3 provides more detail on the Work Programme itself, though the important feature to highlight here is that the initiative was targeted at the long-term unemployed, and so the results are not necessarily generalisable beyond this group. The NAO (2016b) study, nevertheless, was able to exploit an experimental feature in the design of the Work Programme, whereby claimants were randomly allocated to different providers who themselves made varying use of sanctions. Exploiting this randomisation through the use of instrumental variables regression, the study finds that JSA sanctions are associated with an increased probability of employment up to a year after a sanction has been imposed, though no effect is observed regarding earnings. A reduction in the number of days that individuals claim JSA is also observed, though this effect is driven by both increases in days in employment as well as slightly larger increases in days neither in employment nor claiming benefits.

The results of the NAO (2016b) study are complemented by two additional UK studies that are carried out at the aggregate-level. Loopstra et al. (2015b) analyse local authority-level data using fixed effects models, and find that increases in the application of sanctions are associated with increases in



individuals exiting JSA. Similarly, whilst this effect is partly explained by transitions into employment, a larger effect is found in terms of transitions to unknown non-work destinations, which the authors consider to be explained by individuals remaining unemployed whilst not claiming benefits. Indeed, the results indicate that increased sanctioning is associated with falls in the JSA claimant count, but no relationship is found to exist for local authority employment or unemployment rates. Taulbut et al. (2018), furthermore, analyse time-series data using multivariate structural vector auto-regression models, and find that increases in the threat and imposition of sanctions has a positive impact on flows into work in the short-term (below six months) but not in the long-term (up to 18 months). No impact is observed on unemployment rates, however, suggesting little effect on the functioning of the labour market itself. Importantly, an interrupted time-series analysis suggests that the harsher sanctions regime brought about by the Welfare Reform Act 2012 had no impact on flows from JSA into work.

There is a limited amount of evidence in the UK literature on sanctions relating to disabled people. The previously discussed NAO (2016b) study, for example, also looks at labour market impacts for long-term ESA WRAG claimants who participated in the Work Programme. Contrary to the findings for JSA claimants, which showed both positive and negative labour market effects, the findings for ESA claimants are uniquely negative. That is, up to a year after a sanction has been imposed, sanctions are found to: reduce the probability of employment; reduce earnings; increase the number of days claiming benefits and not working; increase the number of days neither in employment nor claiming benefits; and reduce the number of days both claiming and employed. Reeves (2017), furthermore, provides additional evidence on sanctions imposed on JSA claimants who self-declare as having a physical or mental health problem that seriously affects their day-to-day life, though for reasons unknown are not claiming ESA. Through analysis of local authority-level data and using fixed effects models, the findings suggest that increases in sanctions are associated with rises in the economically inactive disability rate, whilst no clear relationship is observed in terms of the employed disability rate.

In addition to the UK evidence, there is a larger literature from across other European social security systems that focuses on unemployment-related sanctions. A US literature also exists, though these studies are not discussed here given the use of full-family sanctions in the US social security system (Fording et al., 2013; Wu et al., 2014). This restriction is helpful in terms of comparability. It is important to re-iterate the point previously highlighted in Chapter 2, however, that sanctions in the mainland European studies considered in this section are less severe than in the UK, with the exception of Finland. These studies are also unable to benefit from the type of natural experiment design utilised by the NAO (2016b) study. In that investigation, randomisation is exploited to help overcome the risk that sanctioned and non-sanctioned individuals may differ in important ways that are relevant to their labour market outcomes. In contrast, the quantitative studies discussed in the remainder of this section rely on the timing-of-events model developed by Abbring and van den Berg (2003). Within this approach, both the duration until sanctioning and the duration of unemployment are modelled simultaneously. It seeks to control for unobserved characteristics that affect both the duration until sanctioning and the speed at which an individual finds a job once sanctioned, such as skill level, preferences and motivation. By controlling for such unobserved characteristics, the model aims to reduce potential bias affecting the estimated effect of sanctions on labour market outcomes.

In terms of employment re-entry, van den Berg et al. (2004) conduct one of the earliest studies into the imposition effects of sanctions that uses Dutch administrative data on means-tested unemployment benefit recipients, as opposed to unemployment insurance (UI) recipients. Within the Dutch system, sanctions for those on means-tested unemployment benefits consist of temporary reductions of 5%, 10% or 20% for a potential maximum of six months but usually of only one or two months. In this context, the study finds large employment effects. Sanctions more than double the transition rate from benefits to employment, which increases by over 140%. However, the harsher 20% sanctions are not found to be associated with stronger effects compared with the 5% sanctions. Abbring et al. (2005) conduct a similar study using administrative data on UI recipients in the Netherlands. Though the UI benefit is more generous than the means-tested benefit, UI sanctions are slightly harsher,

in that temporary partial reductions range from 5% for four weeks to 30% for 13 weeks, whilst permanent reductions are either full or partial. Interestingly, Abbring et al. (2005) find slightly smaller employment effects, which range from increases in the transition rate of between 36% and 98% depending on the sample analysed.

A study by Svarer (2011), furthermore, indicates that the unemployment exit effects of benefit sanctions can operate in response to the imposition of relatively small financial penalties. Using administrative data from Denmark, where UI sanctions last for up to 3 days or - in fewer instances - for 3 weeks, the results indicate that the exit rate increases by more than 100% in response to a sanction. In addition, the effects of sanctions are shown to differ depending on claimant characteristics. Danish citizens and single unemployed people in the sample, for example, appear to respond more strongly to sanctions than non-citizens or married individuals. Perhaps most importantly, however, is the finding that the effect of sanctions decreases over time and does not persist in the long-term, since the imposition effects of sanctions are no longer statistically significant after three months. As Svarer (2011) highlights, a possible explanation of this finding is that those individuals who are already closest to the labour market are the ones who find employment following a sanction, whilst those who find it harder to find employment remain unemployed. Whilst a plausible interpretation, the data used by Svarer (2011) does not distinguish between unemployment exits into employment or outside the labour force itself.

Busk (2016), in contrast, is able to compare both means-tested and UI benefit sanctions as well as investigate a wide range of specific labour market effects, considering impacts on the exit rate from unemployment to work, involvement in ALMPs and to outside the labour force. Administrative data from Finland is analysed, where complete benefit loss is imposed for between 30 and 150 days depending on a variety of circumstances. Importantly, the findings indicate that the effect of sanctions differs according to the type of benefit claimed. In particular, ongoing sanctions increased the transition rate to work by 84% for the means-tested recipients and 25% for UI recipients, whilst completed sanctions increased the rate by 34% for the former group but had no effect on the latter. Sanctions also led to an 11% increase in the probability of participating in an

ALMP for means-tested recipients, but had no effect on UI recipients. For both benefit types, sanctions increased the rate of transition out of the labour force, but the effect was particularly strong for UI recipients, who were three times more likely to exit the labour force (82%) than to return to work (25%).

In addition to imposition effects, Lalive et al. (2005) are able to consider threat effects. Using Swiss data, they can observe the date when a sanction warning is announced to an individual and the date when the maximum 60 day sanction is actually enforced. The results indicate that the exit rate from unemployment increases by 25% following a warning and by an additional 20% if a sanction is subsequently imposed. The key limitation of this study, however, is that it does not distinguish between the types of exit from UI benefits, focusing solely on unemployment duration. A study by Arni et al. (2013), in contrast, is able to overcome this limitation, considering threat and imposition effects on employment re-entry, employment stability and earnings for a period of two years following unemployment exit. Using administrative data from Switzerland, the results suggest that threat and imposition effects are associated with increases in the rate of job re-entry in the short-term. Relatively larger impacts, however, are observed on the exit rate to non-employment. In the longer-term, furthermore, it appears that the pressure to accept job offers more quickly leads to reduced employment stability and lower earnings. These findings are supported in a study by van den Berg and Vikström (2014), which finds negative impacts on wages, occupation-level and on hours worked up to four years following unemployment exit.

The literature on the labour market impacts of sanctions is extensive, though limited in relation to threat effects, and the discussion in this section does not aim to describe all relevant studies. From the literature discussed, however, some clear conclusions emerge in relation to sanctions and unemployment benefits that are consistent with several existing reviews and summaries (Griggs and Evans, 2010; McVicar, 2014; NAO, 2016a; Geiger, 2017). First, sanction warnings and actual impositions are associated with increased employment re-entry in the short-term. With regard to imposition effects, this finding appears to hold across means-tested and UI schemes, though the available evidence suggests that harsher sanctions do not lead to greater employment effects.

Second, sanctions are also associated with increased exits out of the labour force, an outcome that is often larger than the employment effect itself. Third, evidence on the longer-term impacts of sanctions suggests that they have negative impacts on job quality, in terms of wages, stability and hours. This finding is arguably an unsurprising one, given the wider labour market context within which sanctions policies have proliferated, as discussed in Chapter 2. These findings are summarised in Table 4.2, which contrasts with the theoretical summary detailed in Table 4.1.

**Table 4.2:** summary of the evidence of the labour market impacts of benefit sanctions

	Job re-entry	Post-unemployment earnings	Job stability	Labour force attachment
<i>Threat effect:</i>	Increase	Decrease	Nil	Decrease
<i>Imposition effect:</i>	Increase	Decrease	Decrease	Decrease

As McVicar (2014) highlights, nevertheless, a number of gaps in the evidence base remain. Importantly, for example, there is relatively little evidence on whether labour market impacts differ across separate groups of unemployed individuals. It could be important to compare, for example, sanction outcomes for individuals with differing prospects in the labour market. The limited UK evidence discussed in this section suggests that this is likely to be important, given the uniquely negative impacts observed for ESA claimants and JSA claimants with physical or mental health problems. More broadly, there is a clear need for an expanded UK evidence base in light of the fact that international evidence is not necessarily applicable to the UK, given differences in the generosity of benefits, the size and length of sanctions, interactions with other social security benefits as well as differing labour market and economic contexts. The available international and UK evidence, nevertheless, clearly highlights the potential negative labour market outcomes associated with sanctions, therefore increasing the likelihood that the wider expected benefits of sanctions will themselves not be realised. Evidence on the wider impacts of sanctions is the focus of the next section.

## 4.3 Wider impacts

### 4.3.1 Empirical evidence

There is a limited but growing literature that investigates the wider impacts of benefit sanctions. Primarily UK and US focused, this literature observes consistently negative outcomes that will be discussed in turn here, including: financial hardship and debt; food bank usage; survival crime; third-party impacts on children; and adverse mental health impacts. It is important to highlight in advance, however, that the studies in this section make less use of the research designs exploited in Section 4.2. There, natural experiments and timing-of-events methods were used to underpin more robust causal inferences. The reliance on less sophisticated research designs in the wider literature on sanctions is a weakness with regard to the estimation of causal impacts.

Another important point to note relates to the scope of the evidence discussed in this section. Various third-sector organisations in the UK, for example, provide evidence on a range of negative impacts on claimants using surveys and in-depth interviews (see, for example: CAB, 2013; Homeless Link, 2013; Hale, 2014; Stephenson, 2014; Batty et al., 2015; Beatty et al., 2015; Fitzpatrick et al., 2016; Loopstra and Lalor, 2017; Rabindrakumar and Dewar, 2018). The discussion here, however, will focus primarily on the policy and academic literature. Indeed, given the particular focus on JSA sanctions in the empirical chapters of this thesis, evidence relating to this group is foregrounded. Due to the limited size of the literature, nevertheless, some reliance is placed on US research where full-family sanctions are imposed on claimants with dependent children. Indeed, in the discussion on the mental health impacts of sanctions, wider consideration is given to groups other than JSA claimants, as well as to the impact of conditionality more widely.

Since the introduction of JSA in 1996, several investigations carried out for separate UK government departments have considered the impacts of sanctions on claimants, and consistently find negative impacts in terms of financial hardship (Vincent, 1998; Saunders et al., 2001; Peters and Joyce, 2006; Dorsett et al., 2011). As highlighted in Section 4.2, the financial implications of benefit sanctions form a core part of their policy rationale, whereby the withdrawal of

benefit income is assumed to induce particular behavioural responses. Evidence in terms of financial hardship is nevertheless discussed in this section on wider impacts. This is carried out due to the fact that the overriding policy belief is that benefit sanctions will straightforwardly be accompanied by beneficial labour market outcomes for claimants, and should therefore not entail significant financial hardship beyond the initial withdrawal of benefit income. As will become clear, furthermore, financial hardship has an important role as a determinant of the additional wider impacts that will be discussed.

In the first of the UK JSA sanctions reports carried out, Vincent (1998: 28) interviews 30 JSA claimants and finds that sanctions “impose real hardship” on individuals, who are forced into a variety of financial coping strategies in response to their reduced income, which they must carry out in order to avoid falling into debt or adding to existing debts. Similarly, Saunders et al. (2001) interview 50 sanctioned JSA claimants and detail the significant financial impacts that are incurred, including debts of up to £800. As the authors note, the financial impacts depended on a number of factors, such as whether claimants found work, received information regarding hardship payments, lived with their parents or had a partner and/or children. In the largest of these studies, Peters and Joyce (2006) present findings from over 3,000 survey respondents and 70 in-depth interviews with individuals claiming JSA. Over two-thirds (68%) of the survey interviewees who had been sanctioned reported that they had experienced financial hardship, whilst many were forced to borrow money from friends and family as a coping mechanism. Sanctioned claimants had difficulty paying utility bills, rent and managing debt. Indeed, many had already been struggling to get by financially on JSA itself, a situation that was severely aggravated following the imposition of a sanction.

Further evidence on the potentially severe financial implications of benefit sanctions is provided by a series of US studies investigating the Temporary Aid to Needy Families (TANF) programme, which provides temporary financial assistance to low-income families with one or more dependent children (Cook et al., 2002; Kalil et al., 2002; Pavetti et al., 2003; Lee et al., 2004; Reichman et al., 2005). Kalil et al. (2002), for example, use panel data on 562 individuals and find that sanctions are associated with claimants having their gas or electricity

shut off because they could not afford to pay their bills, despite efforts to engage in a variety of hardship-mediating activities. Lee et al. (2004), furthermore, use longitudinal data on 1,123 individuals and find that those who are sanctioned are three times more likely to experience food hardship - reporting that they sometimes, or often, do not have enough to eat - than those who are not sanctioned. Given that recipients of TANF have dependent children, it is not surprising that such food insecurity is found to extend to children themselves (Reichman et al., 2005).

The US evidence regarding TANF sanctions is supported by more recent UK evidence, which identifies sanctions as a key factor driving demand for food banks in both qualitative and quantitative research (Lambie-Mumford, 2014; Loopstra et al., 2015a; Garratt et al., 2016; Loopstra et al., 2018). Loopstra et al. (2018), for example, investigate the relationship between JSA sanctions and food bank usage by linking sanctioning rates in local authorities to area-level food bank usage data from the Trussell Trust Network, the largest food bank network in the UK. Using fixed effects models, the authors find that sanctions are associated with increases in the rate of food bank usage. In particular, every additional 10 sanctions per 100,000 adults within local authorities are associated with an additional 3.36 adults fed by food banks. The results of the study are strengthened by the fact that increases in sanctions in the previous quarter are associated with subsequent increases in food bank usage, whilst decreases in the previous quarter are associated with subsequent decreases in food bank usage. The availability of food distribution sites, furthermore, is shown to affect the relationship between sanctions and food bank usage. In areas with few distribution sites, rising sanctions lead to smaller increases in food bank usage, as would be expected.

It has also been argued that benefit sanctions have the potential to push some claimants towards survival crime (Meacher, 1974; Eardley et al., 2005). Indeed, this is recognised even in the UK policy literature, where Vincent (1998: 30) notes that several sanctioned JSA claimants in their study had been “driven to an extremity in which crime might be their last resort”. Machin and Marie (2006) investigate the relationship between JSA sanctions and both property and violent crime, using aggregate-level data from police force areas in England and Wales.



First, the authors adopt a difference-in-differences (DiD) model to consider the impact of the introduction of JSA in October 1996, which introduced benefit sanctions as discussed in Chapter 3. Importantly, the results indicate that crime rates in areas more affected by the policy change rose by more than areas that were less affected. Second, fixed effects models are used to investigate the post-JSA introduction period, which indicate that sanctions are associated with increases in the rate of crime. The effect is strongest in the quarters following the introduction of JSA, before becoming statistically insignificant towards the end of the period of study. These quantitative findings are additionally supported by more recent qualitative evidence based on the harsher sanctions penalties introduced in 2012 (Batty and Fletcher, 2018; Johnsen et al., 2018).

As previously indicated, sanctions have the potential to impact third parties such as the friends and family of individuals who receive them, and of particular concern are impacts on the children of claimants. US quantitative research considering the effects on the children of TANF claimants finds consistently negative impacts, though it is important to highlight that these studies suffer from weak designs in terms of substantiating causal inferences. Larson et al. (2011), for example, investigate the impact on educational outcomes and find that children from sanctioned families have significantly more school disruptions and lower attendance than children from non-sanctioned families. Lohman et al. (2004), furthermore, examine whether sanctions affect the well-being of children from single mother families, and find that they are associated with lower cognitive achievement scores for pre-school age children when their mothers remain on benefits, and severe behavioural problems when their mothers stop claiming entirely. Paxson and Waldfogel (2003), lastly, investigate the relationship between sanctions and child maltreatment using aggregate-level data across states in the US. The results indicate that sanctions in general and the use of sanctions of longer duration are associated with increases in maltreatment, though these effects are not consistent across the different measures and are not always robust to small changes in the sample.

An issue that is attracting growing attention relates to the mental health impacts for claimants of both sanctions and behavioural conditionality more broadly, both in terms of academic research as well as in wider debates. During

the ‘great sanctions drive’ period described in Chapter 3, for example, the UK media frequently covered claims about the impact of sanctions in relation to anxiety, depression and suicide (Cowburn, 2015; Stone, 2015; Mills, 2018). Many submissions from claimants, academics and third sector organisations to the House of Commons Work and Pensions Committee (2015; 2018) reports into benefit sanctions and the House of Commons Committee of Public Accounts (2017) report, furthermore, highlighted the adverse mental health impacts of sanctions, for those both with and without pre-existing mental health conditions. Groups such as the British Psychological Society (BPS, 2017) have called on the government to suspend the use of sanctions and commission an independent review of their mental health impacts. At the time, the DWP resisted such calls, and appeared to take a much different view in terms of mental health impacts. Neil Couling, in the capacity of the DWP’s Work Services Director, for example, gave evidence to the Scottish Parliament Welfare Reform Committee (2014: 11) and argued that “many benefit recipients welcome the jolt that a sanction can give them”.

Interestingly, however, UK departmental reports themselves identify adverse emotional and psychological impacts resulting from benefit sanctions. Vincent (1998: 30), for example, notes that some claimants reported being “made to feel like a criminal”, and felt powerless and impotent in the face of benefit sanctions. Typical reactions to sanctions included claimants feeling: angry and frustrated; stressed and panicked; treated without respect and degraded; and bitter because they had told the truth and been penalised for it. Saunders et al. (2001: 40), furthermore, report that some sanctioned claimants felt that the “impact on their mental health had been quite serious”, leading them to claim Income Support (IS) because of depression or relying on prescription drugs to cope. In addition, respondents reported adverse effects on relationships with partners, friends and family resulting from benefit sanctions that led to further stress, an outcome also observed by Dorsett et al. (2011). Peters and Joyce (2006), lastly, identify emotional and psychological impacts affecting nearly a tenth (9%) of sanctioned individuals in the study, which include: stress; anxiety; anger and humiliation; and depression. The authors speculate that emotional impacts such as stress, anxiety and depression were more pronounced for those who already experienced related mental health problems.

In the academic literature, Stewart and Wright (2018) conduct longitudinal qualitative interviews with 43 JSA claimants, over half of whom had been sanctioned at least once. Whilst mental health impacts are not the unique focus of their study, the authors nonetheless find that sanctions are commonly associated with effects such as anger, stress, anxiety and depression, caused both by the fear of and the actual imposition of benefit sanctions. These findings reinforce the observations made at the first stage of their research, in which 64 JSA claimants were interviewed for the first time, again over half of which had been sanctioned (Wright and Stewart, 2016). At this stage, similarly, sanctions were associated with “severe and acute negative emotional effects” (Wright and Stewart, 2016: 4), including: anger; powerlessness, due to not being listened to; the perception of being punished unfairly; low mood; and anxiety or depression. The authors also argue that negative emotional impacts are most likely to occur in cases where individuals feel that their sanction is “unjustified, unfair or disproportionate” (Wright and Stewart, 2016: 5) to the purported infringement. Additional qualitative research identifies similar negative psychological and emotional impacts of sanctions that are imposed on groups such as lone parents, disabled people and homeless people (Dwyer, 2018; Dwyer et al., 2018; Johnsen and Blenkinsopp, 2018; Johnsen et al., 2018; Dwyer et al., 2020).

Quantitative research in this area has focused more broadly on the impact of work-related behavioural conditionality on various groups, as opposed to the impact of sanctions specifically. Katikireddi et al. (2018), for example, investigate the impact of job search requirements on lone parents in the UK using a natural experiment design, by exploiting the step-wise reductions over time in the lower age limit at which Lone Parent Obligations (LPOs) apply. The findings suggest that those newly exposed to LPOs saw their mental health worsen compared to two control groups, those not exposed and those already exposed to LPOs. Davis (2019) provides supporting evidence from the US, using fixed effects models to show that mental health is worse for low-educated single mothers in states with stricter behavioural conditionality, measured in terms of sanctions, job search requirements and expenditure on welfare-to-work policies. Barr et al. (2016), finally, investigate the impact of Work Capability Assessments (WCAs) for ill and disabled people in the UK, a fit-for-work test that determines eligibility for ESA. Using fixed effects models and aggregated local authority-

level data, the study finds that higher rates of WCAs are associated with higher rates of self-reported mental health problems, antidepressant prescribing and suicides.

### **4.3.2 Overarching research question**

As Chapter 5 will go on to detail, the empirical research in this thesis will contribute to the existing literature on the impacts of benefit sanctions through a quantitative investigation of the relationship between JSA sanctions and mental health outcomes, with a particular focus on anxiety and depression. Informed by the evidence discussed throughout this chapter, the overarching research question that motivates the empirical investigation carried out in this thesis is as follows:

[RQ]: Are benefit sanctions associated with adverse mental health impacts?

Whilst this chapter has identified the need for more systematic evidence on the relationship between sanctions and mental health outcomes, the research discussed throughout this chapter, both in terms of labour market and wider impacts, arguably leads to the expectation that sanctions will be associated with adverse mental health impacts. Indeed, material deprivation, debt, economic precariousness and food insecurity, for example, have all been consistently linked with detrimental effects on mental health (Fitch et al., 2011; O'Campo et al., 2015; McKee et al., 2017; Renahy et al., 2018; Loopstra et al., 2019). As this section has demonstrated, furthermore, existing qualitative research provides evidence that sanctions are associated with adverse mental health impacts, which this thesis seeks to contribute to by adding a more specifically focused investigation than is provided by the limited quantitative research in this area. Before investigating this issue empirically, however, it is first useful to consider the precise mechanisms that might explain the link between sanctions and mental health. This is an important exercise that will provide the empirical investigation with a stronger theoretical grounding, and is the focus of the next and final section of this chapter.

## 4.4 Sanctions, mental health and causal mechanisms

### 4.4.1 Mental health

Different perspectives exist in terms of conceptualising mental health, which remains a highly contested topic subject to terminological as well as ontological and epistemological disagreement (Rogers and Pilgrim, 2010). Arguably, two main approaches to understanding mental health can be distinguished within the sociological literature, which considers both the social determinants of mental and emotional distress as well as the ways in which such terms are socially constructed (Warner, 2009). Social causation perspectives have been criticised for uncritically accepting medical diagnoses such as ‘depression’ as facts representing an objective reality, using prevalence measures to investigate the relationship between social disadvantage and mental distress through consideration of factors such as social class, ethnicity, gender and age. Social constructivist perspectives, in contrast, have problematised the basis of various medical diagnoses by highlighting how reality emerges as a product of human activity, challenging naïve forms of realism by emphasising the relationships of power that are involved in the creation of medical categories. Consequently, social constructivist perspectives have been influential in foregrounding and critiquing the role that medical professionals and the legal system have historically played in categorising mental ‘disorder’ in ways that ultimately serve to enforce “how people ought to think, feel and act as part of an ideal moral order” (Rogers and Pilgrim, 2010: 272).

Indeed, the inherently value-laden nature of medical categorisation is evident in attempts to define mental health itself, where even broad definitions inevitably situate mental health in relation to ‘normal’ functioning within the expectations of a particular social context. To give one prominent example, the World Health Organisation (WHO) defines mental health as:

“a state of well-being in which the individual realizes his or her own abilities, can cope with the normal stresses of life, can work productively and fruitfully, and is able to make a contribution to his or her community”. (WHO, 2014: 12)

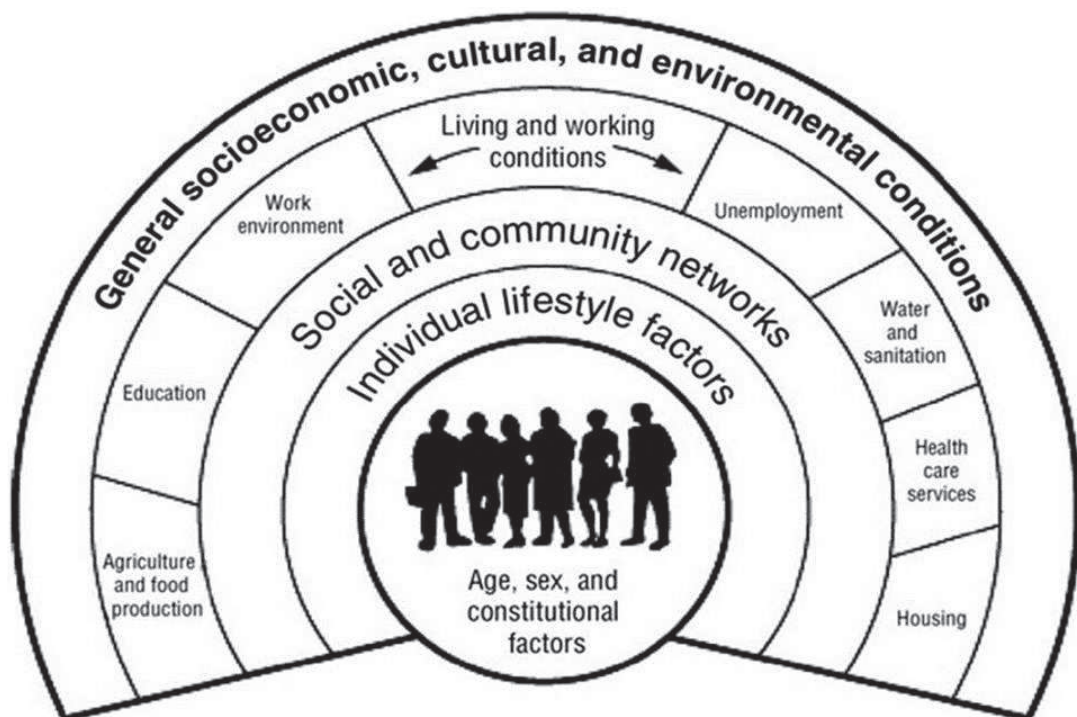
Social constructivist accounts of mental health, however, have themselves been critiqued. Primarily, this criticism relates to the view that strong variants of constructivism ultimately serve to deny the ‘reality’ of the pain experienced by those struggling with mental and emotional distress, given the apparent commitment to the claim that such distress is “*only* a category and does not refer to any objective reality” whatsoever (Busfield, 2000: 547). Whilst debate between competing approaches to understanding mental health is ongoing, less polarised perspectives have developed to support the view that operationalising definitions of mental health in empirical research does not necessarily need to be accompanied by an uncritical approach to the concepts investigated or the conclusions reached. Indeed, more theoretically grounded empirical work has itself been crucial in identifying the social - over and above the biological - as a determinant of mental health, and thus in advancing possible social responses to mental health problems (Wilkinson and Marmot, 1999; 2003).

A further distinction that can be made is between the positive and negative dimensions of mental health. In the first, mental health is understood as a “state of psychological wellbeing”, whilst in the second it is viewed in terms of “mental health problems” (Pilgrim, 2017: 3). Warner (2009) highlights that a range of different terms are used to discuss mental health in the negative sense, including: mental illness; mental disorder; and mental and emotional distress. Importantly, particular applications in empirical research have tended to imply adherence to distinct perspectives on the nature of mental health problems. Usage of the term ‘mental illness’, for example, has often been accompanied by a lack of recognition of the contested nature of medical categories and an uncritical “commitment to the idea that distress can be identified, diagnosed, and treated through medical intervention” (Warner, 2009: 361). Broader terms such as ‘mental health problems’ and ‘mental and emotional distress’ provide alternatives that will be used throughout this thesis. Chapter 5 provides more discussion of anxiety and depression specifically, which form the basis of the outcomes that will be investigated in the empirical chapters of this thesis. In the remainder of this section, however, the focus remains on understanding the potential relationship between sanctions and mental health problems more broadly.

#### 4.4.2 Unemployment: material and psychosocial mechanisms

In order to understand the relationship between sanctions and mental health, it is important to first recognise that unemployment itself - independently of sanctions - is associated with negative mental health outcomes. A widely used framework for understanding the key social determinants of both physical and mental health is the Dahlgren-Whitehead (1991) rainbow model (Bambra et al., 2010), which maps the relationship between individuals and the various influences on their health. This is displayed in Figure 4.1.

**Figure 4.1:** the rainbow model of the social determinants of health



Source: Dahlgren and Whitehead (1991)  
Used with permission of the Institute for Futures Studies

Clearly, the discussion here is most concerned with the ‘Unemployment’ section of the Dahlgren-Whitehead (1991) rainbow model, which forms part of the ‘Living and working conditions’ segment. Before discussing this issue, however, it is important to emphasise that a focus on the mental health consequences of unemployment and sanctions does not necessarily imply that work itself is straightforwardly associated with positive mental health outcomes. The DWP, in contrast, take a singular view regarding the health benefits of employment, arguing that:

“work, and the improved incomes that flow from it, have beneficial effects in terms of people’s health and well-being ... It is difficult to quantify these effects precisely but their existence is not in doubt.”  
(DWP, 2010: 5)

As the ‘Work environment’ section of Figure 4.1 implies, however, it is not work itself but rather the quality of work that is likely to be important for an individual’s mental health. Indeed, this is an intuition that is supported by various systematic reviews (Allen et al., 2014; WHO, 2014; Silva et al., 2016). Recent evidence, furthermore, highlights the potential that poor quality employment might be as worse, or potentially even worse, for mental health than unemployment itself (Kim and von dem Knesebeck, 2015; Kim and von dem Knesebeck, 2016; Chandola and Zhang, 2018).

In terms of unemployment, an extensive literature demonstrates that there is a negative association between unemployment and mental health outcomes (for systematic reviews, see McKee-Ryan et al., 2005; and Paul and Moser, 2009). Paul and Moser (2009), for example, note that the association is observed for a number of different dimensions, including: symptoms of anxiety and depression; distress and hopelessness; self-esteem; feelings of control; and subjective well-being. Although the exact nature of the causal influences at play is still a debated issue, the literature broadly agrees that the mental health effects of unemployment are at least partly causal. That is, unemployment exerts an independent causal effect on people’s mental health, over and above the fact that some unemployed individuals are also more likely to be affected by mental health problems in the first place (Fryer, 2014).

In order to explain this causal relationship, furthermore, researchers have distinguished between two separate pathways through which unemployment can impact on mental health, referred to as the material and psychosocial routes (Sage, 2013). As its name suggests, the material pathway is concerned with the income and poverty effect of unemployment, highlighting the evident financial implications that unemployment involves and the various forms of suffering that follow from a lack of material resources. According to one definition, for example, the material explanation is based on the observation that “money buys



health-promoting goods and the ability to engage in a social life in ways that enable people to be healthy” (Benzeval et al., 2014: 1). Financial resources, that is, provide access to key necessities that underpin good mental health, such as adequate housing, warmth, food, exercise and recreational activities. The psychosocial pathway, in contrast, is concerned that there is something damaging about the experience of unemployment itself, irrespective of directly material factors. Stated broadly, psychosocial mechanisms are concerned with attempting to understand the “way in which people’s social environment makes them feel” (Benzeval et al., 2014: 4).

Psychosocial mechanisms are thought to operate as a result of two distinct sub-pathways. First, being unemployed and living on a low income is highly stressful, which relates to a growing body of research that investigates how financial stress gets “under the skin” (Sturgeon et al., 2016: 134) of individuals to impact their mental health. Clearly, this sub-pathway is closely associated with material concerns, and indeed material factors have been understood as central determinants with psychosocial factors providing an explanatory pathway that connects financial circumstances to health outcomes (Smith and Anderson, 2018). The second psychosocial sub-pathway, in contrast, argues that there is something qualitatively specific about the experience of unemployment that leads to poor health outcomes (Sage, 2013). Arguably, stress is likely to play an important role in the second sub-pathway as well, though the primary concern of theories in this route is to identify the psychosocial needs that are inadequately met through the experience of unemployment. Sage (2018), for example, categorises these in terms of loss of the functions of paid work, loss of agency and loss of social status, the latter of which includes stigma.

In order to develop this categorisation, Sage (2018) identifies three prominent attempts to explain the health impacts of unemployment in terms of psychosocial needs. First, functionalist approaches focus on the social institution of employment and consider the implications for health when individuals are excluded from it. Jahoda (1982), for example, advances the concept of the latent functions of employment, in which paid work is viewed as an institution that meets individuals’ basic psychological needs through the provision of a variety of social goods, including: time structure; social activity; collective

endeavour; regular activity; and status and identity. Unemployment, in this view, results in harmful psychological effects by depriving people of these beneficial experiences. Fryer (1986) criticises the functionalist approach on the grounds that it ignores the meanings that individuals ascribe to their own experiences of unemployment, ultimately treating them as passive actors in the face of their social circumstances. In contrast, Fryer's (1986: 23) agency model foregrounds individuals as intrinsically motivated agents who experience unemployment in terms of their own "beliefs, intentions, and goals for self-actualisation". In this view, health impacts should be understood in the context of agency restriction, whereby unemployment places constraints on people's ability to plan and enact their personal agency and autonomy.

Ezzy (1993) argues that the functionalist and agency theories both suffer from too narrow a focus, overemphasising social institutions or individual agency at the expense of the other. To overcome this limitation, Ezzy (1993) outlines an account in which unemployment is conceived of as a status transition from the valued social position of employment to a less valued social position. Within this account, the impact of unemployment on an individual's mental health ultimately depends on "the interaction between a person's objective social environment and their subjective interpretation of this environment and their place within it" (Ezzy, 1993: 48). Whilst these three theories are often portrayed as being in competition with one another, Sage (2018) seeks to integrate their varying perspectives into an overarching account of the psychosocial impacts of unemployment. To do so, Sage (2018: 1048) conceptualises unemployment as an "overarching process of loss", drawing on qualitative research in which participants report experiencing unemployment in terms of loss of "income, control, autonomy, status, respect, dignity, structure and skills". As indicated above, Sage (2018) develops this account in order to recast the viewpoints of Jahoda (1982), Fryer (1986) and Ezzy (1993) in terms of loss of the functions of paid work, loss of agency and loss of social status.

### 4.4.3 Sanctions: material and psychosocial mechanisms

Importantly, recognition of the separate material and psychosocial mechanisms that explain how unemployment affects health highlights the routes through which the social security system itself might play a mediating role in influencing the experiences of benefit claimants. Indeed, in addition to the literature discussed in Section 4.3 in relation to conditionality, previous research has considered the influence of benefit generosity on claimants' health as well as the role of ALMPs in shaping claimants' experiences of unemployment (Coutts et al., 2014; O'Campo et al., 2015; Sage, 2015a; 2015b; Carter and Whitworth, 2017; Renahy et al., 2018). Arguably, the mental health impacts of sanctions themselves can be expected to operate through material and psychosocial mechanisms, in terms of both threat and imposition effects, which is the focus of the remainder of this section. Their elucidation here - similar to their application in relation to unemployment - helps support the claim that the mental health impacts of sanctions are likely to be at least partly causal. That is, sanctions can be expected to exert an independent causal effect on people's mental health, over and above the fact that individuals who are sanctioned might be more likely to be affected by mental health problems in the first place.

First, the material route will clearly be influential following a sanction, which holds serious financial implications for individuals both in terms of JSA withdrawal and on additional knock-on effects such as managing debt. The adequacy level of JSA is already low, having declined slightly in real terms since its introduction in 1996 and much more significantly as a proportion of average earnings (Rutherford, 2013). Real terms falls have occurred from 2013 onwards, furthermore, as a result of below inflation uprating of 1% a year followed by a freeze from 2016 onwards (McInnes, 2019). Currently, this means that a four-week sanction amounts to the loss of over £230 for an individual aged 18-24 and over £290 for somebody aged 25 and over, figures which are likely to constitute a large proportion of a claimant's monthly income. Indeed, the financial implications of benefit sanctions have the potential to be compounded by a recurring issue relating to the wrongful cancellation of Housing Benefit and Council Tax Reduction. Following a sanction, the Jobcentre contacts the claimants' Local Authority, who might stop these benefits until the individual

confirms their new income. The Oakley Review (2014), for example, received evidence of many instances in which claimants had their claims to these additional benefits wrongly ended following a sanction, and recommended that the DWP take steps to resolve the issue. The House of Commons Public Accounts Committee (2017), unfortunately, again received evidence of errors occurring in this regard, leading them to recommend that the DWP take further steps to fully resolve the issue.

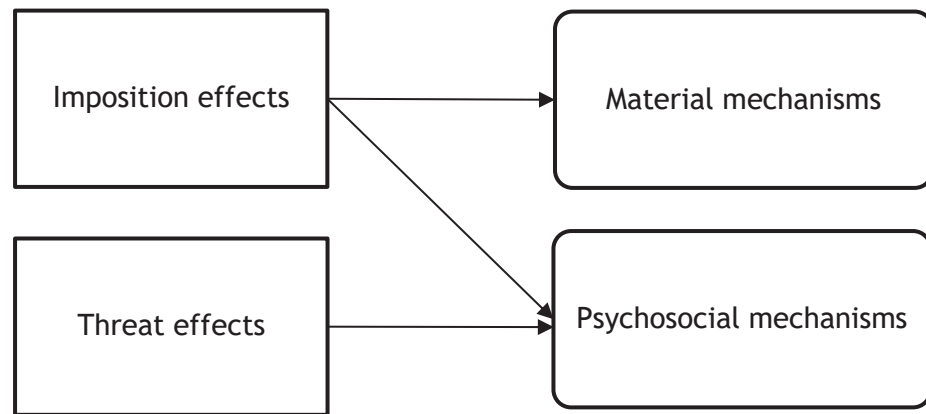
The influence of the material pathway will depend on a number of potentially moderating factors, such as whether an individual can rely on financial support from friends or family, or the extent to which they are feasibly able to make financial adjustments to their living expenses. Hardship payments, furthermore, offer an institutional route through which claimants can attenuate the financial implication of a sanction, though claimants themselves have to prove that they are at risk of financial hardship and unable to pay for essential items (DWP, 2015b). For those who successfully apply, hardship payments are paid at a rate of 60% of JSA for most claimants and 80% for claimants who are pregnant or seriously ill. For claimants who are deemed 'vulnerable', such as those who are pregnant, responsible for dependent children or who suffer from a chronic health condition or disability, hardship payments are payable immediately though they are not automatic. For claimants not in the 'vulnerable' group, however, hardship payments only begin in the third week of the sanction period. As Webster (2014) has highlighted, the official DWP Decision Makers' Guide (2009: para. 35099) itself recognises that this two-week wait can be expected to cause a "healthy adult to suffer some deterioration in their health". In terms of the proportion of sanctioned claimants who receive hardship payments, fewer than 10% received them before the Welfare Reform Act 2012, a figure which rose to over 40% by the end of the Coalition government (Webster, 2015).

Whilst the material route can be expected to operate following a sanction, the psychosocial route will be relevant in terms of both the threat and imposition of benefit sanctions. Informed by the previous discussion, this can be explained through two psychosocial sub-pathways, in terms of stress - both financial and otherwise - as well as in terms of loss of agency and social status. The third of Sage's (2018) categorisation, loss of the functions of paid work, is not directly

relevant here given that sanctioned claimants are already unemployed. Similar to the material pathway, the financial stress resulting from sanctions is self-evident, and indeed research previously discussed in Section 4.3 confirmed this in terms of both threat and imposition effects (Wright and Stewart, 2016; Stewart and Wright, 2018). The same research, furthermore, highlights the impact on claimants' sense of agency and social status resulting from sanctions. Many sanctioned JSA claimants disagreed with the reasons behind their sanction, viewing the circumstances as unfair and reporting feelings of powerlessness and stigmatisation, and it is in these circumstances that the authors considered the psychological impact of sanctions to be most severe. Redman (2020: 90) observes similar effects, and additionally finds that sanctions serve to convey "pejorative inferences" that lead claimants to internalise stigmatisation and shame due to perceived "undeservingness".

Drawing on the above discussion, Figure 4.2 depicts the separate routes through which benefit sanctions can be thought to impact the mental health of individuals in terms of both threat and imposition effects. As previously alluded to in this section, the material and psychosocial pathways can be expected to interact in important ways and so their clear separation in Figure 4.2 is made for clarity of depiction only. As previously highlighted in relation to material mechanisms, moderating factors such as support networks and hardship payments will influence the eventual financial implications of a sanction and thus the overall impact on mental health. The same is true in terms of psychosocial mechanisms, the relevance of which may depend on a variety of individual circumstances or characteristics. The possibility exists, of course, that claimants will not respond to sanctions in terms of the "abjection" identified by Redman (2020: 88), but rather with the forms of "everyday resistance" (Shaw et al., 2008: 83) that have been observed in response to sanctions and welfare reform more broadly (Patrick, 2016; Peterie et al., 2019). Without wishing to discount diverse responses to sanctions, it is nevertheless clear from the present discussion that sanctions will play a role in terms of agency restriction and social status for many, whilst even those who are more resistant to such effects are likely to have their mental health impacted through financial stress.

**Figure 4.2:** mechanisms underpinning the mental health impacts of sanctions



## 4.5 Conclusion

This chapter has discussed the literature that investigates the impacts of benefit sanctions on claimants, and has distinguished between the broad categories of labour market and wider effects. The dominant view of policymakers is that the threat and imposition of sanctions will lead to improved labour market outcomes for claimants, primarily in terms of faster re-entry into employment. The available evidence from UK and mainland European research, however, complicates this straightforward assumption. With regard to employment impacts, the threat and imposition of sanctions do appear to be associated with increased transitions into employment in the short-term, an outcome that is achievable with relatively small financial penalties. This short-term employment effect, however, is accompanied by increased transitions out of the labour force altogether, an effect that is counterproductive from the point of view of sanctions policy. The available evidence on long-term impacts, furthermore, suggests that sanctions are associated with decreases in wages, job stability and working hours. Viewed as a whole, the available literature does not support a particularly benign interpretation of sanctions in terms of labour market outcomes. Indeed, it increases the likelihood that sanctions policy will lead to a range of wider negative impacts.

The literature into the wider impacts of benefit sanctions is less well-developed, and is often limited by research designs that are not able to take into account the influence of confounding factors. Nevertheless, empirical research

consistently observes adverse wider impacts, in terms of financial hardship, food bank usage, survival crime and third-party impacts on children. Although there is a need for additional research into such impacts that is better able to estimate causal effects, it is of evident concern that such adverse consequences are so consistently linked with sanctions policy. A growing area of concern relates to the mental health impacts of the threat and imposition of benefit sanctions. These are observed in existing qualitative research in relation to emotional and psychological problems such as stress, anxiety and depression. The discussion highlights the need for a larger-scale quantitative investigation into the relationship between sanctions and mental health outcomes, which is the focus of the remaining chapters in this thesis.

To support this investigation, the final section of this chapter has outlined the routes through which sanctions can be expected to impact mental health, in terms of both material and psychosocial mechanisms. This provides the empirical research with a stronger theoretical grounding in relation to considering causal inferences. This will be further discussed in the next chapter, which details the methodology pursued in the empirical investigation.

## **Chapter 5. Methodology**

### **5.1 Introduction**

This chapter details the methodological approach that is adopted to investigate the overarching research question of this thesis, as previously identified in Chapter 4. The methodological approach that will be outlined underpins the empirical analyses carried out in Chapters 6 to 9, which focus on the relationship between benefit sanctions and mental health outcomes such as antidepressant prescribing, anxiety and depression. First, this chapter begins by briefly distinguishing between qualitative and quantitative approaches within social research, highlighting the latter perspective as the method that will be relied upon throughout this thesis. Due to the investigation's particular concern with the impacts of sanctions, the discussion then explores how causal inferences have been understood within quantitative research. Next, it broadly outlines the various methods that are ultimately used to help improve the basis for causal inferences in the empirical investigation itself. The chapter then goes on to detail how - in light of the available data - both mental health outcomes and benefit sanctions are measured in the different empirical chapters, which are the two key variables that influence the particular research designs and methods that are adopted. Finally, the chapter provides a brief overview of the data and methods that are used in each empirical chapter. Given the various differences between Chapters 6 to 9, more specific detail in terms of data and methods is provided in the individual chapters themselves. This aids the interpretation of the findings in each analysis and helps to highlight the specific contribution of each empirical chapter.

### **5.2 Quantitative research**

The empirical investigation in this thesis applies quantitative methods to understand the relationship between benefit sanctions and mental health outcomes. A familiar distinction that is made when discussing research methods is one that contrasts quantitative and qualitative approaches. In this regard, Maxwell (2019: 132) argues that attempts to differentiate between the two via appeals to “‘essentialist’ criteria such as numbers versus words, objectivity versus subjectivity, and postpositivism versus constructivism” are ultimately



misguided. Pointing to the diversity of paradigms and methods that are used within quantitative and qualitative research respectively, Maxwell (2019: 133) argues that a key difference between the two approaches resides in the “mental model” - what can be understood as the “underlying framework or logic of justification for social research” (Greene, 2007: 53) - that researchers characteristically align with. As highlighted by Maxwell (2019), Mohr (1982), for example, contrasts variance and process theories, which are two different though potentially complementary ways of conceiving of social phenomena that provide a broad basis for distinguishing between quantitative and qualitative research. Whilst the variance perspective emphasises the measurement of variables and investigates relationships between variables, the process perspective foregrounds explanations of how things occur in particular contexts, emphasising both the processes that connect them as well as particular interpretations of social phenomena.

Consistent with this distinction, the empirical chapters in this thesis apply a variety of quantitative methods that are linked to the variance theory account of explanation through use of regression models, as will be discussed in further detail later in this chapter. Each selection of methods comes with its own strengths and weaknesses, and it is therefore important to briefly highlight what is missed out when relying on the quantitative approach in this particular context. McNeill et al. (2017: 177), for example, have argued that to be able to “fully understand the impact of the extension of conditionality in the UK ... it is vital to give voice to those with direct experience of the welfare system”. This is an end that the quantitative analysis in this thesis does not foreground, and indeed is one that quantitative methods can struggle to achieve more generally. In a similar vein, Patrick (2020: 251) identifies the underutilised potential of participatory research in the area of poverty and social security, a research tradition in which “experts by experience” of poverty take a more central role throughout the research process. By conducting research “*with* rather than *on* people” (Patrick, 2020: 252), the participatory approach provides an important route for promoting undervalued sources of expertise, contesting entrenched misrepresentations of those living in poverty and ultimately in better supporting attempts to improve policy itself.

Qualitative and quantitative approaches can, nevertheless, be complementary, and indeed this thesis has previously drawn on investigations that have sought to highlight the experiences of unemployed benefit claimants, which has been crucial in developing the overall rationale for this research. Chapter 4, for example, surveyed a wide range of quantitative and qualitative evidence into the impacts of benefit sanctions, ultimately leading to the identification of mental health impacts as an emerging area of concern and one that contradicts the assumptions made by policymakers themselves. Given the current extent of the empirical literature on the impacts of benefit sanctions specifically, a quantitative approach is arguably valuable in terms of investigating mental health outcomes as it can be used to provide additional insight to that achieved by the qualitative research discussed in Chapter 4. The research in this thesis, for example, is able to investigate the relationships concerned using data from different sources across the period of Coalition government (2010-15). Consequently, it is able to investigate mental health impacts by exploiting variations in both the frequency and strictness of sanctions through time, as well as focus on the possibility of identifying causal effects using the data available. In light of this, the potential for the analysis to demonstrate the causal impact of sanctions is the subject of the following section.

## **5.3 Causal Inferences**

### **5.3.1 Causation as robust dependence**

It is important to highlight that different conceptions of causality have been developed within the area of quantitative research, and debate on this issue is ongoing. Goldthorpe (2001), for example, distinguishes between three particular accounts: causation as robust dependence; causation as consequential manipulation; and causation as generative process. First, the robust dependence perspective argues that whilst correlation between variables does not imply causation, causation is not possible without correlation being present. Arguably, therefore, the absence of correlation provides grounds to falsify a particular theory or hypothesis. Causal inferences can be made, in contrast, where such a theory is not falsified. This occurs when correlations are found between two variables even after controlling for a variety of additional explanatory variables, which is a familiar technique that is adopted within regression analysis. As

Charlwood et al. (2014) point out, however, a key limitation of this view is that there can be competing theories that each explain an observed correlation. Whilst absence of correlation can arguably be used to falsify a theory, therefore, robust dependence on its own does not provide sufficient grounds for the identification of causal effects.

The limitations of the robust dependence view are particularly apparent in instances where regression analyses are affected by unobserved heterogeneity. This occurs when important determinants of the outcome variable in question are not included in the analysis, leading to omitted variable bias. In such circumstances, any observed correlation between the variables of interest might be entirely spurious. Indeed, it is highly unlikely that any particular regression analysis will be able to control for all relevant independent variables and will therefore be affected by some degree of unobserved heterogeneity. Omitted variable bias is particularly problematic where the omitted variable is a confounder, in that it is a determinant of both the dependent variable and the key independent variable of interest. In the current context, for example, a crude comparison of the mental health outcomes of sanctioned and non-sanctioned claimants would not identify the causal impact of sanctions on mental health outcomes since the two groups may differ systematically in terms of unobserved characteristics. Claimants who are sanctioned may differ in important respects that are not accounted for, such that they are both more likely to be sanctioned and to suffer poorer mental health than claimants who are not sanctioned. Terminological usage on this topic varies by discipline, with some authors referring to omitted variable bias as a particular form of selection bias (Angrist and Pischke, 2009).

### **5.3.2 Causation as consequential manipulation**

Given these limitations, Goldthorpe (2001) highlights attempts to improve on causal inferences through consequential manipulation. Primarily, this occurs using experimental methods such as randomised control trials (RCTs), in which participants are randomly assigned into 'treatment' and 'control' groups that are assumed to be similar in terms of both observed and unobserved characteristics. Typically, having sorted individuals into such groups, the

treatment group is exposed to a particular intervention whilst the control group is not, so that any subsequent difference on the outcome of interest can be interpreted as being directly caused by the intervention. Despite the advantages of consequential manipulation in terms of causal inference, and indeed RCTs are often considered to provide an “experimental ideal” (Angrist and Pischke, 2009: 11) by quantitative researchers, Goldthorpe (2001: 6) highlights a number of issues with this approach. The separation of participants into treatment and control groups, for example, has the potential to affect their behaviour in ways that might influence differences in outcomes between the groups. Whilst researchers can take steps to limit this form of bias, RCTs themselves function through the creation and manipulation of a very specific experimental context, meaning that it is not always clear how results from RCTs apply to real-world social contexts.

The use of RCTs within empirical research, furthermore, is often limited by issues relating to cost and ethical concerns. Based on these and additional limitations, Goldthorpe (2001: 6) warns against appeals to the claim that there can be “no causation without manipulation” in the context of quantitative research. One alternative that has been pursued by researchers is to exploit random assignment that occurs in so-called natural experiment contexts. As alluded to in Chapter 4, for example, a study into sanctions by the National Audit Office (NAO, 2016b) is able to benefit from random assignment in the design of the Work Programme, in which the DWP divided Britain into separate areas within which different providers were made responsible for supporting participants into work. Claimants were randomly assigned to a provider within these areas, and the NAO (2016b) study makes use of this feature to estimate labour market impacts. To do so, it exploits variation in referral rates between providers within the same area, such that it can compare outcomes for otherwise similar claimants who are at a greater or lesser risk of being sanctioned. The specific strategy adopted within the NAO (2016b) study is described as an instrumental variables approach. This attempts to minimise omitted variable bias by identifying a third variable that is exogenous to the model in question, which is thought to influence the outcome variable of interest only through the key independent variable (Angrist and Pischke, 2009).

If the NAO (2016b) study has identified a truly exogenous instrumental variable, then this approach also helps it deal with the additional issue of reverse causality. As Chapter 4 detailed, previous research indicates that benefit sanctions are likely to have a negative impact on mental health outcomes. Additionally, however, it could also be the case that the direction of causality runs in the opposite direction. The possibility exists, for example, that claimants already suffering from mental health problems are more likely to be sanctioned, meaning that any regression analysis will overstate the direct effect of sanctions on mental health. Clearly, neither explanation of the relationship between sanctions and mental health would be a positive outcome from the point of view of sanctions policy. Differentiating between the two, however, would be useful in terms of establishing causal relationships and informing appropriate policy responses. Unfortunately, constraints in terms of data availability mean that the empirical analysis in this thesis relies on observational data and is not able to benefit from random assignment or instrumental variables estimation in order to deal with the issues discussed here. Indeed, as Section 5.5 will detail, data availability means that local authority-level sanctions data - as opposed to individual-level sanctions data - is relied on to investigate the relationship between sanctions and mental health outcomes, which places further limitations on the types of causal inferences that can be made.

### **5.3.3 Responding to the influence of bias**

Whilst the analysis in this thesis is not able to benefit from the strategies described in the previous sub-section, various steps are nevertheless undertaken in order to minimise the influence of bias. The first step relates to the time period that is selected for the analysis, which is restricted to investigating sanctions policy during the Coalition government (2010-15). As Chapter 3 detailed in some depth, sanctions rates varied dramatically during this period in a manner that is unlikely to be explained by changes in claimant characteristics or by claimant behaviour itself. As argued in Chapter 3, decisions made by policymakers themselves were clearly influential throughout the period, involving factors such as internal benchmarking, Jobcentre managers' focus on referral rates and the operation of the Work Programme and its sub-contracted providers. Such influences led to considerable temporal and spatial variations in

sanction rates throughout the Coalition period, which provide a natural experiment-type context that helps limit the influence of omitted variable bias and reverse causality on the results obtained in the empirical analyses in this thesis. Indeed, as Loopstra et al. (2018: 442) argue, changes in the frequency of sanctions throughout the period represent a “largely exogenous source of variation” driven by policy decisions, which provides a context within which it is possible to better estimate the independent effect of sanctions on outcomes of interest.

In addition to the time period investigated, the analysis makes use of several different methods to take into account the role of omitted variables bias and reverse causality. These methods will be broadly outlined in the remainder of this section. Next, Section 5.4 and Section 5.5 will detail the data used in relation to mental health outcomes and sanctions respectively, whilst Section 5.6 will go on to provide a summary of the data and methods that are specific to each chapter. In order to make the discussion in this section and in the remainder of this chapter as clear as possible, Table 5.1 provides a summary of the separate empirical investigations that are carried out in Chapters 6 to 9. It details the level of analysis that is involved (local authority-level; multi-level; individual-level), the mental health outcome that is analysed, the main quantitative method that is applied and the additional robustness tests that are carried out (falsification, placebo and Granger tests). As previously highlighted, additional detail in terms of data sources will be provided later in this chapter, whilst the remainder of this section deals in a more broad sense with the methods that are adopted to deal with sources of bias that might affect the results of the analysis.

**Table 5.1:** summary of analysis by chapter

	Level of analysis	Outcome variable	Main method	Robustness tests
Chapter 6	Local authority-level	Antidepressant prescribing	Fixed effects regression	Falsification and Granger tests
Chapter 7	Local authority-level	Anxiety and/or depression	Fixed effects regression	Falsification and Granger tests
Chapter 8	Multi-level	Anxiety and/or depression	Random intercept regression	Falsification test
Chapter 9	Individual-level	Anxiety and/or depression	Difference-in-differences	Falsification, placebo and matching

In Chapter 6 and Chapter 7, two separate longitudinal ecological analyses are carried out. In these chapters, the primary method for dealing with omitted variable bias is the use of fixed effects regression analysis, which controls for the influence of time-invariant unobserved characteristics. Fixed effects models function by controlling for differences between groups and exploiting within-group variation over time (Allison, 2009; Angrist and Pischke, 2009). Both entity fixed effects and time fixed effects models are used in the analysis. The former control for the influence of factors that are constant over time but that vary between local authorities, whilst the latter control for factors that are constant across local authorities but that vary over time. In so doing, the analysis is able to eliminate omitted variable bias arising from unobserved variables that are constant over time and that are constant across local authorities. This represents a significant advantage over standard OLS regression, though is achieved by making slightly more restrictive assumptions. In the current context, this requires that the effect of sanctions on mental health is additive and constant. At the local authority-level, this implies that a one per cent increase in the sanctions rate will have the same impact on mental health outcomes at all levels of sanction rates, whether or not the increase is from a low or high base rate. When analysing longitudinal data, however, this limitation is outweighed by the need to deal with the unobserved confounding factors identified.

One alternative to fixed effects is the use of random effects regression models, which are able to exploit both within-group and between-group variation but only by making the additional assumption that any omitted variables are

uncorrelated with the explanatory variables included in the regression (Greene, 2008). Whilst random effects models benefit in efficiency over fixed effects models, meaning that the model estimates generally exhibit smaller variances, the stricter assumption they make regarding the error term is often unrealistic in social settings. The Hausman (1978) test provides one means of testing whether this assumption is met, which is carried out as part of the analysis in Chapters 6 and 7. There, both fixed effects and random effects models are estimated as part of the initial modelling process. Fixed effects are ultimately favoured over random effects, though the initial consideration of random effects models provides a useful sensitivity check on the findings. In contrast to the analysis in Chapters 6 and 7, Chapter 8 conducts a multi-level analysis using individual-level data on mental health outcomes and local authority-level data on sanctions. This is primarily carried out to provide a robustness check on the results obtained in Chapter 7, which may have been influenced by changes in the composition of JSA claimants across the period of analysis. In Chapter 8, random effects models provide a better means of dealing with the clustering of individuals within local authorities, and are thus preferred. This will be explained in much greater detail in Chapter 8 itself.

The analysis in Chapter 9 carries out a difference-in-differences (DiD) analysis, using individual-level data on mental health outcomes. DiD analyses use observational data to approximate experimental research designs, by comparing ‘treatment’ and ‘control’ groups before and after a particular treatment occurs (Angrist and Pischke, 2009). In the standard two-period design, for example, researchers exploit situations in which a treatment group is exposed to a particular intervention in the second period as a result of a policy change, whilst a comparable control group is selected that is not exposed to the intervention in either period. The DiD estimate is then obtained by comparing the average change in the outcome of interest for the two groups. If the “common trends assumption” (Angrist and Pischke, 2009: 245) is met, then the observed change in outcomes for the control group serves as a counterfactual for what would have happened to outcomes for the treatment group in the absence of treatment. By comparing the average change for the two groups, the DiD estimator attempts to identify the causal effect of a particular intervention by removing the influence of both selection bias and time trends in the outcome for



the group of interest. Chapter 9 explains in more depth the exact details of the DiD analysis that is carried out, including the particular sanctions policy change that is exploited, as well as the formation of ‘treatment’ and ‘control’ groups.

In addition to the main methods described above, each chapter carries out robustness tests that are designed to take into account particular sources of bias. In terms of omitted variable bias, for example, each chapter conducts falsification tests using the “non-equivalent dependent variable” approach (Shadish et al., 2002: 184). This test functions by identifying an additional dependent variable that is not expected to be influenced by variations in the key independent variable of interest, but that is likely to be influenced by the same unobserved confounders as the original dependent variable. If such a variable can be identified, results of the initial analysis are re-run using the non-equivalent dependent variable. The plausibility of the original associations estimated will be enhanced if no effect between the key independent variable and non-equivalent dependent variable is observed. This would lessen the potential that alternative explanations are the true cause of the effects originally observed. Importantly, however, the selected non-equivalent dependent variable must be similar enough to the original dependent variable in order to be influenced by related unobserved confounding factors. Otherwise, its selection would be “merely arbitrary” (Coryn and Hobson, 2011: 33) and its use as part of a falsification test would not strengthen causal inferences. Given the various differences between each empirical chapter, separate non-equivalent dependent variables are used at each stage, as will be explained in the analyses themselves.

In Chapter 9, use is also made of a placebo test (de Chaisemartin and D’Haultfoeuille, 2018). The DiD analysis carried out in Chapter 9 exploits a particular change in sanctions policy that occurred at the beginning of the Coalition government in 2010. In this context, a placebo test involves repeating the results of the main analysis in a period not marked by any significant changes in sanctions policy. By choosing a time period that was relatively stable in terms of sanctions policy, the results of the main analysis are supported if the placebo DiD analysis does not find evidence of an intervention effect. The use of a placebo test is also useful as it avoids potential doubts that might affect

falsification tests, regarding the appropriateness of the non-equivalent dependent variable that is selected. In a further robustness test, Chapter 9 also carries out a matching technique (Angrist and Pischke, 2009). This seeks to limit the influence of differences between the treatment and control groups by using groups that are as similar as possible in terms of observed characteristics. Whilst matching provides a useful robustness test on the main results it does not, as is occasionally assumed in quantitative analyses, reduce bias resulting from unobserved confounding factors. The precise details of the matching technique that is applied will be explained in more detail in Chapter 9.

Lastly, the longitudinal ecological analyses in Chapter 6 and Chapter 7 are supported by Granger causality tests. The Granger test is carried out in two steps. First, it tests whether lagged values of the key independent variable are jointly associated with the dependent variable, as is implied by the notion that cause precedes effect. Second, it tests whether lagged variables of the dependent variable are jointly associated with the key independent variable, in order to provide an assessment of whether the model is affected by reverse causation. The original test developed by Granger (1969; 1980) was designed for use with time series data, and so the test carried out in Chapter 6 and Chapter 7 uses an extension to panel data developed by Dumitrescu and Hurlin (2012) using the Stata command 'xtgcause' created by Lopez and Weber (2017). The Granger test is premised on a specific notion of causality, based particularly on the predictive content of variables. A positive Granger test, therefore, cannot by itself rule out reverse causality entirely. Causal inferences, nevertheless, are strengthened if it is shown that the key independent variable Granger-causes the dependent variable and not vice versa. Indeed, the use of Granger tests is favoured over explicitly including lagged sanctions variables in the explanatory models themselves. Fixed effects models have been shown to be extremely sensitive to the specification of temporal lags, which undermine attempts to ascertain the correct lag specification (Bellemare et al., 2017; Vaisey and Miles, 2017).

### 5.3.4 Causation as generative process

Thus far, this section has contrasted Goldthorpe's (2001) categories of causation as robust dependence and causation as consequential manipulation. It has been highlighted that the empirical analysis in this thesis is not able to benefit from the forms of randomised allocation that are central to the consequential manipulation approach. Various steps, nevertheless, have been outlined that will be used in order to limited sources of bias and improve upon the scope for causal inference that is possible when using observational data. It is important to highlight, however, Goldthorpe's (2001) third account of causation as generative process. This approach seeks to identify "what must be *added* to any statistical criteria before an argument for causation can convincingly be made" (Goldthorpe, 2001: 8). Accordingly, Goldthorpe (2001) outlines a three-stage but overlapping process for establishing causal claims within quantitative research. This begins by substantiating the existence of empirical regularities through theoretically informed empirical work, followed by hypothesising the causal mechanisms that might give rise to such empirical regularities before then going on to test the mechanisms themselves.

Importantly, the research presented in this thesis is able to contribute to the first two stages of this process. Chapter 4 has considered the separate material and psychosocial mechanisms that might plausibly link sanctions with adverse mental health impacts, drawing on existing empirical and theoretical work in relation to the mental health impacts of sanctions and unemployment. The analyses that are carried out in Chapters 6 to 9, furthermore, consider different mental health outcomes and use methods that combine to provide a robust and thorough investigation into the mental health impacts of benefit sanctions. This triangulation of data sources and research designs is a key strength of this thesis, which uses data from across the period of Coalition government (2010-15) to substantiate empirical regularities in this area. Given data availability, however, the investigation is not able to directly test the role of the separate mechanisms identified, which relates to the third phase of Goldthorpe's (2001) notion of causation as generative process. This is an avenue that would require additional research and data to achieve, which could be used to help strengthen any causal inferences resulting from the investigation carried out in this thesis.

Having outlined the issue of causal inference and the methods that will be adopted in this regard, the discussion now turns to the measurement of important variables. The first of these relates to mental health outcomes, as explained in the next section.

## **5.4 Mental health: measuring anxiety and depression**

Previously, Chapter 4 outlined ongoing debates in relation to the definition and conceptualisation of mental health in broad terms, which are equally prevalent in attempts to understand the nature and status of the mental distress experienced by people affected by anxiety and depression. Kokanovic et al. (2013: 377), for example, identify the various tensions that exist “across the entire spectrum of lay and medical belief”, which range from viewing anxiety and depression as ‘normal’ responses to life’s challenges to forms of potentially serious mental illnesses. In the medical sphere, for example, attempts have been made to develop symptom-based diagnostic categorisations in order to improve upon the reliability of medical diagnoses and therefore guide treatment. In the UK, official guidance from the National Institute for Health and Care Excellence (NICE, 2009: 6) recognises the “broad and heterogeneous” nature of depression, and discusses two classification systems, the ICD-10 and the DSM-IV, which identify symptoms such as: low mood; loss of interests, pleasure or energy; feelings of worthlessness; and suicidal thoughts or actions. The ICD-10 requires four out of ten depressive symptoms to be present for a formal diagnosis of depression to be made, whilst the DSM-IV requires five out of nine, though the NICE (2009) guidelines advise against merely symptom counting. They also recognise that depressive symptoms below the threshold criteria can be significantly distressing, especially when experienced on a consistent basis.

The NICE (2009: 7) guidance adopts a cautious and stepped approach to the diagnosis and treatment of anxiety and depression, noting the “wide range of biological, psychological and social factors” that diagnostic classification systems struggle to capture accurately. The development and application of such diagnostic systems in a broader context has nevertheless been widely criticised, given concerns that they reflect a misguided overemphasis on bio-chemical

explanations of depression that have led to a rapid rise in diagnoses, over-medicalisation and a heyday for the pharmaceutical industry (Blazer, 2005; Horwitz and Wakefield, 2007; Horwitz, 2011). Critics point, for example, to the difficulty in precisely defining and categorising depression given the overlapping nature of the symptoms involved in various mental health-related diagnoses. Indeed, several have questioned whether distinguishing between the categories of ‘anxiety’ and ‘depression’ is itself meaningful, given the similarities in the symptoms and the populations affected by them (Kasper, 2001; Shorter and Tyrer, 2003; Das-Munshi et al., 2008). Similarly, critics have highlighted the value-laden and contested nature of medical diagnoses, pointing to various lay accounts that seek to resist the implication that depression represents an illness or an abnormal or inappropriate response to stress of various forms (Pilgrim and Bentall, 1999; Pilgrim, 2007; Kokanovic et al., 2013). Others have even gone as far, furthermore, as arguing that “no one can say with any authority what depression actually is” (Ridge, 2018: 147).

The above discussion provides some insight into the polarised nature of the debate in this area, and highlights the importance of recognising how mental distress is measured and operationalised in the current research. Given data availability, two indicators of mental health are used in the empirical chapters in this thesis, which include: a measure of antidepressant prescribing (Chapter 6); and a measure of self-reported anxiety and/or depression (Chapters 7 to 9). These measures will be explained further in the remainder of this section, as well as in more detail in the individual empirical chapters themselves. It is important to emphasise, however, that the analysis is not able to contribute to or resolve the ongoing debates in this area. As previously indicated, these relate to the appropriate treatment of anxiety and depression, the status of such mental health problems as distinct issues and their relationship to ‘normal’ functioning in the context of an individual’s social environment. Partly driven by the reliance on quantitative methods, the measures of mental health utilised in this thesis must in some sense be taken at face value, as indications that individuals are experiencing mental or emotional distress of some - potentially quite serious - form. Following from this, and informed by the discussion in Chapter 4, the view that mental health can be influenced by important social

determinants suggests that a basic expectation of the social security system is that it should not add - inadvertently or otherwise - to such mental distress.

In Chapter 6, data on the number of antidepressant items prescribed in each local authority are accessed from NHS Digital (2018), which publishes monthly administrative data from GPs. In particular, the analysis makes use of 'Selective Serotonin Re-Uptake Inhibitors' (SSRIs) as the specific antidepressant measure. These are the first-line medication for treating depression and anxiety (NICE, 2015). They are also the most appropriate indicator for capturing impacts on such mental health problems since the broader total antidepressant measure includes items prescribed to treat non-psychiatric health conditions such as chronic pain (Spence et al., 2014). Clearly, not all individuals suffering from anxiety and/or depression will be prescribed antidepressant medication, since there are differences in the likelihood of individuals recognising and reporting mental health problems, as well as differences in GP prescribing behaviour and the exploration of alternative treatments (Hyde et al., 2005). Nevertheless, the antidepressant measure used in Chapter 6 provides a readily understandable measure of mental distress, in the form of people who do in fact receive medical treatment. In the context of the research itself, antidepressant usage is a useful measure in that it provides an indication of resource usage through the National Health Service (NHS), therefore allowing the investigation to highlight the potential public expenditure implications of benefit sanctions.

In contrast to the administrative antidepressant prescribing data used in Chapter 6, Chapters 7 to 9 rely on a self-reported measure of anxiety and/or depression using the Quarterly Labour Force Survey (QLFS) (ONS, 2018c). Section 5.6 in this chapter provides more specific detail on the structure of the QLFS itself, whilst the present discussion focuses specifically on the questionnaire question relating to anxiety and depression, as detailed in Box 5.1.

**Box 5.1:** QLFS questionnaire health-related question

a) In the QLFS, respondents are first asked:

Do you have any health problems or disabilities that you expect will last for more than a year?

From the second quarter in 2013 onwards this question was changed slightly to:

Do you have any physical or mental health conditions or illnesses lasting or expecting to last 12 months or more?

b) Respondents are then asked to select which particular health problem(s) that they suffer from using a pre-specified list, as follows:

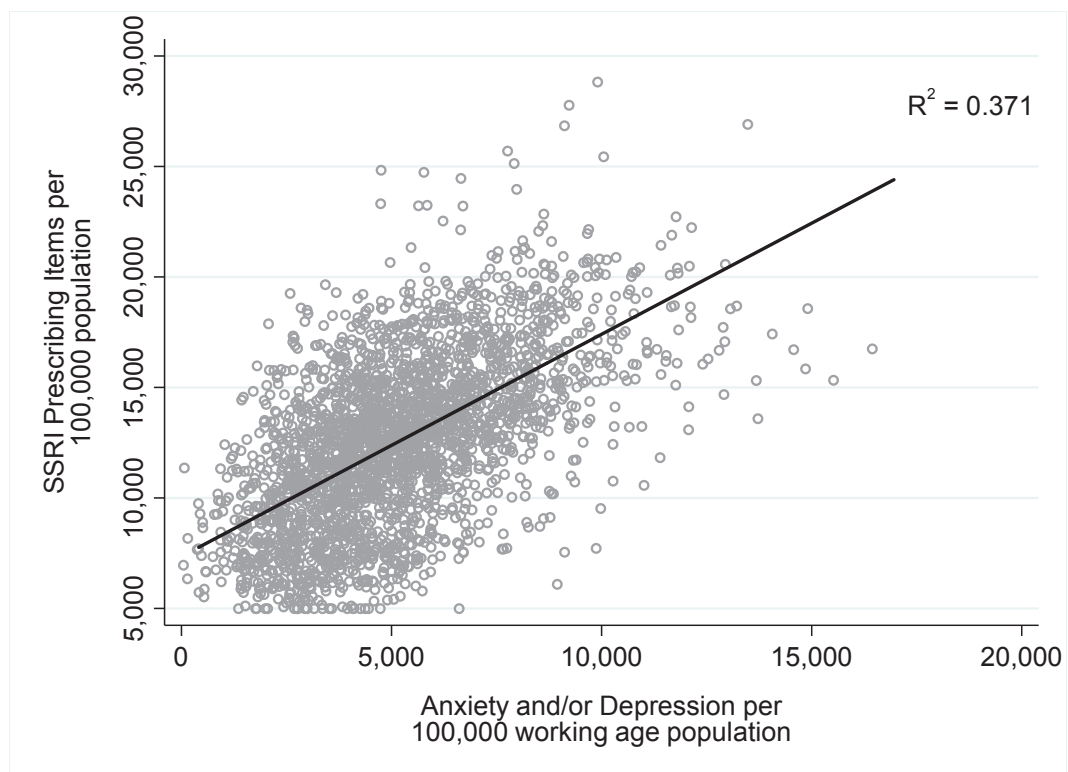
1. Problems or disabilities (including arthritis or rheumatism) connected with arms or hands;
2. ... legs or feet;
3. ... back or neck;
4. Difficulty in seeing (while wearing spectacles or contact lenses);
5. Difficulty in hearing;
6. Speech impediment;
7. Severe disfigurements, skin conditions, allergies;
8. Chest or breathing problems, asthma, bronchitis;
9. Heart, blood pressure or blood circulation problems;
10. Stomach, liver, kidney or digestive problems;
11. Diabetes;
12. Depression, bad nerves or anxiety;
13. Epilepsy;
14. Severe or specific learning difficulties;
15. Mental illness or suffer from phobias, panics or other nervous disorders;
16. Progressive illness not included elsewhere (e.g. cancer not included elsewhere, multiple sclerosis, symptomatic HIV, Parkinson's disease, Muscular Dystrophy); and
17. Other health problems or disabilities.

*Source:* ONS (2014)

As Box 5.1 details, mental health status in the QLFS is elicited using a self-reported measure, in which respondents are first asked whether or not they have any health problems that they expect will last for more than a year. This question changed slightly from the second quarter in 2013, which the analysis takes into account where necessary and as explained in the specific empirical chapters themselves. Following this, respondents are then asked to select which particular health problems they suffer from using a pre-specified list, from which the primary outcome of interest in Chapters 7 to 9 relates to respondents who self-report as suffering from 'Depression, bad nerves or anxiety' (option 12). Although option 15 'Mental illness or suffer from phobias, panics or other nervous disorders' also relates to mental health, the fact that this option combines mental health problems such as phobias with the very broad term

‘mental illness’ meant that this self-reported measure was not ultimately investigated in the analysis, since it is unclear what mental health problems it measures or what the potential relationship with benefit sanctions is expected to be. As Figure 5.1 indicates, there is a high correlation between rates of self-reported anxiety and/or depression and antidepressant prescribing at the local authority-level ( $r = 0.609$ ,  $p < 0.001$ ). The two measures are not perfectly correlated, however, which provides the basis for separate analyses in this thesis.

**Figure 5.1:** relationship between rates of anxiety and/or depression and antidepressant prescribing



*Note:* quarterly rates for 148 local authority counties, Q3 2010 – Q4 2014

Bentley et al. (2016) criticise the self-reported measure of anxiety and depression available through the QLFS on the grounds that it only provides a subjective and indicative response to the survey sub-question. In contrast, they argue that a diagnostic assessment would provide a more robust measure. As the discussion at the beginning of this section indicates, an evident weakness in the criticism made by Bentley et al. (2016) is that it ignores the fact that medical diagnoses of depression and anxiety are themselves continually contested in the context of what has been described as an ongoing “diagnostic fuzziness” (Kokanovic et al., 2013: 380). Self-reported and diagnostic measures provide different means of capturing mental health. Whilst each have their strengths and



limitations, it would arguably be misguided to assume that either one provides a particular degree of validity that the other lacks. The use made of the self-reported measure in this thesis does of course have certain implications. On the one hand, for example, the self-reported measure risks not identifying individuals who are in fact suffering from anxiety and/or depression, because they are either unaware that what they are suffering might be described as such or because they are unwilling to report suffering from such problems. On the other hand, nevertheless, this measure will potentially identify those who self-identify as suffering from some form of mental distress but who have not had this affirmed by a formal medical diagnosis.

Arguably, the biggest limitation of the self-reported measure of anxiety and depression used in Chapters 7 to 9 is that it provides only a binary indication of an individual's mental health status. Mental health problems such as anxiety and depression can be conceptualised as both a category and a continuum (Wheaton, 2001), and thus binary measures ignore the extent to which individuals might suffer mental and emotional distress as a matter of degree as opposed to one of kind. This binary measure of depression and anxiety is therefore limited in that it provides no information about the severity of the mental health problems being reported, such that the mental health of somebody who has already specified that they suffer from such problems cannot be observed to worsen.

This limitation is compounded by the fact that the QLFS questionnaire asks respondents whether or not they expect the particular health problem to last more than a year. In this respect, this measure risks underestimating the prevalence of mental health problems given that it may not identify individuals who are suffering from anxiety or depression but who do not expect such issues to last as long as a year. Despite these limitations, the QLFS measure is a useful one in that it is consistently available on a quarterly basis throughout the time period of interest. Indeed, it is arguably relatively unproblematic to assume that if increases in rates of self-reported anxiety and/or depression using this measure are the result of increases in rates of sanctioning, then this is indicative of adverse impacts in terms of worsening mental health.

Other sources of mental health outcomes were considered as part of the research, though ultimately not investigated within the analysis. The UK Household Longitudinal Study (Understanding Society, the successor to the British Household Panel Survey (BHPS)), for example, permits investigation of mental health outcomes that are measured through the General Health Questionnaire (GHQ-12), which provides a scalar measure of mental distress ranging from zero (least distressed) to 36 (most distressed), based on answers to 12 mental health-related questions (Brown et al., 2018). Delaney et al. (2017), however, have carried out initial - as yet unpublished - analysis of the relationship between mental health and local authority-level sanctions using Understanding Society, whilst the annual nature of the datasets mean that it would not be possible to exploit the high degree of quarterly variation in benefit sanctions in the analysis. The same limitation applies to the Annual Population Survey (APS) subjective well-being datasets, which provide measures of life satisfaction, life worth, happiness and anxiety on a zero to ten scale (ONS, 2012). The APS well-being data are also only available from 2011-12 onwards, therefore missing out on the initial change in sanction frequency at the onset of the Coalition.

## **5.5 Sanctions data**

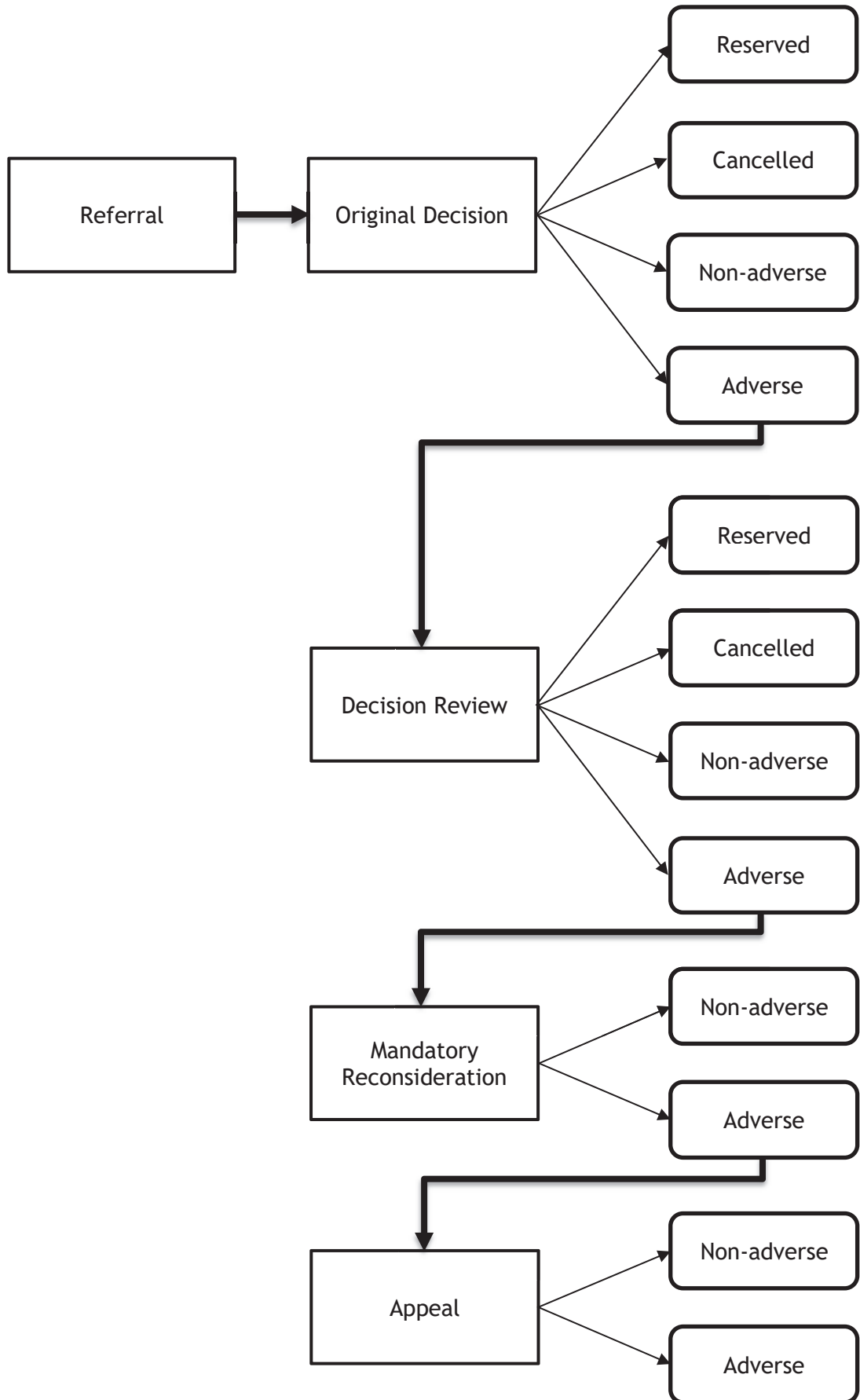
In addition to mental health outcomes, a key variable in this research is the measurement of benefit sanctions themselves. Data on the number of JSA sanctions used throughout this thesis were accessed from the DWP's Stat-Xplore database (DWP, 2018c), which publishes data on the monthly frequency of sanctions at the local authority-level, according to the residence of JSA claimants. As previously highlighted, in both Chapter 4 and in Section 5.3, there are important theoretical reasons for focusing specifically on JSA sanctions during the period of Coalition government (2010-15). Using the Stat-Xplore database, furthermore, local authority-level data for other claimant groups, such as ESA, IS and UC sanctions, are not available during this period and could therefore not be included in the analysis. Data on IS sanctions are only available from October 2016 onwards, whilst data on UC sanctions are only available from August 2015 onwards. Aggregated data on ESA sanctions are accessible throughout the time period analysed in this thesis, however such data could not

be included in the analysis due to the quality of the local authority-level data available. In particular, an extremely high proportion of the monthly ESA sanctions data available through Stat-Xplore are recorded as zero, or had their true figure suppressed due to disclosure concerns, due to the relatively lower level of ESA sanctioning that occurred throughout the period. This issue affected the data to such an extent that the use of ESA sanctions in the analysis would be highly unreliable, which also explains its non-use in the wider UK quantitative literature.

The JSA benefit sanctions data that are available are limited in several ways by how they are recorded and published. The manner in which the DWP has reported on the available sanctions statistics has drawn repeated criticism as well as freedom of information requests (FOIs) from researchers. Criticism has even prompted the UK Statistics Authority (UKSA, 2015) to write to the department recommending that it improve various aspects of its published sanctions statistics, both in terms of transparency and in terms of gaps in areas such as hardship payments. Indeed, key gaps at the local authority-level remain regarding statistics on the length of sanctions, hardship payments and distinguishing between sanctions and disentitlements, meaning that the analysis in this thesis relies on data that records the frequency of sanctions only. Again, this is a feature of the wider UK quantitative literature into sanctions that was discussed in Chapter 4.

Of central importance here is that the Stat-Xplore database records only the latest decision for each sanction case. To elucidate the implication of this recording decision, Figure 5.2 depicts the sanctions process for an individual following a sanctions referral by their Jobcentre Work Coach or Work Programme provider (DWP, 2018a).

**Figure 5.2:** stages in the sanctions process following a referral



Source: adapted from DWP (2018a)

As highlighted in Figure 5.2, Decision Makers within Jobcentre Plus offices can make one of four decisions once they receive a referral for a claimant to be sanctioned. These include:

- i. *Adverse*: a decision to impose a benefit sanction;
- ii. *Non-Adverse*: a decision not to impose a benefit sanction;
- iii. *Cancelled*: a decision to cancel the referral, given that it is deemed inappropriate; and
- iv. *Reserved*: a decision that a sanction cannot be imposed because the claimant is not currently receiving benefits. The claimant will be re-referred if they claim again in the future.

Following an 'Original Decision' to impose a sanction, three further steps are possible if the sanctioned claimant decides to challenge the decision made. First, claimants can offer an explanation of a sanction decision or provide additional evidence not yet considered, leading to a 'Decision Review'. If an adverse decision is upheld, claimants are able to challenge the decision through a formal 'Mandatory Reconsideration' process. Following this, if an adverse decision is again upheld, claimants can then appeal the decision at an independent tribunal. Mandatory reconsiderations were introduced at the end of October 2013, at the height of sanctioning as depicted in Figure 3.1 in Chapter 3. Prior to this time, a claimant could appeal following either an original or a reconsidered decision, though since the introduction of mandatory reconsiderations appeals are only permitted once they have received a decision in that regard.

As highlighted above, the Stat-Xplore database records only the latest decision for each sanction case, and therefore updates the status of each case to the point in time in which the most recent decision has been made on it. The implication of this recording system is that sanctions that have gone through the review, reconsideration or appeals process will be recorded at a later point in time from the original sanctioning decision, meaning that it is not possible to ascertain when the original adverse decision was made. Consequently, it is also

not possible to calculate the total number of adverse sanctions before review, reconsideration or appeal for any given month, which is the point in time at which claimants first lose their benefit income and therefore an important measure in investigating the impacts of sanctions. Although a successful review, reconsideration or appeal should result in the claimant being refunded their lost income, the claimant in question will still have endured a period of time - weeks, potentially months - without any financial resources. An additional consequence is that it is not possible to calculate the number of sanction referrals in any given month, which are not published separately within Stat-Xplore. An indicative measure can be calculated by adding together all the original decisions made (adverse, non-adverse, cancelled and reserved), though this more accurately measures original decisions as opposed to referrals and also remains an underestimate given the issue highlighted in relation to adverse sanctions.

Given the data limitations described, there are several imperfect measures of local authority-level sanctions available for the purposes of the analysis. Each will be affected by the fact that the original adverse sanctions figure represents an underestimate of the true rate, given that this figure represents only those original decisions to impose a sanction that were not later reviewed, reconsidered and/or appealed. The first option is to use data on the remaining original adverse sanctions themselves, which is the approach taken by other quantitative studies in the literature (de Vries et al., 2017; Reeves, 2017; Reeves and Loopstra, 2017; Loopstra et al., 2018; Taulbut et al., 2018), and which is also the main approach that will be taken here. The data on the remaining original adverse sanctions (henceforth, original adverse sanctions) have the advantage of being precisely aligned with their original decision month, despite providing an underestimate of the true level of sanctions that were imposed. The size of this underestimate is not unsubstantial. Kennedy and Keen (2016), for example, estimate that under a fifth of original adverse decisions were challenged in any given month during the period of analysis. Importantly, nevertheless, the NAO (2016a: 32) notes that this proportion stayed “broadly the same” during this period, meaning that the remaining original adverse sanctions rate provides a consistent measure of sanctions through time.

As a sensitivity check, two additional sanctions rates are initially considered in the analysis in Chapter 6. The second rate that is used is the total adverse sanctions figure. For any given month, these include the remaining original adverse sanctions described above, as well as adverse sanctions resulting from a review, reconsideration or appeal. This sanctions measure is fuzzier than the original adverse measure, as it includes adverse sanctions decisions that had been originally imposed prior to the month in question. It also suffers from the fact that the rate of successful challenges increased through the period that is analysed in the investigation (Kennedy and Keen, 2016; NAO, 2016a), which therefore adds an additional degree of inconsistency into the analysis. The third rate that is considered is the indicative referral rate (henceforth, referral rate), which is measured by adding together all original decisions (adverse, non-adverse, cancelled and reserved). As previously explained, this measure provides an underestimate of the true referral rate given the recording issue relating to original adverse sanctions, whilst it more accurately measures original decisions as opposed to the referrals themselves. One limitation of this measure in the current context is that it does not provide a clear expected relationship between sanctions and mental health outcomes. The mental health impact of a sanction referral, for example, will be very different once an individual knows that they have actually received a non-adverse decision.

In their quantitative study, Taulbut et al. (2018) investigate both the referral rate as well as the original adverse sanctions figure, describing the former in terms of the threat of sanctions, with the implication being that the latter represents the imposition effect of sanctions. Arguably, however, it is not possible to investigate threat and imposition effects separately using the aggregate-level data that is available. Chapter 4 distinguished between threat and imposition effects for individual claimants, noting that threat effects have been thought of in terms of both the general threat of sanctions as well as the impact of an actual warning that a sanction might be imposed. Imposition effects, in contrast, occur once sanctions have actually been applied. The aggregate-level data that is available for the investigation in this thesis, in contrast, is better thought of as being able to capture an overall combined threat and imposition effect resulting from sanctions, as opposed to identifying one or the other. Consider, for example, the original adverse sanctions figure. A

quarterly increase in this sanction rate implies that more JSA claimants have received a sanction (an imposition effect), which itself is a form of increased threat effect for those JSA claimants who did not have a sanction imposed. Similarly, the referral rate does not isolate the threat effect of sanctions, since original adverse sanctions represents one of its key components.

## **5.6 Overview of empirical chapters**

The empirical chapters in this thesis investigate the relationship between benefit sanctions and mental health outcomes during the Coalition government (2010-15). As highlighted in Chapter 4, the overarching research question that these empirical chapters seek to address is the following:

[RQ]: Are benefit sanctions associated with adverse mental health impacts?

In order to address this research question, each chapter exploits changes in the frequency and the strictness of sanctions that occurred across the period. These sources of variation are used to better understand the specific relationship that is being investigated in each chapter, as well as strengthen any causal inferences that are made, as will be explained in this section. In light of the various data limitations that have been highlighted throughout this chapter, and in order to maximise the potential of the data that is available, several different analyses are carried out across Chapters 6 to 9. These were previously summarised in Table 5.1. Benefit sanctions data in each chapter are sourced from Stat-Xplore (DWP, 2018c), and the separate analyses will be briefly outlined in turn in this section, including: two longitudinal ecological analyses (Chapter 6 and Chapter 7); a multi-level analysis (Chapter 8); and a difference-in-differences analysis (Chapter 9). More in-depth discussion of the various aspects of each investigation will be provided in the specific chapters themselves.

### **5.6.1 Longitudinal ecological analyses**

The analyses in Chapter 6 and Chapter 7 are conducted using similar research designs but consider different aspects of mental health. The former investigates antidepressant prescribing and the latter investigates rates of self-reported



anxiety and/or depression. In both, panel datasets are constructed that permit the investigation of the relationship between benefit sanctions and mental health outcomes through time at the local authority-level, which is enabled by the considerable spatial and temporal variation in sanction rates that occurred throughout the period. In addition, the implementation of the Welfare Reform Act 2012 increased the average length of the sanctions that could be imposed on claimants, and so both chapters consider whether the observed relationship between sanctions and mental health differs in the pre- and post-reform period. The particular research questions that inform the investigation in Chapter 6 are the following:

[RQ 6.1]: Are benefit sanctions associated with higher rates of antidepressant prescribing at the local authority-level?

[RQ 6.2]: Does the observed relationship strengthen following the implementation of the Welfare Reform Act 2012?

Similarly, the research questions that inform the investigation in Chapter 7 are as follows:

[RQ 7.1]: Are benefit sanctions associated with higher rates of anxiety and/or depression at the local authority-level?

[RQ 7.2]: Does the observed relationship strengthen following the implementation of the Welfare Reform Act 2012?

Section 5.3 highlights the twin concerns of omitted variable bias and reverse causality which the analyses in Chapter 6 and Chapter 7 take various steps to take into account. In terms of omitted variable bias, additional local authority-level variables are sourced from Nomis, Stat-Xplore and specific UK government departments and included in both fixed and random effects regression models, whilst falsification tests are carried out in robustness checks on the results of the main analysis. Granger tests are also carried out to consider the issue of reverse causality.

Ecological analyses are subject to well-known limitations that affect their scope for causal inference. Primarily, this relates to the fact that correlations that hold at the area-level do not necessarily apply at the individual-level. Since the investigations carried out in Chapter 6 and Chapter 7 are based on data at the local authority-level, it is not possible to know whether the individuals being sanctioned in each local authority are the same people who suffer impacts in terms of their mental health. Indeed, the risk of drawing incorrect inferences about individual-level relationships based on correlations that hold at the aggregate-level is commonly described as an “ecological fallacy” (Pearce, 2000: 326). It is important, therefore, to be cognisant of this issue when interpreting the results in these chapters. Aggregate-level studies, nevertheless, are important in circumstances where individual-level data are unavailable. Such studies can, for example, provide initial tests of hypotheses and help to identify policy issues that need to be considered through additional individual-level research (Pearce, 2000). Indeed, aggregate-level analyses are a feature of the wider quantitative literature into both conditionality and sanctions, which has highlighted concerns regarding the impacts of recent social security reforms (Barr et al., 2016; Loopstra et al., 2018; Taulbut et al., 2018).

Previously, Section 5.4 outlined the particular measures of mental health outcomes that will be adopted in each chapter. First, Chapter 6 uses administrative data from GPs on antidepressant prescribing that is then aggregated to the local authority-level (NHS Digital, 2018). Chapter 7, in contrast, uses a self-reported measure of anxiety and/or depression that is sourced from the QLFS (ONS, 2018c). In particular, use is made of the secure access version of the QLFS, which - amongst other advantages over the separate publicly available datasets - provides researchers with more fine grained geographical information such as the local authority in which survey respondents live. The QLFS itself is a nationally representative, quarterly household survey in which each quarterly dataset contains approximately 100,000 individuals (ONS, 2016). The survey is carried out as a repeated cross-sectional study, though it adopts a rotational sampling design in which each household is included for five consecutive quarters, with the final interview carried out one year after the first. Each quarterly cross-sectional dataset contains five waves of data, made up of individuals at the five separate stages of the interview process. An

individual is in Wave 1 if it is their first interview and Wave 5 if it is their last. A fifth of the sample is replaced each quarter. Using the third quarter of 2010 as an example quarter, which is the beginning of the period of analysis in Chapter 7, Table 5.2 depicts the wave pattern that is implied by the rotational sampling design used in the QLFS.

**Table 5.2:** wave structure of the QLFS, Q3 2010 dataset example

	Q3 2009	Q4 2009	Q1 2010	Q2 2010	Q3 2010
Cohort 1:	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
Cohort 2:		Wave 1	Wave 2	Wave 3	Wave 4
Cohort 3:			Wave 1	Wave 2	Wave 3
Cohort 4:				Wave 1	Wave 2
Cohort 5:					Wave 1

*Source:* adapted from ONS (2016)

The potential to exploit the longitudinal aspects of the QLFS has important implications for the analysis in Chapter 8 and Chapter 9, though in Chapter 7 use is made of the QLFS by pooling separate cross-sectional datasets. Using the secure access version of the QLFS, it is possible to estimate local authority estimates of rates of anxiety and/or depression using QLFS survey weights. Weights function by assigning each survey respondent a number indicating how many people in the population that that individual represents, which enables population inferences to be made. The QLFS weights aim to take into account both sample design and non-response bias. The former compensates for probability of selection into the QLFS whilst the latter compensates for differential non-response for different groups. Using the QLFS weights, Chapter 7 estimates the proportion of working age individuals that from anxiety and/or depression at the local authority-level. Although based on survey estimates, one advantage of the analysis in Chapter 7 over that of Chapter 6 is that the antidepressant prescribing data is not available with any population characteristics detailed. This means that the investigation in Chapter 6 is limited to a less precise outcome variable expressed as a rate per total local authority population.

### 5.6.2 Multi-level analysis

In contrast to Chapter 7, the multi-level analysis in Chapter 8 combines individual-level data on JSA claimants from the QLFS with local authority-level benefit sanctions data, to investigate the relationship between individual-level mental health of claimants and the area-level sanctions rate. The research questions that inform the investigation at this stage of the analysis are the following:

[RQ 8.1]: Are area-level sanction rates associated with adverse mental health impacts for JSA claimants?

[RQ 8.2]: Does the observed relationship strengthen following the implementation of the Welfare Reform Act 2012?

The analysis in Chapter 8 pools separate cross-sectional QLFS datasets over the period of investigation. Given the rotational sampling design described above, this introduces a longitudinal element into the analysis given the fact that JSA claimants will be present in the sample between one and five times, depending on how long they were claiming JSA. Chapter 8 therefore begins by analysing a three-level data structure, in which occasions of measurement are nested within individuals who are themselves nested within local authorities. Such clustering needs to be explicitly taken into account in the modelling process, as such observations are likely to be highly correlated with each other and will therefore violate the assumption of independence that underpins single-level multiple regression models. Chapter 8 itself provides more detailed discussion regarding the modelling approach, which estimates three-level models as well as a two-level model that retains JSA claimants according to the first occasion in which they are present in the sample.

The main contribution of Chapter 8 with regard to supporting the identification of benefit sanction impacts is that it provides an additional robustness check on the analysis conducted in Chapter 7. Given that the longitudinal analysis in Chapter 7 is carried out solely at the ecological-level, it encounters the additional risk of being influenced by compositional bias. In this context, there is a risk that any observed association between sanctions and anxiety and/or

depression is explained by unobserved changes in the characteristics of the JSA claimant group throughout the time period, as opposed to being driven by sanctions themselves. The use of individual-level data on JSA claimants helps attenuate this risk, as the regression models are able to explicitly control for claimant characteristics. The analysis in Chapter 8 is supported by an additional falsification test, though it is important to highlight that reverse causality could still be influential. Although the combined threat and imposition effect of the area-level sanctions rate might adversely affect claimants' mental health, for example, it could also be the case that claimants at a higher risk of mental health problems are more likely to be sanctioned, which itself contributes to the area-level sanctions rate.

### **5.6.3 Difference-in-differences analysis**

Chapter 9, finally, carries out a difference-in-differences (DiD) analysis. It investigates the impact of a change in sanctions policy at the onset of the Coalition in 2010 on the self-reported mental health of JSA claimants. The move towards a harsher sanctioning environment at this time is constituted by two changes in sanctions policy, including an increase in the penalty for non-attendance at advisory interviews and an increase in the rate of sanctions. The research question that informs the investigation at this stage of the analysis is the following:

[RQ 9.1]: Are harsher sanctioning environments associated with adverse impacts on the mental health of JSA claimants?

In contrast to Chapter 8, Chapter 9 makes use of the two-quarter longitudinal LFS datasets (ONS, 2018a). These link information on working age survey respondents who respond in two consecutive quarters and provide longitudinal survey weights that account for differential attrition. Whilst both two- and five-quarter longitudinal LFS datasets are published, the analysis makes use of the two-quarter datasets given the larger sample size that they provide. Each two-quarter dataset contains approximately 35,000 individuals compared with approximately 5,000 for the five-quarter datasets. Using the two-quarter longitudinal LFS datasets, the investigation carries out a DiD analysis that compares the changes in the mental health of a 'treatment' group of JSA

claimants before and after the onset of the Coalition with the experience of a comparable ‘control’ group. As Section 5.3 highlighted, the aim of this approach is to minimise the influence of both selection bias and time trends in the outcome for the group of interest. More depth on the precise nature of the DiD analysis that is carried out is provided in Chapter 9 itself, regarding the ‘treatment’ and ‘control’ groups that are used and the particular sanctions policy change that is exploited.

## **5.7 Conclusion**

This chapter has detailed the data and methods that provide the basis for the empirical analyses carried out in Chapters 6 to 9 of this thesis. Together, these conduct a quantitative investigation into the impact of benefit sanctions on mental health outcomes such as antidepressant prescribing, anxiety and depression. Particular attention has been given to the issue of causal inferences, due to the use of observational data throughout the analysis and in light of the reliance on local authority-level data on sanctions. Informed by Goldthorpe’s (2001) categorisation of different accounts of causation within quantitative research, the discussion has argued that the investigation is able to contribute to the establishment of robust empirical regularities in this area and is supported by the development of impact mechanisms carried out in Chapter 4. Four separate empirical analyses are carried out, and it is argued that the triangulation of data sources and research designs that the investigation is able to achieve is a particular strength. Whilst limitations have been highlighted, causal inferences are supported by the time period selected, which is characterised by exogenously influenced changes in sanctions policy. Methods such as fixed effects, multi-level and difference-in-differences analysis are used to take into account several sources of bias, which are reinforced through additional robustness checks. It is to these empirical chapters that this thesis now turns.

## Chapter 6. Longitudinal ecological analysis I: antidepressant prescribing

### 6.1 Introduction

This chapter investigates the relationship between JSA benefit sanctions and antidepressant prescribing during the period of Coalition government (2010-15), using data at the local authority-level. Informed by the discussion in the previous chapters of this thesis, the research questions associated with this stage of the analysis are as follows:

[RQ 6.1]: Are benefit sanctions associated with higher rates of antidepressant prescribing at the local authority-level?

[RQ 6.2]: Does the observed relationship strengthen following the implementation of the Welfare Reform Act 2012?

Before investigating these research questions, the analysis in this chapter begins by considering the broader determinants of antidepressant prescribing beyond the key focus on benefit sanctions. This is an important exercise, as it helps to identify the additional explanatory variables that need to be included in the regression modelling stage of the analysis. Next, the chapter outlines the data and methods that will be used in the analysis, providing more specific detail to that previously outlined in Chapter 5. The results of the analysis are then presented and discussed. In the summary and discussion, consideration is given to the issue of whether the data and methods used permit causal inferences to be drawn from the results obtained. The analysis presented and discussed in this chapter forms the basis of the following journal article:

Williams, E. (2019). Unemployment, sanctions and mental health: the relationship between benefit sanctions and antidepressant prescribing. *Journal of Social Policy*, forthcoming. Available at: <https://doi.org/10.1017/S0047279419000783>

## 6.2 Determinants of antidepressant prescribing

Antidepressant medication is primarily prescribed to treat individuals who are suffering from anxiety and depression, and rates of prescribing have been on the rise in the UK in recent decades (Middleton et al., 2001). In the last ten years, for example, the number of antidepressant items prescribed more than doubled in England (NHS Digital, 2017), whilst the upward trend in prescribing increased following the 2008 recession (Barr et al., 2015). Increasing rates of antidepressant prescribing have been regularly reported upon in the media, though GPs themselves disagree on whether their rise is indicative of an over-medicalisation of common mental health problems or in fact represents an improved response to rising levels of anxiety and depression in the population (Reid, 2013; Spence, 2013). At the local authority-level, antidepressant prescribing is positively correlated with the prevalence of mental health problems such as anxiety and depression, as shown in Figure 5.1 in Chapter 5. As previously highlighted, however, the two do not correlate perfectly because not all individuals suffering from these mental health problems will actually be prescribed antidepressant medication. This might occur for a variety of reasons, which relate to: the likelihood that people recognise and report mental health problems; access to health services; GP prescribing behaviour; and the exploration of alternative treatments (Hyde et al., 2005).

This section provides an overview of the separate determinants of antidepressant prescribing, which will be used to inform the identification of additional explanatory variables that will be included in the regression analysis. Given the distinction that has been made between the prevalence of mental health problems and actual rates of antidepressant prescribing, which is a difference that is likely to vary by country, the discussion primarily draws on findings from UK-specific studies, as they will be most relevant to the statistical modelling carried out here. Existing quantitative research in the UK considers the factors that explain variations in prescribing rates at the GP practice-level, and takes into account a combination of the characteristics of registered patients and of the GP practice itself as well as area-level determinants. Spence et al. (2014), for example, find that antidepressant prescribing is higher in GP practices that have patients with a higher prevalence of depression, as well as



higher proportions of older people, women and white people. Morrison et al. (2009), furthermore, find that greater proportions of GPs who are female, young or born in the UK are factors that are associated with higher levels of antidepressant prescribing. In addition, recent rises in the overall numbers of people receiving antidepressants is partly explainable by changes in GP prescribing behaviour (Spence et al., 2014) as well as the increasing numbers of individuals that are receiving long-term treatment (Moore et al., 2009; Mars et al., 2017).

As highlighted in Chapter 5, data availability means that the analysis in this chapter is carried out solely at the local authority-level. Unfortunately, there is a risk involved in the selection of explanatory variables for a local authority-level analysis using empirical insights from research that is based at different levels, such as the individual and GP-level. Mars et al. (2017), for example, find that the prevalence of antidepressant prescribing increases with age, though there is no guarantee that the same relationship will hold when controlling for separate age proportions at the local authority-level. This is the reverse of the problem of ecological bias described in Chapter 5. Usefully, nevertheless, additional research considers area-level determinants, including differences by age, gender and ethnicity. Sreeharan et al. (2013), for example, find that antidepressant prescribing is higher in areas with higher proportions of older people and white people, though in contrast to previous findings, they also find it is lower in areas with higher female populations. A particularly important determinant is area-level socio-economic deprivation, where prescribing increases along with increased deprivation. The role of area-level deprivation is confirmed in additional research, which also identifies urban environments as an important factor associated with higher rates of prescribing (Morrison et al., 2009; McKenzie et al., 2013).

In addition to demographic characteristics, deprivation and urban-rural classification, area-level research highlights the influence of labour market and economic factors on rates of antidepressant prescribing. Barr et al. (2016), for example, find that antidepressant prescribing is higher in areas with higher unemployment rates, lower wages and lower rates of economic output. Indeed, this finding confirms the well-established link between factors such as

unemployment and poor mental health, as well as in relation to antidepressant prescribing, that is found at the individual-level (von Soest et al., 2012). As argued in Chapter 4, furthermore, the structure of the social security system itself is likely to play an important role in terms of both mental health and consequent impacts on antidepressant prescribing. As previously highlighted, for example, the particular focus of the study by Barr et al. (2016) is on work-related conditionality in the form of Work Capability Assessments (WCAs). In their study, rates of antidepressant prescribing are higher in local authorities with a greater cumulative proportion of WCAs for claimants of the main out-of-work disability benefit. The particular focus of this chapter, furthermore, is on benefit sanctions in relation to antidepressant prescribing. Chapter 4 argued that both the threat and actual imposition of a sanction is expected to impact mental health outcomes, operating through a mixture of material and psychosocial mechanisms. The specific data and methods used in the analysis are the subject of the next section.

## **6.3 Data and methods**

### **6.3.1 Analytic sample**

As outlined in Chapter 5, the analysis in this chapter and the following empirical chapters in this thesis focuses on the period of Coalition government (2010-15). Importantly, this represents the period of the ‘great sanctions drive’ described in Chapter 3, which - it has previously been argued - serves to limit the overall influence of omitted variable bias and reverse causality on the results obtained. Antidepressant prescribing data itself is only available from June 2010 onwards for GP practices in England (NHS Digital, 2018). Since the investigation in this chapter is carried out using quarterly data, the time period for the analysis begins at the third quarter of 2010 (Q3: July-September), which coincides with the early months of the Coalition government and the initial rise in rates of JSA sanctions. At the other end of the time period, furthermore, it is important to highlight the influence of the rollout of Universal Credit (UC) on the JSA claimant count and JSA sanctions figures. February 2015 marked the start of the national expansion of UC, which - as previously described in Chapter 3 - began to replace six existing means-tested benefits including JSA (DWP, 2015c). Importantly, the rollout of UC systematically altered the composition of the

remaining JSA claimant group by initially only being open to younger unemployed individuals without dependent children (DWP, 2014).

As Chapter 5 explained, however, data on UC claims and sanctions are not available for the time period in question and consequently cannot be included in the regression models. In order to minimise the potential influence of compositional change on the results of the analysis, therefore, data are included up to and including the fourth quarter of 2014, prior to the national rollout of UC. In the remaining pre-2015 sample, 31 local authorities were affected by the Pathfinder phase of UC that began in April 2013, which impacts 78 local authority quarters in the sample. These local authority quarters are removed in the analysis presented here, though the results were also re-run with them included as a sensitivity check. Importantly, the findings remain substantively unchanged with or without their inclusion, which is unsurprising given the small number of individuals actually claiming UC by December 2014. England is divided into 326 local authority districts, though the City of London and the Isles of Scilly are excluded from this analysis, since their small population size means that many observations for important variables are either missing from the data available or are highly unreliable. These exclusions imply that the analysis is based on 324 local authority districts across 18 quarters (Q3 2010 - Q4 2014). This provides a maximum sample size of 5,754 local authority quarters, once the 78 local authority quarters have been removed.

### **6.3.2 Dependent variable and falsification variable**

Data on the number of antidepressant items prescribed by GP practices were accessed from NHS Digital (2018), which publishes data at monthly intervals for all practices in England starting in June 2010. Comparable data for GP practices in Scotland are published, however are only publicly available from October 2015 onwards, which is beyond the time period of analysis. Antidepressant prescribing items were identified using Section 4.3 ‘Antidepressant Drugs’ of the British National Formulary (BNF) code (BMA and RPS, 2018). In particular, the analysis uses a subset of antidepressant items categorised within BNF Section 4.3.3 ‘Selective Serotonin Re-Uptake Inhibitors’ (SSRIs), which are the first-line medication for treating anxiety and depression (NICE, 2015). SSRIs are the most

appropriate indicator to capture impacts on anxiety and depression since the broader antidepressant measure includes items prescribed to treat non-psychiatric health conditions such as chronic pain (Spence et al., 2014). Prescription items themselves are single supplies of a medicine that generally refer to month-long prescriptions, though the length of prescription items will vary depending on the length of treatment or quantity of medicine prescribed (HSCIC, 2015). They nevertheless represent a standard measure of antidepressant prescribing rates and are used widely in epidemiological research (Middleton et al., 2001; Moore et al., 2009; Spence et al., 2014).

Using the GP practice-level data, rates of SSRI prescribing were constructed for each local authority in the sample. First, each GP practice was linked to the local authority in which it was located, using a postcode to local authority look-up table published by the ONS (2015). Whilst this method represents the most straightforward option of linking GP-level data to local authorities given the data available, the approach nevertheless suffers the weakness that not all patients will necessarily live in the local authority in which their GP practice is based. This limitation is likely to introduce some degree of uncertainty to the measure, though is unlikely to systematically bias the findings obtained. Next, quarterly rates of SSRI prescribing per 100,000 local authority population were constructed for each local authority by aggregating the monthly data across quarters and using mid-year population estimates available through Nomis (ONS, 2018b). A key limitation of the NHS Digital prescribing data is that they do not contain any patient-related information, meaning that it was not possible to construct prescribing rates per working age population, which is the group who are at risk of sanctioning if claiming JSA. Consequently, the SSRI prescribing rate - as well as sanctions and additional explanatory variables included in the analysis - are expressed as quarterly rates per 100,000 population.

In addition to the main dependent variable, a robustness check on the main results of the analysis is carried out in the form of a falsification test, using the non-equivalent dependent variable approach. The rationale behind this approach was explained in detail in Chapter 5, though it is worth briefly re-iterating that it tests for omitted variable bias by identifying an additional dependent variable that should not be affected by sanctions but that could be influenced by the

same potential unobserved confounding factors as for SSRI prescribing rates. Following Barr et al. (2016), the rate of cardiovascular drug prescribing (BNF Section 2.0) is used as a non-equivalent dependent variable, on the basis that it is unlikely that the health conditions treated by such items will be affected by sanctions, especially in the short-term. Cardiovascular prescribing is arguably not an arbitrary choice of variable, however, as it might be expected to be affected by potential unobserved confounders to SSRI prescribing, such as changes in access to primary healthcare across the study period, or changes in the propensity of individuals to report health problems to their GP. Similar to the other variables in the analysis, rates of cardiovascular prescribing are calculated as quarterly rates per 100,000 local authority population.

### **6.3.3 Sanctions data and additional explanatory variables**

Data on the monthly number of JSA sanctions at the local authority-level were accessed from Stat-Xplore (DWP, 2018c), and aggregated into quarterly rates per 100,000 population. The various limitations of the sanctions data that are available were explained in detail in Chapter 5, and will not be repeated in full here. It is worth re-iterating, nevertheless, that the sanctions measures that are available can be thought of as capturing the combined threat and imposition effects of sanctions, as opposed to uniquely identifying one or the other. Indeed, given that the analysis is carried out at the ecological-level, the sanction impacts that are observed do not necessarily identify effects on JSA claimants alone, but may also capture any wider impacts on the friends and family of sanctioned individuals. Sanctions data, furthermore, do not pertain to individuals, meaning that the same claimant could have received more than one sanction in the same quarter. As previously highlighted, three sanctions indicators are investigated as part of the analysis. The main sanctions indicator that will be used is referred to as original adverse sanctions, which represents original decisions to impose a sanction that were not later reviewed, reconsidered and/or appealed. This indicator is prevalent in the wider quantitative literature into benefit sanctions, and is used throughout the analysis in this chapter and the other empirical chapters in this thesis. As a sensitivity check, nevertheless, two additional indicators are briefly considered

in this chapter, referred to as total adverse sanctions and sanction referrals as previously explained.

Additional explanatory variables are included in the analysis, informed by the discussion in Section 6.2. To capture the role of demographic characteristics, data on age and gender were accessed through Nomis (ONS, 2018b), which publishes annual local authority-level data. Data on the proportion of separate ethnic groups were also accessed through Nomis, which publishes annual estimates that are updated at quarterly intervals. Robust data were only available for proportions of white UK born individuals and so this is the particular variable that is included in the analysis. Next, measures were sought to account for variations in local labour market and economic conditions. Data on the number of JSA claimants were accessed from Nomis, which publishes data at monthly intervals at the local authority-level, which were then averaged across the quarter to provide quarterly estimates. In addition, local authority data on unemployment, employment and economic inactivity were accessed through Nomis, which publishes annual estimates that are updated each quarter. In contrast to the claimant count, the ILO unemployment rate includes both claimants and unemployed non-claimants, and therefore provides a broader measure of unemployment. As will be explained in Section 6.4, the claimant count and unemployment rate were included separately in the initial modelling process, with the unemployment rate ultimately preferred.

Annual rates of GVA per head - which is a local authority-level equivalent of GDP that measures economic activity - were accessed through the ONS (2018b). Whilst previous research had indicated that wages would also be an important determinant of antidepressant prescribing at the local authority-level, their inclusion alongside GVA in this analysis did not add to the explanatory power of the estimated models and so were not ultimately included. Indeed, since GVA is calculated using an income approach that includes data on the compensation of employees, this result is arguably not a surprising one. In addition to demographic and economic factors, quarterly rates of Work Capability Assessments (WCAs) were accessed from Stat-Xplore and quarterly rates of antibiotic prescribing (BNF Section 5.1) were accessed through NHS Digital (2018). Spence et al. (2014) use antibiotic prescribing as a proxy for the

propensity of GPs to prescribe in general, since antibiotics are seen to reflect discretionary prescribing behaviour. Areas with higher rates of antibiotic prescribing are therefore also expected to have higher levels of antidepressant prescribing.

Finally, data from the Index of Multiple Deprivation (IMD) 2010 were accessed from the Department for Communities and Local Government (DCLG, 2011) and data on rural-urban classification 2011 were accessed from the Department for Environment, Food and Rural Affairs (Defra, 2014). In contrast to the previous explanatory variables discussed, these two measures are time-invariant in that they are measured only once during the period that is analysed. The next subsection details how the influence of these two variables is considered within the statistical analysis itself.

In terms of measurement, the IMD captures levels of deprivation based on an aggregation of separate indicators (Noble et al., 2006). The IMD 2010 ranks small areas (LSOAs) in England according to an overall score based on separate dimensions: income; employment; health and disability; education and training; barriers to housing and services; living environment; and crime. The ranking of local authorities is calculated by the DCLG using a population-weighted average of the combined LSOA scores. In the current analysis, the overall ranking is divided into quintiles that each represent 20% of local authorities. Importantly, the health domain of the IMD is partly determined by the number of people suffering from anxiety or mood-related problems, and so the inclusion of the IMD in the analysis can be thought of in some sense as controlling for initial differences in mental health that exist between local authorities. In terms of the rural-urban classification, data from the 2011 census are used by Defra to categorise local authorities into six separate groups based on the proportion of the resident population in each area that live in rural or urban settings. In this analysis, these six separate categories are collapsed into three broader categories that include: predominantly rural; urban with significant rural; and predominantly urban.

Table 6.1 summarises the separate variables that are included in the analysis, their data source and summary statistics.

**Table 6.1:** summary statistics for 324 local authorities, Q3 2010 – Q4 2014

	N	Mean	St.d Dev.	Min.	Max.	Source
<i>Dependent variable:</i>						
SSRI prescribing	5,754	12,946	3,411	5,114	28,830	NHS Digital
<i>Sanctions variables:</i>						
Original adverse	5,754	223	139	9	969	Stat-Xplore
Adverse	5,754	258	162	9	1,125	Stat-Xplore
Referrals	5,754	481	298	43	2,369	Stat-Xplore
<i>Control variables:</i>						
Claimants	5,754	1,851	964	287	6,033	Nomis
Unemployment	5,459	3,514	1,393	603	10,044	Nomis
Economic Inactivity	5,754	13,809	3,133	5,618	25,575	Nomis
Employment	5,754	45,363	3,587	28,553	59,802	Nomis
Work Capability Assessments	5,754	248	129	26	1,173	Stat-Xplore
GVA	5,754	22,886	14,435	11,876	235,244	ONS
Age						Nomis
0-15 year olds	5,754	18,586	1,837	13,712	26,967	
16-29 year olds	5,754	17,358	3,846	11,644	32,959	
30-49 year olds	5,754	27,132	2,817	18,670	37,897	
50-64 year olds	5,754	18,741	2,433	9,145	24,038	
65 and above	5,754	18,182	4,385	6,018	31,854	
Female	5,754	50,829	697	45,813	52,562	Nomis
White UK born	5,754	82,636	15,482	13,921	99,042	Nomis
Antibiotics prescribing	5,754	17,347	3,117	8,788	38,915	NHS Digital
Index of Multiple Deprivation						DCLG
Quintile 1	1,166					
Quintile 2	1,157					
Quintile 3	1,140					
Quintile 4	1,165					
Quintile 5	1,126					
Rural-Urban Classification						Defra
Predominantly rural	1,620					
Urban with significant rural	959					
Predominantly urban	3,175					
<i>Falsification variable:</i>						
Cardiovascular prescribing	5,754	144,487	36,595	58,061	288,986	NHS Digital

*Note:* suppression of values for the unemployment estimates leads to the fall in the sample size.



All variables in Table 6.1 are measured as rates per 100,000 population, except GVA per head, IMD and rural-urban classification, the latter two of which are categorical variables.

As the discussion has indicated, the additional explanatory variables that are included in the regression analysis represent important determinants of antidepressant prescribing, and their inclusion therefore helps explain variation in antidepressants throughout the period over and above that which is explainable by sanctions alone. Importantly, furthermore, some of the explanatory variables detailed in Table 6.1 are factors that have been identified as influencing sanction rates themselves, and their inclusion therefore helps to limit the influence of omitted variable bias on the results of the analysis. As explained in Chapter 5, omitted variable bias occurs when a variable that is a determinant of both the dependent variable and the key independent variable of interest is not included in the analysis. In the current context, this would be the case if the omitted variable is a characteristic of local authorities that drives both antidepressant prescribing rates and sanction rates. Although research in this area is limited, the most recent investigation carried out at the local authority-level finds that younger people, men and ethnic minorities are more likely to be sanctioned than other groups (de Vries et al., 2017). As the authors emphasise, the reasons that explain why these demographic inequalities in rates of sanctioning occur remain unexplained, though they highlight possible explanations in the form of structural factors, differences in claimant behaviour and differential treatment of claimants by caseworkers. Whatever the reason, their inclusion here helps limit the concern that the estimated relationship between sanctions and antidepressant prescribing is in fact explained by the influence of some omitted third variable.

In contrast to the need to control for confounding factors, the possibility exists that there is a risk in over-controlling for factors that are relevant to sanctions and mental health outcomes. For example, sanctions could positively affect mental health through the short-term employment impacts discussed in Chapter 4, which would not be captured in the analysis because it controls for employment. This issue is complicated, however, by countervailing influences. As also discussed in Chapter 4, for example, the literature on the social

determinants of health does not suggest that all employment is conducive to good mental health, but rather that it is the quality of employment that is important. Indeed, the available evidence suggests that sanctions are associated with negative impacts on job quality, in terms of wages, stability and hours (Arni et al., 2013; van den Berg and Vikström, 2014), which might be expected to incur adverse mental health impacts (Allen et al., 2014; WHO, 2014; Silva et al., 2016). The increased short-term employment effect of sanctions for some claimants, furthermore, has also been observed to be smaller than increases in economic inactivity for other claimants (Arni et al., 2013; Busk, 2016). This latter effect might also be expected to have adverse mental health impacts. To better consider the complex interactions between sanctions, the labour market and mental health outcomes, sufficiently detailed individual-level data are necessary. This issue is further discussed in the conclusion to this thesis. To consider the risk of over-controlling in this chapter, the main analysis will be re-run without the labour market control variables included. The results of this sensitivity check are discussed in the results section.

### 6.3.4 Statistical approach

The first stage of the analysis investigates the relationship between sanctions and antidepressant prescribing across the time period, which responds to the first research question identified in the introduction to this chapter. To do so, fixed effects models are estimated, as described in a basic form in Equation 6.1:

$$SSRI_{i,t} = \beta_0 + \beta_1 Sanctions_{i,t} + \beta' X_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \quad (6.1)$$

In Equation 6.1,  $i$  denotes the local authority and  $t$  denotes the quarter.  $SSRI$  is the SSRI prescribing rate per 100,000 population,  $Sanctions$  is the JSA sanctions rate per 100,000 population and  $X$  represents a vector of additional explanatory variables. The remaining symbols are as follows:  $\mu$  denotes local authority fixed effects;  $\lambda$  denotes time fixed effects; and  $\varepsilon$  represents the error term. The inclusion of local authority fixed effects controls for time-invariant unobserved differences between local authorities, meaning that the analysis ultimately estimates the average association between sanctions and antidepressants within local authorities across the time period. The inclusion of time fixed effects, furthermore, controls for the influence of factors that are constant across local

authorities but that vary over time, such as national policy changes affecting all local authorities or national trends in antidepressant prescribing. The advantage of time fixed effects over the inclusion of linear and quadratic time trends is that it is not necessary to impose a particular functional form on the relationship between SSRI prescribing and time. All estimated models use Driscoll-Kraay standard errors (Driscoll and Kraay, 1998), which are robust to heteroscedasticity, correlation through time within local authorities as well as general forms of cross-sectional dependence (Hoechle, 2007).

As part of the initial modelling process, three separate regression models were first estimated, which are detailed in Table 6.2. The discussion here broadly describes this process of model selection, whilst the next section will discuss the results of the model that is ultimately preferred. First, Model 6.1 estimates a fixed effects model that incorporates the sanctions rate as well as a number of additional time-variant explanatory variables, which include: rates of unemployment and economic inactivity (with rates of employment left out due to perfect collinearity between the three variables); Work Capability Assessments (WCAs); GVA; age, gender and ethnicity; and antibiotic prescribing.

Next, Model 6.2 repeats this model but also includes the two time-invariant variables that are available for the analysis: the Index of Multiple Deprivation quintiles; and rural-urban classification. In fixed effects regression models, time-invariant variables are perfectly collinear with the local authority fixed effects, and are therefore automatically excluded from the model itself. However, it is possible to include separate time trends by quintile of deprivation and rural-urban classification, by interacting the two time-invariant variables with time. This interaction captures the possibility that the influence of deprivation and rurality on antidepressant prescribing may have changed over the period, which is an issue that may have emerged due to the uneven impact of austerity policies or additional factors (Gray and Barford, 2018; Thomson et al., 2018). Indeed, this approach is carried out elsewhere in the UK literature that considers the impacts of welfare reform over a similar period (see, for example, Barr et al., 2016). The coefficients for these interactions estimate how the effect of deprivation and rurality change over the period, with their main baseline effects absorbed into the local authority fixed effects as in Equation 6.1 (Allison, 2009).

**Table 6.2:** relationship between sanctions and SSRI prescribing, initial model selection

	Model 6.1: Fixed effects	Model 6.2: Fixed effects	Model 6.3: Random effects
Sanctions	0.465* (0.206)	0.371*** (0.079)	0.478** (0.180)
Unemployment	-0.012 (0.015)	-0.013 (0.012)	-0.013 (0.013)
Economic Inactivity	0.009*** (0.002)	0.005* (0.002)	0.009 (0.008)
WCAs	0.440 (0.595)	0.199 (0.412)	0.528*** (0.162)
GVA	-0.054*** (0.013)	-0.021* (0.008)	-0.038** (0.013)
Age			
16-29	-0.001 (0.067)	-0.168*** (0.035)	0.104 (0.100)
30-49	-0.261* (0.090)	-0.589*** (0.075)	-0.147 (0.137)
50-64	-0.208** (0.071)	-0.519*** (0.072)	-0.020 (0.142)
65 and over	0.144** (0.047)	0.011 (0.034)	0.229* (0.095)
Female	0.145 (0.108)	0.558*** (0.069)	0.083 (0.161)
White UK born	0.002 (0.003)	-0.0001 (0.002)	0.015* (0.006)
Antibiotic Prescribing	0.111*** (0.019)	0.086*** (0.015)	0.133*** (0.021)
Index of Multiple Deprivation			
Quintile 2			370.56 (368.396)
Quintile 3			1,268.77*** (371.098)
Quintile 4			2,215.73*** (398.461)
Quintile 5			3,052.26*** (469.230)
Urban-Rural Classification			
Urban with significant rural			-419.259 (405.112)
Predominantly urban			-1224.087*** (362.712)

Index of Multiple  
Deprivation

Quintile 2 × Quarter		37.508*** (2.112)	
Quintile 3 × Quarter		60.046*** (3.042)	
Quintile 4 × Quarter		75.667*** (4.671)	
Quintile 5 × Quarter		114.015*** (7.715)	
Urban-Rural Classification			
Urban with significant rural × Quarter		-22.709*** (2.189)	
Predominantly urban × Quarter		-30.276*** (3.388)	
$R^2$ (within)	0.866	0.889	0.865
LA Quarters	5,459	5,459	5,459

*Note:* Robust standard errors in brackets. Model 6.1 and Model 6.2 include local authority and time fixed effects. Model 6.3 includes time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Arguably, the comparison of Model 6.1 and Model 6.2 motivates the inclusion of the IMD and rural-urban interactions in the analysis. The coefficients for the interactions are statistically significant at the 0.1% level ( $p < 0.001$ ), whilst the within- $R^2$  increases from 0.866 to 0.889. This indicates that Model 6.2 explains more of the variance in antidepressant prescribing within local authorities than Model 6.1. In an additional sensitivity test, finally, Table 6.2 details the results of a random effects model in Model 6.3. As explained in Chapter 5, random effects models provide an alternative to fixed effects in that they are able to take into account both within-group and between-group variation. They are able to adjust for time-invariant factors, for example, by making the relatively stricter assumption that any omitted variables are uncorrelated with the included explanatory variables. A Hausman test of this assumption, however, indicates that a random effects framework should not be favoured over fixed effects in the current analysis ( $p < 0.001$ ). Consequently, out of the three models discussed here, Model 6.2 is ultimately favoured for the purposes of the analysis, the results of which will be discussed in more depth in the following section. A full set of regression diagnostic checks of this model are detailed in the appendix for Chapter 6 (Section A6.1), which will also be referred to in the next section.

The next stage of the analysis considers the second research question that is outlined in the introduction to this chapter. It investigates the impact of the Welfare Reform Act 2012, which introduced a harsher sanctions regime by increasing the average length of sanctions that could be imposed. The reforms, which are detailed in Chapter 3, increased both the minimum and maximum length of JSA sanctions, meaning that the sanctions data used in this analysis are qualitatively different in the post-reform period. To capture this effect, Equation 6.2 modifies the initial fixed effects analysis through inclusion of an interaction term between *Sanctions* and *Reform*:

$$SSRI_{i,t} = \beta_0 + \beta_1 Sanctions_{i,t} + \beta_2 (Sanctions_{i,t} * Reform_t) + \beta' X_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \quad (6.2)$$

In Equation 6.2, *Reform* is a dummy variable that marks the quarters before and after the implementation of the harsher sanctions regime brought about by the Welfare Reform Act 2012. It is coded 1 for quarters Q4 2012 onwards and 0 before that date. The post-reform relationship between sanctions and

antidepressant prescribing is then arrived at through calculating a linear combination of  $\beta_1$  and  $\beta_2$ , using the Stata command ‘`lincomest`’ created by Newson (2002) (see also, Reeves and Loopstra, 2017). The remainder of this chapter details the results of these fixed effects regressions and then goes on to discuss the implications of the findings obtained.

## 6.4 Results

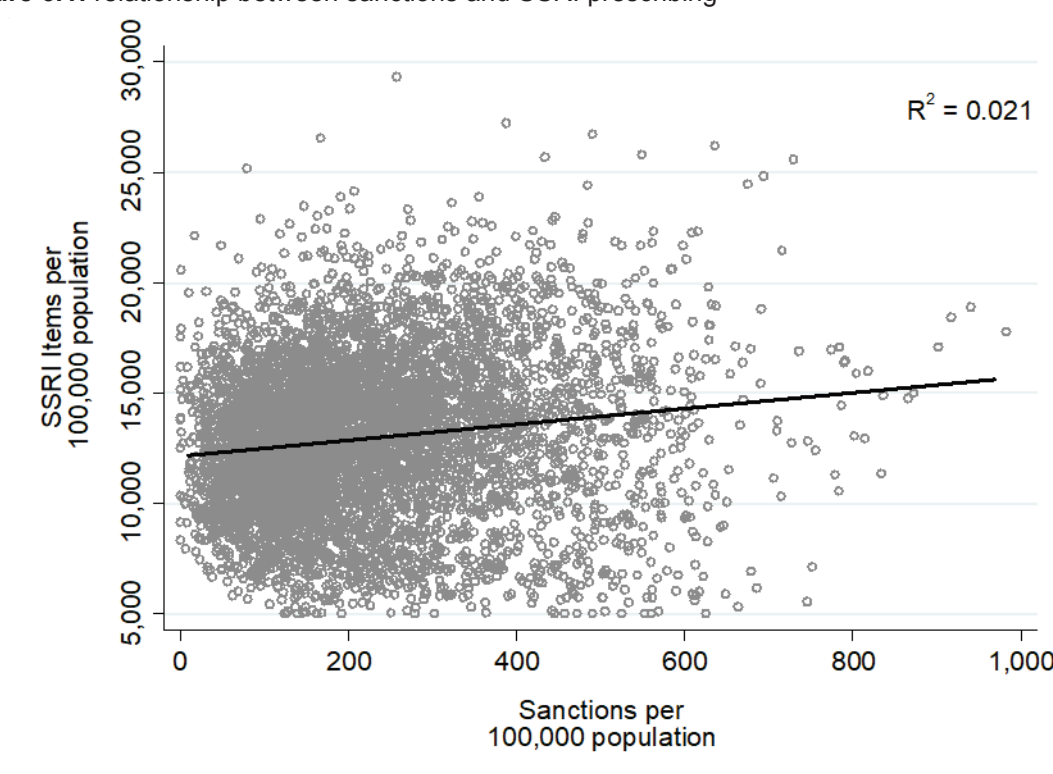
### 6.4.1 Summary statistics

Table 6.1, above, details the summary statistics for the variables that are included in the analysis. It indicates that across the sample there are an average of 223 sanctions and 12,946 SSRI items per 100,000 population per quarter in local authorities. There is a large degree of variation, nevertheless, in rates of both antidepressant prescribing and sanctions around these averages. Blackpool, for example, stands out in particular with an average of 520 sanctions and 24,567 SSRI items per 100,000 population per quarter, which are 2.3 and 1.9 times the respective averages. Beatty and Fothergill (2013) estimate that, along with other seaside areas (Torbay, Hastings, Great Yarmouth and Thanet), Blackpool was badly hit in financial terms by Coalition government welfare reforms. All of these areas display high rates of both antidepressant prescribing and sanctions in the sample. Beatty and Fothergill (2013) argue that the increased exposure of these local authorities to welfare reform is due to the high proportions of working-age adults claiming out-of-work benefits in these areas, who migrated there due to the availability of cheap private rental sector accommodation. There is a risk that the inclusion of these local authorities plays an undue influence on the results of the main analysis. In a sensitivity test in the Chapter 6 appendix, therefore, these local authorities are removed from the sample and the results compared with those of the main findings. The substantive results remain unchanged, however, and so this section discusses the findings from the full sample.

Elsewhere in England, local authorities that have high rates of sanctions are not necessarily the same local authorities that have high rates of SSRI prescribing, and vice versa. Several local authorities in London, for example, exhibit quite high rates of sanctions but very low rates of antidepressant prescribing,

indicating that additional factors are influential. Evidently, therefore, sanctions and antidepressant prescribing are not expected to correlate perfectly, though there is nevertheless an expectation that higher rates of sanctions will on average be correlated with higher rates of antidepressant prescribing. Indeed, this is confirmed in the scatterplot in Figure 6.1, which depicts the correlation between sanctions and SSRI prescribing in each local authority quarter across the period. Figure 6.1 indicates that in local authority quarters where the rate of sanctioning is higher, so too are rates of SSRI prescribing ( $r = 0.146$ ;  $p < 0.001$ ). Whilst the association is positive, the actual correlation is quite low. A basic linear regression model using the two variables indicates that only 2.1% of the variance in SSRI prescribing is explained by sanctions. This motivates the need to examine the relationship more formally as part of an expanded regression framework, which is carried out in the remainder of this section.

**Figure 6.1:** relationship between sanctions and SSRI prescribing



*Note:* quarterly rates for 324 local authority districts, Q3 2010 – Q4 2014

#### 6.4.2 Regression models: full time period

Following the process of model selection that was explained in the previous section, Table 6.3 reproduces the results of Model 6.2 for the purposes of the current discussion.



**Table 6.3:** relationship between sanctions and SSRI prescribing, fixed effects model

	Model 6.2
Sanctions	0.371*** (0.079)
Unemployment	-0.013 (0.012)
Economic Inactivity	0.005* (0.002)
WCAs	0.199 (0.412)
GVA	-0.021* (0.008)
Age	
16-29	-0.168*** (0.035)
30-49	-0.589*** (0.075)
50-64	-0.519*** (0.072)
65 and over	0.011 (0.034)
Female	0.558*** (0.069)
White UK born	-0.0001 (0.002)
Antibiotic Prescribing	0.086*** (0.015)
Index of Multiple Deprivation	
Quintile 2 × Quarter	37.508*** (2.112)
Quintile 3 × Quarter	60.046*** (3.042)
Quintile 4 × Quarter	75.669*** (4.671)
Quintile 5 × Quarter	114.015*** (7.715)
Urban-Rural Classification	
Urban with significant rural × Quarter	-22.709*** (2.189)
Predominantly urban × Quarter	-30.276*** (3.388)
$R^2$ (within)	0.889
LA Quarters	5,459

*Note:* Robust standard errors in brackets. Model includes local authority and time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Importantly, Model 6.2 indicates that sanctions are associated with increases in antidepressant prescribing rates. For every 10 additional sanctions applied per 100,000 population, the rate of SSRI prescribing is 3.71 items higher per 100,000 population, a result that is statistically significant at the 0.1% level ( $p < 0.001$ ). The 95% confidence interval around this estimate, furthermore, ranges from 2.03 to 5.38. A full diagnostic check of Model 6.2 is carried out in the appendix for Chapter 6 (Section A6.1). As Section A6.1 details, the diagnostic checks do not highlight any obvious issues that would undermine the results obtained. In addition, the models themselves use standard errors that are robust to heteroscedasticity, correlation through time within local authorities as well as general forms of cross-sectional dependence, as previously indicated. The additional explanatory variables included in Model 6.2, furthermore, generally conform to the expected relationship with antidepressant prescribing as informed by Section 6.2. That is, increases within local authorities in rates of economic inactivity, females, antibiotic prescribing and WCAs are associated with increases in SSRI prescribing, though this latter result is non-significant at the 5% level. Increases in GVA per head, furthermore, are associated with decreases in SSRI prescribing.

In contrast to these variables, however, several of the estimated coefficients in Model 6.2 require additional attention. A zero coefficient, for example, is observed for rates of white UK born, whereas the expectation is for a positive relationship to exist. In this situation, it is useful to compare the Model 6.2 coefficient for white UK born with the estimated random effects coefficient in Model 6.3 in Table 6.2. In Model 6.3, the association is positive and significant at the 5% level. This suggests that the zero coefficient in Model 6.2 is explained by the fact that fixed effects models are not well-placed to estimate the effects of slowly changing variables, which is something that random effects models are better able to achieve (Plümper and Troeger, 2007). The next counterintuitive result relates to the separate age group coefficients, which are progressively negative before becoming positive and non-significant for the proportion aged 65 and over. The age groupings are affected by a similar issue as described for the white UK born variable, in that they exhibit a low degree of quarterly variation through the time period. In addition, however, pairwise correlations indicate that the separate age groups are affected by a high degree of multicollinearity

with one another. To check for the potential influence of this multicollinearity on the key sanctions coefficient, the results of Model 6.2 were re-run without the age groupings included, though this exclusion did not affect the main substantive results and so are included in Model 6.2 in any case.

Perhaps the key counterintuitive result in Model 6.2 that requires additional discussion is the negative and non-significant coefficient for the rate of unemployment, given the well-established link between unemployment and poor mental health, which also exists with antidepressant prescribing at the individual-level (von Soest et al., 2012). As previously discussed, there are two unemployment-related variables available for the analysis: the JSA claimant count; and the ILO unemployment rate. Model 6.2 includes the ILO unemployment rate. In a separate model that uses the JSA claimant count, however, the estimated claimant count coefficient is negative and statistically significant at the 5% level (Table A6.2 in the Chapter 6 appendix (Section A6.2)). In this model, the sanctions coefficient remains substantively unchanged. In a further sensitivity check, the results are re-run without the labour market control variables included, to consider the previously discussed risk of over-controlling (Table A6.3 in the Chapter 6 appendix (Section A6.3)). Again, the sanctions coefficient remains substantively unchanged.

The ILO unemployment rate is favoured for use in the main analysis because it represents a wider measure of unemployment than the claimant count. The claimant rate also has a high degree of collinearity with the rate of sanctions ( $r = 0.793$ ). This is of concern, due to the fact that when two variables are highly and positively correlated, it is likely that their slope coefficient estimators will be highly and negatively correlated (Berry and Feldman, 1985). The correlation between ILO unemployment and sanctions is lower ( $r = 0.586$ ), suggesting that multicollinearity is not driving the counterintuitive result of concern.

Indeed, this view is confirmed by the additional regression results detailed in the Chapter 6 appendix (Section A6.4, Table A6.4). In Table A6.4, the results of Model 6.2 are re-run with unemployment excluded (Model A6.6) and with sanctions excluded (Model A6.7), with little observed impact on the estimated coefficients. A possible explanation of the counterintuitive result under

discussion therefore, arguably relates to the previously identified risk of ecological bias on the findings of the analysis. Aggregate-level analyses are limited by the fact that correlations that hold at the area-level do not necessarily apply at the individual-level. The fact that unemployment has been observed to have a positive relationship with antidepressant prescribing at the individual-level in previous research, therefore, does not imply that the same will apply at the local authority-level. Indeed, existing area-level research into the relationship between unemployment and antidepressant prescribing finds contradictory results (Lundin and Hansson, 2014; Spence et al., 2014; Barr et al., 2016). This issue in relation to unemployment highlights an important limitation on the results of the current study more broadly, and emphasises the need for additional individual-level analysis to better understand the relationships that are investigated throughout this thesis.

Before moving on to discuss how the relationship between sanctions and antidepressant prescribing is affected by the Welfare Reform Act 2012, an additional sensitivity check is carried out that contrasts the results in Model 6.2 with the two additional sanctions indicators described in Section 6.3. The results are detailed in Table 6.4, which reports the coefficients for the separate sanctions indicators. Each model includes the same explanatory variables as in Model 6.2, though these are not displayed in the table itself. As Table 6.4 make clear, the original adverse sanctions variable (Model 6.2) provides the strongest estimated relationship between sanctions and antidepressant prescribing. The sanctions coefficient falls from 0.371 in Model 6.2, to 0.283 using the total adverse sanction rate to 0.211 using the referrals figure. All three of the sanctions coefficients are statistically significant at the 1% level. Comparison of the different sanctions measures provides a useful sensitivity check on the results of the main analysis, which is arguably supported by the fact that the estimated relationship with antidepressant prescribing is attenuated - but nevertheless still positive - when the two additional sanction rates are used. This is because the additional sanctions indicators provide fuzzier measures than the original adverse rate. Total adverse sanctions, for example, include adverse sanctions that were originally applied in previous time periods, whilst referrals include several types of sanction decisions in addition to adverse decisions (see Chapter 5).

As highlighted in Chapter 5, the original adverse sanctions rate used in Model 6.2 is widely applied in the UK quantitative literature that investigates the impacts of benefit sanctions. Despite underestimating the true adverse sanctions rate, it nevertheless provides the best figure given the available data, and will be used throughout the remainder of the empirical analyses in this thesis.

**Table 6.4:** relationship between sanctions and SSRI prescribing, comparison of measures

	Original Adverse (Model 6.2)	Total Adverse	Referrals
Sanctions	0.371*** (0.079)	0.283** (0.075)	0.211*** (0.043)
$R^2$ (within)	0.889	0.889	0.889
LA Quarters	5,459	5,459	5,459

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and additional explanatory variables not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

### 6.4.3 Regression models: influence of the Welfare Reform Act 2012

Next, the analysis examines whether the observed association between sanctions and SSRI prescribing is stronger in the post-reform period. The full results of this fixed effects regression model are displayed in Model 6.4 in Table 6.5, which reproduces Model 6.2 for ease of comparison. The results indicate that in the pre-reform period, for every 10 additional sanctions applied per 100,000 population the rate of SSRI prescribing is 1.74 items per 100,000 population higher, though this result is non-significant at the 5% level. Following the implementation of the harsher sanctions regime, however, the association increases by 2.82 prescribing items, so that every 10 additional sanctions applied per 100,000 population are associated with 4.57 additional SSRI prescribing items ( $p < 0.001$ ). The 95% confidence interval around this estimate, furthermore, ranges from 2.14 to 6.99. The coefficients for the additional explanatory variables, furthermore, are very similar between Model 6.4 and Model 6.2. Importantly, the results in Model 6.4 are consistent with the expectation that the harsher sanctions regime brought about following the implementation of the Welfare Reform Act 2012 would have a stronger association with antidepressant prescribing than the pre-reform period. Indeed, they indicate that the sanction effect observed in the Model 6.2 for the full time period are in fact driven by the influence of the post-reform period.

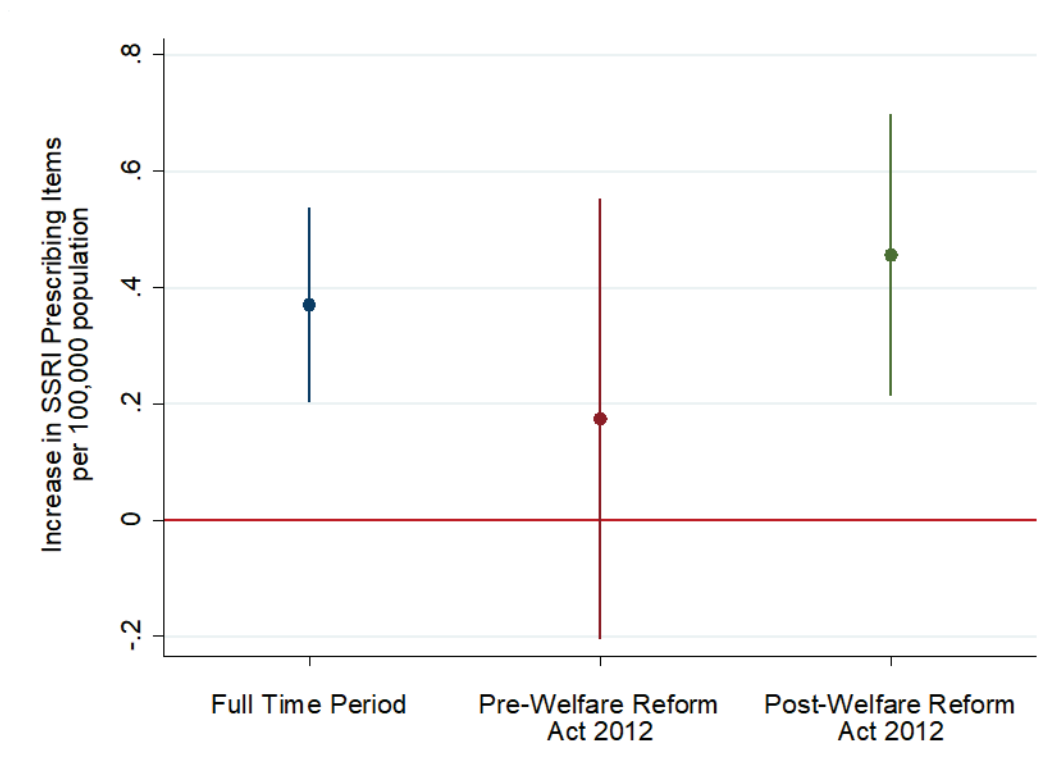
**Table 6.5:** relationship between sanctions and SSRI prescribing, fixed effects models

	Model 6.2	Model 6.4
Sanctions	0.371*** (0.079)	0.174 (0.179)
Sanctions x Reform		0.282 (0.252)
Unemployment	-0.013 (0.012)	-0.013 (0.012)
Economic Inactivity	0.005* (0.002)	0.005* (0.002)
WCAs	0.199 (0.412)	0.186 (0.413)
GVA	-0.021* (0.008)	-0.021* (0.008)
Age		
16-29	-0.168*** (0.035)	-0.170*** (0.035)
30-49	-0.589*** (0.075)	-0.590*** (0.076)
50-64	-0.519*** (0.072)	-0.525*** (0.073)
65 and over	0.011 (0.034)	0.014 (0.035)
Female	0.558*** (0.069)	0.548*** (0.063)
White UK born	-0.0001 (0.002)	-0.0001 (0.002)
Antibiotic Prescribing	0.086*** (0.015)	0.084*** (0.014)
Index of Multiple Deprivation		
Quintile 2 × Quarter	37.508*** (2.112)	36.465*** (2.271)
Quintile 3 × Quarter	60.046*** (3.042)	57.878*** (3.400)
Quintile 4 × Quarter	75.669*** (4.671)	72.009*** (5.015)
Quintile 5 × Quarter	114.015*** (7.715)	107.853*** (8.275)
Urban-Rural Classification		
Urban with significant rural × Quarter	-22.709*** (2.189)	-23.077*** (2.128)
Predominantly urban × Quarter	-30.276*** (3.388)	-30.891*** (3.212)
$R^2$ (within)	0.889	0.889
LA Quarters	5,459	5,459

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

The results detailed in Table 6.5 are summarised in Figure 6.2, which displays the estimated sanctions coefficients for the full time period (Model 6.2), as well as the pre- and post-Act periods (Model 6.4).

**Figure 6.2:** relationship between sanctions and SSRI prescribing, sanctions coefficients



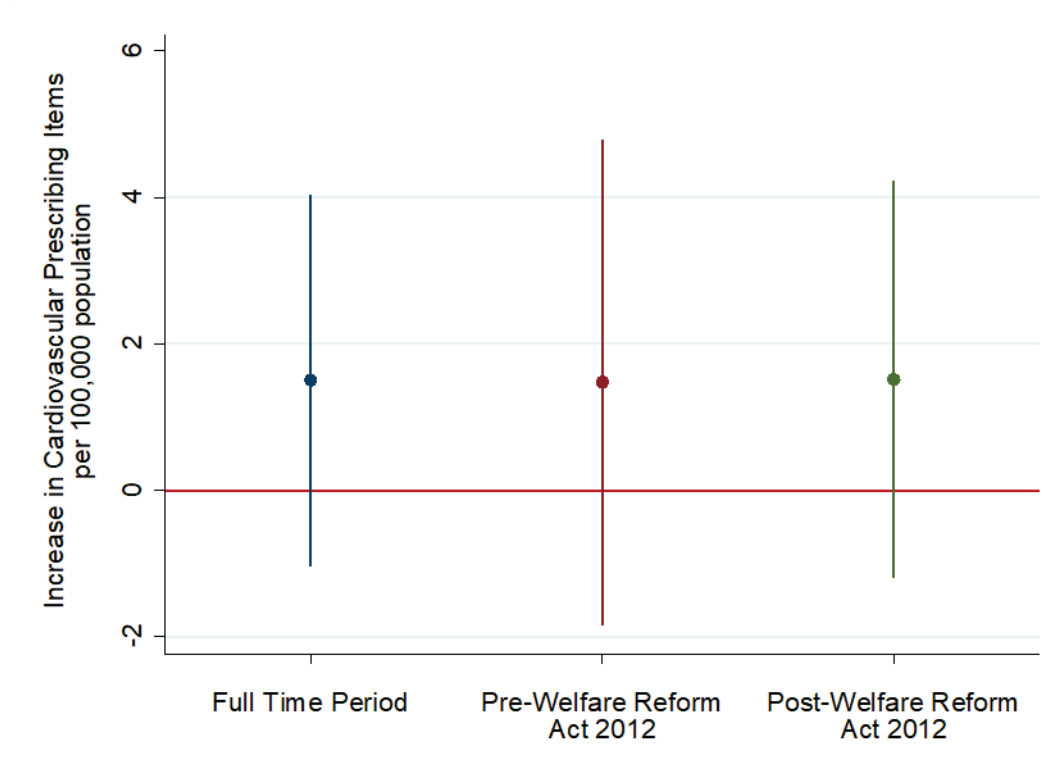
Note: Point estimates for sanctions are derived from Table 6.5  
Vertical bars represent 95% confidence intervals

#### 6.4.4 Robustness tests

In addition to the various sensitivity checks that have already been discussed, two further tests are carried out to consider the robustness of the main substantive findings. First, as previously explained, a falsification test is carried out using cardiovascular prescribing as a non-equivalent dependent variable. The full results of the falsification test are detailed in the appendix to Chapter 6 (Table A6.5 in Section A6.5), which re-runs the regression analyses previously carried out in Model 6.2 and Model 6.4. The results for the sanctions coefficients are displayed in Figure 6.3. Assuming that cardiovascular prescribing provides a good choice of non-equivalent dependent variable, the substantive results of the main analysis are supported by the fact that no statistically significant relationship is found between sanctions and cardiovascular prescribing in Table A6.5, either across the time period (Model A6.8) or in the pre- and post-Welfare Reform Act periods (Model A6.9). Whilst the estimated coefficients are positive,

the effect size does not increase in the post-reform period in the manner observed in the main results. Arguably, therefore, the results of this falsification test strengthen the claim that the findings from the main analysis are not driven by omitted variable bias.

**Figure 6.3:** relationship between sanctions and cardiovascular prescribing, sanctions coefficients



Note: Point estimates for sanctions are derived from Table A6.5  
Vertical bars represent 95% confidence intervals

Second, a Granger test for reverse causality is carried out, which considers whether the main analysis specifies the correct direction of causal inference. As discussed in Chapter 5 in terms of individual-level relationships, sanctions are expected to negatively impact the mental health of claimants. It might also be true, however, that claimants who are already suffering from mental health problems are themselves more likely to be sanctioned. This possibility may also apply at the local authority-level in the context of the current analysis. That is, there might be an increased risk of sanctions in areas with higher levels of individuals already suffering from poor mental health who are already being prescribed antidepressants. In this scenario, there is a risk that the substantive results discussed throughout this chapter overstate the direct effect of sanctions on mental health. As explained in Chapter 5, the Granger test provides a useful test of causality based on the predictive content of variables, though it cannot



by itself be used to rule out reverse causality entirely. Here, using a lag structure of four quarters, sanctions are found to Granger-cause SSRI prescribing ( $p < 0.01$ ) whilst SSRI prescribing is not found to Granger-cause sanctions ( $p = 0.775$ ). This result supports the local authority-level inferences made in the main analysis, though clearly individual-level data and a different research design are necessary to fully account for this issue.

## 6.5 Conclusion

The results discussed in this chapter suggest that increases in sanction rates within local authorities are associated with increases in SSRI prescribing. First, results for the full time period indicate that every 10 additional sanctions applied per 100,000 population are associated with approximately 3.71 additional SSRI prescribing items, a result that is significant at the 1% level ( $p < 0.001$ ) (95% CI: 2.03 to 5.38). Additional results, however, indicate that this association is driven by the harsher sanctions regime that followed the implementation of the Welfare Reform Act 2012. In the pre-reform period, sanctions are associated with higher rates of SSRI prescribing, though the relationship is non-significant at the 5% level. In the post-reform period, however, the results indicate that every 10 additional sanctions applied per 100,000 population are associated with approximately 4.57 additional SSRI prescribing items, a result that is significant at the 1% level ( $p < 0.001$ ) (95% CI: 2.14 to 6.99). The average length of a prescribing item is one month, meaning that one person can receive three prescribing items per quarter. Consequently, this estimated quarterly relationship with SSRI prescribing items approximately translates to between one and two additional people receiving treatment. Since the analysis makes use of a sanctions indicator that underestimates the true quarterly rate, a best guess estimate would imply that every 10 additional sanctions applied per 100,000 population are associated with approximately one additional person receiving treatment (see Chapter 6 appendix Section A6.6 for a more in-depth mathematical explanation).

As highlighted in Chapter 5 and as re-emphasised throughout this chapter, nevertheless, the analysis itself is subject to certain limitations in terms of its attempt to investigate the impact of sanctions on antidepressant prescribing.

Principally, these limitations relate to ecological bias, omitted variable bias and reverse causality. Regarding the first of these, the analysis is carried out at the local authority-level, meaning that it is not possible to ascertain whether the people being sanctioned are the same as those who are ultimately prescribed antidepressants. It has also been highlighted, however, that one consequence of this fact is that the findings therefore capture impacts on both JSA claimants as well as wider effects on the friends and family of sanctioned individuals. Ecological bias, nevertheless, is an important limitation on the current study. Indeed, the risk of mistakenly applying area-level associations to individual-level relationships was highlighted when discussing the results, and reinforces the need for additional individual-level research. It is an important finding, however, that the scale and severity of sanctions following the implementation of the Welfare Reform Act 2012 are sufficient to have observable impacts even at the local authority-level. Arguably, the findings are at least indicative of individual-level effects, since it is not immediately clear why such relationships would hold at the area- but not the individual-level.

The results are estimated as part of fixed effects regression models that control for a number of additional explanatory variables. They are also supported by a variety of sensitivity checks, as well as a falsification test and a Granger test for reverse causality. Given that the analysis relies on observational data, however, it is not possible to completely rule out the influence of either omitted variable bias or reverse causality. In order to better account for these sources of bias, for example, individual-level data combined with a research design that could exploit sources of random allocation would be needed, as previously described in Chapter 5. In the current context, nevertheless, it is useful to consider whether there are any obvious unaccounted for factors that might serve as confounders of the results obtained. In this regard, it is important to highlight that for omitted variable bias to affect the main estimate of the relationship between sanctions and antidepressant prescribing, any omitted variable would have to be systematically correlated with both sanctions and antidepressant prescribing through time. There are various GP-level factors that the analysis does not account for, for example, such as numbers of GPs and GP characteristics, though the fixed effects control for initial differences between local authorities and it is

unlikely that changes in these factors across the period - if they occurred to any significant degree at all - would be related to the sanctions rate.

Arguably, a possibly greater threat to the analysis is the fact that it is not able to control for rates of local authority-level ESA sanctions throughout the period. As Chapter 3 highlighted, ESA sanctions varied in frequency during the Coalition whilst their severity increased from December 2012. Whilst this omission is of concern, it was previously highlighted that variation in ESA sanctions occurred at a much lower level than those of JSA sanctions. In an additional check of this issue, Figure A6.3 in the Chapter 6 appendix (Section A6.7) compares the rate of JSA and ESA sanctions that are relevant to this analysis: original adverse sanctions in England during the Coalition period. As Figure A6.3 indicates, the two sanction rates follow quite different trajectories across the period. The rate of ESA sanctions in fact falls from the beginning of the Coalition to mid-2011 and then stays broadly level at a rate below 1% for the remainder of the Coalition government. There are, of course, other reforms during the Coalition government that the analysis is not able to take into account. In addition to sanctions changes, for example, the Welfare Reform Act 2012 provided for the introduction of UC, a benefit cap, changes to Housing Benefit (the 'bedroom tax') and the replacement of Disability Living Allowance with Personal Independence Payments. Again, however, it is not clear why these factors would be systematically correlated with sanction rates from October 2012 onwards.

In light of this discussion, therefore, it is arguably reasonable to draw preliminary causal inferences from the results obtained, since it is not clear what other factors would plausibly explain them. This makes the findings more useful in terms of informing public policy, particularly in terms of considering the impact of the sanctions reforms that were introduced in the Welfare Reform Act 2012. The analysis in this chapter has focused on the relationship between JSA sanctions and antidepressant prescribing. The positive and robust association that is observed in this regard indicates that sanctions lead to adverse mental health impacts for claimants and others. Some of these individuals ultimately receive medical treatment for issues relating to anxiety and depression, meaning that the findings imply that sanctions policy knock-on effects in terms of wider public expenditure, given the additional demand that they create on the NHS.

This latter issue will be discussed in more depth in the concluding chapter of this thesis, which will also go into more detail regarding the policy recommendations that emerge from the empirical investigation as a whole. The next empirical chapter continues this investigation, and focuses on the relationship between benefit sanctions and mental health problems such as anxiety and depression.

## Chapter 7. Longitudinal ecological analysis II: anxiety and/or depression

### 7.1 Introduction

This chapter investigates the relationship between JSA benefit sanctions and rates of self-reported anxiety and/or depression during the period of Coalition government (2010-15), using data at the local authority-level. The specific research questions that are associated with this stage of the analysis are as follows:

[RQ 7.1]: Are benefit sanctions associated with higher rates of anxiety and/or depression at the local authority-level?

[RQ 7.2]: Does the observed relationship strengthen following the implementation of the Welfare Reform Act 2012?

As previously highlighted in Chapter 5, the analyses in Chapter 6 and Chapter 7 follow quite similar research designs and apply similar methods as part of their respective investigations into the impacts of sanctions. Indeed, this is reflected in the structure of this chapter, which broadly mirrors that established in Chapter 6. First, it begins by briefly considering the broader determinants of anxiety and depression at the local authority-level beyond the key focus on benefit sanctions. Next, the chapter outlines the data and methods that are used in the analysis, and then goes on to describe and discuss the results of the fixed effects regression analysis. Overall, the discussion is more succinct in comparison with the previous chapter, as it focuses on what is different between the two analyses. Although certain aspects are shared, there are important differences between the two that will be made clear in the course of this chapter. In addition to focusing on anxiety/depression, for example, the analysis in this chapter relies on survey estimates as opposed to administrative data. One advantage of this is that it is able to calculate local authority rates per working age population, as opposed to rates per total population as previously. The use of survey estimates, however, also leads to the consideration of two different levels of local authority geography: district-level, as in the previous chapter; and the more aggregated county-level.

## 7.2 Determinants of anxiety and depression

This section provides a brief overview of the separate factors that are associated with anxiety and depression. Previously, Figure 4.1 in Chapter 4 depicted the Dahlgren-Whitehead (1991) rainbow model of the social determinants of health, which maps the relationship between individuals and the various influences on their physical and mental health. Age and gender are at the centre of the model, which progresses outwards through the following determinants: individual lifestyle factors; social and community networks; living and working conditions; and general socio-economic, cultural and environmental conditions. In contrast to this multi-level framework, however, and similar to the previous chapter, the analysis in the current investigation is carried out solely at the local authority-level, which means that it is limited to the inclusion of area-level variables. As a consequence, this again makes it necessary to include factors at the area-level that are based on findings from individual-level studies. Chapter 4 has already highlighted the role that working conditions, unemployment and the nature of the social security interventions play in relation to mental health. In terms of the two characteristics that are at the centre of the Dahlgren-Whitehead model, furthermore, existing empirical research indicates that rates of anxiety and depression are higher on average for women, whilst they are fairly consistent across people of working age before falling for older age groups (Allen et al., 2014; Silva et al., 2016; Baker, 2018).

Additional research considers area-level determinants of psychological distress. Similar to the evidence discussed in relation to antidepressant prescribing, for example, Barr et al. (2016) highlight the role of labour market and economic factors, as well as the social security system itself. In their study, rates of mental health problems such as anxiety and depression are found to be higher in areas with higher unemployment rates, lower wages, lower rates of economic output and higher rates of Work Capability Assessments (WCAs). At the individual-level, furthermore, mental health follows a social gradient, in that mental health problems are more common the more disadvantaged people are in terms of their socio-economic status (Mattheys et al., 2016). The same is true at the area-level, where mental health problems have been found to increase along with increasing levels of deprivation (Mattheys et al., 2016; Silva et al., 2016).

Rural-urban classification is also important, in that urban environments are associated with higher rates of psychological distress, including anxiety and depression (Gong et al., 2016). Lastly, Rees et al. (2016) note that there is a limited UK evidence base on rates of anxiety and depression across separate ethnicities, which does not provide consistent results. At the area-level, the “ethnic density hypothesis” suggests that mental health for minority ethnic groups is improved when such individuals live in areas with higher proportions of people of the same ethnicity, though again UK evidence in this regard is mixed (Silva et al., 2016: 278).

## **7.3 Data and methods**

### **7.3.1 Analytic sample**

The sample in this chapter mirrors that selected for the antidepressant analysis carried out in Chapter 6, namely the quarters including and between the third quarter of 2010 and the end of 2014 (Q3 2010 - Q4 2014). This time period is selected since the third quarter of 2010 coincides with the onset of the Coalition government, whilst early 2015 marks the beginning of the national rollout of UC. As previously described, furthermore, this period saw important changes in the frequency and severity of sanctions, which provides the context within which to estimate the independent effect of sanctions on mental health. The investigation in this chapter is also restricted to English local authorities. In Chapter 6, the sample restriction to England was driven by data availability for antidepressant prescribing, which is not an issue here. The restriction in this chapter, nevertheless, results from a similar issue of data availability relating to important explanatory variables: deprivation and rural-urban classification at the local authority-level. One consequence of this is to reduce the sample size available for the analysis, since there are 32 local authority districts in Scotland and 22 local authority districts in Wales. The restriction to England in both chapters, nevertheless, permits a greater degree of comparability between the two investigations, which is useful when carrying out a sense-check on the results obtained in this chapter.

In addition, furthermore, the need to restrict the sample to England has the advantage of allowing the analysis to be carried out at two different levels of

local authority geography, which is not possible in Scotland or Wales given their single-tiered system of local governance. As explained in Chapter 5, the dependent variable in this chapter is based on QLFS survey estimates of anxiety and/or depression. Lower levels of geography are subject to more sampling variation than more aggregated levels, which - as Section 7.4 and Section 7.5 will discuss in more depth - has important impacts on the results obtained. There are two levels of local authority geography available for the analysis: 326 local authority districts that are organised into 152 county areas. The investigation in Chapter 6 used administrative data and therefore focused on local authority districts. Here, however, both district- and county-level models are estimated given the issue alluded to above. At the county-level, the QLFS combines the area of Cheshire East with Cheshire West and Chester into Cheshire, and the area of Bedford with Central Bedfordshire into Bedfordshire, which leaves 150 counties. Since the analysis excludes the City of London and the Isles of Scilly for reasons previously described, there are 324 local authority districts and 148 local authority counties in the two samples.

Finally, and similar to the analysis in Chapter 6, the remaining pre-2015 sample contains areas that were affected by the Pathfinder phase of UC from April 2013, which altered the composition of the remaining JSA claimant group in those areas. These local authority quarters are removed in order to minimise the influence of compositional bias on the results of the analysis, which totals 78 district quarters and 69 county quarters. These exclusions imply a maximum sample size of 5,754 local authority district quarters and 2,595 county quarters. Again, the findings remain substantively unchanged with or without the inclusion of the local authority quarters that are affected by the UC Pathfinder, given the small number of individuals actually claiming UC by December 2014.

### **7.3.2 Dependent variable and falsification variables**

As detailed in Chapter 5, quarterly estimates of the number of people in each local authority who self-report as suffering from anxiety and/or depression are produced using the QLFS (ONS, 2018c). QLFS survey respondents are first asked whether or not they suffer from 'health problems', and can then identify as suffering from 'depression, bad nerves or anxiety' from a pre-specified list (see



Box 5.1 in Chapter 5), referred to throughout this analysis as anxiety and/or depression. Using survey weights provided by the QLFS, furthermore, quarterly local authority estimates of the number of working age individuals suffering from anxiety and/or depression are constructed, and converted to rates per 100,000 working age population using mid-year population estimates available through Nomis (ONS, 2018b). The fact that this chapter uses rates per working age population represents an advantage over the antidepressants analysis in Chapter 6, which was only able to consider rates per total local authority population. Given the issue of sampling variation, however, local authority quarters where the estimates produced zero estimates were removed from the sample, which affected 78 district quarters and 3 county quarters. Given the small number of local authority quarters affected, the substantive findings remain unchanged with or without these exclusions.

Since the QLFS question relating to ‘health problems’ was altered slightly from Q2 2013 onwards, all regression models include a dummy variable coded 0 for quarters prior to Q2 2013 and 1 thereafter. As detailed in Box 5.1 (Chapter 5), prior to Q2 2013 respondents were asked: ‘Do you have any health problems or disabilities that you expect will last for more than a year?’ From Q2 2013 onwards, this changed to: ‘Do you have any physical or mental health conditions or illnesses lasting or expecting to last 12 months or more?’ Though the new health question refers more explicitly to respondent mental health than the previous one, the inclusion of the dummy variable to take into account this change ultimately has no substantive impact on the results of the analysis.

A falsification test is carried out to provide a robustness check on the main results of the analysis, using the non-equivalent dependent variable approach. Following Barr et al. (2016), two falsification variables are used: the rate of anxiety and/or depression per 100,000 population aged 65 and over; and the rate of ‘Heart, blood pressure or blood circulation problems’ per 100,000 working age population (henceforth ‘cardiovascular problems’). The falsification tests carried out in this chapter are limited by the availability of variables through the QLFS. The two non-equivalent dependent variables that are chosen arguably provide plausible candidates, nevertheless, particularly the rate of anxiety and/or depression in the population aged 65 and over. The assumption is

that these outcomes will not be affected by sanctions, especially in the short-term, but might be affected by potential unobserved confounders affecting the results of the main analysis. These might include factors such as changes in people's willingness to report health problems, for example, and/or changes in their access to healthcare. In this sense the rate of anxiety and/or depression in the population aged 65+ is a preferable choice, as it may be affected by changes in people's willingness to report mental health problems specifically, as opposed to health problems more generally. Here, given the issue of sampling variation, local authority quarters where the estimates produced zero estimates are again removed from the sample.

### **7.3.3 Sanctions data and additional explanatory variables**

Data on sanctions are sourced from Stat-Xplore and constructed in a similar way as in Chapter 6, though are calculated as a quarterly rate per 100,000 working age population. Given the separate results for the three different sanctions measures that were considered in Chapter 6, the analysis in this chapter focuses solely on the main original adverse sanctions figure. Along with sanctions, additional explanatory variables are included in the analysis that the previous discussion in Section 7.2 indicated as being important determinants of anxiety and/or depression. Given the close relationship between these mental health problems and antidepressant prescribing, the explanatory variables are similar to those identified in Chapter 6, and include: rates of JSA claimants, ILO unemployment, economic inactivity, employment, Work Capability Assessments, GVA per head, demographic characteristics (age, gender and ethnicity); and the Index of Multiple Deprivation (IMD) and rural-urban classification. The main difference here is that rates of antibiotic prescribing are not considered, since this variable was included in Chapter 6 as a proxy for discretionary GP prescribing behaviour, which is not relevant in the current context. Data are available, furthermore, for proportions of white working age individuals in each local authority, and so this variable is used as opposed to the white UK born measure that was available for the analysis in Chapter 6.

Table 7.1 summarises the separate variables that are included in the analysis, their data source and summary statistics. All variables are measured as rates per 100,000 work age population, except GVA per head, IMD and rural-urban classification, the latter two of which are categorical variables. Table 7.1 details summary statistics at the county-level only. The equivalent information for the district-level is detailed in Table A7.1 in the appendix for Chapter 7 (Section A7.1). The county-level summary statistics are displayed here given the fact that the analysis itself ultimately focuses mainly on the county-level results, for reasons that will become clear in the discussion that follows.

### 7.3.4 Statistical approach

Similar to Chapter 6, the first stage of the analysis in this chapter investigates the relationship between sanctions and anxiety and/or depression across the time period. This responds to the first research question identified in the introduction, by estimating fixed effects models as described in a basic form in Equation 7.1:

$$Anxiety_{i,t} = \beta_0 + \beta_1 Sanctions_{i,t} + \beta' X_{i,t} + Q_t + \mu_i + \lambda_t + \varepsilon_{i,t} \quad (7.1)$$

In Equation 7.1,  $i$  denotes the local authority and  $t$  denotes the quarter. *Anxiety* is the rate of anxiety and/or depression per 100,000 working age population, *Sanctions* is the sanctions rate per 100,000 working age population and  $X$  represents a vector of additional explanatory variables.  $Q$  is a dummy variable coded 0 for quarters prior to Q2 2013 and 1 thereafter, to take into account the change in the QLFS survey question at that point in time. The remaining symbols are as follows:  $\mu$  denotes local authority fixed effects,  $\lambda$  denotes time fixed effects and  $\varepsilon$  represents the error term. The meaning and implication of including these fixed effects terms was previously explained in Chapter 6. In addition, regression diagnostic checks are carried out, as detailed in the appendix for Chapter 7 (Section A7.3), and as a result of these checks all estimated models use Driscoll-Kraay standard errors, which are robust to heteroscedasticity, correlation through time within local authorities as well as general forms of cross-sectional dependence.

**Table 7.1:** summary statistics for 148 local authority counties, Q3 2010 – Q4 2014

	<i>N</i>	Mean	St.d Dev	Min.	Max.	Source
<i>Dependent variable:</i>						
Anxiety and/or depression	2,592	5,161	2,255	411	16,959	QLFS
<i>Sanctions variable:</i>						
Original adverse	2,595	434	226	22	1,510	Stat- Xplore
<i>Control variables:</i>						
Claimants	2,595	3,610	1,544	566	9,051	Nomis
Unemployment	2,590	6,141	1,851	1,877	12,694	Nomis
Economic Inactivity	2,595	23,428	4,033	13,821	36,524	Nomis
Employment	2,595	69,408	5,168	51,682	81,121	Nomis
Work Capability Assessments	2,595	439	216	54	1,887	Stat- Xplore
GVA	2,595	24,903	20,395	12,791	238,714	Nomis
Age						Nomis
16-29 year olds	2,595	29,189	4,493	21,199	43,970	
30-49 year olds	2,595	43,331	3,049	35,066	52,332	
50-64 year olds	2,595	27,480	5,107	12,407	37,484	
Female	2,595	50,118	827	46,464	52,555	Nomis
White	2,595	83,285	15,708	22,145	99,831	Nomis
Index of Multiple Deprivation						DCLG
Quintile 1	507					
Quintile 2	531					
Quintile 3	516					
Quintile 4	510					
Quintile 5	531					
Rural-Urban Classification						Defra
Predominantly rural	338					
Urban with significant rural	349					
Predominantly urban	1,908					
<i>Falsification variables:</i>						
Anxiety and/or depression (age 65+)	2,201	1,577	1,553	92	35,295	QLFS
Cardiovascular health	2,595	7,945	2,582	935	22,584	QLFS

*Note:* Local authority quarters where the QLFS estimates produced zero estimates were removed from the sample.

The next stage of the analysis considers the second research question outlined in the introduction to this chapter, and investigates the impact of the Welfare Reform Act 2012. To capture the effect of this reform, Equation 7.2 modifies the initial fixed effect analysis through inclusion of an interaction term with *Sanctions* and *Reform*:

$$Anxiety_{i,t} = \beta_0 + \beta_1 Sanctions_{i,t} + \beta_2 (Sanctions_{i,t} * Reform_t) + \beta' X_{i,t} + Q_t + \mu_i + \lambda_t + \varepsilon_{i,t} \quad (7.2)$$

In Equation 7.2, *Reform* is a dummy variable that marks the quarters before and after the implementation of the harsher sanctions regime brought about by the Welfare Reform Act 2012. It is coded 1 for quarters Q4 2012 onwards and 0 before that date. The post-reform relationship between sanctions and anxiety and/or depression is then arrived at through calculating a linear combination of  $\beta_1$  and  $\beta_2$ , as previously explained. The remainder of this chapter details the results of these fixed effects regressions, both at the district- and the county-level, and then goes on to discuss the implications of the findings obtained.

## 7.4 District-level results

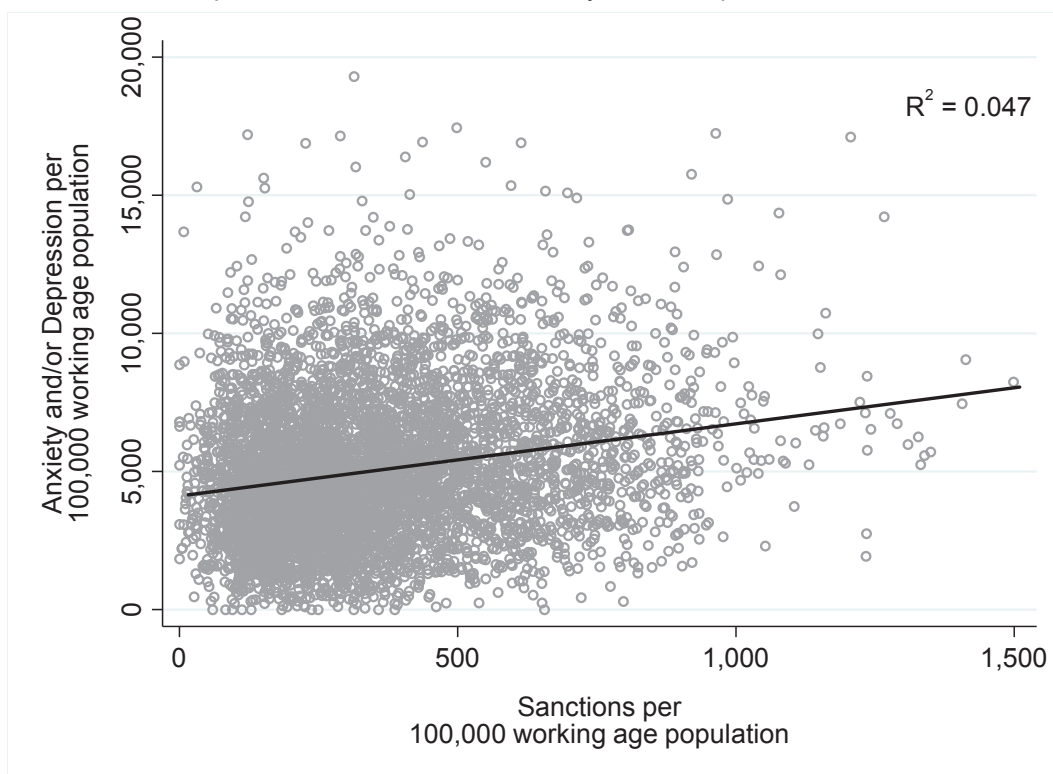
This section details the results from the district-level analysis, including summary statistics and the regression estimates from both across the time period and in the pre- and post-reform periods. As will become clear in the course of the discussion, the regression results in relation to the key sanctions variable of interest are consistently non-significant. Consequently, the analysis in this section is relatively brief, focusing on the main sanctions coefficients only. The full regression tables themselves are detailed in the appendix for Chapter 7 (Section A7.1). The following section details the results of the county-level analysis in more depth.

### 7.4.1 Summary statistics

Figure 7.1 depicts the correlation between sanctions and anxiety and/or depression in each local authority quarter across the period, and indicates that in local authority quarters where the rate of sanction is higher, so too are rates of anxiety and/or depression ( $r = 0.218$ ;  $p < 0.001$ ). Indeed, this correlation is stronger than in the equivalent Figure 6.1 in Chapter 6 regarding the relationship

between sanctions and SSRI prescribing. The positive association is nevertheless still quite low, with only 4.7% of the variance in anxiety and/or depression explained by sanctions, which motivates the application of an expanded fixed effects regression framework.

**Figure 7.1:** relationship between sanctions and anxiety and/or depression



*Note:* quarterly rates for 324 local authority districts, Q3 2010 – Q4 2014

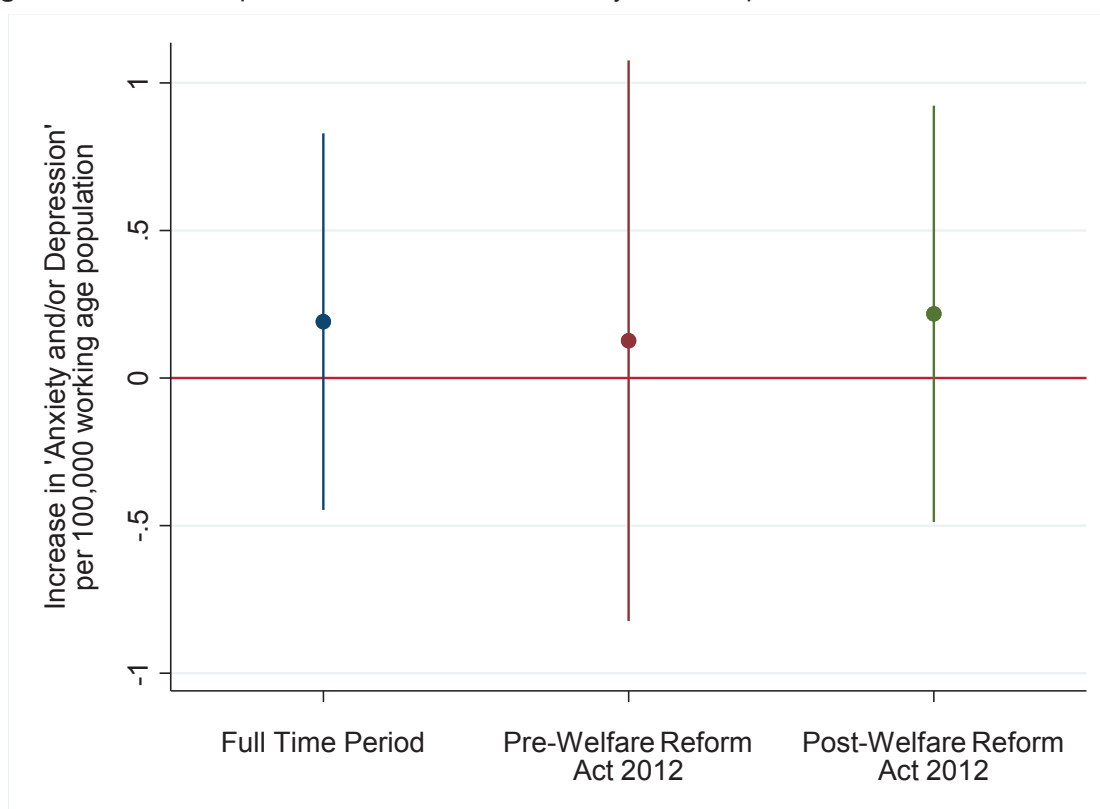
#### 7.4.2 Regression models: full time period

Estimates from the district-level fixed effects analysis are displayed in Table A7.3 in the appendix for Chapter 7 (Section A7.1). The detail is quite similar to the modelling carried out in Chapter 6, whilst the focus in this section is on the sanctions coefficients specifically. The results in Table A7.3 (Model A7.2) indicate that increases in sanction rates within local authorities are associated with increases in rates of anxiety and/or depression, though this effect is non-significant at the 5% level. For every 10 additional sanctions applied per 100,000 working age population, the rate of anxiety and/or depression increases by 1.91 per 100,000 working age population ( $p = 0.536$ ). Whilst the positive association is the expected one, it is not possible to place much confidence in the estimated coefficient itself. This is made clear in the graphical representation of the estimated sanctions coefficient for the full-time period in Figure 7.2.

### 7.4.3 Regression models: influence of the Welfare Reform Act 2012

Next, the analysis examines whether the observed association between sanctions and anxiety and/or depression is stronger in the period following the implementation of the Welfare Reform Act 2012. The full Model A7.4 results are displayed in Table A7.3 in the appendix for Chapter 7 (Section A7.1). Similar to the results for the full time period, the estimated relationship between sanctions and anxiety and/or depression is not statistically significant at the 5% level in either the pre- or post-reform periods. In the pre-reform period, for every 10 additional sanctions applied per 100,000 working age population the rate of anxiety and/or depression is 1.27 per 100,000 working age population higher ( $p = 0.782$ ). Following the reform, the association increases by 0.91 per 100,000 working population, so that for every 10 additional sanctions applied per 100,000 population the rate of anxiety and/or depression is 2.18 per 100,000 working age population higher ( $p = 0.524$ ). These results are summarised in Figure 7.2, which displays the estimated sanctions coefficient for the full period, as well as the pre-and post-Act periods.

**Figure 7.2:** relationship between sanctions and anxiety and/or depression



Note: Point estimates for sanctions are derived from Table A7.3  
Vertical bars represent 95% confidence intervals

As highlighted in this section, the results at the district-level do not find a statistically significant association between sanctions and anxiety and/or depression. It is at least indicative, however, that the estimated coefficients conform to the expected relationship. Not only is the association between sanctions and anxiety and/or depression positive, the estimated coefficient is larger in the post-reform period. Clearly, however, the confidence intervals displayed in Figure 7.2 are too large for any reliance to be placed on the results in question. One plausible explanation of the large standard errors is the role of sampling variability from one quarter to the next, derived from the fact that the analysis estimates anxiety and/or depression using QLFS survey weights. When producing area-level QLFS estimates, the estimates themselves become less and less reliable at lower levels of geography (ONS, 2016). One option in this context would be to use estimates derived from the Annual Population Survey (APS), which benefits from a larger sample size. However, the APS is designed for annual as opposed to quarterly analysis, and is therefore not suited to producing estimates that can be utilised in the present analysis. Instead, and as previously indicated, the next section focuses on county-level estimates of anxiety and/or depression using the QLFS. By considering the association between sanctions and anxiety and/or depression at a more aggregated geographical level, the role of sampling variation is reduced and more confidence can be placed in the results obtained.

## **7.5 County-level results**

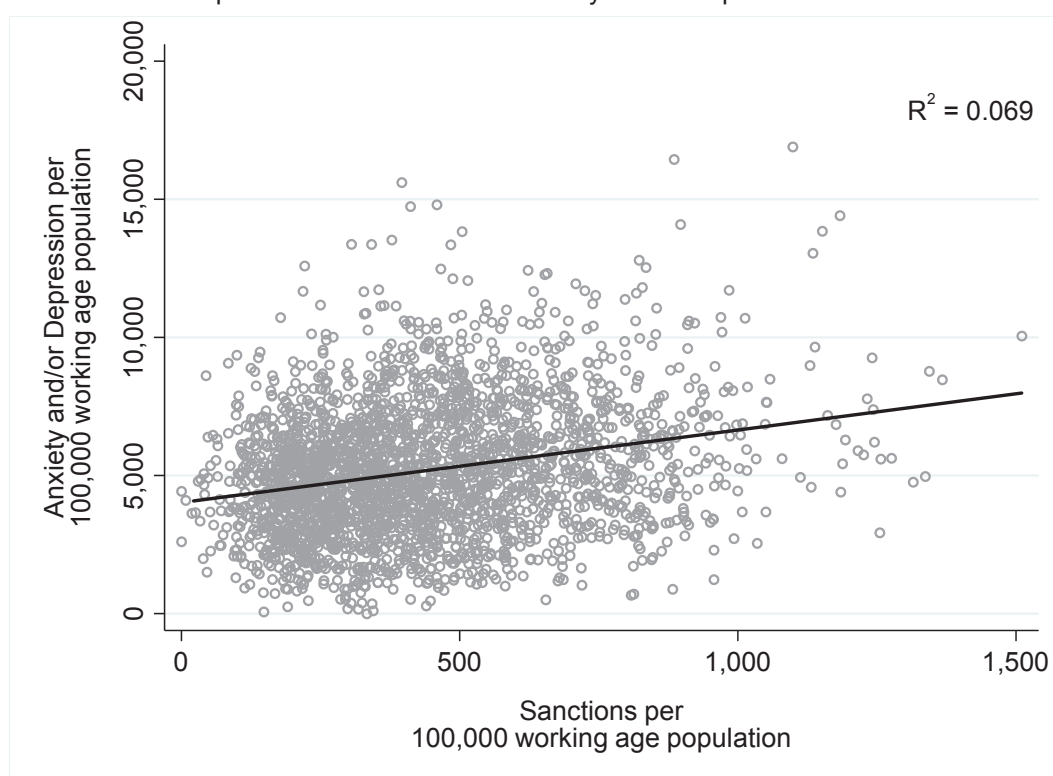
### **7.5.1 Summary statistics**

Table 7.1, above, details the summary statistics for the variables included in the county-level analysis. It indicates that across the sample there are an average of 434 sanctions and 5,161 people experiencing anxiety and/or depression per 100,000 working age population per quarter in local authorities. There was a large degree of variation, nevertheless, in rates of both sanctions and anxiety and/or depression around these averages. Again, for example, Blackpool stands out with an average of 833 sanctions and 8,770 people experiencing anxiety and/or depression per 100,000 working age population per quarter. Equally, however, local authorities with high rates of sanctions are not necessarily the same local authorities with high rates of anxiety and/or depression, and vice



versa. Rutland, for example, has a low rate of sanctions but a relatively high rate of people experiencing anxiety and/or depression, which indicates that factors other than sanctions are important. The scatterplot in Figure 7.3 depicts the correlation between sanctions and anxiety and/or depression in each local authority quarter across the period, which is slightly stronger than that observed with the district-level scatterplot displayed in Figure 7.2. As before, in local authority quarters where the rate of sanction is higher, so too are rates of anxiety and/or depression ( $r = 0.263$ ;  $p < 0.001$ ). A basic linear regression model using the two variables indicates that only 6.9% of the variance in anxiety and/or depression is explained by sanctions, which motivates the application of an expanded fixed effects regression framework.

**Figure 7.3:** relationship between sanctions and anxiety and/or depression



*Note:* quarterly rates for 148 local authority counties, Q3 2010 – Q4 2014

### 7.5.2 Regression models: full time period

The initial modelling process that is carried for the county-level results follows that previously explained in Chapter 6. That is, three separate models are initially estimated, which are detailed in Table A7.4 in the appendix for Chapter 7 (Section A7.2). The discussion itself will ultimately focus in more detail on the preferred model. First, a fixed effects model (Model A7.5) is estimated that

incorporates the sanctions rate as well as a number of additional time-variant explanatory variables, which include: rates of unemployment and economic inactivity; Work Capability Assessments (WCAs); GVA; age, gender and ethnicity. Second, this model is repeated with the inclusion of IMD quintiles and rural-urban classification (Model A7.6), both interacted with time. As previously explained, this interaction captures the possibility that the influence of deprivation and rurality on anxiety and/or depression may have changed over the period. Third, as an additional sensitivity test, a random effects model is estimated (Model A7.7). Similar to the analysis in Chapter 6, the fixed effects model with the inclusion of IMD quintiles and rural-urban classification interactions is preferred. Again, a Hausman test indicates that a random effects framework should not be favoured over fixed effects in the current analysis ( $p < 0.001$ ). Equally, Model A7.6 is preferred over Model A7.5 due to the significance of the IMD and rural-urban interactions with time, as well as the increase in within- $R^2$ . The Model A7.6 results are reproduced in Table 7.2 (Model 7.1), for the purposes of the current discussion.

Importantly, given the transition from a district-level to a county-level analysis, the results in Model 7.1 indicate that sanctions are associated with increases in rates of anxiety and/or depression. For every 10 additional sanctions applied per 100,000 working age population, the rate of anxiety and/or depression increases by 7.77 per 100,000 working age population. Unlike at the district-level, furthermore, the results are now statistically significant at the 0.1% level ( $p < 0.001$ ) (95% C.I.: 3.57 to 11.97). A full diagnostic check of Model 7.1 is carried out in the appendix for Chapter 7 (Section A7.3), which tests the various fixed effects model assumptions. Importantly, the checks do not indicate that there are any clear issues that would undermine the results obtained. The models themselves also use standard errors that are robust to heteroscedasticity, correlation through time within local authorities as well as general forms of cross-sectional dependence, as previously indicated.

**Table 7.2:** relationship between sanctions and anxiety and/or depression, fixed effects models

	Model 7.1	Model 7.2
Sanctions	0.777*** (0.199)	0.690 (0.459)
Sanctions x Reform		0.119 (0.515)
Unemployment	-0.009 (0.041)	-0.009 (0.041)
Economic Inactivity	0.074** (0.020)	0.074** (0.020)
WCAs	0.216 (0.536)	0.212 (0.549)
GVA	-0.059*** (0.014)	-0.060*** (0.013)
Age		
30-49	-0.407*** (0.104)	-0.404** (0.109)
50-64	0.105 (0.108)	0.104 (0.109)
Female	0.504 (0.473)	0.498 (0.467)
White	0.025 (0.019)	0.025 (0.019)
Index of Multiple Deprivation		
Quintile 2 x Quarter	51.541** (14.751)	50.416** (16.222)
Quintile 3 x Quarter	56.931*** (6.926)	54.829*** (12.786)
Quintile 4 x Quarter	66.666*** (16.652)	63.880** (18.871)
Quintile 5 x Quarter	80.834** (21.423)	76.581* (29.328)
Urban-Rural Classification		
Urban with significant rural x Quarter	10.867 (12.300)	10.519 (13.054)
Predominantly urban x Quarter	-50.008** (14.758)	-50.531** (15.344)
$R^2$ (within)	0.847	0.847
LA Quarters	2,587	2,587

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and LFS question change dummy not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Several of the explanatory variables included in Model 7.1 conform to the relationship that is expected with regard to rates of anxiety and/or depression. In terms of the statistically significant results, for example, increases within local authorities in rates of economic inactivity are associated with increases in anxiety and/or depression, whilst increases in GVA are associated with decreases. A positive relationship exists, furthermore, in terms of rates of WCAs, females and proportions of white people, though these results are non-significant at the 5% level. Similar to the results in Chapter 6, however, a counterintuitive result in Model 7.1 is the negative and close to zero estimated coefficient for the rate of unemployment, given the well-established link between unemployment and poor mental health. As with previously, the results are re-run using the JSA claimant count as opposed to ILO unemployment, as detailed in the Chapter 7 appendix (Table A7.6, Section A7.4). In this model, the estimated claimant count coefficient remains negative and non-significant at the 5% level. The sanctions coefficient also remains substantively unchanged.

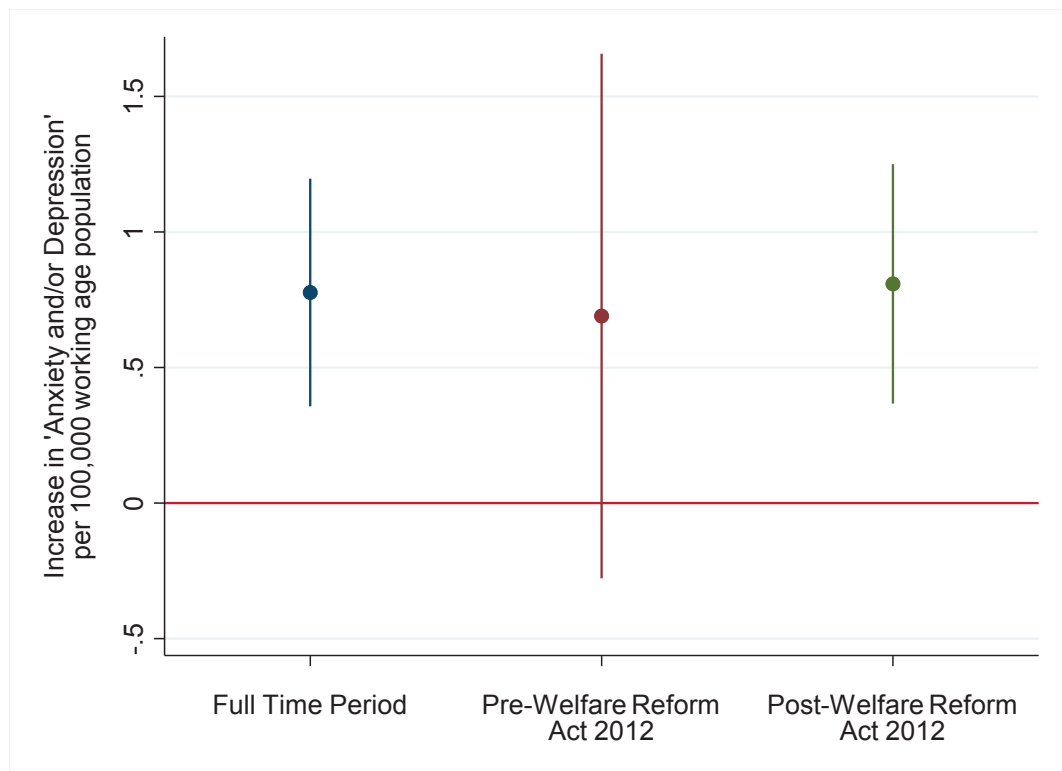
The wider ILO measure of unemployment is again favoured over the narrower JSA claimant rate in the analysis for two key reasons. As well as capturing a narrower set of unemployed individuals, the claimant rate has a high degree of collinearity with sanctions ( $r = 0.750$ ) that has the potential to drive the negative estimated relationship between the claimant rate and anxiety and/or depression. The correlation between unemployment and sanctions is lower ( $r = 0.653$ ), though still high, but separate regressions with and without each variable included indicate that multicollinearity is not driving the counterintuitive estimated relationship that is observed here. Again, a possible explanation of the result relates to the previously identified and discussed risk of ecological bias, whereby correlations that hold at the area-level do not necessarily apply at the individual-level. The ongoing threat of ecological bias emphasises the need for additional individual-level analysis to better understand the relationships that are investigated throughout this thesis.

### **7.5.3 Regression models: influence of the Welfare Reform Act 2012**

Next, the analysis examines whether the observed association between sanctions and anxiety and/or depression is stronger in the period following the

implementation of the Welfare Reform Act 2012. The full results of this fixed effects regression model are displayed in Model 7.2 in Table 7.2. They indicate that before the implementation of the Act, for every 10 additional sanctions applied per 100,000 working age population the rate of anxiety and/or depression is 6.90 per 100,000 working age population higher, though - similar to the analysis in Chapter 6 - this result is not significant at the five per cent level ( $p = 0.151$ ). Following the reform, however, the association increases by 1.19 per 100,000 working age population, so that for every 10 additional sanctions applied per 100,000 working age population the rate of anxiety and/or depression is 8.09 per 100,000 working age population higher ( $p < 0.001$ ) (95% C. I.: 3.67 to 12.50). These results are consistent with the expected impact of the Welfare Reform Act 2012. The coefficients for the additional explanatory variables in Model 7.2, furthermore, are very similar to those estimated in Model 7.1. These results are summarised in Figure 7.4, which displays the estimated sanctions coefficient for the full time period (Model 7.1), as well as the pre-and post-Act periods (Model 7.2).

**Figure 7.4:** relationship between sanctions and anxiety and/or depression, sanctions coefficients

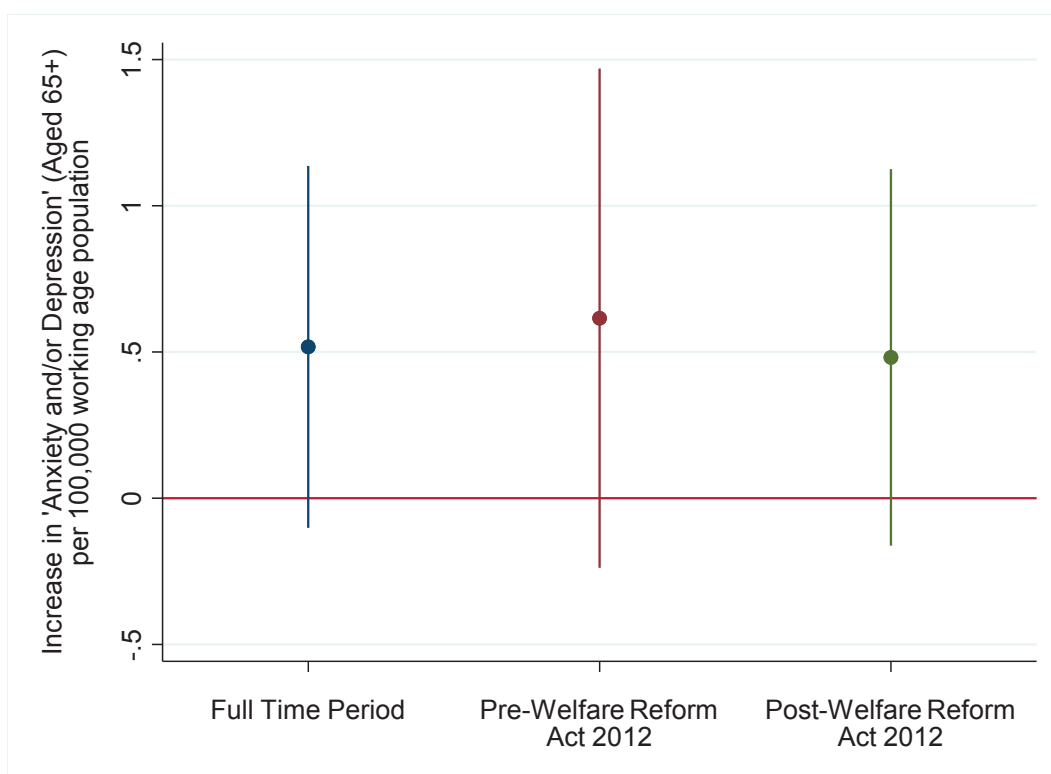


Note: Point estimates for sanctions are derived from Table 7.2  
Vertical bars represent 95% confidence intervals

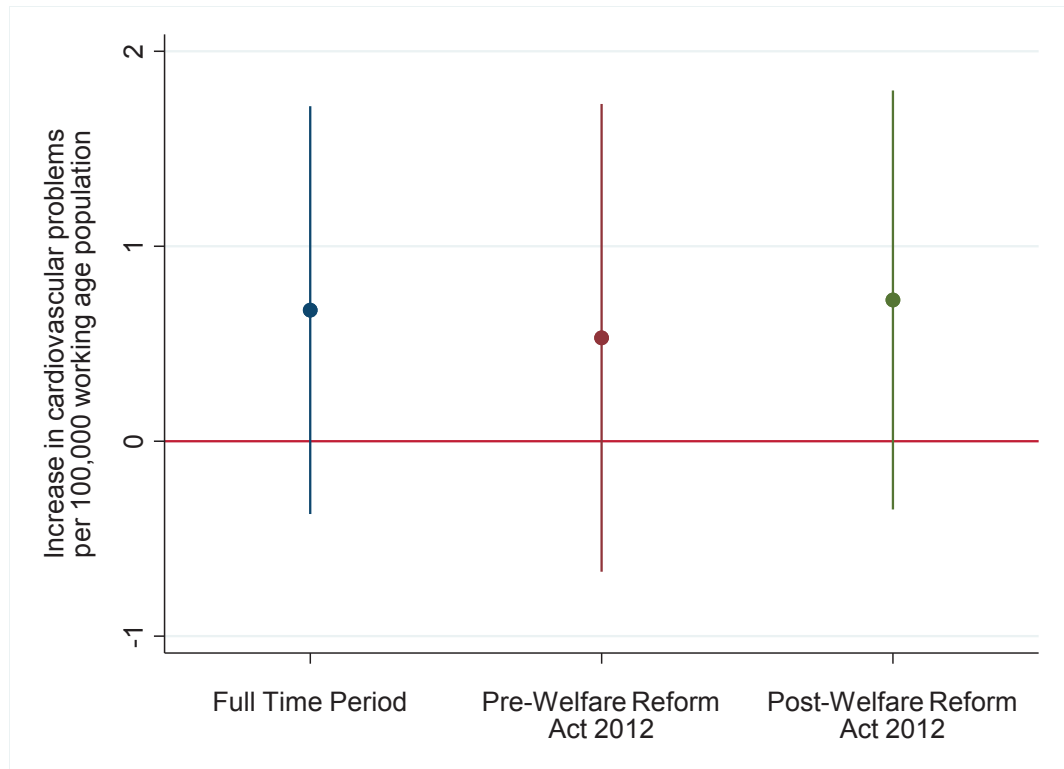
### 7.5.4 Robustness tests

Two falsification tests are carried out using rates of anxiety and/or depression in the population aged 65 and over, as well as rates of cardiovascular problems in the working age population. The full results are detailed in the appendix for Chapter 7 (Table A7.7 and Table A7.8, Section A7.5). For both variables, no statistically significant relationship is found in terms of sanctions either across the time period or in the pre- and post-reform periods. These results are displayed in Figure 7.5 and Figure 7.6. Unfortunately, use of anxiety and/or depression in the population aged 65 and over is limited by the issue of zero estimates (Figure 7.5). As previously highlighted, local authority quarters with estimates of zero are removed from the sample as they are likely to reflect issues relating to sampling variability. This leads to a 15% drop in the sample size available for the analysis when carrying out the falsification test, which undermines its usefulness as a non-equivalent dependent variable. This is unfortunate, since it was previously highlighted that this measure arguably represents a better non-equivalent dependent than cardiovascular problems.

**Figure 7.5:** relationship between sanctions and anxiety and/or depression (aged 65+), sanctions coefficients



Note: Point estimates for sanctions are derived from Table A7.7  
Vertical bars represent 95% confidence intervals

**Figure 7.6:** relationship between sanctions and cardiovascular health, sanctions coefficients

Note: Point estimates for sanctions are derived from Table A7.8  
Vertical bars represent 95% confidence intervals

Second, a Granger test for reverse causality is carried out using a lag structure of four quarters, which indicates that sanctions are found to Granger-cause anxiety and/or depression ( $p = 0.013$ ), whilst rates of anxiety and/or depression are not found to Granger-cause sanctions ( $p = 0.608$ ). This result supports the local authority-level inferences made in the analysis here. It is important to reiterate, however, that the Granger test provides only a limited test of reverse causality, which would require individual-level data and a different research design to be fully taken into account.

## 7.6 Conclusion

The findings presented in this chapter suggest that higher sanction rates are associated with increases in rates of anxiety and/or depression within local authorities. Results for the full time period indicate that for every 10 additional sanctions applied per 100,000 working age population, the rate of anxiety and/or depression increases by 7.77 per 100,000 working age population ( $p < 0.001$ ) (95% C.I.: 3.57 to 11.97). Similar to the analysis in Chapter 6, however, additional models suggest that a stronger relationship exists following the

implementation of the Welfare Reform Act 2012. In the pre-reform period, that is, sanctions are associated with higher rates of anxiety and/or depression but the relationship is not significant at the 5% level. In the post-reform period, the results indicate that for every 10 additional sanctions applied per 100,000 working age population, the rate of anxiety and/or depression increases by 8.09 per 100,000 working age population ( $p < 0.001$ ) (95% C. I.: 3.67 to 12.50). Whilst this local authority-level association might be considered to be quite high, it has been highlighted that this level of analysis captures impacts on both JSA claimants as well as wider effects on the friends and family of sanctioned individuals. As previously detailed in the appendix for Chapter 6 (Section A6.6), furthermore, the analysis uses a sanctions indicator that underestimates the true quarterly rate, which implies that it slightly overestimates the true relationship between sanctions and - in this case - anxiety and/or depression.

The level of analysis and methods used in Chapter 6 and Chapter 7 are similar, and as such the results here are limited by a similar set of concerns to those previously discussed, which include: ecological bias; omitted variable bias; and reverse causality. The role of each of these issues was highlighted in Chapter 6 (Section 6.5), and won't be repeated in the discussion here, though it is important to reiterate that the results themselves are estimated as part of fixed effects models that control for a number of additional factors and are supported by sensitivity and robustness checks. A key difference between the two analyses, nevertheless, is that the results in this chapter rely on survey estimates of anxiety and/or depression and are run at both the district- and county-level of local authority geography, with statistically significant effects observed at the county-level only. Even at the district-level, however, the estimated non-significant sanctions coefficients conform to the expected pattern. The difference between the district- and county-level results is also explainable by the fact that QLFS estimates are less reliable at lower levels of geography. It is an important finding, therefore, that the scale and severity of sanctions following the implementation of the Welfare Reform Act 2012 are sufficient to have observable impacts at the local authority-level. Indeed, the findings in Chapter 6 and Chapter 7 are both observed despite the fact that the rate of hardship payments increased in the post-reform period (Webster, 2015).



An additional issue that was previously identified in Chapter 5 relates to compositional bias. Because the analyses in Chapter 6 and Chapter 7 are carried out at the local authority-level, there is a risk that the estimated association between sanctions and mental health outcomes are partly driven by unobserved changes in the characteristics of the JSA claimant group throughout the period of analysis, as opposed to by sanctions themselves. This is an unavoidable concern in the context of ecological analyses. As highlighted in both chapters, the analysis has sought to limit the potential influence of compositional bias by accounting for the rollout of Universal Credit (UC). This altered the composition of the JSA claimant group towards the end of the period as UC was initially only open to younger unemployed individuals without dependent children. To further investigate this issue, the analysis in the next empirical chapter considers the relationship between the area-level sanctions rate and the mental health of individual JSA claimants. Using individual-level QLFS data and controlling for claimant characteristics, this provides an additional robustness check on the findings in this chapter.

## Chapter 8. Multi-level analysis: anxiety and/or depression

### 8.1 Introduction

This chapter investigates the relationship between JSA benefit sanctions and rates of anxiety and/or depression during the period of Coalition government (2010-15), using individual-level data on JSA claimants and local authority-level data on benefit sanctions. As highlighted in the summary to Chapter 7, there is a risk that the estimated association in that chapter between sanctions and anxiety and/or depression is partly driven by compositional bias, given that it relies on longitudinal ecological data. To consider this issue further, the investigation in this chapter carries out a multi-level analysis that is able to control for individual-level characteristics of JSA claimants. Whilst this is primarily carried out to provide an additional robustness check on the findings in Chapter 7, the specific research questions associated with this stage of the analysis can be stated as follows:

[RQ 8.1]: Are area-level sanction rates associated with adverse mental health impacts for JSA claimants?

[RQ 8.2]: Does the observed relationship strengthen following the implementation of the Welfare Reform Act 2012?

In the process of investigating these research questions, this chapter begins by outlining the specific modelling approach that will be pursued, in light of the fact that the analysis makes use of both individual- and area-level data. This discussion provides more depth into the multi-level modelling approach than was previously given in Chapter 5. Next, the chapter outlines additional details relating to the data and methods that are used in the investigation, and then goes on to describe and discuss the results of the multi-level analysis itself.

### 8.2 Multi-level analysis

The analysis in this chapter uses QLFS data (ONS, 2018c), which - when combined with sanctions data from Stat-Xplore (DWP, 2018c) - permit

investigation into the relationship between quarterly sanction rates at the local authority-level and the mental health of individual JSA claimants. This initial combination of individual- and area-level data involves the analysis of a two-level data structure in which JSA claimants are clustered within local authorities. As previously highlighted, furthermore, the primary aim of this chapter is to provide a robustness check on the longitudinal ecological analysis carried out in Chapter 7, and so the investigation pools QLFS datasets between Q3 2010 and Q4 2014. One implication of pooling quarterly datasets, combined with the rotational sampling design adopted by the QLFS, is that some individuals will be present in the sample more than once. As previously explained in Chapter 5, the QLFS uses a rotational sampling design in which each household is interviewed for five consecutive quarters. Consequently, if an individual is a JSA claimant for five consecutive quarters then they will be included in the current sample five times, whereas if they are a claimant in only one quarter then they will be included in the sample only once. The investigation, therefore, involves the analysis of a hierarchical three-level data structure in which occasions of measurement are nested within individuals which are themselves nested within local authorities.

Importantly, both the repeated observations of individuals and the clustering of individuals within local authorities must be explicitly taken into account during the modelling process, as such observations are likely to be highly correlated with each other. Such observations serve to violate the assumption of independence that underpins single-level multiple regression models, and if this clustering is ignored the standard errors of the regression coefficients are likely to be underestimated, which increases the likelihood of committing a Type I statistical error by falsely rejecting a true null hypothesis (Steele, 2008b). There are at least two strategies for dealing with such clustering in the current context. First, it is possible to estimate fixed effects regression models, as previously carried out in Chapter 6 and Chapter 7. An alternative strategy is to explicitly model for dependency within individuals and local authorities using a multi-level modelling approach, which implies the use of random effects regression analysis. As previously highlighted in Chapter 5, when attempting to choose between the two approaches, it is important to compare the fact that whilst random effects models provide a more efficient and parsimonious means

of dealing with clustering, they are also underpinned by stricter assumptions. In order to exploit both within- and between-group variation, for example, random effects models make the relatively stricter assumption that any omitted variables are uncorrelated with the included explanatory variables. That is, the residual regression errors are assumed to be independent of the random effects, whilst also being normally distributed with a mean of zero and a constant variance (Steele, 2008b).

Despite its relatively stricter assumptions, there are clear grounds in the current context for adopting a random effects framework over that of fixed effects. Importantly, for example, there are several disadvantages that affect the use of individual and local authority fixed effects given the data that is being analysed. The inclusion of individual fixed effects, for example, would involve including  $N-1$  individual dummy variables and use up many degrees of freedom due to the large number of individuals in the dataset. In addition, the use of individual fixed effects would involve essentially ignoring the explanatory power of those individuals who appear in the dataset only once, whilst the estimates of the local authority fixed effects are likely to be unreliable where the within-local authority sample size is small (Steele, 2008a). Here, individuals are perfectly nested within local authorities, meaning that the analysis will only be able to include individual-level fixed effects due to perfect multicollinearity between the two levels. Consequently, the clustering of individuals within local authorities would have to be dealt with using a ‘cluster-robust’ regression method, which provides only a limited response to the issue of clustering via post-estimation technical adjustments to the standard errors (Cameron and Miller, 2015).

Random effects models, in contrast, not only avoid these pitfalls but also, by more explicitly recognising the nested structure of the data available, are better able to accurately estimate the standard errors of the key explanatory variable, which is the area-level sanctions rate. A key strength of the multi-level framework in the current context, furthermore, is that it does not require balanced data nor does it require the spacing of occasions to be the same between individuals (Rasbash, 2008). In the current sample, individuals might be claiming JSA at one or more quarters that are not necessarily equally spaced

apart. When conducting multi-level analysis it is possible to estimate both random intercept and random slope models (Steele, 2008b). In random intercept models, the intercept is allowed to vary randomly across groups, in this case individuals and local authorities, meaning that it can take on values from a distribution. The effect of explanatory variables included in the model are nonetheless fixed, in the sense that they do not vary across groups. The effect of age or gender on mental health, for example, is assumed to be the same no matter what local authority an individual is in. In random slope models, in contrast, such coefficients are allowed to vary randomly across groups. This approach is not pursued here, however, given the additional assumptions that these models entail and the central focus on the area-level sanctions rate.

## **8.3 Data and methods**

### **8.3.1 Analytic sample**

As previously highlighted, the analysis in this chapter uses pooled QLFS data (ONS, 2018c), using information on JSA claimants present in the QLFS datasets between Q3 2010 and Q4 2014. Consistent with the analysis in Chapter 7, furthermore, JSA claimants in the City of London and the Isles of Scilly are removed, as well as JSA claimants in quarters affected by the roll-out of Universal Credit. This latter restriction leads to a loss of 250 JSA claimant observations from the sample (1.9% of the post-reform sample), though the substantive results are again similar with or without this exclusion, given the limited rollout of UC that had occurred by the end of 2014.

In the initial analysis, all instances of an individual claiming JSA are retained, which implies that the investigation analyses a three-level data structure in which occasions of measurement are nested within individuals which are nested within local authorities. As a sensitivity check on the three-level analysis, however, the investigation also considers a two-level data structure in which JSA claimants are observed only once, relating to the first time that they enter the sample. Partly, this sensitivity check is carried out in response to the potential that the initial analysis could be affected by the sample size available at each level of occasion, individual and local authority. As Hox (2010) notes, for example, the estimates of coefficients and their standard errors become more

accurate with increasing sample sizes at all levels, and points towards rules of thumb that have been developed which suggest: at least 30 observations per group with 30 groups; at least 20 observations per group with 50 groups; and at least 10 observations per group with 100 groups. Whilst it is important to emphasise that these sample size suggestions provide rules of thumbs only, the role of sample size is investigated in a sensitivity check by requiring least 10 observations per group. Since JSA claimants can only be present in the original sample for a maximum of 5 times, the minimum sample size requirement restricts the multi-level analysis to a two-level model of individual JSA claimants clustered within local authorities. This restriction has an added benefit in that the survey weights available in the QLFS are not strictly designed for use within longitudinal analyses.

### **8.3.2 Dependent variable, sanctions and additional explanatory variables**

Consistent with the analysis in Chapter 7, the outcome of interest in this chapter relates to self-reported anxiety and/or depression. Unlike in Chapter 7, however, the focus here is on individual-level responses as opposed to rates per local authority population, which is captured through use of a dependent variable in which JSA claimants are coded as 1 if they report suffering from anxiety and/or depression and 0 if they do not. Given that the analysis in this chapter is primarily intended to provide a robustness check on the analysis in Chapter 7, a falsification test is not strictly necessary. For completeness, however, the analysis carries out a falsification test that uses cardiovascular problems as the non-equivalent dependent variable, constructed in a similar fashion to the main dependent variable.

The main explanatory variable of interest is the quarterly local authority-level rate of JSA sanctions, calculated as a proportion of the number of JSA claimants. This sanctions rate differs from that used in Chapters 6 and 7, which constructed a rate as a proportion of the local authority population. Given the focus on the individual-level mental health of JSA claimants in this analysis, however, the selected sanctions rate arguably provides a better measure of the combined threat and imposition effects of sanctions in this context than a population-based rate would provide. As part of the sensitivity tests, nevertheless, the

analysis is re-run using the population-based rate and the substantive results are similar. Of course, the use of a local authority-level sanctions measure elides variation in sanctioning that occurs at the Jobcentre-level. Prior to 2018, there were 784 Jobcentre Plus offices in Britain, approximately two per local authority (Bate, 2017). In addition, it is possible for a JSA claimant to live in a different local authority to the Jobcentre that is administering their claim. Clearly, therefore, a better measure of the sanctions rate would be the sanctions rate as a proportion of JSA claimants at the Jobcentre-level. However, this is not possible using the QLFS as it is not possible to identify the Jobcentres at which each JSA claimant in the sample is registered. This issue reinforces the need for further research in this area using more fine-grained individual-level data.

In addition to the key explanatory variable of interest, the analysis includes a number of individual-level variables that are available through the QLFS and that have previously been shown to influence mental health (Sage, 2015b; Reeves et al., 2016; Carter and Whitworth, 2017), including: age, gender, ethnicity, education, disability, married/cohabiting, having children and socio-economic status. Previous research has indicated that there exists a ‘U-shaped’ relationship between age and mental health, in which an individual’s mental health is lowest in their middle-age, being higher when they are younger and older (Blanchflower and Oswald, 2008). Rates of anxiety and depression in the UK, however, are fairly consistent across people of working age (Baker, 2018). To investigate this issue, the analysis initially considered age-squared as an additional control variable. This measure is not included in the models presented in this chapter, however, due to the fact that its inclusion did not add to the overall model fit and that the estimated coefficient was consistently non-significant. Disability is measured in the QLFS based on long-term disabilities and health problems that substantially limit the day-to-day activities of respondents, as well as the kind or amount of work that they might engage in. Socio-economic status is measured based on the National Statistics Socio-economic classification (NS-SEC).

### 8.3.3 Statistical approach

Binary outcome variables are most commonly analysed through the estimation of logistic regression models. The current analysis, however, is carried out using linear regression in response to the growing recognition of the problems that affect logistic regression models (Angrist and Pischke, 2009; Hellevik, 2009; Mood, 2010; Breen et al., 2018; Gomila, 2019). Mood (2010), for example, details three issues that result from omitted variable bias and which affect the estimation of coefficients in logistic models. As detailed in Chapter 5, unobserved heterogeneity is a well-known source of bias in both linear and logistic regression, which occurs when omitted variables are correlated with the dependent and independent variables that are observed and included in the model. In logistic regression models, however, unobserved heterogeneity impacts regression estimates even when omitted variables are not correlated with the included independent variables. This occurs as a direct consequence of how coefficients are standardised as part of logistic regression, so that the regression residuals follow a logistic distribution (see Mood (2010) for a technical elaboration). This has three important consequences, as Mood (2010: 67-68) details:

- i. It is problematic to interpret log-odds ratios (LnOR) or odds ratios (OR) as substantive effects, because they also reflect unobserved heterogeneity.
- ii. It is problematic to compare LnOR or OR across models with different independent variables, because the unobserved heterogeneity is likely to vary across models.
- iii. It is problematic to compare LnOR or OR across samples, across groups within samples, or over time - even when we use models with the same independent variables - because the unobserved heterogeneity can vary across the compared samples, groups, or points in time.

The issues detailed above have important implications for the current analysis. In a general sense, they indicate that there are concerns with regard to logistic regression estimates and what can be drawn from them in terms of substantive effects, which is clearly a central preoccupation for the current analysis. In



addition, logistic regression estimates cannot be compared across groups within samples or over time, which is a particular issue given the need to compare the impact of sanctions before and after the implementation of the Welfare Reform Act 2012. Importantly, Mood (2010) argues that the use of linear probability models are an acceptable alternative to logistic regression that can be used to overcome these issues, which is also the recommendation of several additional investigations (Hellevik, 2009; Breen et al., 2018; Gomila, 2019).

In light of the above discussion, the analysis of the results in the next section foreground the estimates from linear probability models. Indeed, this approach is commonly adopted in economics and is gaining greater application in sociological and epidemiological research (see, for example, Reeves et al., 2016). Of course, the use of linear probability models to analyse binary dependent variables is itself not necessarily uncontroversial. This relates to the fact that such models can predict unrealistic values that are higher than 1 or lower than 0, can suffer from heteroscedastic and non-normal residuals, and clearly mis-specify the functional form that exists between the dependent and independent variables. Such issues are arguably of lesser importance than the biases introduced in logistic regression models, however: unrealistic predicted values occur in linear regression models even without binary responses; heteroscedasticity can be corrected for; and misspecification is not of overriding concern where the investigation is primarily interested in the “sign and significance of an effect, or of an average effect estimate ... and not in the non-linearity of the relation *per se*” (Mood, 2010: 78). Indeed, estimates from linear probability models also benefit in terms of the immediate interpretability of effect sizes, which is not true of the coefficients or odds-ratios that result from logistic regression. As a sensitivity test, nevertheless, the results of the main analysis are re-run using logistic regression in order to check for the consistency of the results obtained.

To investigate the relationship between area-level sanctions and anxiety and/or depression, the analysis estimates multi-level random intercept models as described in its most basic form in Equation 8.1:

$$Anxiety_{i,j,k} = \beta_0 + \beta_1 Sanctions_{i,j,k} + \beta' X_{i,j,k} + v_k + u_{j,k} + \varepsilon_{i,j,k} \quad (8.1)$$

In Equation 8.1,  $i$  denotes the occasion of measurement (level 1),  $j$  denotes the individual (level 2) and  $k$  denotes the local authority (level 3). *Anxiety* is the self-reported mental health status of an individual  $j$  at occasion  $i$  in local authority  $k$ . *Sanctions* denotes the local authority-level sanctions rate as a proportion of JSA claimants, whilst  $\nu$  is the between-local authority residual (the level 3 random effect),  $u$  is the between-individual residual (the level 2 random effect) and  $\varepsilon$  is the within-individual residual (the level 1 random effect).  $X$  represents a vector of additional individual-level explanatory variables.

The analysis carried out in Chapter 7 highlights the importance of the Welfare Reform Act 2012 with regard to estimating the effect of sanctions on anxiety and or/depression. Previously, the effect of the reform was captured through an interaction term, which interacted the sanctions variable with a dummy variable indicating quarters before and after Q4 2012. A similar strategy is possible here, however for clarity of presentation the analysis divides the sample into the pre- and post-reform periods, repeating the regression models in Equation 8.1 for each period and comparing the results obtained. Whilst the aggregate-level analysis in Chapter 7 considered the same local authorities through time, the multi-level analysis carried out here involves largely different individuals in the pre- and post-reform periods, and so splitting the sample aids interpretation. Splitting the sample has the downside of losing statistical power, however additional checks indicate that the substantive results remain similar whichever method is used.

As in Chapter 7 (see discussion in Section 7.3), a dummy variable coded '0' for quarters prior to Q2 2013 and '1' thereafter is included to capture the slight change in the wording of the health-related question at this point in time. In this analysis, this dummy only needs to be included in the models that relate to the post-reform sample, given the fact that the sample is split as explained above. As was the case in Chapter 7, nevertheless, the substantive results of the analysis carried out here remain unchanged with or without the inclusion of this question change dummy variable.

All regression models include person weights that are calculated and provided by the QLFS, which aim to permit population inferences by taking into account the

survey's sample design as well as differential non-response among different groups (ONS, 2016). As previously indicated, furthermore, the fact that the weights provided are not specifically designed for use in longitudinal analysis partly motivates a sensitivity check in which the main three-level analysis is repeated in a two-level form, based on unique occasions of JSA claimants clustered within local authorities.

## **8.4 Results**

### **8.4.1 Summary statistics**

Summary statistics for the pre- and post-reform samples are detailed in Table 8.1. There are 10,352 individuals in the pre-reform sample and 7,921 individuals in the post-reform sample, which form the basis of the statistics displayed in Table 8.1. In the pre-reform sample, repeat observations mean that the overall sample size available for the multi-level modelling consists of 15,807 occasions of measurement: 6,766 individuals were present in the sample just once and 3,586 were present two or more times. In the post-reform sample, repeat observations mean that the overall sample size available for the multi-level modelling consists of 12,669 occasions of measurement: 4,910 individuals were present in the sample just once and 3,011 were present two or more times. As Table 8.1 indicates, the post-reform period saw a higher proportion of JSA claimants reporting that they suffered from anxiety and/or depression, rising from 7.5% to 12.4% of the sample. The average local authority-level sanctions rate was also higher in the post-reform period, rising from an average of 10.9% to 13.1%.

**Table 8.1:** summary statistics, JSA claimants in the pre- and post-reform samples

	Pre-reform (N = 10,352)	Post-reform (N = 7,921)
<i>Dependent variable:</i>		
Anxiety and/or Depression	7.47	12.37
<i>Sanctions Variable:</i>		
Original adverse	10.93	13.15
<i>Control variables:</i>		
Age	35.29	36.50
Female	36.34	40.93
Ethnicity		
White	84.21	83.07
Mixed	1.59	1.73
Asian or Asian British	6.11	6.72
Black or Black British	5.57	5.98
Chinese	0.38	0.14
Other	2.14	2.36
Disability	25.77	32.43
Qualifications		
Higher education	15.99	15.62
A-Level or equivalent	17.53	17.56
GCSE or equivalent	28.65	28.83
Other qualifications	17.87	16.99
No qualification	18.59	19.13
Don't know	1.37	1.87
Partner	28.51	27.46
Dependent children	31.82	34.09
Socio-economic status		
Never worked	45.66	49.80
Routine and semi-routine occupations	28.56	26.39
Lower supervisory and technical	5.04	4.53
Small employers and own account workers	3.83	3.53
Intermediate occupations	6.27	6.34
Managerial and professional	10.64	9.41
<i>Falsification variable:</i>		
Cardiovascular problems	7.21	7.92

*Note:* table reports unweighted sample percentages with the exception of age and the local authority-level sanctions rate, which report the unweighted means

### 8.4.2 Three-level models

Estimates from the three-level analysis are displayed in Table 8.2, which contrasts between the pre- and post-reform periods in Model 8.1 and Model 8.2 respectively. Separate to the models displayed in Table 8.2, null models without any covariates included are run. These show that - as remains the case in Table 8.2 - there is larger individual- than local authority-level variation, though both are statistically significant at the 5% level in both the pre- and post-reform samples. The results in Model 8.1 indicate that the area-level sanctions rate is associated with increased rates of anxiety and/or depression for JSA claimants in the pre-reform period, though the effect is non-significant at the 5% level. Specifically, a one percentage point increase in the sanctions rate is associated with a 1.1 percentage point increase in the likelihood that a JSA claimant suffers from anxiety and/or depression ( $p = 0.213$ ). Model 8.2 indicates, however, that in the post-reform period a one percentage point increase in the sanctions rate is associated with a 2.1 percentage point increase in the likelihood that a JSA claimant suffers from anxiety and/or depression ( $p < 0.05$ ). These findings support the results of the aggregate-level analysis carried out in Chapter 7, which also only observed statistically significant sanction effects following the implementation of the Welfare Reform Act 2012.

As part of sensitivity analyses, the results of Model 8.1 and Model 8.2 are re-run with time fixed effects and additional area-level factors included (IMD quintiles and rural-urban classification). Due to data availability, the inclusion of the additional area-level factors restricts the analysis to JSA claimants in England. These additional models are detailed in the Chapter 8 appendix (Table A8.1, Section A8.1). The substantive results of the analysis, however, remain unchanged, and so the models including only the local authority-level sanctions rate are detailed here.

**Table 8.2:** relationship between sanctions and anxiety and/or depression

	Model 8.1: Pre-reform	Model 8.2: Post-reform
Sanctions	0.011 (0.008)	0.021* (0.009)
Age	0.008*** (0.001)	0.010*** (0.002)
Female (ref: Male)	0.029*** (0.005)	0.050*** (0.007)
Ethnicity (ref: White)		
Mixed	0.028 (0.018)	-0.021 (0.025)
Asian or Asian British	-0.022* (0.010)	-0.050*** (0.014)
Black or Black British	-0.044*** (0.010)	-0.088*** (0.014)
Chinese	-0.108** (0.041)	-0.068 (0.102)
Other	-0.029* (0.015)	-0.074*** (0.021)
Disability (ref: No disability)	0.220*** (0.005)	0.262*** (0.006)
Qualifications (ref: Higher education)		
A-Level or equivalent	0.001 (0.008)	0.010 (0.011)
GCSE or equivalent	0.010 (0.007)	0.013 (0.010)
Other qualifications	0.011 (0.008)	0.047*** (0.010)
No qualification	0.019* (0.008)	0.011 (0.011)
Don't know	0.007 (0.019)	-0.041 (0.021)
Partner (ref. No partner)	-0.022*** (0.006)	-0.034*** (0.008)
Dependent children (ref. No dependents)	-0.012* (0.005)	-0.012 (0.007)
Socio-economic status (ref: Never worked)		
Routine and semi-routine occupations	-0.004 (0.005)	0.008 (0.006)
Lower supervisory and technical	-0.006 (0.009)	0.008 (0.012)
Small employers and own account workers	-0.024* (0.011)	-0.020 (0.014)

Intermediate occupations	-0.011 (0.009)	-0.021 (0.011)
Managerial and professional	-0.006 (0.008)	-0.018 (0.010)
<hr/>		
Within individual variance	0.015	0.014
Between individual variance	0.044	0.070
Between local authority variance	0.0004	0.001
Occasion, <i>n</i>	15,807	12,669
Individual, <i>n</i>	10,352	7,921
Local Authority, <i>n</i>	376	372
<hr/>		

*Note:* Robust standard errors in brackets. Constant and LFS question change dummy (post-reform model) not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

### 8.4.3 Two-level models

Next, the analysis considers a two-level model, which - as previously indicated - is carried out in order to respond to possible concerns regarding the available sample size at each level in the original three-level analysis. An additional benefit of moving to a two-level model without repeat observations for individuals is that the survey weights available in the QLFS are not designed for use within longitudinal analyses. Accordingly, the models estimated in Table 8.2 are re-run with the additional requirement of at least 10 observations per group, with JSA claimants retained according to the first time that they enter the original sample. This leads to the loss of 83 local authorities in the pre-reform period and 140 local authorities in post-reform period, and a final sample size of 9,831 and 6,699 individuals in each period respectively. Estimates from the two-level analysis are displayed in Table 8.3, which finds similar results to those in the original three-level analysis. That is, the relationship between the area-level sanctions rate and anxiety and/or depression for JSA claimants is positive but not statistically significant in the pre-reform period. In the post-reform period, however, a one percentage point increase in the sanctions rate is associated with a 2.5 percentage point increase in the likelihood that a JSA claimant suffers from anxiety and/or depression ( $p < 0.05$ ).

As was previously the case, furthermore, these substantive results are consistent in sensitivity analyses in which Model 8.3 and Model 8.4 are re-run with time fixed effects and additional area-level factors included. These are detailed in the Chapter 8 appendix (Table A8.2, Section A8.1).



**Table 8.3:** relationship between sanctions and anxiety and/or depression

	Model 8.3: Pre-reform	Model 8.4: Post-reform
Sanctions	0.008 (0.016)	0.025* (0.012)
Age	0.009*** (0.001)	0.010*** (0.002)
Female (ref. Male)	0.026*** (0.005)	0.050*** (0.007)
Ethnicity (ref. White)		
Mixed	0.013 (0.019)	-0.010 (0.026)
Asian or Asian British	-0.022* (0.010)	-0.053*** (0.014)
Black or Black British	-0.042*** (0.011)	-0.080*** (0.015)
Chinese	-0.115** (0.043)	-0.046 (0.117)
Other	-0.015 (0.016)	-0.072** (0.023)
Disability (ref. No disability)	0.233*** (0.006)	0.303*** (0.008)
Qualifications (ref. Higher education)		
A-Level or equivalent	0.001 (0.009)	0.011 (0.013)
GCSE or equivalent	0.013 (0.008)	0.002 (0.012)
Other qualifications	0.005 (0.009)	0.031* (0.013)
No qualification	0.015 (0.009)	0.0005 (0.013)
Don't know	0.004 (0.022)	-0.025 (0.028)
Partner (ref. No partner)	-0.023*** (0.006)	-0.034*** (0.009)
Dependent children (ref. No dependents)	-0.013* (0.006)	-0.014 (0.008)
Socio-economic status (ref: Never worked)		
Routine and semi-routine occupations	0.001 (0.006)	-0.013 (0.009)
Lower supervisory and technical	-0.012 (0.011)	-0.005 (0.017)
Small employers and own account workers	-0.029* (0.013)	-0.056** (0.020)

Intermediate occupations	-0.008 (0.011)	-0.035* (0.015)
Managerial and professional	-0.009 (0.009)	-0.030* (0.014)
Between individual variance	0.056	0.081
Between local authority variance	0.0004	0.001
Individual, <i>n</i>	9,831	6,699
Local Authority, <i>n</i>	293	232

*Note:* Robust standard errors in brackets. Constant and LFS question change dummy (post-reform model) not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

#### 8.4.4 Robustness tests

In addition to the sensitivity checks already alluded to, two further tests are carried out in order to underpin the robustness of the results discussed. First, the models of the main multi-level analysis are re-run using logistic regression, the results of which are detailed in Table A8.3 in the appendix for Chapter 8 (Section A8.2). Given the issues relating to sample size and survey weights highlighted above, Table A8.3 details results from two-level logistic regression models. Consistent with the previous analysis, nevertheless, results across the three- and two-level models are broadly similar. The results in Table A8.3 support the findings of the main analysis, in that a positive sanctions coefficient is estimated in both the pre- and post-reform periods, whilst the result is larger and statistically significant at the 5% level in the post-reform period (Model A8.6). These results indicate that the findings of the main analysis are not arbitrarily driven by the decision to estimate models using linear regression.

Finally, a falsification test is run using cardiovascular problems as the non-equivalent dependent variable, as detailed in Table A8.4 in the appendix for Chapter 8 (Section A8.3). Again, this details the results from two-level models. The results of the main analysis are supported by the fact that the sanctions coefficients in the pre- and post-reform periods (Model A8.7 and Model A8.8, respectively) are non-significant at the 5% level. Contrary to the results of the main analysis, furthermore, the sanctions coefficient is smaller and very close to zero in the post-reform period.

### 8.5 Conclusion

This chapter has investigated the relationship between the local authority-level sanctions rate and rates of anxiety and/or depression for individuals claiming JSA. Primarily, the analysis has been carried out in order to provide a robustness check on the aggregate-level investigation detailed in Chapter 7, which is open to the concern that the estimated association between sanctions and anxiety and/or depression at the local authority-level is driven by compositional bias. Importantly, the findings presented in this chapter support - or at the very least do not undermine - the results of the ecological analysis previously carried out. Results from the three-level models, for example, indicate that higher sanction

rates are associated with higher rates of anxiety and/or depression for JSA claimants. In the post-reform period, a one percentage point increase in the sanctions rate is associated with a 2.1 percentage point increase in the likelihood that a JSA claimant suffers from anxiety and/or depression ( $p < 0.05$ ). Similar results are observed in two-level models that include each JSA claimant only once, where it is estimated that in the post-reform period a one percentage point increase in the sanctions rate is associated with a 2.5 percentage point increase in the likelihood that a JSA claimant suffers from anxiety and/or depression ( $p < 0.05$ ). These results are supported, furthermore, in additional robustness tests using logistic regression and a falsification test.

Whilst the analysis in this chapter has been primarily carried out in order to provide a robustness check on the results estimated in Chapter 7, it is an interesting finding in and of itself that a positive association exists between the area-level sanctions rate and rates of anxiety and/or depression for JSA claimants. A separate but limited literature, for example, has considered the potential for area-level unemployment rates to partially moderate the impact of unemployment on mental health, which are hypothesised to operate in response to social norm effects and/or the development of effective support networks (Clark, 2003; Flint et al., 2013). Whilst Strandh et al. (2011: 799) find “no coherent effect” of area-level unemployment rates on mental health in Sweden, Flint et al. (2013: 1) find that in the UK, living in areas with higher unemployment rates provides a “degree of protection” against the mental health impact of unemployment. Such dynamics have not been the focus of the analysis in this chapter, though the findings suggest that claiming JSA in areas with higher sanctions rates is associated with higher rates of anxiety and/or depression, which pertains despite the influence of any countervailing moderation effects that may be operating.

Given the data available, it is not possible to interpret the relationship investigated in this chapter in terms of the causal effect of sanctions on mental health. As previously highlighted, for example, it would be more meaningful to study the Jobcentre-level sanctions rate in this context. In addition, one possible alternative explanation for the results obtained is that higher rates of anxiety and/or depression may themselves increase the likelihood of claimants being

sanctioned. Arguably, this concern is partly limited by the fact that a stronger relationship between the local authority-level sanctions rate and rates of anxiety and/or depression is observed in the post-reform period. This is important, since this finding is consistent with the expectation that harsher sanctions will have a stronger impact on mental health outcomes, but is harder to explain if the direction of causality runs solely in the other direction. Given that this limitation in terms of causal inference persists, however, the investigation in the next and final empirical chapter aims to provide a better estimate of the causal effect of sanctions on claimant mental health. This is carried out through the use of a difference-in-differences research design, as is explained in Chapter 9.

## Chapter 9. Difference-in-differences analysis: anxiety and/or depression

### 9.1 Introduction

This chapter investigates the impact of benefit sanctions on mental health outcomes in a manner that is distinct to the analyses carried out in Chapters 6 to 8. The previous chapters considered variations in the frequency and severity of sanctions across the period of Coalition government (2010-15). This chapter, in contrast, investigates the impact of changes in sanctions policy that occurred at the onset of the Coalition government in 2010. It uses individual-level data on the self-reported mental health of JSA claimants from the two-quarter longitudinal Labour Force Survey (LFS) datasets (ONS, 2018a), as opposed to the separate quarterly datasets that were used in Chapter 7 (ONS, 2018c). The specific research question that is associated with this stage of the analysis is the following:

[RQ 9.1]: Are harsher sanctioning environments associated with adverse impacts on the mental health of JSA claimants?

To investigate this research question, the analysis makes use of the difference-in-differences (DiD) method, which was previously described in broad terms in Chapter 5. This chapter begins by explaining the DiD method in greater detail, outlining how its use of a ‘treatment’ and a ‘control’ group enables it to better estimate the causal effects of policy interventions. The analysis carried out in this chapter therefore aims to contribute in terms of causal inferences, given the limitations in this regard that have been discussed in the previous empirical chapters. Next, the chapter outlines the particular changes in sanctions policy that occurred at the onset of the Coalition government, justifying why this period was selected over other stages of the ‘great sanctions drive’ described in Chapter 3. The chapter then outlines the data and statistical approach that will be used in the analysis, providing more specific detail to that previously outlined in Chapter 5. The results of the analysis are then presented and discussed.

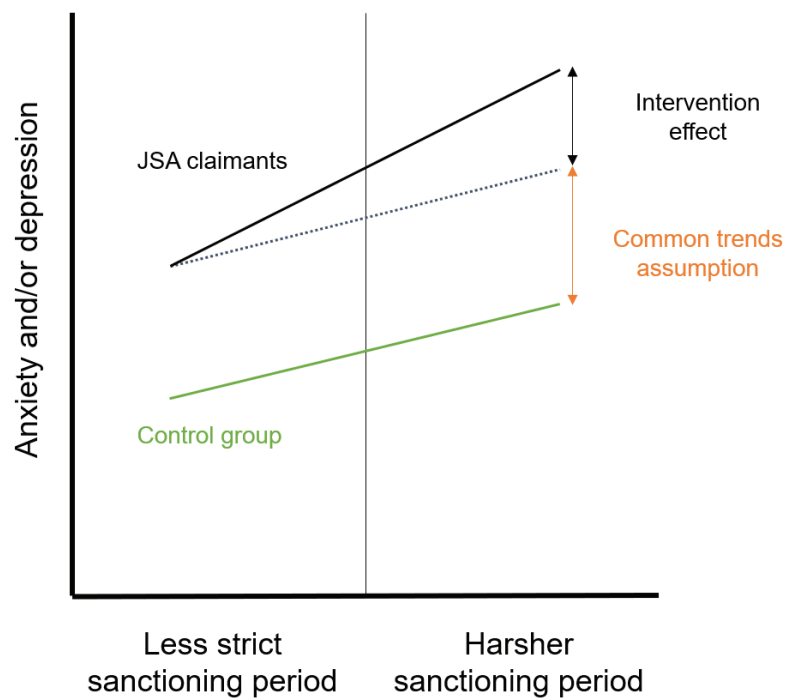
## 9.2 Difference-in-differences research design

As explained in Chapter 5, DiD analyses attempt to approximate the ‘consequential manipulation’ that forms the basis of experimental research designs, and aim to improve upon the inferences that can be made when relying on observational data (Angrist and Pischke, 2009). In the standard two-group and two-period design, a ‘treatment’ and ‘control’ group are identified and compared before and after a policy intervention occurs that affects the treatment group in some way. DiD analyses proceed in two steps. First, outcomes for the treatment group are compared before and after the policy intervention. In the current context, this involves comparing the self-reported mental health of JSA claimants before and after a particular change in sanctions policy, which will be described below. The issue with relying on this initial difference, however, is that it does not take into account the fact that outcomes for the treatment group may have changed over the period, irrespective of the policy intervention itself. This issue is dealt with by comparing the average change in the outcome for the treatment group with the average change in the outcome for a comparable control group that is unaffected by the policy intervention. If the common trends assumption is met, in that the two groups can be assumed to have followed similar trends in the outcome in the absence of the policy intervention, then this comparison identifies the intervention effect of the policy change.

Figure 9.1 portrays a stylised DiD research design, using the policy context that forms the basis of the analysis in this chapter. In particular, this involves an increase in the harshness of the sanctioning environment at the onset of the Coalition government in 2010. As displayed in Figure 9.1, the analysis will compare the self-reported mental health of JSA claimants in a less strict sanctioning period prior to the onset of the Coalition with those of JSA claimants in the later period. This first comparison provides the observed change in the outcome for the treatment group that is indicated in Figure 9.1. The DiD intervention effect is then arrived at through comparison of this change with the change in mental health observed in a comparable control group. Section 9.3 in this chapter provides more detail of the specific control group that will be used in the analysis itself. The remainder of this section details the policy change that

occurred at the onset of the Coalition government in 2010. In many DiD designs, the treatment group goes from a state of being fully untreated in the pre-intervention period to fully treated in the post-intervention period. In this context, however, the JSA treatment group moves from a less strict sanctioning environment to a harsher sanctioning environment, and as such the current investigation can be understood as adopting a “fuzzy difference-in-differences” design (de Chaisemartin and D’Haultfoeuille, 2018: 999).

**Figure 9.1:** difference-in-differences diagram



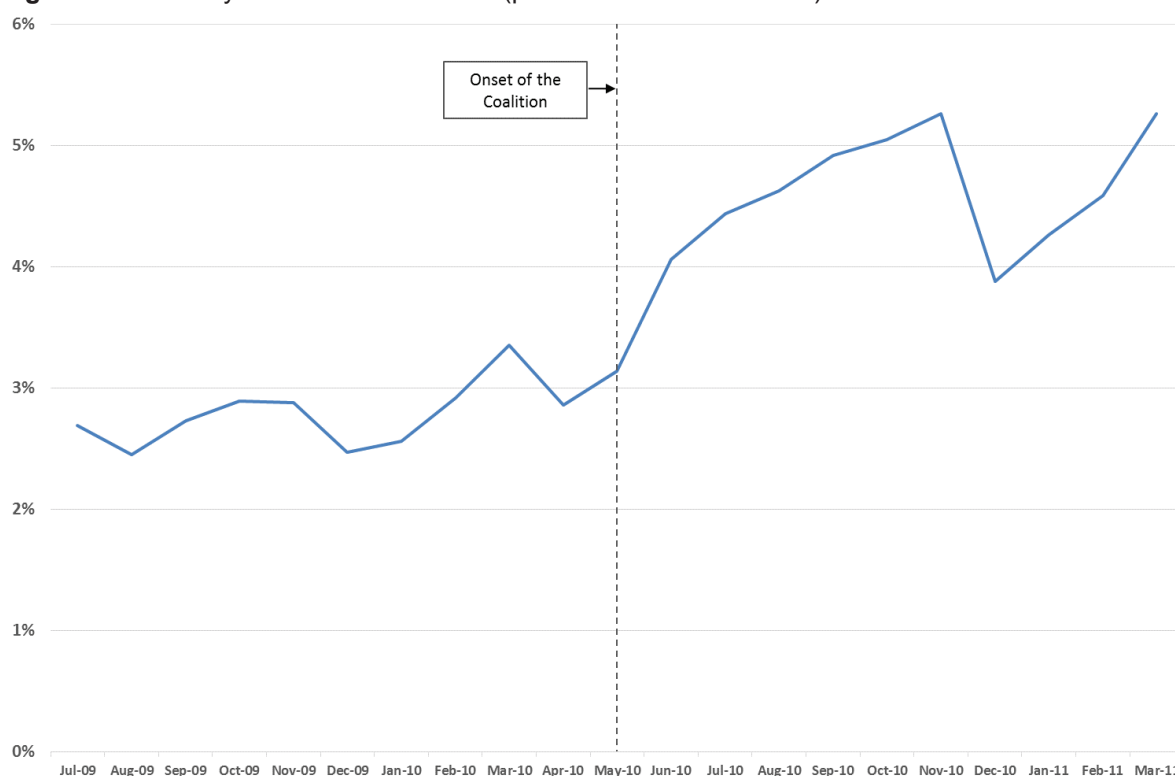
Source: created by the author

The move to a harsher sanctioning environment at the onset of the Coalition government in May 2010 is constituted by two changes in sanctions policy. First, in April 2010, the penalty for JSA claimants for non-attendance at interviews with JSA advisers was changed from disqualification to a fixed length sanction of one week for a first ‘failure’ and two weeks for subsequent ‘failures’ (DWP, 2013b). As previously highlighted in Chapter 3, under the old disqualification rules the claimant’s existing claim was discontinued but they were then able to re-claim immediately, leading to very limited benefit loss for the claimant (DWP, 2011a). Thus, the change to a fixed sanction period resulted in an increased penalty for one of the most common reasons for a sanction being imposed. In addition to this change in the sanction rules, furthermore, the months following the onset of the Coalition in May 2010 saw an increase in the



frequency of benefit sanctions being applied. As Figure 9.2 demonstrates, for example, the JSA sanctions rate increased from approximately 3% in May 2010 to over 5% in November 2010, having generally been below 3% in the year prior to the onset of the Coalition. As previously discussed in Chapter 3, this marks the beginning of the ‘great sanctions drive’ observed under the Coalition government. Developments at the beginning of this period are described by Webster (2016: 2) as representing an “unannounced change of policy” in relation to the increase in the frequency of sanctions.

**Figure 9.2:** monthly rate of JSA sanctions (per cent of JSA claimants)



Source: author's calculations using DWP Stat-Xplore data (DWP, 2018c)

Taken together, the change in sanctioning rules and the increase in the rate of sanctions mean that the sanctioning environment in the period following the onset of the Coalition government was much harsher than that of the one that preceded it. In the process of the analysis, alternative time periods throughout the Coalition government were also considered. However, the combined increase in the severity and frequency of sanctions that coincided with the onset of the Coalition provided the best policy change for an intervention effect to be estimated. Following the implementation of the Welfare Reform Act 2012, for example, the severity of sanctions were hugely increased but the sanctions rate initially declined (see Figure 3.1 in Chapter 3). These are circumstances that do

not provide as clear developments for a DiD analysis as those exploited here. Arguably, the sanctioning environment brought about by the onset of the Coalition government can be considered to represent an exogenous shock in terms of the experience of JSA claiming, in which the severity, threat and imposition of sanctions increased. Indeed, because this change in sanctions policy was not plausibly driven by changes in the mental health of JSA claimants themselves, this period provides a good context for the application of the DiD design and its use in supporting causal inferences regarding the impact of sanctions on mental health.

## **9.3 Data and methods**

### **9.3.1 Datasets**

In order to carry out the DiD analysis, the investigation makes use of two-quarter longitudinal Labour Force Survey (LFS) datasets (ONS, 2018a). As previously explained in Chapter 5, the rotational sampling design used in the QLFS means that each household is interviewed for five consecutive quarters and the ONS produces two- and five-quarter datasets that link information on working age survey respondents who respond in consecutive quarters. The analysis makes use of the two-quarter datasets given the larger sample size that they provide. Each two-quarter dataset contains approximately 35,000 individuals, compared with approximately 5,000 individuals in each five-quarter dataset. The result of linking the quarterly LFS datasets introduces the risk of two main biases: first, sample attrition will occur as a result of non-response; and second, response errors will produce spurious flows between different states of economic activity. The first of these biases will be mainly problematic where there is differential attrition, in which particular groups of individuals are more likely than other groups to drop out of the survey from one quarter to the next. To deal with such differential non-response bias, the ONS produce longitudinal weights that are applied in the analysis carried out here. The risk of spurious flows between quarters, furthermore, is minimised by the way in which the two-quarter datasets are ultimately used in the analysis, as explained below.

Preferably, DiD analyses should use longitudinal data on the same individuals in the pre- and post-intervention time periods, as this helps minimise the influence

of compositional bias. Repeated cross-sectional designs are also common, however, that analyse data on different individuals in each time period. Such designs make the additional assumption that individuals in the period-one cross-section are comparable to those in the period-two cross-section for both the treatment and control groups respectively. As a result of the timings of the sanctions policy change described above, and in conjunction with sample size concerns relating to the size of the JSA claimant group captured within the LFS, a cross-sectional DiD design forms the basis of the analysis in this chapter. A longitudinal element, nevertheless, is introduced by combining two different two-quarter LFS datasets from the pre- and post-intervention periods, which make it possible to observe and compare the change in mental health for JSA claimants from one quarter to the next. Consequently, the analysis is able to investigate the intervention effect that is associated with being a JSA claimant at a point during the harsher sanctioning environment, and to do so in terms of how that experience impacts the mental health of those JSA claimants into the following quarter.

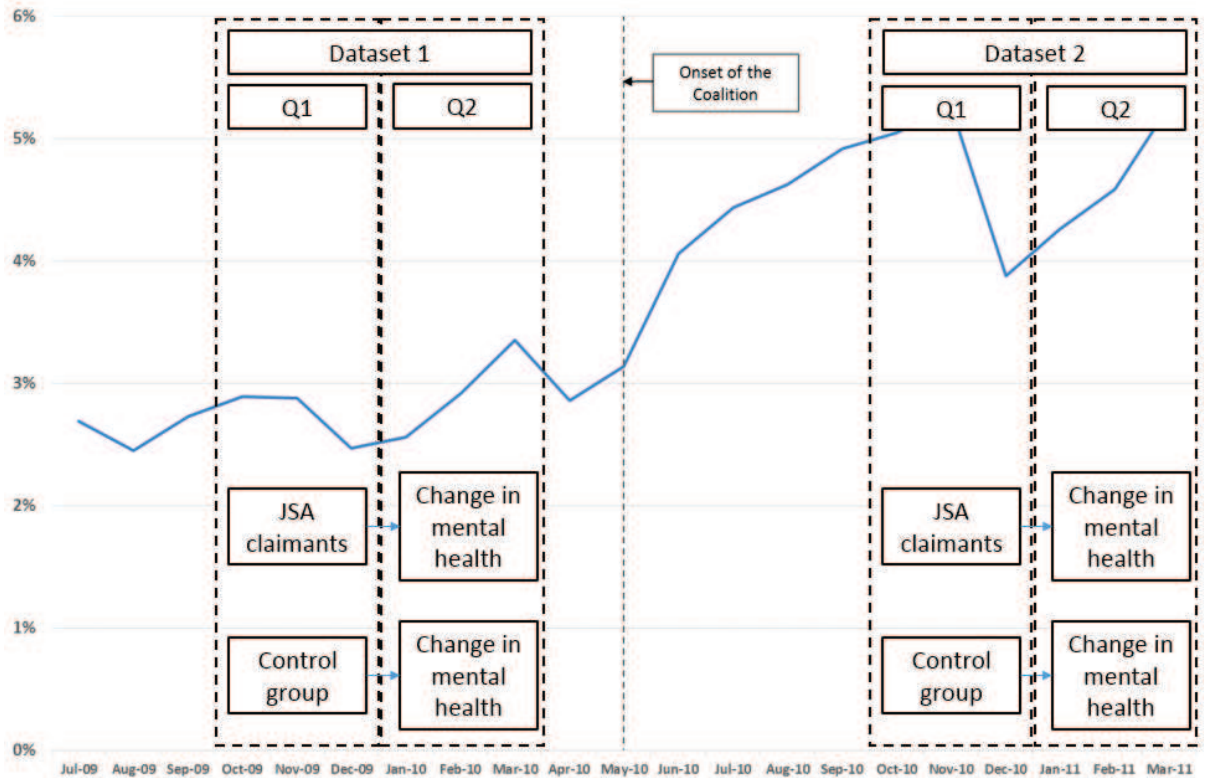
The findings in this chapter therefore differ from the results in the previous empirical chapters, in that they are able to capture lagged - as opposed to contemporaneous - sanction impacts. The findings in the previous chapters were primarily based on quarterly variation in sanction rates at the local authority-level, the effects of which were captured by quarterly variation in mental health outcomes in the corresponding quarters. They therefore focused on the impact of sanctions in terms of levels of mental health outcomes. This chapter, in contrast, brings an additional element to the analysis, as it considers the impact of sanctions on changes in mental health. The research design does not therefore rely on there being a contemporaneous impact on mental health for those claiming JSA in the harsher sanctioning environment. Rather, it assumes that claiming JSA in this period may have an impact into the next quarter. As the next sub-section details, this allows the analysis to focus solely on those JSA claimants who initially did not report suffering from anxiety and/or depression, which helps deal with the influence of pre-existing mental health problems. Future research, of course, could conduct a similar DiD analysis to the one carried out here, but instead focus on levels of anxiety and/or depression using the QLFS as opposed to the longitudinal LFS datasets used in this chapter.

Table 9.1 details the specific two-quarter longitudinal LFS datasets that are combined as part of the analysis. The selection of these particular datasets is informed by a number of considerations. First, the dataset in the post-intervention period is selected to coincide with the exogenous shock provided by the harsher sanctioning environment (Dataset 2). Dataset 2 starts with the October-December 2010 quarter as opposed to the July-September 2010 quarter in order to most clearly capture the influence of this shock. Second, the pre-Coalition Dataset 1 is exactly a year before Dataset 2, which is necessary in order to avoid introducing biases relating to seasonality. Last, it is important to highlight that the claimant rate was level across the two time periods selected (NAO, 2016a). This means that the two different groups of JSA claimants that form the treatment group are likely to share a similar distribution of characteristics, which minimises the influence of compositional bias on the results that are ultimately obtained.

**Table 9.1:** two-quarter longitudinal LFS datasets used in the difference-in-differences analysis

	Quarter 1	Quarter 2
<u>Dataset 1:</u> Pre-intervention (less strict sanctioning)	October-December 2009	January-March 2010
<u>Dataset 2:</u> Post-intervention (harsher sanctioning)	October-December 2010	January-March 2011

Figure 9.3 provides a graphical depiction of how these datasets align with changes in sanctions policy as previously shown in Figure 9.2. Having identified the specific two-quarter longitudinal LFS datasets that are used within the investigation in this chapter, the remainder of this section goes on to explain other important aspects of the DiD analysis, including: how mental health is measured and operationalised; the construction of the treatment and control groups; and the statistical models that are ultimately estimated.

**Figure 9.3:** two-quarter longitudinal LFS datasets used in the difference-in-differences analysis

Source: created by the author

### 9.3.2 Dependent variable and falsification variable

Similar to the analysis in Chapter 7 and Chapter 8, the mental health status of respondents is identified using a self-reported measure, in which LFS survey respondents are first asked whether or not they suffer from ‘health problems’. They can then identify as suffering from ‘depression, bad nerves or anxiety’ from a pre-specified list (see Box 5.1 in Chapter 5), which is referred to throughout this chapter as anxiety and/or depression. As previously indicated, the analysis is specifically concerned with changes in the mental health status of JSA claimants from one quarter to the next before and during the harsher sanctioning environment. In order to consider changes in mental health, two new indicators were initially investigated as part of the analysis. This is necessitated by the fact that the measure of anxiety and/or depression provides a binary indication of an individual’s mental health status. The first measure of change can be described in terms of ‘worsening’ mental health. This measure is coded ‘1’ if an individual did not report as suffering from anxiety and/or depression in the first quarter but did report as suffering from anxiety and/or depression in the second quarter. Conversely, it is coded ‘0’ if an individual did not report as suffering from anxiety and/or depression in either quarter. This

measure is summarised in Table 9.2 and is used to investigate whether or not the experience of claiming JSA in the harsher sanctioning period is associated with an increased prevalence of newly experiencing anxiety and/or depression.

**Table 9.2:** construction of ‘worsening’ mental health variable

Anxiety and/or depression (Q1)	Anxiety and/or depression (Q2)	‘Worsening’ mental health variable
No	No	0
No	Yes	1

Similarly, the second outcome variable considered can be described as an ‘improving’ mental health variable. As summarised in Table 9.3, this measure is coded ‘1’ if an individual suffered from anxiety and/or depression in the first quarter but did not in the second quarter, and is coded ‘0’ if they suffered from anxiety and/or depression in both quarters. This measure is used to investigate whether or not the experience of claiming JSA in the harsher sanctioning period is associated with a reduced prevalence of improvements in mental health, from suffering to not suffering from anxiety and/or depression.

**Table 9.3:** construction of ‘improving’ mental health variable

Anxiety and/or depression (Q1)	Anxiety and/or depression (Q2)	‘Improving’ mental health variable
Yes	Yes	0
Yes	No	1

Initially, both the ‘worsening’ and ‘improving’ measures of change in mental health status were considered as part of the analysis. Unfortunately, however, there are very few instances of improvements in mental health observed for JSA claimants in the sample, which does not provide a sufficient number of observations to carry out a robust DiD analysis. The sample sizes for the JSA treatment group are displayed in Table 9.4, which displays the number of observations for the ‘worsening’ and ‘improving’ measures in both the pre- and post-intervention periods. With regard to the ‘improving’ measure, for example, there are only 40 JSA claimants present in the harsher sanctioning environment, 35 of which suffered from anxiety and/or depression in both Q1 (October-December 2010) and Q2 (January-March 2011) and 5 of which saw their mental health improve between the quarters. The sample size available for the

‘worsening’ measure, in contrast, is substantially higher, and as a result this is the dependent variable that forms the basis of the analysis conducted throughout this chapter.

**Table 9.4:** sample size for ‘worsening’ and ‘improving’ mental health variables, JSA claimants

	‘Worsening’ mental health		‘Improving’ mental health	
	Maintained	Worsened	Maintained	Improved
Pre-intervention (less strict sanctioning)	909	13	32	6
Post-intervention (harsher sanctioning)	786	25	35	5

*Note:* table reports unweighted sample *Ns*

The sample size available for the ‘worsening’ mental health measure is larger than that of the ‘improving’ variable, though Table 9.4 indicates that only a small proportion of JSA claimants report that their mental health status worsens from one quarter to the next. In terms of the ‘worsening’ measure, there are 811 JSA claimants present in the harsher sanctioning environment, 25 of which report that they suffer from anxiety and/or depression in Q2 having not suffered from it in Q1 (approximately 3% of the sample). This issue re-emphasises limitations previously discussed in Chapter 5, where it was noted that the available measure provides only a binary account of mental health.

Consequently, anxiety and/or depression is treated as a category as opposed to a continuum, meaning that if an individual’s mental health does worsen between the quarters this will only be captured if they record it in the terms specified in the QLFS questionnaire. This issue is compounded by the fact respondents are asked whether or not they expect their health problem to last more than a year. This further reduces the likelihood that an individual’s mental health will be observed to worsen from one quarter to the next even where such a deterioration does in fact take place. Together, these limitations increase the likelihood that the DiD analysis will observe no intervention effect on mental health, even where such an impact may in fact exist.

In a robustness check on the main results of the analysis, a falsification test is carried out that uses the non-equivalent dependent variable approach. Similar to the analysis in Chapter 7 and Chapter 8, responses in terms of people suffering

with ‘Heart, blood pressure or blood circulation problems’ (henceforth ‘cardiovascular problems’) are used as a falsification variable. This is constructed in a similar fashion to the ‘worsening’ mental health measure as detailed in Table 9.2. In addition to a falsification test, the analysis carries out a placebo test. As previously explained in Chapter 5, this involves repeating the results of the main analysis in a period that is not marked by any significant changes in sanctions policy. By choosing a time period that was relatively stable in terms of sanctions policy, the results of the main analysis are supported if the placebo DiD analysis does not find evidence of an intervention effect. The placebo test provides a better check than the falsification test in the current context, since the latter may always be open to the concern that it is based on a poor choice of non-equivalent dependent variable. In the analysis carried out here, a period is selected in which the JSA claimant count was stable and in which there were no changes in either the severity or frequency of sanctions, ultimately leading to the use of two-quarter LFS datasets prior to the 2007-08 financial crisis and recession. The exact details of the datasets used as part of the placebo test are detailed in the appendix for Chapter 9 (Section A9.5).

### **9.3.3 Treatment and control groups**

As previously indicated, the analysis in this chapter carries out a cross-sectional version of the DiD design in which the treatment and control groups are formed of different individuals in the pre- and post-intervention periods. First, the treatment group is formed of JSA claimants in Quarter 1 for each dataset detailed in Table 9.1. These are a group of individuals who are claiming JSA in October-December 2009 and a different set of individuals who are claiming JSA in October-December 2010. These are the JSA claimants that form the sample detailed in Table 9.4. In terms of the ‘worsening’ mental health measure, for example, there are 922 JSA claimants in the pre-intervention period and 811 JSA claimants in the post-intervention period. For each group, no restrictions are placed on their economic status in the following Quarter 2 when their mental health status is measured again. This decision is taken to capture the expectation that the experience of claiming JSA in the harsher sanctioning environment will impact an individual’s mental health, and will do so irrespective of that individuals’ circumstances in the next quarter. In addition,



however, this construction has additional advantages in terms of sample size, as well as in helping to deal with the second source of bias that affect the two-quarter longitudinal LFS datasets. As previously described, potential response errors may lead to erroneous flows between different states of economic activity, which are not dealt with through the use of longitudinal weights.

A crucial component in a DiD analysis is an appropriate control group. In order to take into account the role of time trends, the treatment and control groups must be expected to follow a common trend in mental health outcomes in the absence of the policy change. If this condition is not met, the DiD estimator will be biased and will not accurately estimate the causal impact of the policy intervention. The remainder of this sub-section details the process of identifying a suitable control group for the DiD analysis. As highlighted in Chapter 4, elsewhere in the sanctions literature Machin and Marie (2006) use a DiD design to investigate impacts on crime. Their study benefits from the fact that claimants in particular pre-JSA duration categories were more affected by the introduction of JSA than others, which provided the variation that enabled areas to be split into treatment and control groups depending on their exposure to the policy change. Such geographical variation, however, is not present in the current context. In the process of the investigation, geographical variation in rates of JSA sanctions in the pre- and post-May 2010 periods was initially investigated. However, whilst some geographical variation was observed, rates of JSA sanctions increased in all areas following the onset of the Coalition, consistent with the view that the change in sanctions policy was an exogenous and centrally-determined decision. Indeed, the April 2010 rule change regarding the penalty for non-attendance at an advisory interview applied nationally, further limiting the scope for a DiD analysis based on geographical variation.

In light of this, a variety of control groups are considered throughout the analysis, which will be explained in turn here. The first option considered is JSA claimants in Northern Ireland. Importantly, the 'great sanctions drive' described in Chapter 3 relates to sanctions policy in Britain specifically. In Northern Ireland, the frequency of sanctions was much lower than in the rest of the UK and did not see any dramatic variations throughout the period, whilst changes in the severity of sanctions were less harsh and applied at a later date (Webster,

2018). The same applies in the context considered in this chapter, given that the April 2010 changes that were introduced in Britain were only implemented in Northern Ireland in March 2012 (HM Government, 2012b). Whilst JSA claimants in Northern Ireland provide a plausible control group for JSA claimants in the rest of the UK, their use in the current context is limited by their low number of observations in the datasets used in the analysis. Specifically, there are 54 and 58 JSA claimants in Northern Ireland in the pre- and post-intervention samples respectively. Additional concerns relate to their geographic separation from JSA claimants in the rest of the UK and wider differences in policy context. Whilst JSA claimants in Northern Ireland are retained in the analysis, these issues motivate the identification of alternative control groups.

Several additional control groups are considered in the form of non-JSA claimant groups that are identifiable within the two-quarter LFS datasets, which include: Income Support (IS) claimants; Incapacity-related Benefit (IB) claimants; Employment and Support Allowance (ESA) claimants; Working Tax Credit (WTC) claimants; and unemployed non-claimants of JSA. For the purposes of the analysis, the group of IS claimants are restricted to the main lone parent group, whilst the IB group are formed of the three benefits that were replaced by ESA: Incapacity Benefit claimants; Severe Disablement Allowance; and Income Support paid because of an illness or disability. Each control group is constructed in a similar way to that described for the JSA treatment group, as detailed above. Admittedly, none of these potential control groups provide an ideal counterfactual for the JSA treatment group, since it can be argued that each might tend to differ systematically from JSA claimants in terms of particular observed and unobserved characteristics. This issue, unfortunately, represents an unavoidable limitation on the DiD analysis that is carried out in this chapter. Additional investigation, however, is carried out in order to limit the overall influence of this issue on the results obtained.

First, the suitability of each control group is considered by checking whether or not their circumstances remained the same in the pre- and post-intervention periods. Like JSA claimants, several of the control groups considered are subject to work-related behavioural conditionality, as discussed in Chapter 3. It is important to ensure that there were no significant policy changes for these

groups over the period, as this helps strengthen the claim that any estimated intervention effect can be plausibly linked to the harsher sanctioning environment. Table 9.5 summarises relevant developments for each group. It indicates that - out of the various options considered in addition to JSA claimants in Northern Ireland - the best control groups available are the Working Tax Credit (WTC) group and the unemployed non-claimants of JSA group. This is because both of these groups were not affected by any important policy changes during the period. IS, IB and ESA claimants, on the other hand, were subject to a number of reforms that serve to weaken their usefulness as control groups. Indeed, the validity of comparing JSA claimants to claimants of lone parent benefits or sickness and disability benefits might itself be questioned, and so these groups are ultimately not used in the investigation. With regard to the remaining WTC and unemployed non-JSA claimant control groups, furthermore, it is useful to consider what similarities and differences these groups have with the JSA treatment group in order to assess their suitability, which will be considered in turn here.

**Table 9.5:** relevant developments for possible control groups, October 2009 – March 2011

Control group	Policy developments
JSA claimants (NI)	Sanctions policy did not change during the period of analysis.
WTC claimants	No changes occurred during the period of analysis. Key alterations to WTC rates, thresholds and tapers began in April 2011 (Hood and Phillips, 2015).
Unemployed (non-JSA claimants)	As non-claimants, there are no particular changes that affected this group during the period of analysis, whilst the ILO unemployment rate remained broadly level (Phillips, 2017).
IS claimants	In October 2009, eligibility for IS was changed, ending for lone parents with a youngest child aged 10 from the previous limit of 12 years old. This was further reduced to a youngest child aged seven in October 2010 (CPAG, 2016).
IB claimants	Work Capability Assessments for existing IB claimants began in October 2010, with a trial in Aberdeen and Burnley (McInnes, 2012). This was followed by a full national rollout in April 2011, which is beyond the period of analysis. Garthwaite et al. (2014: 311) describe a “period of substantial change” for IB claimants that takes place during the period of analysis.
ESA claimants	Work Capability Assessments for ESA began at its creation in October 2008. Rates of initial assessments, as well as results of the assessments (support group versus work-related activity group) remained very similar across the period of analysis (DWP, 2011b). The only sanctions for ESA claimants - for failure to attend a mandatory interview - were falling rapidly at the onset of the Coalition, and began rising again as a result of the new ‘non-participation in work related activity’ sanction introduced at the onset of the Work Programme in June 2011 (Webster, 2016).

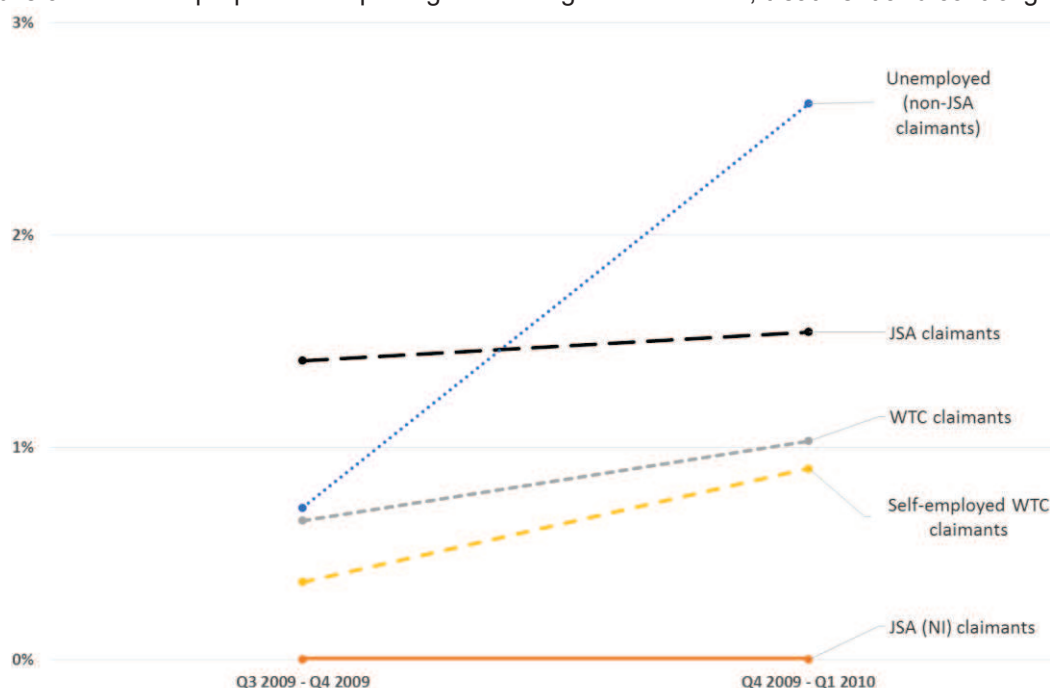
Working tax credits are available for individuals on low incomes who are working at least 16 hours a week, whilst JSA is available for the unemployed or those working less than 16 hours a week. Given the differences that might be expected in the circumstances of these two groups, however, an additional comparison group of the self-employed WTC claimants is included in the analysis, which arguably serves as a better control group than the WTC group as a whole. Following the 2007-08 recession, the UK experienced a dramatic rise in the numbers of recorded self-employment and it has been argued that many of the newly self-employed can be considered to represent a hidden form of unemployment, in the sense that they registered as such because they could not

find employment and/or wanted to avoid claiming the more onerous JSA benefit (Hatfield, 2015). Indeed, this process has been described by Jordan (2018: 597) in terms of “zero-work self-employment”, who notes that in these circumstances JSA claimants and self-employed WTC claimants will receive benefits of a similar value. Admittedly, robust empirical evidence on this phenomenon is lacking, though D’Arcy and Gardiner (2014) estimate that this effect accounts for up to a quarter (24%) of the growth in self-employment following the recession. With this in mind, the analysis constructs a control group of self-employed WTC claimants, which provides a means of identifying this ‘hidden unemployed’ group and thus potentially providing a stronger comparison group than WTC claimants as a whole.

With regard to unemployed non-claimants of JSA, these represent unemployed individuals who - for a variety of reasons - are not themselves claiming JSA. There are several technical differences between ILO unemployment and the claimant count, though the gap between the two measures began growing in the 1990s, arguably driven by the introduction of JSA and the associated increase in work-related behavioural conditionality (Phillips, 2017). By 2010, overall unemployment was two-thirds higher than the claimant count, and as the gap has grown the age and gender differences between the two groups have narrowed (Clancy and Stam, 2010). Whilst this latter development is a useful one in the context of the current analysis, it of course remains the case that the two groups might differ in important respects. Unemployed non-JSA claimants, for example, are likely to enjoy a slightly better access to financial resources than JSA claimants, due to savings or support from friends and family, with such access meaning that they are either ineligible for JSA or don’t require financial support from the state. The possibility exists, furthermore, that some individuals in the unemployed non-JSA claimant group may have been subjected to sanctions themselves and stopped claiming JSA as a result, which weakens their validity as a control group. Whilst these limitations are recognised, the unemployed non-JSA claimant group is nonetheless retained in the analysis, and serves as a useful sensitivity check on the results for the other control groups investigated.

To consider this issue further, Figure 9.4 displays the trend in the proportion of individuals who experienced ‘worsening’ mental health in the period prior to the onset of the Coalition, using Dataset 1 (October 2009 - March 2010) and the previous two-quarter longitudinal LFS dataset (July 2009 - December 2009). With DiD analyses, the control group functions as a counterfactual for the treatment group. As such, the common trends assumption is not itself directly observable or verifiable. It is useful, however, to consider whether or not the treatment and control group(s) follow similar trends in the pre-intervention period. If they do, this helps support the claim that the two groups would have followed parallel trends in the post-intervention period. As shown in Figure 9.4, the proportion of JSA claimants reporting worsening mental health is broadly consistent in the pre-intervention period, rising only very slightly between the two datasets used. With regard to the possible control groups, trends for JSA claimants in Northern Ireland, WTC claimants and self-employed WTC claimants are not perfectly parallel with the JSA treatment group, though do not appear to diverge dramatically from the common trends assumption. The same does not pertain, however, for unemployed non-JSA claimants. This group saw an increase in the proportion reporting worsening mental health far greater than that observed for the JSA treatment group. This divergence further weakens the validity of the unemployed non-JSA claimant group as a control group in the analysis.

**Figure 9.4:** trend in proportions reporting ‘worsening’ mental health, treatment and control groups



*Note:* sample proportions use longitudinal weights

Lastly, a robustness test on the main investigation is carried out that seeks to limit the influence of differences between the treatment and control groups through the use of a matching technique. As explained in Chapter 5, this method aims to reduce bias by estimating the intervention effect using treatment and control groups that are as similar as possible in terms of observed characteristics. Three broad types of matching methods have been developed, referred to as exact, Mahalanobis and propensity score matching (Stuart, 2010). As Stuart (2010) details, exact matching is viewed as the ideal form in that the method involves one-to-one matching of individuals from both groups. Inevitably, requiring one-to-one matches means that very few individuals will ultimately be matched, leading to larger biases than if inexact matching were carried out. Coarsened exact matching (CEM) has been developed to overcome this limitation, which involves temporarily coarsening data through re-coding continuous variables as categorical variables and balancing the sample on that basis (Iacus et al., 2012). This method increases the number of matched individuals whilst overcoming recognised limitations to the Mahalanobis and propensity score methods, which cannot guarantee improvements in balance (Iacus et al., 2011). In the analysis, the CEM method is implemented using the ‘cem’ Stata command developed by Blackwell et al. (2009).

### 9.3.4 Statistical approach

As previously outlined, the DiD analysis carried out in this chapter investigates the intervention effect that is associated with the harsher sanctioning environment at the onset of the Coalition government in 2010. In particular, the analysis investigates whether or not the experience of claiming JSA in the harsher sanctioning period is associated with an increased prevalence of newly experiencing anxiety and/or depression. As described throughout this section, this involves comparing the change in the self-reported mental health of JSA claimants before and after important developments in sanctions policy, and comparing this difference to similar changes observed for a comparable control group. This intervention effect is shown in Equation 9.1:

$$\begin{aligned} \text{Intervention Effect} = & \\ & (\Delta \text{Anxiety}_{\text{Post}, \text{JSA}} - \Delta \text{Anxiety}_{\text{Pre}, \text{JSA}}) - \\ & (\Delta \text{Anxiety}_{\text{Post}, \text{Control}} - \Delta \text{Anxiety}_{\text{Pre}, \text{Control}}) \end{aligned} \quad (9.1)$$

In order to estimate the intervention effect specified in Equation 9.1, the analysis carries out a DiD regression model as shown in Equation 9.2:

$$\Delta Anxietiy_{i,t} = \beta_0 + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 (Treat_i * Post_t) + \beta' X_{i,t} + \varepsilon_{i,t} \quad (9.2)$$

In Equation 9.2,  $i$  denotes the individual and  $t$  denotes the quarters in the pre- and post-intervention time periods.  $\Delta Anxietiy$  is a measure of ‘worsening’ mental health that is constructed as detailed in Table 9.2.  $Treat$  is a dummy variable that is equal to ‘1’ for JSA claimants in Q1 in the pre- and post-intervention time periods and ‘0’ for the control group.  $Post$  is a dummy variable that is equal to ‘1’ if the individual is from the post-treatment period, and ‘0’ if they are from the pre-treatment period. By interacting  $Treat$  and  $Post$  it is possible to estimate  $\beta_3$ , which is the DiD estimate of the intervention effect described in Equation 9.1. This is the main coefficient of interest in the models that are estimated in this chapter.  $X$  represents a vector of additional explanatory variables. The analysis considers both a basic version of the DiD analysis without additional explanatory variables, as well as a regression model with additional explanatory variables included. These variables are similar to those used in Chapter 8, and include: gender; age; ethnicity; education; disability; marital status; dependent children; socio-economic status; and region of residence. In contrast to Chapter 8, marital status is used as opposed to relationship status in this chapter, which is due to variable availability in the two-quarter longitudinal LFS datasets.

Similar to the analysis in Chapter 8, furthermore, linear probability models are estimated despite the fact that the investigation is based on a binary measure of anxiety and/or depression. As previously explained, linear models are estimated in order to overcome the limitations of logistic regression models as identified by Mood (2010). Indeed, this response is carried out in other DiD analyses in the literature (see, for example, Reeves et al., 2016). In the current context, the use of linear models are not particularly affected by the risk of under- or over-prediction, given that the main coefficient of interest - the  $\beta_3$  intervention effect - is a dummy term that is bounded between zero and one. Logistic regression models, nevertheless, are estimated as part of sensitivity analyses.



## 9.4 Results

### 9.4.1 Summary statistics

Table 9.6 details the summary statistics for the measure of ‘worsening’ mental health that is used throughout the analysis. Table A9.1 in the appendix for Chapter 9 (Section A9.1) details the summary statistics for the additional explanatory variables included in the extended DiD models. As shown in Table 9.6, 13 of the 922 JSA claimants in the pre-intervention period experienced a worsening in their mental health, which is approximately 1% of the sample. In the post-intervention period, in contrast, approximately 3% of JSA claimants experienced a worsening in their mental health (25/811). This very basic ‘before’ and ‘after’ comparison, therefore, provides an initial indication that the harsher sanctioning environment might be associated with an increase in JSA claimants newly experiencing anxiety and/or depression, which will be further investigated in the DiD analysis. In terms of the various control groups that are considered, clearly the group of JSA claimants in Northern Ireland is limited by the small sample size that is available for the analysis, though the remaining control groups benefit from larger sample sizes. Table A9.1 in the appendix for Chapter 9, furthermore, indicates that the treatment and control groups are not identical in terms of the distribution of the characteristics captured by the explanatory variables, motivating the inclusion of the explanatory variables as well as the matching DiD analysis that will be carried out.

**Table 9.6:** summary of ‘worsening’ mental health for JSA treatment and comparison groups

	Pre-intervention (less strict sanctioning)		Post-intervention (harsher sanctioning)	
	Maintained	Worsened	Maintained	Worsened
<i>Treatment group:</i>				
JSA claimants	909	13	786	25
<i>Control groups:</i>				
JSA claimants (NI)	54	0	58	0
WTC claimants	4,229	43	3,421	23
Self-employed WTC claimants	528	5	450	2
Unemployed (non-JSA claimants)	1,063	27	904	6

Note: table reports unweighted sample Ns

### 9.4.2 Difference-in-differences regression models

Table 9.7 details the results of the initial DiD analysis, reporting estimates for the separate control groups in models with and without additional explanatory variables included. In particular, Table 9.7 reports the estimated intervention effect for each model, which is  $\beta_3$  in Equation 9.2. As previously highlighted, the intervention effect in this context is understood as the impact on the mental health of JSA claimants that is associated with claiming during the harsher sanctioning environment that followed the onset of the Coalition government. An estimated positive coefficient, for example, indicates that the harsher sanctioning environment is associated with an increase in the prevalence of JSA claimants newly experiencing anxiety and/or depression. Whilst Table 9.7 reports only the estimated intervention effect from the separate models, Table A9.2 in the appendix for Chapter 9 (Section A9.2) details the full regression results for each control group, based on the models that include additional explanatory variables (Models 9.2).

Importantly, across the separate models summarised in Table 9.7, the DiD analysis finds a positive and statistically significant intervention effect. First, in the basic models that do not include covariates (Models 9.1), the estimated intervention effect ranges from an increase of 1.4 to 3.3 percentage points, depending on the control group that is used. Using JSA claimants in Northern Ireland, for example, the estimated intervention effect is an increase of 1.4 percentage points. Although this effect is not significant at the 5% level, it is close to it ( $p = 0.067$ ). Indeed, with covariates included (Models 9.2), the intervention effect using this control group becomes significant at the 5% level. When comparing across Models 9.1 and Models 9.2, there is a high degree of consistency in the size of the estimated coefficients for each control group. The latter set of models estimate an intervention effect that ranges from an increase of 1.7 to 3.3 percentage points, all of which are significant at the 5% level. In Models 9.2, furthermore, the estimated coefficients are similar across the control groups of JSA claimants in Northern Ireland, WTC claimants and self-employed WTC claimants. For the control group of unemployed non-JSA claimants, however, the estimate is higher, at an increase of 3.3 percentage points ( $p < 0.001$ ). As indicated in Section 9.3, there are issues with this control

group in terms of differences in circumstances with the JSA treatment group, as well as very dissimilar pre-intervention trends in mental health. Such issues may be contributing to the larger estimated intervention effect, which puts the finding into question.

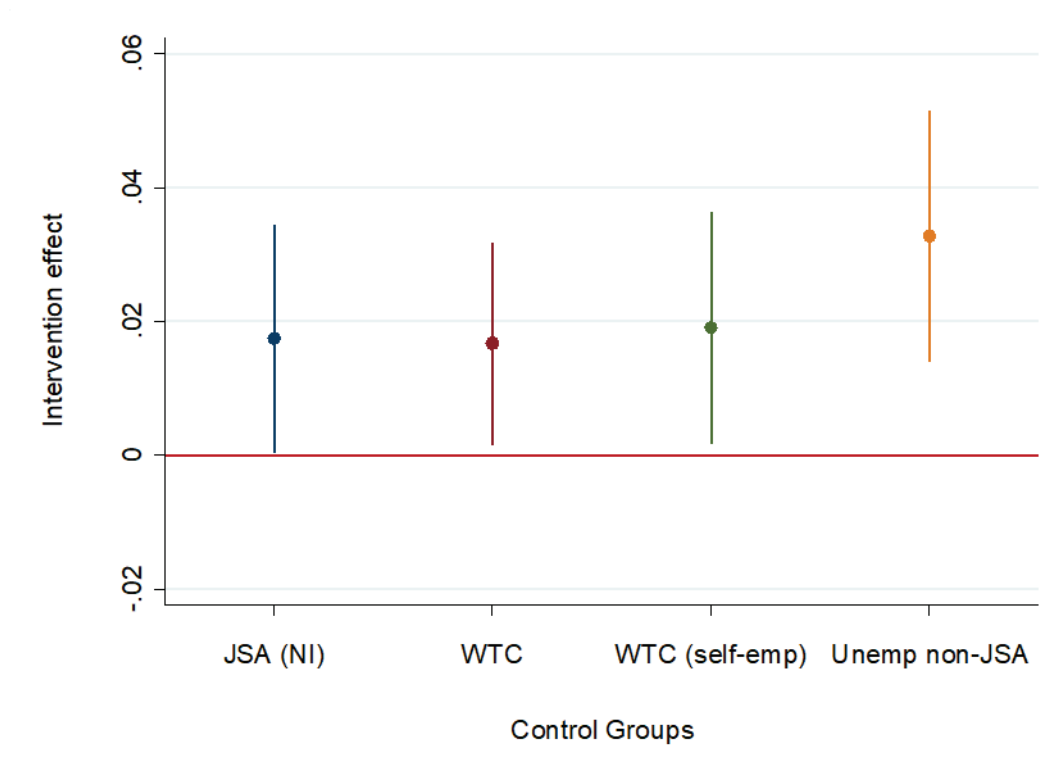
**Table 9.7:** estimated intervention effect on the self-reported mental health of JSA claimants

Control Group	Model(s) 9.1: No covariates	Model(s) 9.2: Covariates
JSA claimants (NI)	0.014 (0.007)	0.017* (0.009)
WTC claimants	0.017* (0.008)	0.017* (0.008)
Self-employed WTC claimants	0.018* (0.009)	0.019* (0.009)
Unemployed (non-JSA claimants)	0.033*** (0.010)	0.033*** (0.010)

*Note:* Robust standard errors in brackets. Covariates: gender; age; ethnicity; qualification level; disability; marital status; dependent children; socio-economic status and region of residence. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

The intervention effects that are estimated across the separate control groups considered in Models 9.2 are displayed in Figure 9.5.

**Figure 9.5:** estimated intervention effect on the self-reported mental health of JSA claimants



*Note:* Point estimates are derived from Models 9.2 in Table 9.7  
Vertical bars represent 95% confidence intervals

### 9.4.3 Robustness tests

As a robustness check on the results in Table 9.7, the models are re-estimated using the coarsened exact matching (CEM) method, which was previously described in Section 9.3. The treatment and control groups are matched using all the additional explanatory variables previously described. In the case of JSA claimants in Northern Ireland, however, region of residence is not used for obvious reasons. For the self-employed WTC claimant group, furthermore, socio-economic status is not used given that this group overwhelmingly responds according to their self-employed status ('Small employers and own account workers'), leading to very few matches when this variable is used. The results for the CEM differences-in-differences analysis are displayed in Table 9.8, which reports similar findings to those previously detailed in Table 9.7. Importantly, the coefficients are very similar in terms of both the direction of the intervention effect as well as the scale. For the control group of JSA claimants in Northern Ireland, however, the estimated intervention effect - an increase of 1.7 percentage points - is non-significant at the 5% level. Arguably, this finding is not surprising given the already low sample size for this control group in the absence of matching. The coefficient for the control group of self-employed WTC claimants, furthermore, is no longer statistically significant at the 5% level, though is statistically significant at the 10% level ( $p = 0.095$ ).

**Table 9.8:** estimated intervention effect on the self-reported mental health of JSA claimants

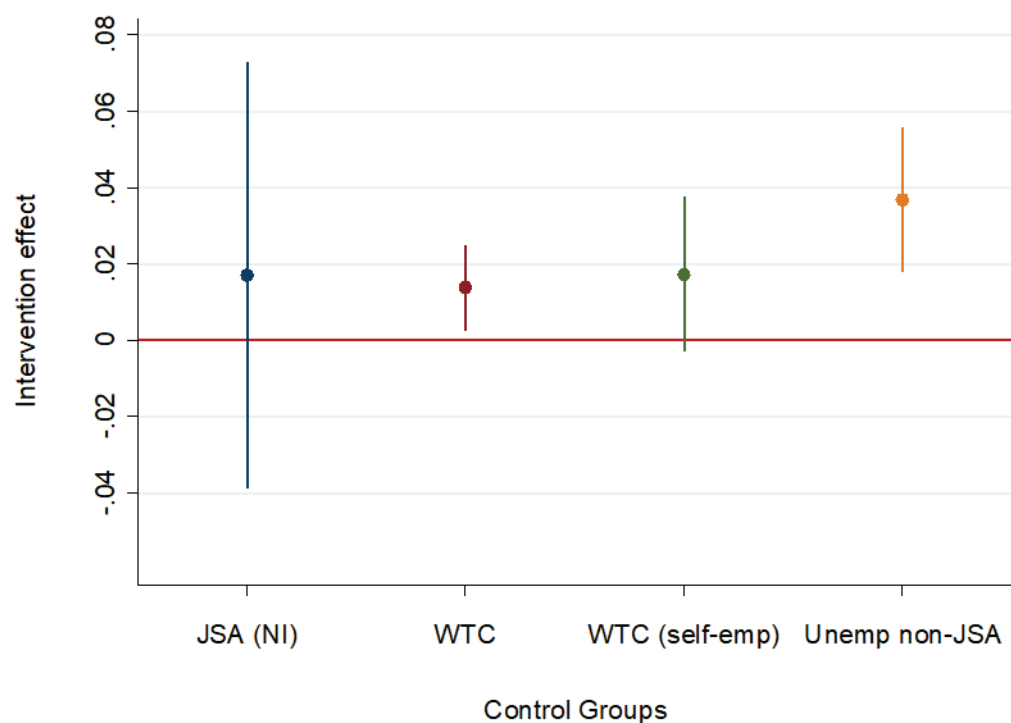
Comparison Group	Model(s) 9.3: Matching
JSA claimants (NI)	0.017 (0.028)
WTC claimants	0.014* (0.006)
Self-employed WTC claimants	0.017 (0.010)
Unemployed (non-JSA claimants)	0.037*** (0.010)

*Note:* Robust standard errors in brackets. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

The intervention effects that are estimated across the separate control groups in Models 9.3 are displayed in Figure 9.6. Arguably, the similarity of the results across Models 9.3 and Models 9.2 is unsurprising, given that the addition of

explanatory variables in Models 9.2 saw little change from the basic DiD analysis in Models 9.1. Indeed, as an additional sensitivity check, the results across these models are re-run using logistic regression. Consistency is again observed, though results for the self-employed WTC claimant group are non-significant at the 5% level (see Table A9.3 in the appendix for Chapter 9 (Section A9.3)). These similarities suggest that observed differences between the treatment and control groups are not driving the results obtained. Arguably, therefore, the intervention effects observed across the models discussed are caused by the harsher sanctioning environment itself, omitted variables bias, or some combination of the two. As previously indicated, the balance of factors will depend on the control group that is considered, given the concerns highlighted in relation to the unemployed non-JSA claimant group.

**Figure 9.6:** estimated intervention effect on the self-reported mental health of JSA claimants, matching models

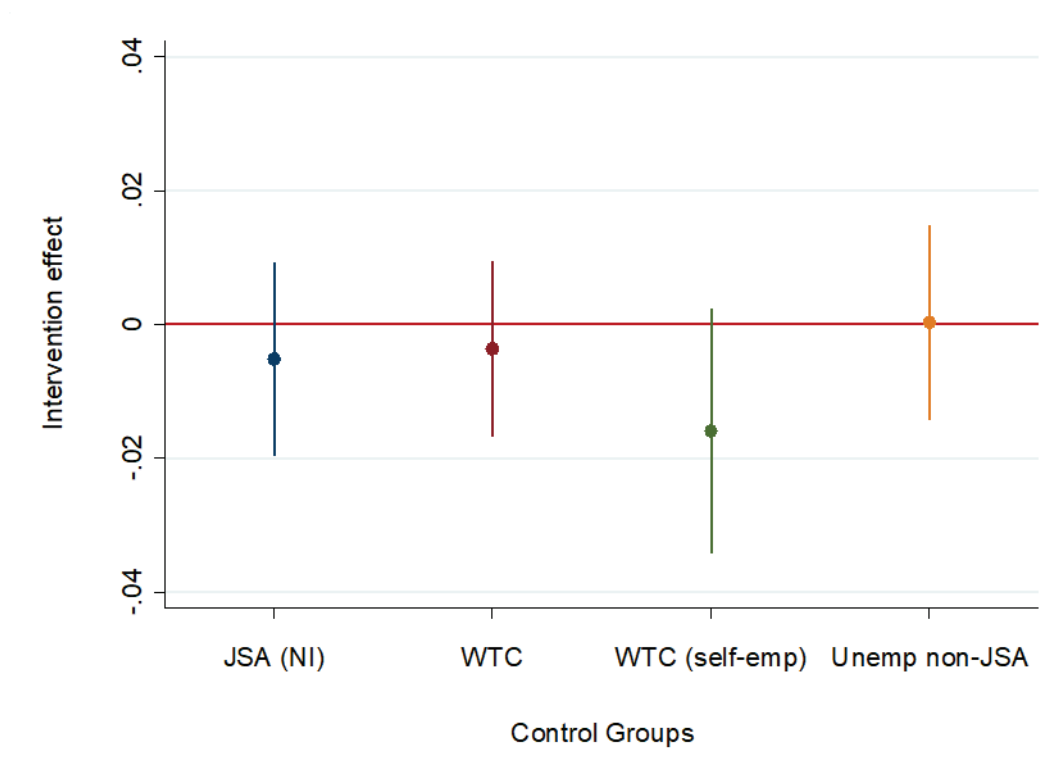


*Note:* Point estimates are derived from Models 9.3 in Table 9.8  
Vertical bars represent 95% confidence intervals

As an additional robustness check, a falsification test is carried out that uses cardiovascular problems as the non-equivalent dependent variable, as previously explained. The results of the falsification test are detailed in Table A9.5 in the appendix for Chapter 9 (Section A9.4). Across the various control groups and DiD models in Table A9.5, no statistically significant intervention effect is observed,

whilst the direction of the effects is negative and/or close to zero. These results strengthen the claim that the harsher sanctioning environment is a key factor in explaining the findings previously discussed, as opposed to some unaccounted for confounding factor. Figure 9.7 displays the results of the falsification test. It depicts the intervention effects that are estimated across the separate control groups, based on the DiD models that have covariates included.

**Figure 9.7:** estimated intervention effect on the self-reported cardiovascular health of JSA claimants

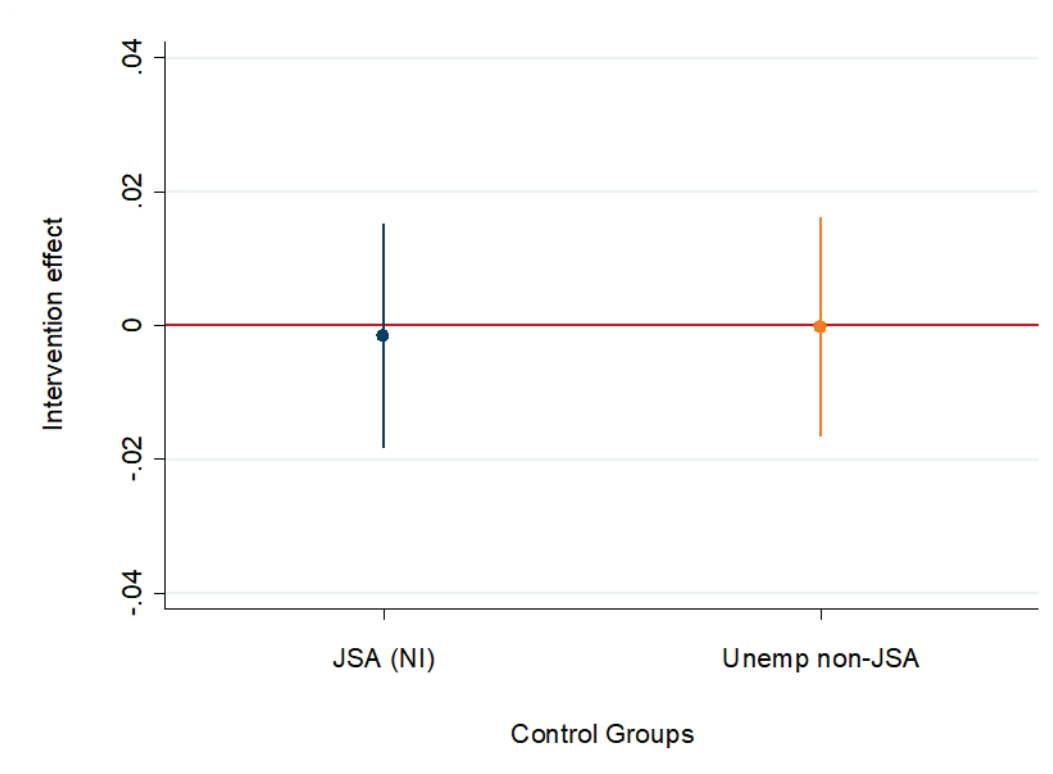


*Note:* Point estimates are derived from Models A9.9 in Table A9.5  
Vertical bars represent 95% confidence intervals

In a final robustness check, a placebo test is carried out, the precise details of which were previously explained in Section 9.3. Briefly, the placebo test re-runs the analysis in a period of relative stability in sanctions policy prior to 2007, at which point an intervention effect would not be expected to be observed. The results of the placebo test are detailed in Table A9.8 in the appendix for Chapter 9 (Section A9.5). Given variable availability within the QLFS, it is only possible to identify two control groups in this period: JSA claimants in Northern Ireland; and unemployed non-JSA claimants. For both control groups and across the various DiD models, no statistically significant intervention effect is observed. The direction and size of the effects is also negative and/or very close to zero. This is depicted in Figure 9.8, which displays the results of the DiD models that have

covariates included. The results strengthen the claim that the harsher sanctioning environment is a key factor in explaining the findings of the main analysis.

**Figure 9.8:** estimated intervention effect on the self-reported mental health of JSA claimants, placebo test



*Note:* Point estimates are derived from Models A9.12 in Table A9.8  
Vertical bars represent 95% confidence intervals

## 9.5 Conclusion

This chapter has carried out a difference-in-differences analysis, with the aim of considering the impact of the harsher sanctioning environment that was brought about at the onset of the Coalition government in 2010. Changes in sanctions policy at that time saw harsher penalties introduced for non-attendance at advisory interviews, as well as an increase in the rate of sanctions actually imposed. Importantly, the findings presented in this chapter suggest that the harsher sanctioning environment is associated with adverse impacts on the mental health of JSA claimants. DiD estimates using a variety of control groups estimate a small but positive and statistically significant intervention effect. Based on the most reliable control groups, the harsher sanctioning environment is associated with an increase of approximately 1 to 2 percentage points in the prevalence of JSA claimants newly experiencing anxiety and/or depression.

These results are relatively consistent across DiD models with and without additional explanatory variables included, as well as in matching models. Indeed, they are further supported by both a falsification test and a placebo test. As expected, an intervention effect is not observed when using a non-equivalent dependent variable and when repeating the analysis during a relatively stable period in sanctions policy. The results of the main DiD analysis are observed, furthermore, despite the reliance on a dependent variable that is limited in its ability to capture variations in mental health.

As highlighted throughout this chapter, the analysis is subject to certain limitations that imply that caution is necessary in terms of interpreting the estimated intervention effect solely in causal terms. A key issue relates to the availability of a suitable control group, which is central to the validity of any DiD analysis. Ultimately, four different control groups are used throughout the various models that are estimated. The suitability of each group was investigated to ensure that they were not affected by policy changes of their own between the pre- and post-intervention periods. In addition, a comparison was made of the trend in proportions reporting worsening mental health for each group in the pre-Coalition period. This comparison can provide some reassurance, albeit not definitive, regarding the plausibility of the common trends assumption. As highlighted throughout the analysis, there are issues with regard to the unemployed non-JSA claimants control group, both in terms of differences in circumstances with the treatment group and diverging pre-intervention trends in mental health. Indeed, when using this control group, the estimated intervention effect is consistently over double that compared with the other control groups. This implies that the results when using this group should be treated as an overestimate of the intervention effect, given the issues highlighted.

Three additional control groups are considered in the analysis: JSA claimants in Northern Ireland; WTC claimants; and self-employed WTC claimants. Unlike the group of unemployed non-JSA claimants, these control groups had similar pre-Coalition trends in mental health outcomes, which strengthens the case for their use in the DiD analysis. The intervention effect that is estimated when using these groups is similar across the various models, in terms of both the direction



and size of the effect observed. Arguably, the estimated increase of approximately 1 to 2 percentage points in the prevalence of JSA claimants newly experiencing anxiety and/or depression can be considered plausible, given the scale of the change in sanctions policy that is considered here. The concern remains, of course, that none of these groups provide ‘ideal’ control groups given that they may differ systematically from the JSA treatment group in important respects. The group of JSA claimants in Northern Ireland, for example, are separated in terms of both geography and policy context from the treatment group, and suffer from a limited sample size in the two-quarter LFS datasets. Whilst this limitation is recognised, it is important to reiterate that the main results are supported by several robustness tests. If the results are explained by the influence of unobserved confounders, therefore, it is not immediately clear what these factors could be.

Despite the acknowledged limitations, therefore, the DiD analysis carried out in this chapter provides a useful addition to the analyses carried out in Chapters 6 to 8 of this thesis. Indeed, in light of the data that is available for the separate empirical analyses, the estimation of an intervention effect in this chapter has an important role to play, primarily in terms of contributing to the consideration of causal inferences. Together, these analyses combine to help identify robust empirical regularities in the area of sanctions and mental health impacts. This will be further discussed in the next chapter, which provides the concluding chapter for this thesis.

## **Chapter 10. Conclusion**

### **10.1 Introduction**

This chapter is the final and concluding chapter of this thesis. It discusses the findings of the empirical analyses that have been carried out in Chapters 6 to 9 together, and reflects on their overall contribution to knowledge in the area of sanctions and their associated impacts. This task is carried out with reference to the research objectives outlined in the introductory chapter, as well as the overarching research question established in Chapter 4. First, this chapter begins with a summary of the results discussed in Chapters 6 to 9, highlighting the strengths and limitations of the analyses that have been carried out. In particular, the first section considers what the results imply regarding the impact of sanctions on mental health, which leads to a discussion of the important areas of future research that follow from the findings in this thesis. Last, this chapter ends with a discussion of the policy implications of the research, followed by a reflection on what the findings imply for sanctions policy more broadly, when considered in conjunction with the existing literature on the labour market and wider impacts of benefit sanctions.

### **10.2 Thesis contribution**

#### **10.2.1 Research objectives and overarching research question**

As identified in Chapter 1, this thesis has had two central research objectives:

- i. To contribute to the existing literature on the wider impacts of benefit sanctions, by using quantitative methods, longitudinal data and causal modelling frameworks to establish robust claims about the causal impacts of sanctions on mental health outcomes.
- ii. To provide evidence that can be used to inform sanctions policy in the UK, specifically in relation to the reforms introduced by the Welfare Reform Act 2012.

In the course of the research, Chapter 4 carried out a review of the existing empirical literature on the labour market and wider impacts of benefit sanctions, which identified the need for more systematic evidence regarding the relationship between sanctions and impacts on mental health. The central aim of the research carried out throughout the investigation, therefore, has been to address this particular gap in the literature. In light of this, the overarching research question that has motivated the separate empirical analyses carried out in the thesis is as follows:

[RQ]: Are benefit sanctions associated with adverse mental health impacts?

Through answering this overarching research question, the thesis has sought to address the two objectives outlined above. Prior to the empirical chapters themselves, Chapter 5 highlighted the fact that individual-level data on sanctions are not currently available for use within empirical research in the UK. In response to this central limitation, four separate quantitative analyses have been carried out in Chapters 6 to 9, each of which have addressed the overarching research question to the extent possible with the available data. The contribution of the empirical chapters will be highlighted in the discussion that follows, which focuses on how they - both individually and in combination - address the overarching research question and the objectives of the thesis.

### **10.2.2 Empirical contribution**

Importantly, the separate analyses carried out in Chapters 6 to 9 have found consistent and mutually reinforcing evidence that links JSA sanctions with adverse mental health impacts, measured in terms of antidepressant prescribing and anxiety and/or depression. The main findings can be summarised as follows:

- i. *Chapter 6 (local authority-level analysis)*: Following the implementation of the Welfare Reform Act 2012, every 10 additional sanctions applied per 100,000 population per quarter are associated with approximately 4.57 additional SSRI prescribing items. This represents approximately one additional person receiving treatment.

- ii. *Chapter 7 (local authority-level analysis)*: Following the implementation of the Welfare Reform Act 2012, every 10 additional sanctions applied per 100,000 working age population per quarter are associated with approximately 8.09 additional people reporting that they suffer from anxiety and/or depression.
- iii. *Chapter 8 (multi-level analysis)*: Following the implementation of the Welfare Reform Act 2012, a one percentage point increase in the local authority-level sanctions rate is associated with a 2-3 percentage point increase in the likelihood that JSA claimants report suffering from anxiety and/or depression.
- iv. *Chapter 9 (difference-in-differences analysis)*: The harsher sanctioning environment that was brought about at the onset of the Coalition government in 2010 is associated with an increase of approximately 1-2 percentage points in the prevalence of JSA claimants newly experiencing anxiety and/or depression.

These findings make an important contribution to the literature on the wider impacts of benefit sanctions. As discussed in detail in Chapter 4, existing empirical research has focused its attention on the labour market impacts of sanctions, though there is a limited and growing research agenda that identifies a range of wider impacts. This wider impacts literature is not extensive overall, be it from evidence relating to the UK or international social security systems. Indeed, mental health impacts specifically have not been investigated as systematically as other areas such as food bank usage, which have been the focus of several qualitative and quantitative analyses (Lambie-Mumford, 2014; Loopstra et al., 2015a; Garratt et al., 2016; Loopstra et al., 2018).

Previous research in the area of sanctions and mental health exists, though is limited in scope. Existing qualitative research, for example, highlights the mental distress that claimants experience in relation to the threat and actual imposition of a sanction, though such sanction-related impacts generally do not represent the primary focus of the available studies (Wright and Stewart, 2016; Dwyer, 2018; Dwyer et al., 2018; Johnsen and Blenkinsopp, 2018; Johnsen et al.,

2018; Stewart and Wright, 2018; Dwyer et al., 2020; Redman, 2020). Existing quantitative research, furthermore, has focused on the broader issue of work-related behavioural conditionality for a variety of claimant groups, as opposed to considering sanction impacts specifically (Barr et al., 2016; Katikireddi et al., 2018; Davis, 2019).

Importantly, the research carried out in this thesis makes a clear contribution to existing knowledge in this area, through conducting a large-scale and longitudinal quantitative investigation into JSA benefit sanctions and their impacts on mental health, with a specific focus on antidepressant prescribing, anxiety and depression. Despite various data limitations, a strength of the thesis is that it has been able to triangulate different data sources and research designs in separate analyses, which together provide strong evidence that JSA sanctions lead to adverse mental health impacts. In so doing, these findings contribute not only to the literature on the impacts of benefit sanctions, but also to wider debates relating to mental health, unemployment and the mediating role of social security systems. As discussed in Chapter 4, unemployment is widely recognised to have detrimental mental health impacts (Paul and Moser, 2009). The research carried out in this thesis supports the view that the social security system has an important role to play in this area, given that it can either serve to compound or alleviate these adverse mental health impacts (Coutts et al., 2014; O'Campo et al., 2015; Sage, 2015a; 2015b; Carter and Whitworth, 2017; Renahy et al., 2018).

As highlighted in Chapter 5, a key concern in this thesis has been to establish robust empirical regularities with regard to sanctions and mental health impacts, with particular consideration given to the scope for making causal inferences. This is important, as evidence of this form, which attempts to move beyond merely measuring associations, is more useful in terms of informing the development of sanctions policy in the UK. Several methods have been pursued in order to contribute to this endeavour, which will be referred to below. These respond to the various issues that affect the estimation of the relationship between sanctions and mental health outcomes given the data analysed, such as ecological bias, omitted variable bias, reverse causality and compositional bias.

A central element in the investigation's response to these issues begins with the time period chosen, which focuses specifically on the period of Coalition government (2010-15). As detailed in Chapter 3 and Chapter 5, this period is marked by significant developments in sanctions policy in terms of both the frequency and the severity with which sanctions were applied. These developments provide the exogenous variation through which it has been possible to better examine the mental health impacts of sanctions in the empirical chapters. In addition to exploiting this policy context, longitudinal data has been analysed throughout Chapters 6 to 9, which have investigated the topic using different levels of data and several quantitative methods such as fixed effects, multi-level and difference-in-differences analysis. The main results of these analyses, furthermore, have been supported through additional robustness checks in the form of falsification tests, placebo tests and Granger tests for reverse causality.

It is important to reiterate, however, that the reliance on local authority-level sanctions data in this thesis is accompanied by additional issues relating to data availability and quality. As detailed in Chapter 5, for example, the publicly available local authority-level sanctions data published by the DWP are limited by the manner in which they are recorded. A central issue is that the Stat-Xplore database records only the latest decision for each sanction case, leading to the use of a JSA sanctions indicator that underestimates the true quarterly figure. As previously highlighted, furthermore, key data gaps at the local authority-level remain regarding statistics that distinguish between disentanglements and sanctions, the different length of sanctions applied as well as the number of hardship payments granted.

Despite the limitations highlighted, the findings in this thesis combine to provide a robust indication that JSA sanctions are associated with adverse mental health impacts. The findings in Chapters 6 to 8, for example, indicate that evidence regarding the relationship between sanctions and adverse mental health impacts is clearest following the implementation of the Welfare Reform Act 2012, and the subsequent increase in the minimum and maximum length of the financial penalties imposed. It is an important finding that the frequency and increased severity of sanctions in the post-reform period are such that associations with

mental health outcomes are observed even when relying on local authority-level data. Whilst the analysis is not able to include hardship payment rates, furthermore, the findings in these chapters are observed despite the fact that the rate of hardship payments increased in the post-reform period. They are also supported by findings from the difference-in-differences analysis in Chapter 9, which considered the impact of an increase in the frequency and severity of sanctions that occurred at the beginning of the Coalition period.

These empirical findings are also supported by the analysis in Chapter 4, which developed an account of the causal mechanisms linking benefit sanctions and adverse mental health impacts. Drawing on the work of Sage (2018), the causal pathways identified are based on a distinction between material and psychosocial mechanisms. Material mechanisms can be expected to operate in response to the imposition effects of sanctions solely, whereas psychosocial mechanisms such as stress, loss of agency and loss of social status can be expected to operate in response to both the threat and imposition effects of sanctions. As this implies, an important distinction within this account relates to the threat and imposition effects of sanctions. As detailed in Chapter 4, the literature on the impacts of sanctions is most developed in the area of imposition effects, which are relatively more straightforward to investigate within empirical research, given that they reflect the impact of sanctions that have actually been applied. Threat effects, in contrast, can be understood in two different ways, either in terms of the general threat of sanctions or a formal warning that a sanction will be applied. As explained in Chapter 5, however, the reliance on local authority-level sanctions data in this thesis means that it is not able to distinguish between threat and imposition effects in its findings, capturing instead an overall combined effect.

The findings of the investigation carried out in this thesis motivate additional individual-level research to better consider the causal relationships involved. The next section in this chapter will go on to detail recommendations for any such future individual-level analysis (Section 10.3). It is important to emphasise here, nevertheless, that - despite the data limitations recognised - the empirical investigation in this thesis, together with its consideration of causal inferences, mean that it is able to make an important contribution to ongoing

policy debates in this area. As detailed in Chapter 3, sanctions policy during the Coalition government has prompted a number of official investigations, inquiries and reviews. These have repeatedly called on the DWP to evaluate the impact of the reforms implemented in 2012, given the lack of evidence that informed the changes at the time and which - unfortunately - currently persists. The research carried out in this thesis contributes to filling this gap in policy knowledge, and challenges the prevailing policymaking assumption that sanctions will be associated with positive health outcomes. After the next section, Section 10.4 will go on to detail recommendations for sanctions policy that follow from the empirical findings discussed here.

### **10.3 Recommendations for future research**

As highlighted in the previous section, the findings of this thesis indicate that there is a clear need for additional individual-level research to investigate the relationship between sanctions and mental health outcomes. In an important recent statement, the DWP has responded to sustained criticism by committing to share administrative data on sanctions with researchers for the purposes of investigation into health impacts (HoC WPC, 2019). This indicates that individual-level research in this area could soon become possible in the UK, which is an extremely promising development. The recommendation for additional individual-level research can also be extended beyond the UK, where there is a need for greater international research into the mental health impacts of sanctions. In the review of the sanctions literature in Chapter 4, it was noted that existing research in terms of the wider impacts of benefit sanctions is largely based on evidence from the US and the UK. This contrasts with the literature on the labour market impacts of sanctions, which benefits from a wider range of studies across separate social security systems, notably across mainland Europe. In light of the UK-specific findings in this thesis, there is a therefore a need for additional research into mental health impacts in a wider range of contexts, where the frequency and severity of sanctions differ.

The investigation carried out in this thesis highlights a number of important areas that it would be useful for any future UK and international research to take into account. One central issue relates to the identification of causal



impacts, since it has been highlighted that whilst sanctions themselves will have an independent impact on mental health, it is important to distinguish this effect from the influence of prior mental health status on the likelihood of claimants being sanctioned. Whilst the existence of each effect has negative implications in terms of the functioning of sanctions policy, it is useful to distinguish between the two in the process of informing effective policy responses. Informed by the methodological discussion carried out in Chapter 5, it is likely that such individual-level research will need to exploit a source of random allocation and/or identify an instrumental variable in order to better identify causal impacts.

An important additional task for future UK and international research in this area relates to the need to test the hypothesised causal mechanisms that are thought to link sanctions and mental health impacts. Depending on data availability, the task of distinguishing between material and psychosocial factors may be a difficult one for quantitative research to achieve. A qualitative research design, in contrast, could be better placed to investigate claimant experiences regarding the separate influence such factors on their mental health, in relation to both the threat and imposition effects of sanctions. This highlights the potential of mixed methods research to make additional contributions in this area. On the topic of mental health itself, furthermore, there is a need to consider additional measures of mental and emotional distress to the ones investigated in this thesis. The main limitation of the self-reported measure of anxiety and/or depression used in Chapters 7 to 9, for example, is that it provides only a binary indication of an individual's mental health status, and therefore overlooks the fact that such mental distress can be experienced along a continuum. As previously highlighted, the GHQ-12 provides a more continuous quantitative measure, though both quantitative and qualitative approaches could usefully consider additional and related issues such as stress and claimant well-being.

In this thesis, a distinction has been maintained between the labour market and wider impacts of benefit sanctions, which reflects the respective focus of two broadly separate areas of existing empirical research. As highlighted in Chapter 4, however, labour market and wider impacts are not necessarily independent of one another, which raises the potential that quite complex interactions exist

between the two. This is particularly true with regard to mental health and the labour market, though due to data availability the research carried out here has had to focus on mental health impacts solely. The mental health impacts of sanctions, for example, are likely to differ according to the separate labour market outcomes for claimants following a sanction, such as in terms of the quality of employment that individuals are able to find (Kim and von dem Knesebeck, 2015; Kim and von dem Knesebeck, 2016; Chandola and Zhang, 2018). Equally, the immediate mental health impacts of sanctions may themselves affect people's ability to search for and consequently secure paid work (Dwyer, 2018; Dwyer et al., 2020).

The data requirements that are necessary to investigate such complex interactions are clearly quite demanding, and imply the need for longitudinal data that is able to measure labour market and mental health outcomes in both the short- and long-term. The impacts considered in this thesis have been fairly short-term in nature, and so future research could usefully consider whether they persist in the long-term. Labour market outcomes will again be relevant here, as the speed with which people return to work and the nature of the employment that they secure will influence whether mental health impacts improve or worsen. Arguably, research of this nature is possible in the UK given the administrative data at the DWP's disposal and the advances that can be achieved by linking existing datasets (Katikireddi and Leyland, 2017; Pattaro et al., 2020). Indeed, data that are available through the Work and Pensions Longitudinal Study (WPLS), when linked to NHS administrative data, would make it possible to investigate sanction impacts for different claimant groups and potentially even to consider third-party impacts on the children of claimants.

Future research into sanctions in the UK also needs to respond to the evolving policy context. The empirical investigation in this thesis has been limited to considering impacts relating to JSA sanctions, capturing impacts for JSA claimants themselves but also - in Chapter 6 and Chapter 7 - potential wider knock-on effects for family and friends. As highlighted throughout this thesis, the rollout of Universal Credit (UC) replaces pre-existing means-tested benefits, such as JSA, ESA, IS and WTC, meaning that conditionality and sanctions now apply within a single benefit to groups such as the unemployed, lone parents,

disabled people and those in-work. Clearly, there is a need to evaluate the impact of sanctions within this new system, in which sanctions have become consecutive as opposed to concurrent, and in which hardship payments have become repayable. Indeed, the need for an evaluation of sanctions in the current policy landscape is a key recommendation of the most recent Work and Pensions Committee inquiries into benefit sanctions, which the DWP itself has now finally accepted (HoC WPC, 2018; 2019). An important area for consideration in this regard relates to how the impacts of sanctions differ for claimants in different circumstances. Existing evidence discussed in Chapter 4, for example, indicates that sanctions are more unambiguously counterproductive in terms of labour market outcomes for ESA WRAG claimants than for JSA claimants (NAO, 2016b). It is likely that diverse responses to sanctions will continue within UC, though this needs to be investigated in the new policy context so that appropriate responses can be developed.

## **10.4 Implications for sanctions policy**

The findings of this thesis hold several implications for contemporary sanctions policy in the UK. Importantly, the empirical investigation provides consistent evidence that sanctions are associated with adverse mental health impacts, particularly following the implementation of the Welfare Reform Act 2012, which increased the minimum length of a sanction from 1 to 4 weeks and the maximum length from 26 to 156 weeks. Although these findings are based on an analysis of data relating to JSA sanctions up until 2015, they nevertheless have relevance with regard to the ongoing rollout of UC, which is itself underpinned by the sanctions regime introduced in 2012. The findings themselves are driven by both the severity and scale of sanctions in the post-reform period. Given that the DWP purports to be concerned about the mental health of claimants, the findings imply that action needs to be taken in both of these areas.

First, there is a clear need to reduce the severity of the sanctions that can be applied, in terms of both the length of sanctions as well as the proportion of a benefit that is removed. This recommendation is made all the more evident by the fact that the international evidence, discussed in Chapter 4 in relation to social security systems with less severe sanctions than the UK, suggests that

harsher sanctions do not lead to greater employment effects. Thus, this suggested policy response, which is based on the findings of this thesis in relation to mental health, is not expected to conflict with the DWP's additional policy concern with labour market outcomes. In a recent important and positive development in this regard, the DWP has ended the use of sanctions that last for 156 weeks, meaning that the harshest sanction has returned to the pre-reform maximum of 26 weeks (HM Government, 2019). As previously highlighted, however, sanctions are consecutive under UC, which implies that some claimants will ultimately have their benefit income removed for longer than this - already quite lengthy - 26 week period.

A key issue that relates to sanction severity, furthermore, is the functioning of the hardship payments system. As highlighted in Chapter 4, the official DWP Decision Makers' Guide itself recognises that the two-week wait for receipt of a hardship payment is likely to result in adverse health impacts for claimants. Indeed, the empirical findings in this thesis are observed despite the fact that the proportion of sanctioned claimants receiving hardship payments increased in the post-reform period. Importantly, however, since October 2017 JSA claimants who are already suffering from mental health problems have been deemed to represent a 'vulnerable' group, and are therefore now eligible for an immediate hardship payment (HM Government, 2017). Whilst this change is welcome, it begs the question of why claimants already suffering from mental health problems are subject to sanctions at all, whilst the findings in this thesis suggest that greater consideration needs to be given to the mental health of all claimants subjected to sanctions. Indeed, under UC, hardship payments are awarded for a restricted set of reasons, must be renewed every month and are repayable, which clearly increases the overall material impact of a sanction relative to the previous system. As Webster (2019) argues, these requirements are likely to explain the fact that less than 20% of sanctioned claimants receive hardship payments within UC, which is under half the proportion that had been receiving them within JSA.

Separate to the issues of sanction severity and associated hardship payments, is the issue of the overall frequency with which sanctions are applied. As argued in Chapter 3, the large variations in the rate of JSA sanctions between 2010 and

2015 cannot be plausibly explained by claimant behaviour alone. An influential role can be attributed to more structural determinants, such as decisions made by policymakers themselves, managerial focus on referral rates and the operation of the - now concluded - Work Programme. The monthly rate of sanctions within UC has also experienced a large degree of variation, falling from a high of approximately 9% in 2015 to 2% in 2019 (Webster, 2019). Using the latest data, Webster (2019) estimates that the monthly sanctions rate for unemployed claimants within UC stands at approximately 2.4%, which is similar to the JSA sanctions rate that generally pertained prior to 2010. The findings from this research motivate the recommendation that the DWP should do everything in its power to - at the very least - maintain this low rate of sanctions, which is clearly an achievable aim. Adler (2018) highlights, furthermore, that there are several straightforward means through which it would be possible to lower the sanctions rate even further. This could be achieved by limiting the number of reasons for which sanctions apply as well as establishing a more lenient conception of 'good reason' for which benefit rules might be contravened.

An additional option that would help achieve this end relates to the introduction of a sanctions warning system. In this, work coaches would be able to issue claimants with a warning - as opposed to a sanction - the first time that they do not meet a particular requirement. In this regard, the DWP has recently agreed to run what it describes as a "Proof of Concept (PoC) of a warning system" (HoC WPC, 2019: 17), which will apply solely in instances where claimants do not attend an appointment. Though limited in scope, this statement of intent appears to represent a positive development from the DWP's previous early warning trial, in which claimants were sent a warning letter requesting that they contact the DWP to provide evidence of good reason against a scheduled sanction and an additional 14 days in which to do this (DWP, 2018b). By providing claimants with a warning that they are at risk of being sanctioned, this opens up the possibility that individuals can take the necessary steps to avoid having a sanction actually imposed. In terms of mental health impacts, the analysis in this thesis indicates that this could have positive effects by helping claimants have a greater sense of control over the process and ultimately help them avoid experiencing the financial consequences of sanctions.

Despite the positive developments identified, the overall response by the DWP to the most recent Work and Pensions Committee inquiry indicates that it is fundamentally committed to a punitive sanctions system based upon escalating financial penalties (HoC WPC, 2019). The empirical findings in this thesis highlight that an important area of concern that will persist within such a system relates to impacts on claimants' mental health, though a further implication of the analysis is that sanctions are associated with increased rates of antidepressant prescribing, which represents additional public expenditure through demand on the NHS. In their review of the UK sanctions regime, the National Audit Office (NAO, 2016a) concludes that the total public expenditure implications that result from the system of sanctioning are unknown. Savings to the government in terms of benefits not paid due to sanctions, for example, must be weighed against administrative costs and unknown levels of additional support for those affected. Though this thesis has not attempted to specify a precise monetary figure associated with increased antidepressant prescribing, the research nevertheless indicates that such additional costs represent an important issue for further consideration. Indeed, this represents an overlooked area within the business case for UC, which assumes that the additional conditionality within UC will result in "gains to the NHS" (DWP, 2018d: 19), based on the claim that UC will lead to increased employment for claimants, which itself is assumed to result in improved health outcomes.

## **10.5 Final reflections**

Chapter 2 of this thesis provided an overview of the ongoing and highly contested debate regarding the ethical legitimacy of behavioural conditionality, and highlighted the importance of empirical evidence within this discussion. Whilst the differences between the various and competing normative claims on this issue are not reducible to empirical concerns alone, evidence regarding the impacts of conditionality are relevant for all the perspectives involved when attempting to provide an overall assessment of this policy agenda. This thesis has focused on the impacts of benefit sanctions specifically. In this regard, Chapter 4 detailed the findings from existing empirical research, in both the UK and internationally, on the labour market and wider impacts of benefit sanctions. As argued in Chapter 4, the UK and international evidence in relation

to labour market impacts is mixed, where sanctions are associated with short-term increases into employment as well as increases out of the labour force itself. In the longer-term, furthermore, sanctions are associated with decreases in wages, job stability and working hours. UK evidence in relation to wider impacts, furthermore, is consistently negative, given that sanctions have been associated with impacts such as financial hardship, food bank usage and survival crime. Building on existing qualitative evidence relating to mental health impacts, furthermore, the quantitative analysis in this thesis indicates that sanctions are associated with increases in both antidepressant prescribing and rates of anxiety and depression. When research in terms of both labour market and wider impacts is considered together, therefore, it would arguably be difficult for any normative perspective to defend the use of sanctions as they currently apply in the UK.

Indeed, whilst this concluding chapter has highlighted the need for more individual-level research in the area of sanctions and mental health, the existing weight of evidence arguably provides clear support for policy action on the grounds of the precautionary principle (Martuzzi and Tickner, 2004). Though the exact nature of cause and effect regarding the impacts of benefit sanctions merits further investigation, the precautionary principle implies that it is not necessary to wait for this debate to be resolved before action is taken, given that the burden of proof rests with those seeking to maintain a sanctions policy that holds clearly articulated risks for claimants. Indeed, Saunders et al. (2017: 29) identify the social security system as a key part of the policy response in terms of the social determinants of health, arguing that it is important to ensure that “conditionality measures and financial sanctions do not harm beneficiaries”. The findings presented in this thesis, when considered in conjunction with the existing empirical research discussed, indicate that this basic requirement is unfortunately not being met. In light of this, lack of meaningful action in this area is inexcusable. With the appropriate will, the aim of minimising the negative impacts of sanctions policy could be achieved within a short period of time, and this conclusion has highlighted several straightforward means through which this might be carried out.

Promisingly, the discussion has indicated some areas where progress has been made with respect to sanctions. The DWP, furthermore, purports to respond to empirical evidence, arguing recently that sanctions policy “should be evidence-based” (HoC WPC, 2019: 10). Whilst this commitment is a positive one in the present context, a gap arguably persists regarding how the DWP responds to the available evidence. Partly, this relates to a misunderstanding of the evidence base itself. In its response to the most recent Work and Pensions Committee inquiry into sanctions, for example, the DWP cites four studies to support its claim that international studies make it “clear” that conditionality and sanctions are associated with positive labour market impacts (HoC WPC, 2019: 3). All four of these studies were discussed in Chapter 4, which provided a more nuanced assessment. Lalive et al. (2005) and Svarer (2011), for example, are not able to distinguish between movements into work or out of the labour force, whilst Arni et al. (2013) and van den Berg and Vikström (2014) indicate that sanctions are also associated with negative longer-term impacts. Based on evidence from workshops with DWP policymakers and analysts involved in developing UC, furthermore, Monaghan and Ingold (2019: 364) argue that perceived constraints in terms of political feasibility continue to drive evidence use, leading to the “danger that vital, emerging evidence will continue to be obscured or omitted” in the process of policy development. Importantly, the authors identify the “austerity paradigm” as a key constitutive element in this process of filtering evidence. It can only be hoped that, as the UK apparently emerges into a post-austerity era, opportunities for a reassessment of the evidence on sanctions will be forthcoming, and that appropriate action will be taken.



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## Chapter 6 appendix

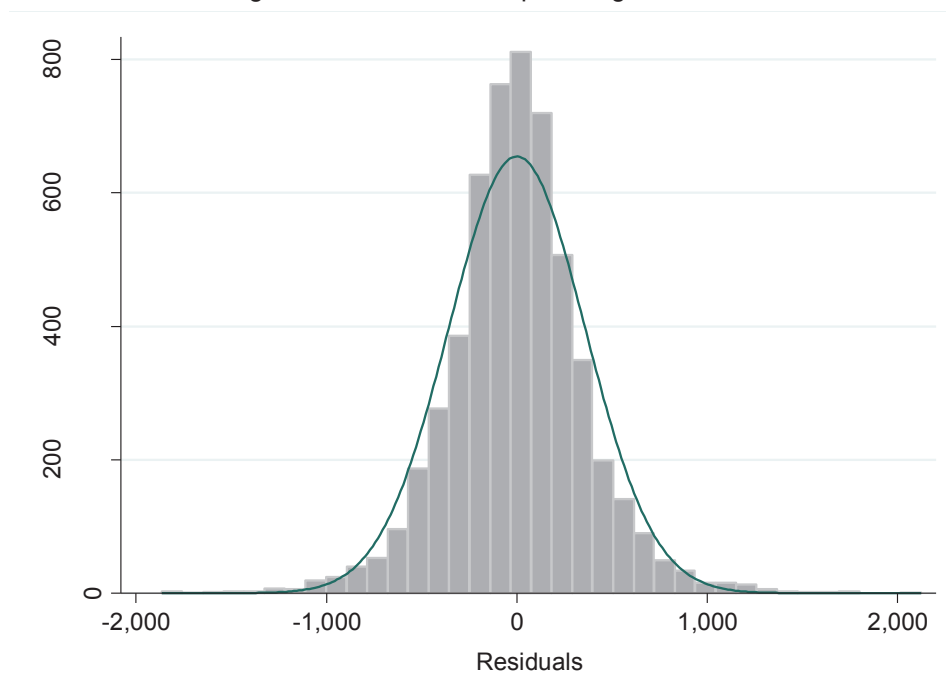
### A6.1 Diagnostic tests

Various diagnostic checks are carried out to test that the fixed effects model assumptions are satisfied, as detailed in Greene (2008). The checks presented here are for regression Model 6.2 in Table 6.3, as discussed in Section 6.4.

#### A6.1.1 Normality of the residuals

Figure A6.1 depicts a histogram of the regression residuals to check for serious deviations from the assumption of normality. Clearly, the residuals do not deviate sufficiently from the ideal of normality to be of concern to the results of the analysis. Three formal tests of normality, a Skewness/Kurtosis test ( $p < 0.001$ ), a Shapiro-Wilk test ( $p < 0.001$ ) and a Shapiro-Francia test ( $p < 0.001$ ) reject the null of normality. However, as Ghasemi and Zahedias (2012) outline, such tests are sensitive to even very small deviations from normality at large sample sizes. The rejection of normality by such tests is therefore not of concern to the analysis, given the distribution that is actually observed.

**Figure A6.1:** distribution of regression residuals compared against normal distribution curve



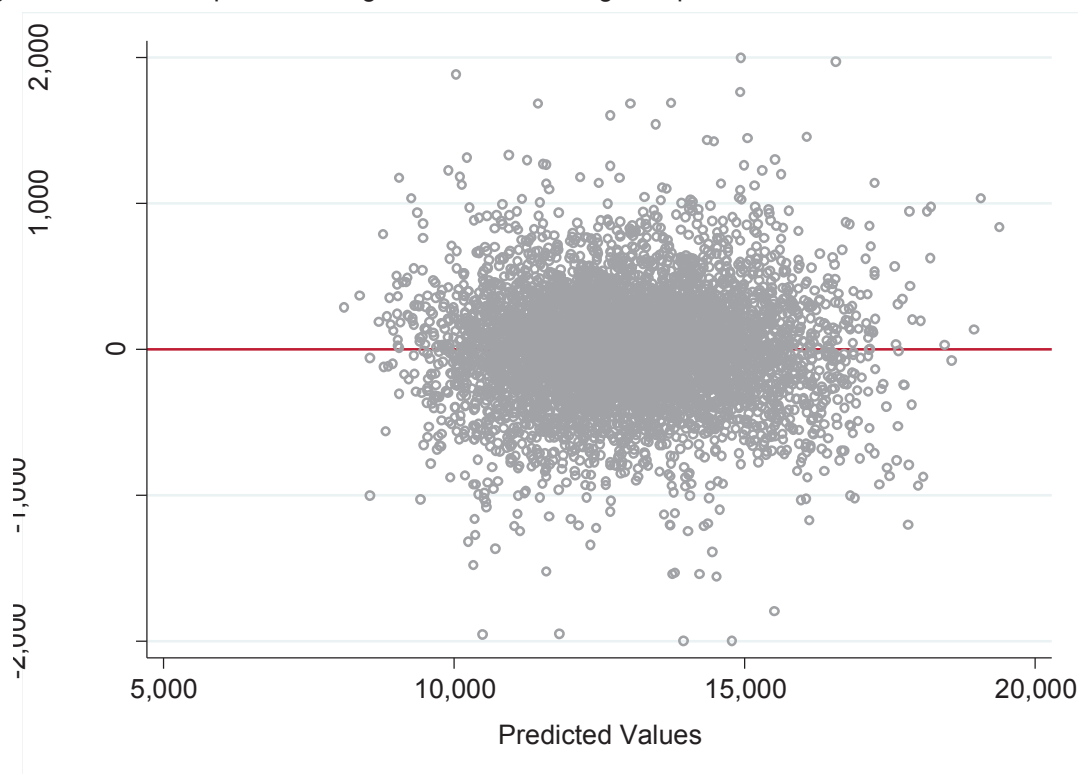


### **A6.1.2 Cross-sectional independence, homoscedasticity, no serial correlation and stationarity**

The tests carried out in this sub-section indicate that the fixed effects models suffer from cross-sectional dependence, heteroscedasticity and autocorrelation, though there are important caveats on the tests themselves that will be explained in more detail in the following discussion. Consequently, the fixed effects regression models estimated throughout Chapter 6 use Driscoll-Kraay standard errors (Driscoll and Kraay, 1998), which are robust to cross-sectional dependence, heteroscedasticity and correlation through time within local authorities. These are implemented using the Stata command 'xtscc', developed by Hoechle (2007).

First, a check for cross-sectional dependence is carried out. The standard test of this issue is the Breusch-Pagan Lagrange multiplier (LM) test, as developed by Breusch and Pagan (1980). This test isn't valid in panels with a large number of observations ( $N$ ) but a small number of observations per cross-sectional unit ( $T$ ), which is the case here ( $N = 324$ ,  $T = 18$ ). Instead, Pesaran's (2004) cross-sectional dependence (CD) test is carried out, using the 'xtcsd' Stata command developed by de Hoyos and Sarafidis (2006), which is compatible with unbalanced datasets. The Pesaran (2004) CD test rejects the null hypothesis of no cross-sectional dependence ( $p < 0.05$ ).

Next, in order to check for heteroscedasticity, a modified Wald test (Greene, 2008) is carried out that tests for group-wise heteroscedasticity in the residuals of fixed effect regression models, using the Stata command 'xttest3' developed by Baum (2001). The modified Wald test rejects the null of homoscedasticity ( $p < 0.001$ ), which indicates that the residuals display heteroscedasticity. This test, however, has a very low power in the context of fixed effects with "large  $N$ , small  $T$ " (Baum, 2001: 102) panels, as is the case here. The result of the modified Wald test should, therefore, be treated with caution. Indeed, a scatter plot of the regression residuals against predicted values, furthermore, suggests that the error term has an approximately constant variance, since there is no sign of a fanning out effect over different predicted values. This is depicted in Figure A6.2.

**Figure A6.2:** scatter plot of the regression residuals against predicted values

Next, in order to check for serial correlation, a Wooldridge (2002) test is carried out using the Stata command ‘xtserial’ developed by Drukker (2003). The Wooldridge (2002) test rejects the null of no autocorrelation ( $p < 0.001$ ), though - like the modified Wald test - is very sensitive in the context of fixed effects with a large  $N$  and small  $T$  panel (Drukker, 2003).

Finally, in order to test for non-stationarity in both sanctions and SSRI prescribing, Pesaran’s (2007) panel unit root test is carried out which - unlike many unit root tests - does not require the assumption of cross-sectional independence to be met. This is carried out using the Stata command ‘pescadf’ developed by Lewandowski (2007), which rejects the null of non-stationarity with or without a time trend included for both variables ( $p < 0.001$ ).

### **A6.1.3 Unusual and Influential Data**

Next, checks for the influence of outliers and extreme observations are carried out. Firstly, observations with residuals that are two standard deviations from the mean in Model 6.2 are removed and the regression models re-estimated (Cousineau and Chartier, 2010). The results are shown in Table A6.1, Model A6.1. To check for the role of extreme observations, furthermore, the results

from Model 6.2 are re-run with the top and bottom one percentiles removed for sanctions (Model A6.2). Finally, the results from Model 6.2 are re-run with the seaside areas discussed in Section 6.4 removed (Blackpool, Torbay, Hastings, Great Yarmouth and Thanet) (Model A6.3). As Table A6.1 indicates, the results across the separate models remain broadly similar to the estimated sanctions coefficient in Model 6.2, suggesting that the results of the main analysis are not adversely impacted by unusual or influential data.

**Table A6.1:** relationship between sanctions and SSRI prescribing

	Model A6.1	Model A6.2	Model A6.3
Sanctions	0.344 *** (0.086)	0.327** (0.109)	0.409*** (0.083)
$R^2$ (within)	0.889	0.889	0.889
LA Quarters	5,265	5,362	5,369

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and additional explanatory variables not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A6.2 Claimant count model

**Table A6.2:** relationship between sanctions and SSRI prescribing

	Model A6.4
Sanctions	0.417*** (0.100)
JSA claimants	-0.203* (0.078)
Economic Inactivity	0.005 (0.002)
WCAs	0.250 (0.426)
GVA	-0.021** (0.006)
Age	
16-29	-0.131*** (0.026)
30-49	-0.549*** (0.073)
50-64	-0.480*** (0.067)
65 and over	0.036 (0.029)
Female	0.537*** (0.077)
White UK born	0.0006 (0.002)
Antibiotic Prescribing	0.080*** (0.015)
Index of Multiple Deprivation	
Quintile 2 × Quarter	37.408*** (1.611)
Quintile 3 × Quarter	60.136*** (3.389)
Quintile 4 × Quarter	71.410*** (4.521)
Quintile 5 × Quarter	109.311*** (7.143)
Urban-Rural Classification	
Urban with significant rural × Quarter	-21.318*** (1.940)
Predominantly urban × Quarter	-29.716*** (3.436)
$R^2$ (within)	0.889
LA Quarters	5,754

*Note:* Robust standard errors in brackets. Model includes local authority and time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A6.3 Employment and over-controlling

**Table A6.3:** relationship between sanctions and SSRI prescribing

		Model A6.5
Sanctions		0.396*** (0.081)
WCAs		0.178 (0.452)
GVA		-0.020** (0.006)
Age		
	16-29	-0.113*** (0.026)
	30-49	-0.513*** (0.072)
	50-64	-0.460*** (0.066)
	65 and over	0.049 (0.028)
Female		0.548*** (0.072)
White UK born		0.0001 (0.002)
Antibiotic Prescribing		0.083*** (0.015)
Index of Multiple Deprivation		
	Quintile 2 × Quarter	38.657*** (1.844)
	Quintile 3 × Quarter	62.560*** (3.729)
	Quintile 4 × Quarter	74.722*** (5.236)
	Quintile 5 × Quarter	113.908*** (8.333)
Urban-Rural Classification		
	Urban with significant rural × Quarter	-20.168*** (1.897)
	Predominantly urban × Quarter	-28.056*** (2.914)
$R^2$ (within)		0.888
LA Quarters		5,754

*Note:* Robust standard errors in brackets. Model includes local authority and time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A6.4 Multicollinearity check

**Table A6.4:** relationship between sanctions and SSRI prescribing

	Model A6.6	Model A6.7
Sanctions	0.397*** (0.081)	
Unemployment		-0.013 (0.012)
Economic Inactivity	0.004 (0.002)	0.005* (0.002)
Work Capability Assessments	0.178 (0.453)	0.231 (0.400)
GVA	-0.020** (0.006)	-0.023** (0.008)
Age		
16-29	-0.115*** (0.026)	-0.169*** (0.038)
30-49	-0.513*** (0.071)	-0.601*** (0.079)
50-64	-0.460*** (0.065)	-0.524*** (0.073)
65 and over	0.049 (0.028)	0.003 (0.037)
Female	0.546*** (0.072)	0.575*** (0.069)
White UK born	0.0003 (0.002)	0.0005 (0.002)
Antibiotic Prescribing	0.083*** (0.015)	0.086*** (0.015)
Index of Multiple Deprivation		
Quintile 2 × Quarter	38.629*** (1.816)	37.292*** (2.167)
Quintile 3 × Quarter	62.515*** (3.739)	59.810*** (2.869)
Quintile 4 × Quarter	74.520*** (5.227)	76.202*** (4.299)
Quintile 5 × Quarter	113.698*** (8.368)	114.310*** (6.947)
Urban-Rural Classification		
Urban with significant rural × Quarter	-20.304*** (1.953)	-22.867*** (2.214)
Predominantly urban × Quarter	-27.963*** (2.878)	-30.551*** (3.495)
$R^2$ (within)	0.888	0.889
LA Quarters	5,754	5,459

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A6.5 Falsification test

**Table A6.5:** relationship between sanctions and cardiovascular prescribing, falsification test

	Model A6.8	Model A6.9
Sanctions	1.503 (1.288)	1.478 (1.685)
Sanctions x Reform		0.035 (1.575)
Unemployment	-0.037 (0.086)	-0.037 (0.086)
Economic Inactivity	-0.054 (0.057)	-0.054 (0.057)
Work Capability Assessments	-2.463** (0.939)	-2.465** (0.937)
GVA	0.099 (0.083)	0.099 (0.082)
Age		
16-29	0.282 (1.154)	0.282 (1.152)
30-49	0.843 (1.710)	0.842 (1.708)
50-64	1.639 (1.587)	1.639 (1.581)
65 and over	2.555* (1.039)	2.555* (1.041)
Female	2.811* (1.134)	2.810* (1.130)
White UK born	-0.008 (0.041)	-0.008 (0.041)
Antibiotic Prescribing	0.677*** (0.163)	0.676*** (0.164)
Index of Multiple Deprivation		
Quintile 2 × Quarter	206.595* (84.586)	206.465* (84.596)
Quintile 3 × Quarter	100.641 (68.404)	100.372 (70.299)
Quintile 4 × Quarter	263.887*** (73.243)	263.433*** (75.009)
Quintile 5 × Quarter	283.358** (93.980)	282.593** (105.256)
Urban-Rural Classification		
Urban with significant rural × Quarter	-175.721* (77.698)	175.766* (77.863)
Predominantly urban × Quarter	-192.588* (77.830)	192.665* (78.036)
$R^2$ (within)	0.631	0.631
LA Quarters	5,459	5,459

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A6.6 Estimated sanctions coefficient

As discussed in Chapter 5, the sanctions measure (original adverse sanctions) that is available for the analysis underestimates the true quarterly rate. This implies that original adverse sanctions ( $S'$ ) capture only a proportion ( $k$ ) of total sanctions ( $S$ ) as indicated below:

$$S' = kS$$

$$\text{where } 0 < k < 1$$

As described in Chapter 6, the analysis investigates the relationship between sanctions and SSRI prescribing, which is written in a basic form below:

$$SSRI = \alpha + \beta \text{Sanctions}$$

$$= \alpha + \beta S$$

Given that the sanctions measure ( $S'$ ) underestimates the true sanctions rate ( $S$ ), the analysis in fact estimates the following:

$$SSRI = \alpha + \beta' S'$$

Which can be re-written as follows:

$$SSRI = \alpha + \beta' kS$$

$$= \alpha + k\beta' S$$

Which implies that:

$$\beta = k\beta'$$

The above implies that the estimated relationship ( $\beta'$ ) between sanctions and antidepressant prescribing in Chapter 6 is higher than what would be estimated if the true sanctions measure had been used ( $\beta$ ), and needs to be re-scaled by the proportion  $k$ .

As highlighted in Chapter 5, Kennedy and Keen (2016) estimate that under a fifth of original adverse decisions were challenged in any given month during the period of analysis, which implies that  $k$  should be set at 0.8 or above. Thus, whilst the finding in Chapter 6 that every 10 additional sanctions applied per 100,000 population are associated with approximately 4.57 additional SSRI

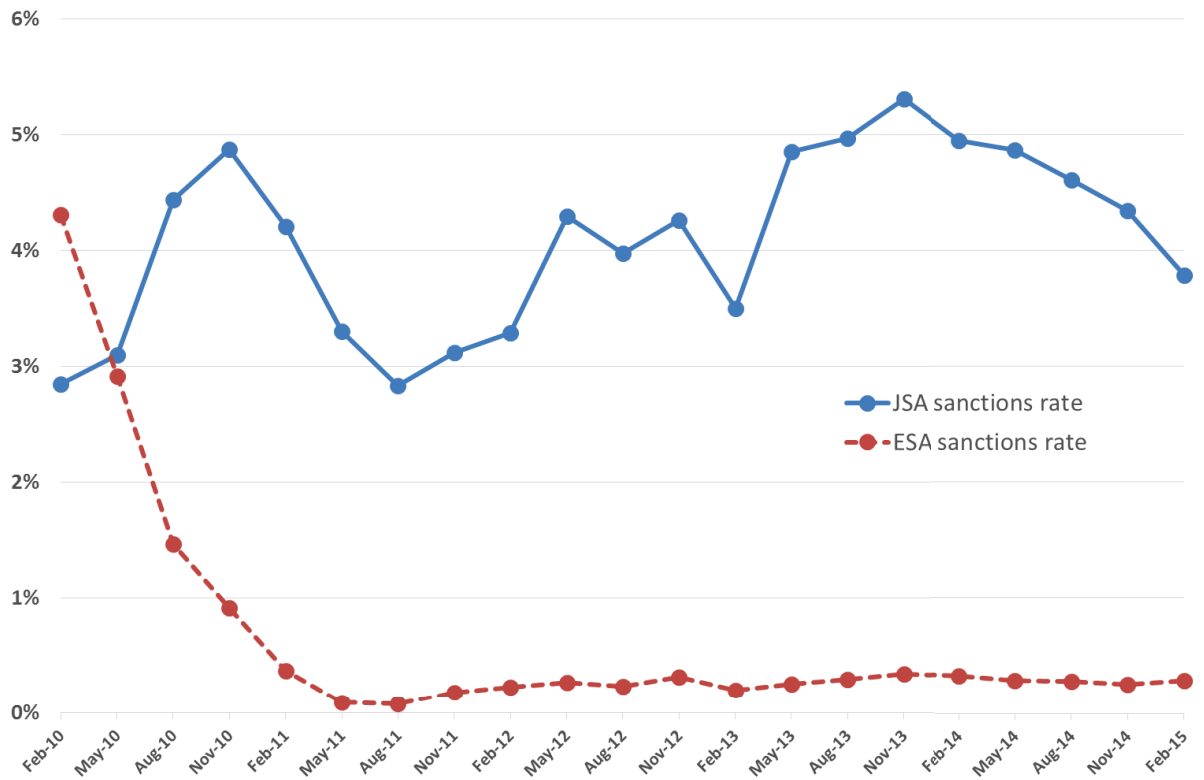


prescribing items translates to between one and two additional people receiving treatment, this needs to be scaled down closer to one.

## A6.7 Comparison of JSA and ESA sanction rates

Figure A6.3 compares the JSA and ESA sanctions rate during the period that is investigated in Chapter 6. Due to changes in how the DWP publish claimant statistics, the rates themselves are only calculable for four particular months during each year (February, May, August and November). JSA and ESA sanction rates are calculated using original adverse sanctions relating to claimants in England only, and measure sanctions as a proportion of JSA claimants and ESA WRAG claimants respectively. The different variations in rates of JSA and ESA sanctions implies that the analysis does not suffer from omitted variable bias due to its inability to include the ESA sanctions rate at the local authority-level in the fixed effects regression models.

**Figure A6.3:** JSA and ESA sanctions rate (per cent of claimants), 2010-2014



Source: author's calculations using Stat-Xplore data

## Chapter 7 appendix

### A7.1 District-level summary statistics and regression tables

**Table A7.1:** summary statistics for 324 local authority districts, Q3 2010 – Q4 2014

	<i>N</i>	Mean	St.d Dev	Min	Max	Source
<i>Dependent variable:</i>						
Anxiety and/or depression	5,676	5,028	2,543	405	19,056	QLFS
<i>Sanctions variable:</i>						
Original adverse	5,754	349	212	16	1,510	Stat- Xplore
<i>Control variables:</i>						
Claimants	5,754	2,900	1,452	491	9,043	Nomis
Unemployment	5,459	5,516	2,097	1,062	16,853	Nomis
Economic Inactivity	5,754	21,777	4,452	9,081	37,519	Nomis
Employment	5,754	71,839	5,559	51,682	90,813	Nomis
Work Capability Assessments	5,754	393	202	43	1,891	Stat- Xplore
GVA	5,754	22,886	14,435	11,876	235,244	Nomis
Age						Nomis
16-29 year olds	5,754	27,260	4,623	19,387	45,662	
30-49 year olds	5,754	42,846	2,912	33,514	52,309	
50-64 year olds	5,754	29,894	5,088	12,407	42,183	
Female	5,754	50,238	842	43,245	52,579	Nomis
White	5,754	89,003	12,567	22,444	100,000	Nomis
Index of Multiple Deprivation						DCLG
Quintile 1	1,166					
Quintile 2	1,157					
Quintile 3	1,140					
Quintile 4	1,165					
Quintile 5	1,126					
Urban-Rural Classification						Defra
Predominantly rural	1,620					
Urban with significant rural	959					
Predominantly urban	3,175					
<i>Falsification variables:</i>						
Anxiety and/or depression (age 65+)	3,918	2,028	2,171	116	35,545	QLFS
Cardiovascular health	5,742	7,745	2,995	702	29,582	QLFS

*Note:* Local authority quarters where the QLFS estimates produced zero estimates were removed from the sample.

**Table A7.2:** relationship between sanctions and anxiety and/or depression, initial model selection

	Model A7.1: Fixed Effects	Model A7.2: Fixed Effects	Model A7.3: Random Effects
Sanctions	0.226 (0.317)	0.191 (0.302)	0.253 (0.392)
Unemployment	0.028 (0.025)	0.029 (0.025)	0.042 (0.031)
Economic Inactivity	0.084*** (0.017)	0.081*** (0.018)	0.085*** (0.015)
WCAs	0.851 (0.454)	0.790 (0.457)	1.028* (0.480)
GVA	-0.034* (0.012)	-0.023 (0.015)	0.0003 (0.003)
Age			
30-49	-0.206** (0.055)	-0.237** (0.080)	-0.098*** (0.023)
50-64	-0.157* (0.071)	-0.164* (0.074)	0.030 (0.027)
Female	0.843*** (0.193)	0.943*** (0.220)	-0.035 (0.075)
White	0.054*** (0.013)	0.055*** (0.013)	0.052*** (0.012)
Index of Multiple Deprivation			
Quintile 2			-18.032 (238.104)
Quintile 3			214.030 (215.671)
Quintile 4			860.261*** (216.510)
Quintile 5			1177.122*** (292.764)
Urban-Rural Classification			
Urban with significant rural			-270.958 (344.405)
Predominantly urban			249.576 (211.915)
Index of Multiple Deprivation			
Quintile 2 x Quarter		17.545 (26.505)	
Quintile 3 x Quarter		7.883 (16.752)	
Quintile 4 x Quarter		33.019*** (8.253)	

Quintile 5 x Quarter		32.799*** (6.846)	
Urban-Rural Classification			
Urban with significant rural x Quarter		59.653** (17.543)	
Predominantly urban x Quarter		8.193 (25.801)	
<hr/>			
$R^2$ (within)	0.823	0.827	0.819
LA Quarters	5,391	5,391	5,391
<hr/>			

*Note:* Robust standard errors in brackets. Model A7.1 and A7.2 include local authority and time fixed effects. Model A7.3 includes time fixed effects. Constant and LFS question change dummy not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A7.3:** relationship between sanctions and anxiety and/or depression, fixed effects models

	Model A7.2	Model A7.4
Sanctions	0.191 (0.302)	0.127 (0.450)
Sanctions x Reform		0.091 (0.479)
Unemployment	0.029 (0.025)	0.029 (0.025)
Economic Inactivity	0.081*** (0.018)	0.081*** (0.018)
WCAs	0.790 (0.457)	0.786 (0.456)
GVA	-0.023 (0.015)	-0.023 (0.016)
Age		
30-49	-0.237** (0.080)	-0.236** (0.079)
50-64	-0.164* (0.074)	-0.164* (0.073)
Female	0.943*** (0.220)	0.940*** (0.214)
White	0.055*** (0.013)	0.055*** (0.013)
Index of Multiple Deprivation		
Quintile 2 x Quarter	17.545 (26.505)	17.020 (26.081)
Quintile 3 x Quarter	7.883 (16.752)	6.756 (16.566)
Quintile 4 x Quarter	33.019*** (8.253)	31.163** (10.531)
Quintile 5 x Quarter	32.799*** (6.846)	29.663 (15.202)
Urban-Rural Classification		
Urban with significant rural x Quarter	59.653** (17.543)	59.414** (17.823)
Predominantly urban x Quarter	8.193 (25.801)	7.794 (25.783)
$R^2$ (within)	0.827	0.827
LA Quarters	5,391	5,391

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and LFS question change dummy not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A7.2 County-level initial modelling process

**Table A7.4:** relationship between sanctions and anxiety and/or depression, initial model selection

	Model A7.5: Fixed Effects	Model A7.6: Fixed Effects	Model A7.7: Random Effects
Sanctions	0.819** (0.265)	0.777*** (0.199)	0.785*** (0.156)
Unemployment	-0.018 (0.038)	-0.009 (0.041)	0.018 (0.047)
Economic Inactivity	0.070** (0.020)	0.074** (0.020)	0.064* (0.022)
WCAs	0.267 (0.520)	0.216 (0.536)	0.656 (0.507)
GVA	-0.041** (0.013)	-0.059*** (0.014)	-0.007 (0.006)
Age			
30-49	-0.448*** (0.091)	-0.407*** (0.104)	-0.139 (0.067)
50-64	0.107 (0.156)	0.105 (0.108)	0.061 (0.033)
Female	0.265 (0.484)	0.504 (0.473)	-0.111 (0.173)
White	0.023 (0.020)	0.025 (0.019)	0.045** (0.012)
Index of Multiple Deprivation			
Quintile 2			176.388 (333.627)
Quintile 3			494.839 (239.399)
Quintile 4			1,228.314*** (308.493)
Quintile 5			1,117.412* (424.947)
Urban-Rural Classification			
Urban with significant rural			-318.679 (223.929)
Predominantly urban			332.084 (300.793)
Index of Multiple Deprivation			
Quintile 2 x Quarter		51.541** (14.751)	

Quintile 3 x Quarter		56.931*** (6.926)	
Quintile 4 x Quarter		66.666*** (16.652)	
Quintile 5 x Quarter		80.834** (21.423)	
Urban-Rural Classification			
Urban with significant rural x Quarter		10.867 (12.300)	
Predominantly urban x Quarter		-50.008** (14.758)	
<hr/>			
$R^2$ (within)	0.839	0.847	0.832
LA Quarters	2,587	2,587	2,587
<hr/>			

*Note:* Robust standard errors in brackets. Model 7.1 and Model 7.2 include local authority and time fixed effects. Model 7.3 includes time fixed effects. Constant and LFS question change dummy not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$



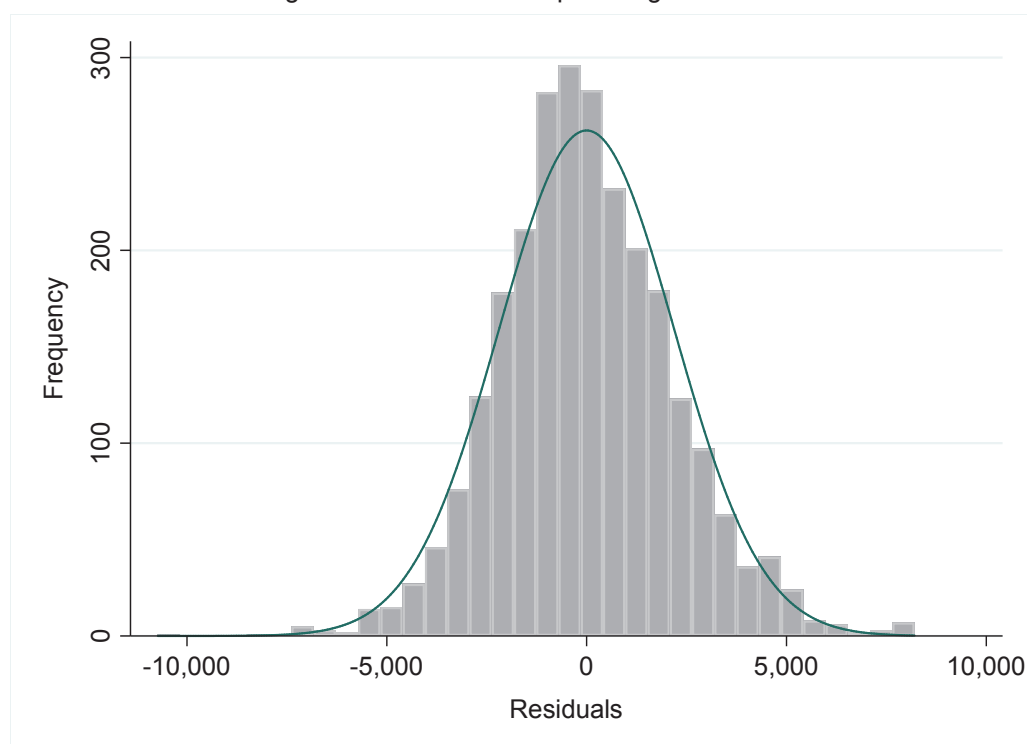
## A7.3 Diagnostic tests

Various diagnostic checks are carried out to test that the fixed effects model assumptions are satisfied, as previously identified in the appendix for Chapter 6 (Section A6.1). The diagnostic checks presented here are for regression Model 7.1, Table 7.2, as discussed in Section 7.5.

### A7.3.1 Normality of the residuals

Figure A7.1 depicts a histogram of the regression residuals to check for serious deviations from the assumption of normality. Clearly, the residuals do not deviate sufficiently from the ideal of normality to be of concern. Three formal tests of normality, a Skewness/Kurtosis test ( $p < 0.001$ ), a Shapiro-Wilk test ( $p < 0.001$ ) and a Shapiro-Francia test ( $p < 0.001$ ) reject the null of normality. However, as previously highlighted, such tests are sensitive to even very small deviations from normality at large sample sizes. The rejection of normality by such tests is therefore not of concern to the analysis, given the distribution that is actually observed.

**Figure A7.1:** distribution of regression residuals compared against normal distribution curve

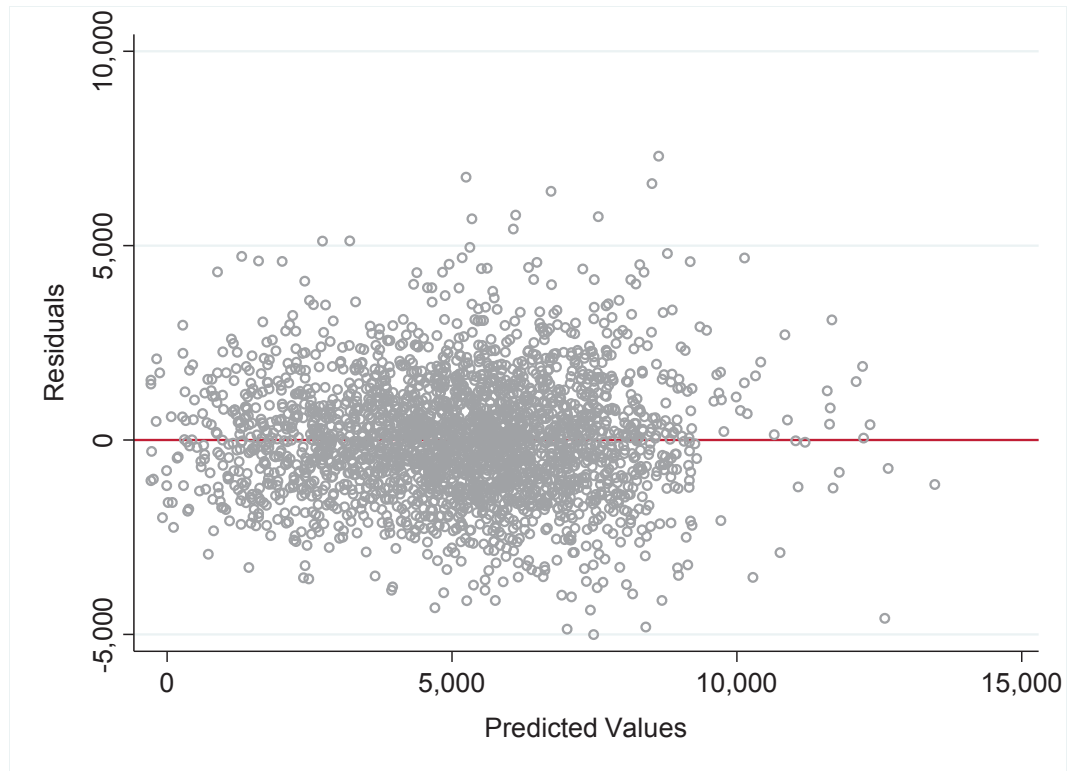


### **A7.3.2 Cross-sectional independence, homoscedasticity, no serial correlation and stationarity**

The tests carried out in this sub-section indicate that the fixed effects models suffer from cross-sectional dependence, heteroscedasticity and autocorrelation, though the previously identified caveats apply regarding the results of such tests. In any case, the fixed effects regression models estimated throughout Chapter 7 use Driscoll-Kraay standard errors (Driscoll and Kray, 1998), which are robust to cross-sectional dependence, heteroscedasticity and correlation through time within local authorities.

The standard test for cross-sectional independence is the Breusch-Pagan Lagrange multiplier (LM) (Breusch and Pagan, 1980). This isn't valid in panels with a large number of observations ( $N$ ) but a small number of observations per cross-sectional unit ( $T$ ), which is the case here ( $N = 148$ ,  $T = 18$ ). Instead, Pesaran's (2004) cross-sectional dependence (CD) test is carried out, which is compatible with unbalanced datasets. The Pesaran CD test rejects the null hypothesis of no cross-sectional dependence ( $p < 0.001$ ).

Next, in order to check for heteroscedasticity, a modified Wald test (Greene, 2008) is carried out that tests for group-wise heteroscedasticity in the residuals of fixed effect regression models. The modified Wald test rejects the null of homoscedasticity ( $p < 0.001$ ), which indicates that the residuals display heteroscedasticity. This test, however, has a very low power in the context of fixed effects with "large  $N$ , small  $T$ " (Baum, 2001: 102) panels, as is the case here. The result of the modified Wald test should, therefore, be treated with caution. Indeed, a scatter plot of the regression residuals against predicted values, furthermore, suggests that the error term has an approximately constant variance, since there is little sign of a fanning out effect over different predicted values. This is depicted in Figure A7.2.

**Figure A7.2:** scatter plot of the regression residuals against predicted values

Next, in order to check for serial correlation, a Wooldridge (2002) test is carried out. The Wooldridge (2002) test rejects the null of no autocorrelation ( $p < 0.001$ ), though - like the modified Wald test - is very sensitive in the context of fixed effects with a large  $N$  and small  $T$  panel (Drukker, 2003).

Finally, in order to test for non-stationarity, in both sanctions and anxiety and/or depression, Pesaran's (2007) panel unit root test is carried out which - unlike many unit root tests - does not require the assumption of cross-sectional independence to be met. This test rejects the null of non-stationarity with or without a time trend included for both variables ( $p < 0.001$ ).

### **A7.3.3 Unusual and influential data**

Next, checks for the influence of outliers and extreme observations are carried out. Firstly, observations with residuals that are two standard deviations from the mean in Model 7.1 (Table 7.2) are removed and the regression models re-estimated (Cousineau and Chartier, 2010). The results are shown in Table A7.5, Model A7.8. Finally, to check for the role of extreme observations the results from Model 7.1 are re-run with the top and bottom one percentiles removed for sanctions (Model A7.9). As Table A7.5 indicates, the results across the two

models remain broadly similar to the estimated sanctions coefficient in Model 7.1, suggesting that the results of the main analysis are not adversely impacted by unusual or influential data.

**Table A7.5:** relationship between sanctions and anxiety and/or depression

	Model A7.8	Model A7.9
Sanctions	0.748*** (0.172)	0.549** (0.180)
$R^2$ (within)	0.854	0.847
LA Quarters	2,568	2,537

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and additional explanatory variables not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A7.4 Claimant count model

**Table A7.6:** relationship between sanctions and anxiety and/or depression

	Model A7.10
Sanctions	0.819*** (0.175)
JSA claimants	-0.166 (0.162)
Economic Inactivity	0.079*** (0.021)
WCAs	0.367 (0.517)
GVA	0.053*** (0.012)
Age	
30-49	-0.402*** (0.096)
50-64	0.093 (0.098)
Female	0.319 (0.402)
White	0.028 (0.019)
Index of Multiple Deprivation	
Quintile 2 × Quarter	46.627** (15.023)
Quintile 3 × Quarter	50.296*** (8.760)
Quintile 4 × Quarter	58.465** (16.002)
Quintile 5 × Quarter	68.498* (25.019)
Urban-Rural Classification	
Urban with significant rural × Quarter	5.738 (13.310)
Predominantly urban × Quarter	-54.160** (14.979)
$R^2$ (within)	0.847
LA Quarters	2,592

*Note:* Robust standard errors in brackets. Model includes local authority and time fixed effects. Constant not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A7.5 Falsification tests

**Table A7.7:** relationship between sanctions and anxiety and/or depression (aged 65+)

	Model A7.11	Model A7.12
Sanctions	0.518 (0.293)	0.616 (0.405)
Sanctions x Reform		-0.134 (0.361)
Unemployment	0.003 (0.027)	0.004 (0.027)
Economic Inactivity	-0.003 (0.020)	-0.003 (0.020)
WCAs	0.492 (0.289)	0.497 (0.279)
GVA	-0.036 (0.021)	-0.037 (0.021)
Age		
30-49	-0.286*** (0.059)	-0.289*** (0.062)
50-64	0.096 (0.098)	0.097 (0.099)
Female	-1.157* (0.511)	-1.150* (0.510)
White	0.018 (0.016)	0.018 (0.016)
Index of Multiple Deprivation		
Quintile 2 x Quarter	-17.367 (10.564)	-16.099 (10.491)
Quintile 3 x Quarter	-3.642 (14.349)	-1.271 (16.876)
Quintile 4 x Quarter	-11.376 (22.597)	-8.235 (24.410)
Quintile 5 x Quarter	4.435 (16.186)	9.231 (23.854)
Urban-Rural Classification		
Urban with significant rural x Quarter	-6.589 (16.770)	-6.198 (16.916)
Predominantly urban x Quarter	0.796 (11.382)	1.386 (11.810)
$R^2$ (within)	0.763	0.763
LA Quarters	2,196	2,196

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and LFS question change dummy not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A7.8:** relationship between sanctions and cardiovascular health

	Model A7.13	Model A7.14
Sanctions	0.672 (0.496)	0.530 (0.569)
Sanctions x Reform		0.194 (0.335)
Unemployment	0.042 (0.041)	0.042 (0.040)
Economic Inactivity	0.086** (0.028)	0.086** (0.028)
WCAs	0.345 (0.397)	0.338 (0.385)
GVA	-0.007 (0.042)	-0.006 (0.042)
Age		
30-49	0.080 (0.140)	0.084 (0.142)
50-64	-0.095 (0.145)	-0.097 (0.144)
Female	-2.281*** (0.479)	-2.291*** (0.473)
White	0.033 (0.018)	0.034 (0.018)
Index of Multiple Deprivation		
Quintile 2 x Quarter	8.100 (22.361)	6.260 (20.553)
Quintile 3 x Quarter	26.953 (26.034)	23.512 (25.569)
Quintile 4 x Quarter	30.900 (29.839)	26.341 (28.315)
Quintile 5 x Quarter	65.340* (22.880)	58.379* (24.805)
Urban-Rural Classification		
Urban with significant rural x Quarter	46.521 (25.803)	45.954 (25.481)
Predominantly urban x Quarter	-16.215 (20.480)	-17.071 (20.117)
$R^2$ (within)	0.849	0.849
LA Quarters	2,590	2,590

*Note:* Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and LFS question change dummy not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## Chapter 8 appendix

### A8.1 Additional control variables

**Table A8.1:** relationship between sanctions and anxiety and/or depression (three-level)

	Model A8.1: Pre-reform	Model A8.2: Post-reform
Sanctions	0.014 (0.009)	0.023* (0.010)
Index of Multiple Deprivation		
Quintile 2	0.003 (0.012)	-0.047* (0.018)
Quintile 3	-0.005 (0.012)	-0.023 (0.018)
Quintile 4	0.007 (0.011)	-0.019 (0.017)
Quintile 5	-0.014 (0.011)	-0.017 (0.017)
Urban-Rural Classification		
Urban with significant rural	-0.0008 (0.011)	-0.009 (0.016)
Predominantly urban	0.021* (0.009)	-0.014 (0.013)
Age	0.007*** (0.001)	0.008*** (0.002)
Female (ref: Male)	0.032*** (0.005)	0.050*** (0.007)
Ethnicity (ref: White)		
Mixed	0.029 (0.019)	-0.028 (0.025)
Asian or Asian British	-0.024* (0.010)	-0.046*** (0.014)
Black or Black British	-0.044*** (0.011)	-0.083*** (0.014)
Chinese	-0.101* (0.041)	-0.068 (0.101)
Other	-0.027 (0.015)	-0.066** (0.022)
Disability (ref: No disability)	0.215*** (0.005)	0.252*** (0.006)
Qualifications (ref: Higher education)		
A-Level or equivalent	0.006 (0.008)	0.003 (0.011)



GCSE or equivalent	0.016* (0.008)	0.012 (0.011)
Other qualifications	0.013 (0.008)	0.040*** (0.011)
No qualification	0.027** (0.009)	0.010 (0.012)
Don't know	0.020 (0.020)	-0.045 (0.024)
Partner (ref. No partner)	-0.015* (0.006)	-0.031*** (0.008)
Dependent children (ref. No dependents)	-0.015* (0.006)	-0.012 (0.007)
Socio-economic status (ref: Never worked)		
Routine and semi-routine occupations	-0.001 (0.005)	0.009 (0.006)
Lower supervisory and technical	-0.002 (0.010)	0.017 (0.013)
Small employers and own account workers	-0.018 (0.012)	-0.023 (0.016)
Intermediate occupations	-0.010 (0.009)	-0.019 (0.012)
Managerial and professional	-0.004 (0.008)	-0.016 (0.011)
Within individual variance	0.015	0.015
Between individual variance	0.044	0.067
Between local authority variance	0.0004	0.001
Occasion, <i>n</i>	13,278	10,672
Individual, <i>n</i>	8,775	6,642
Local Authority, <i>n</i>	324	319

*Note:* Robust standard errors in brackets. Constant, time fixed effects and LFS question change dummy (post-reform model) not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A8.2:** relationship between sanctions and anxiety and/or depression (two-level)

	Model A8.3: Pre-reform	Model A8.4: Post-reform
Sanctions	0.010 (0.019)	0.021* (0.010)
Index of Multiple Deprivation		
Quintile 2	0.019 (0.014)	-0.013 (0.026)
Quintile 3	0.005 (0.014)	-0.010 (0.024)
Quintile 4	0.017 (0.013)	0.008 (0.023)
Quintile 5	0.004 (0.013)	0.011 (0.023)
Urban-Rural Classification		
Urban with significant rural	-0.0002 (0.012)	-0.006 (0.020)
Predominantly urban	0.014 (0.009)	-0.007 (0.015)
Age	0.008*** (0.001)	0.007*** (0.002)
Female (ref. Male)	0.028*** (0.006)	0.051*** (0.008)
Ethnicity (ref. White)		
Mixed	0.007 (0.020)	-0.017 (0.026)
Asian or Asian British	-0.025* (0.011)	-0.051*** (0.015)
Black or Black British	-0.042*** (0.011)	-0.076*** (0.015)
Chinese	-0.109* (0.043)	-0.050 (0.115)
Other	-0.011 (0.017)	-0.068** (0.024)
Disability (ref. No disability)	0.228*** (0.006)	0.284*** (0.009)
Qualifications (ref. Higher education)		
A-Level or equivalent	0.002 (0.009)	0.004 (0.014)
GCSE or equivalent	0.013 (0.009)	-0.005 (0.013)
Other qualifications	0.006 (0.009)	0.022 (0.014)
No qualification	0.018 (0.010)	-0.002 (0.014)

Don't know	0.016 (0.025)	-0.044 (0.031)
Partner (ref. No partner)	-0.017** (0.006)	-0.027** (0.009)
Dependent children (ref. No dependents)	-0.014* (0.006)	-0.019* (0.009)
Socio-economic status (ref: Never worked)		
Routine and semi-routine occupations	0.004 (0.006)	-0.011 (0.009)
Lower supervisory and technical	-0.005 (0.012)	0.011 (0.019)
Small employers and own account workers	-0.019 (0.014)	-0.056** (0.021)
Intermediate occupations	-0.007 (0.012)	-0.030 (0.016)
Managerial and professional	-0.010 (0.010)	-0.024 (0.015)
Between individual variance	0.055	0.079
Between local authority variance	0.0004	0.001
Individual, <i>n</i>	8,311	5,577
Local Authority, <i>n</i>	248	191

*Note:* Robust standard errors in brackets. Constant, time fixed effects and LFS question change dummy (post-reform model) not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A8.2 Logistic regression

**Table A8.3:** relationship between sanctions and cardiovascular problems (two-level)

	Model A8.5: Pre-reform	Model A8.6: Post-reform
Sanctions	0.104 (0.205)	0.498* (0.228)
Age	0.177*** (0.025)	0.138*** (0.026)
Female (ref: Male)	0.469*** (0.092)	0.635*** (0.095)
Ethnicity (ref: White)		
Mixed	0.137 (0.313)	-0.270 (0.367)
Asian or Asian British	-0.519* (0.218)	-0.836*** (0.220)
Black or Black British	-1.142*** (0.280)	-1.597*** (0.315)
Chinese	-10.423 (78.841)	-13.522 (858.781)
Other	-0.217 (0.313)	-1.112** (0.386)
Disability (ref: No disability)	3.253*** (0.121)	3.277*** (0.128)
Qualifications (ref: Higher education)		
A-Level or equivalent	0.020 (0.173)	0.222 (0.178)
GCSE or equivalent	0.237 (0.158)	0.051 (0.163)
Other qualifications	0.177 (0.172)	0.390* (0.173)
No qualification	0.252 (0.166)	0.050 (0.171)
Don't know	0.090 (0.461)	-0.572 (0.494)
Partner (ref. No partner)	-0.373*** (0.115)	-0.425*** (0.116)
Dependent children (ref. No dependents)	-0.315** (0.111)	-0.163 (0.116)
Socio-economic status (ref: Never worked)		
Routine and semi-routine occupations	0.069 (0.106)	-0.130 (0.111)
Lower supervisory and technical	-0.181 (0.231)	-0.022 (0.219)

Small employers and own account workers	-0.552* (0.267)	-0.521* (0.260)
Intermediate occupations	-0.134 (0.217)	-0.478* (0.221)
Managerial and professional	-0.133 (0.173)	-0.389* (0.197)
Between local authority variance	0.113	0.271
Individual, <i>n</i>	9,831	6,699
Local Authority, <i>n</i>	293	232

*Note:* Table reports coefficients (not odds ratios). Robust standard errors in brackets. Constant and LFS question change dummy (post-reform model) not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A8.3 Falsification test

**Table A8.4:** relationship between sanctions and cardiovascular problems (two-level)

	Model A8.7: Pre-reform	Model A8.8: Post-reform
Sanctions	0.110 (0.080)	0.002 (0.089)
Age	-0.010*** (0.001)	-0.010*** (0.002)
Female (ref: Male)	-0.010* (0.005)	-0.006 (0.006)
Ethnicity (ref: White)		
Mixed	-0.023 (0.019)	0.031 (0.023)
Asian or Asian British	0.037*** (0.010)	0.018 (0.012)
Black or Black British	0.021* (0.011)	0.006 (0.013)
Chinese	-0.065 (0.043)	0.128 (0.102)
Other	0.012 (0.016)	0.038 (0.020)
Disability (ref: No disability)	0.160*** (0.006)	0.139*** (0.007)
Qualifications (ref: Higher education)		
A-Level or equivalent	0.003 (0.009)	0.008 (0.011)
GCSE or equivalent	0.017* (0.008)	0.002 (0.010)
Other qualifications	0.009 (0.009)	0.026 (0.011)
No qualification	0.006 (0.009)	0.002 (0.011)
Don't know	-0.002 (0.022)	-0.030 (0.024)
Partner (ref. No partner)	0.007 (0.006)	0.020** (0.008)
Dependent children (ref. No dependents)	-0.002 (0.006)	-0.005 (0.007)
Socio-economic status (ref: Never worked)		
Routine and semi-routine occupations	0.002 (0.006)	-0.020** (0.007)
Lower supervisory and technical	0.008 (0.011)	0.001 (0.015)

Small employers and own account workers	0.016 (0.013)	-0.022 (0.017)
Intermediate occupations	0.005 (0.011)	-0.008 (0.013)
Managerial and professional	-0.003 (0.009)	-0.040*** (0.012)
Between individual variance	0.055	0.062
Between local authority variance	0.001	0.001
Individual, <i>n</i>	9,831	6,699
Local Authority, <i>n</i>	293	232

*Note:* Robust standard errors in brackets. Constant and LFS question change dummy (post-reform model) not shown. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## Chapter 9 appendix

### A9.1 Summary statistics

**Table A9.1:** summary statistics for JSA claimants and control groups, pre- and post-intervention period

	JSA claimants		JSA claimants (NI)		WTC claimants		Self-employed WTC claimants		Unemployed (non-JSA claimants)	
	Pre-	Post-	Pre-	Post-	Pre-	Post-	Pre-	Post-	Pre-	Post-
<i>Dependent variable:</i>										
Worsening anxiety/and or depression	1.4	3.1	0.0	0.0	1.0	0.7	0.9	0.4	2.5	0.7
<i>Control variables:</i>										
Female	31.4	33.1	22.8	31.7	72.3	71.5	51.8	51.8	51.4	53.3
Age	38.6	38.5	34.7	33.4	40.1	40.5	42.1	42.7	34.8	34.5
Ethnicity										
White	88.3	85.8	100.0	100.0	91.1	91.7	89.0	91.6	86.3	85.6
Mixed	0.8	1.3	0.0	0.0	0.7	0.5	0.6	0.7	1.5	1.0
Asian or Asian British	4.6	5.1	0.0	0.0	4.6	4.2	7.4	4.8	5.7	5.8
Black or Black British	4.0	4.8	0.0	0.0	2.6	2.1	1.1	1.5	4.0	4.1
Chinese	0.2	0.5	0.0	0.0	0.3	0.4	1.1	0.4	0.3	0.9
Other ethnic group	2.1	2.6	0.0	0.0	0.8	1.1	0.9	1.1	2.3	2.6
Qualifications										
No qualifications	18.7	20.1	22.8	36.7	5.6	5.5	7.9	7.6	11.7	9.3
GCSE or equivalent	17.4	16.0	12.3	15.0	21.0	21.3	20.6	14.3	21.9	25.9



A-Level or equivalent	14.1	11.8	19.3	5.0	17.1	17.9	14.9	20.3	15.5	15.8
Higher education	16.9	17.8	12.3	10.0	30.5	30.3	27.4	31.5	22.8	21.6
Other qualifications	33.0	34.4	33.3	33.3	25.8	25.0	29.2	26.4	28.1	27.5
Disability	27.6	30.1	24.6	23.3	15.3	18.0	19.5	22.9	20.3	24.0
Marital status										
Single	56.4	59.6	77.2	71.7	20.8	20.7	16.5	19.0	56.1	56.4
Married	42.7	39.1	22.8	28.3	78.1	77.9	81.4	79.1	42.8	41.4
Divorced	0.9	1.1	0.0	0.0	1.1	1.2	1.8	1.9	1.1	1.9
Widowed	0.0	0.2	0.0	0.0	0.1	0.2	0.2	0.0	0.0	0.3
Dependents	34.7	32.4	22.8	46.7	92.2	91.8	86.8	82.3	54.9	56.7
Socio-economic status (NS-SEC)										
Never worked	39.2	49.5	43.9	78.3	1.5	1.7	0.9	1.1	51.1	57.4
Routine and semi-routine occupations	29.3	23.3	35.1	10.0	30.1	31.5	0.0	0.0	18.1	17.1
Lower supervisory and technical	7.9	5.5	7.0	0.0	8.6	8.5	0.0	0.0	4.8	4.3
Small employers and own account workers	5.4	4.4	3.5	3.3	10.3	11.0	82.7	83.2	2.6	3.0
Intermediate occupations	4.5	6.2	5.3	1.7	15.5	16.0	0.0	0.0	7.4	4.9
Managerial	13.8	11.2	5.3	6.7	34.0	31.4	16.4	15.8	15.9	13.4
<i>Falsification variable:</i>										
Worsening cardiovascular problems	2.5	1.8	0.0	0.0	1.4	1.1	0.8	1.6	1.6	0.8

Note: table reports unweighted sample percentages with the exception of age, which reports the mean. Region not reported.

## A9.2 Difference-in-differences regression models

**Table A9.2:** estimated intervention effect on the self-reported mental health of JSA claimants

	Model A9.1: JSA claimants (NI)	Model A9.2: WTC claimants	Model A9.3: Self- employed WTC claimants	Model A9.4: Unemployed (non-JSA claimants)
Treat*Post	0.017* (0.009)	0.017* (0.008)	0.019* (0.009)	0.033*** (0.010)
Treat (ref: Control)	0.054* (0.026)	-0.002 (0.006)	-0.002 (0.008)	-0.012 (0.007)
Post (ref: Pre)	-0.005 (0.005)	-0.004 (0.002)	-0.004 (0.005)	-0.020*** (0.006)
Constant	0.015 (0.017)	0.027* (0.013)	0.051* (0.024)	0.058** (0.022)
Female (ref: Male)	0.002 (0.008)	0.005 (0.002)	0.007 (0.006)	0.005 (0.005)
Age	-0.0002 (0.0004)	-0.00003 (0.0002)	-0.0002 (0.0003)	0.0001 (0.0003)
Ethnicity (ref: White)				
Mixed	-0.021* (0.008)	-0.001 (0.013)	-0.018** (0.006)	0.008 (0.028)
Asian or Asian British	-0.021*** (0.006)	-0.009*** (0.002)	-0.015*** (0.004)	-0.016*** (0.004)
Black or Black British	-0.013 (0.012)	-0.001 (0.007)	-0.010 (0.012)	-0.013* (0.006)
Chinese	0.108 (0.130)	0.014 (0.024)	0.057 (0.068)	0.028 (0.037)
Other ethnic group	0.013 (0.031)	0.002 (0.012)	0.013 (0.027)	0.029 (0.024)
Qualifications (ref: No qualifications)				
GCSE or equivalent	-0.014 (0.013)	-0.003 (0.006)	-0.012 (0.009)	-0.009 (0.010)
A-Level or equivalent	-0.019 (0.012)	0.0003 (0.006)	-0.013 (0.009)	0.002 (0.011)
Higher education	-0.019 (0.013)	-0.002 (0.006)	-0.011 (0.009)	-0.014 (0.009)
Other qualifications	0.007 (0.013)	-0.001 (0.006)	0.007 (0.010)	0.002 (0.010)
Disability (ref: No disability)	0.004 (0.010)	0.006 (0.004)	0.001 (0.006)	0.008 (0.007)
Marital status (ref: single)				

Married	0.001 (0.010)	-0.002 (0.003)	0.002 (0.007)	-0.006 (0.008)
Divorced	0.026 (0.066)	-0.002 (0.012)	0.011 (0.029)	-0.004 (0.027)
Widowed	-0.005 (0.014)	-0.012*** (0.004)	-0.008 (0.009)	-0.022* (0.010)
Dependents (ref: No dependents)	-0.005 (0.008)	-0.003 (0.005)	-0.004 (0.007)	-0.003 (0.005)
NS-SEC (ref: Never worked)				
Routine and semi-routine occupations	-0.008 (0.010)	-0.010 (0.007)	-0.010 (0.009)	-0.007 (0.007)
Lower supervisory and technical	-0.012 (0.013)	-0.005 (0.008)	-0.017 (0.011)	-0.011 (0.009)
Small employers and own account workers	-0.009 (0.014)	-0.013 (0.007)	-0.012 (0.010)	-0.009 (0.010)
Intermediate occupations	-0.013 (0.013)	-0.014* (0.007)	-0.017 (0.013)	-0.017** (0.007)
Managerial	-0.010 (0.009)	-0.014* (0.007)	-0.012 (0.009)	-0.010 (0.006)
R <sup>2</sup>	0.021	0.008	0.018	0.017
N	1,842	9,423	2,711	3,728

Note: Robust standard errors in brackets. Region of residence not shown. \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

## A9.3 Logistic regression

As a sensitivity check, Table A9.3 re-runs the results discussed in Models 9.1-9.3 using logistic regression. Since the control group of JSA claimants in Northern Ireland did not see any worsening in mental health in either period, difference-in-differences models using this control group cannot be estimated with logistic regression due to perfect collinearity with the dependent variable.

**Table A9.3:** estimated intervention effect on the self-reported mental health of JSA claimants

Comparison Group	Model(s) A9.5: No covariates	Model(s) A9.6: Covariates	Model(s) A9.7: Matching
JSA claimants (NI)	-	-	-
WTC claimants	1.087* (0.446)	1.106* (0.456)	1.352* (0.648)
Self-employed WTC claimants	1.325 (0.923)	1.272 (0.944)	1.091 (0.923)
Unemployed (non-JSA claimants)	2.036*** (0.590)	2.067*** (0.606)	2.074*** (0.574)

*Note:* Robust standard errors in brackets. Covariates: gender; age; ethnicity; qualification level; disability; marital status; dependent children; socio-economic status and region of residence. \* p < 0.05, \*\* p < 0.01, \*\*\* p < 0.001

## A9.4 Falsification test

**Table A9.4:** summary of 'worsening' mental health for JSA treatment group and comparison groups

	Pre-intervention (less strict sanctioning)		Post-intervention (harsher sanctioning)	
	Maintained	Worsened	Maintained	Worsened
<i>Treatment group:</i>				
JSA claimants	862	22	767	14
<i>Control groups:</i>				
JSA claimants (NI)	55	0	58	0
WTC claimants	4,112	57	3,294	35
Self-employed WTC claimants	508	4	422	7
Unemployed (non-JSA claimants)	1,066	17	900	7

*Note:* table reports unweighted sample *N*s

**Table A9.5:** estimated intervention effect on the self-reported cardiovascular health of JSA claimants

Comparison Group	Model(s) A9.8: No covariates	Model(s) A9.9: Covariates	Model(s) A9.10: Matching
JSA claimants (NI)	-0.008 (0.007)	-0.005 (0.007)	-0.006 (0.027)
WTC claimants	-0.004 (0.007)	-0.004 (0.007)	0.004 (0.007)
Self-employed WTC claimants	-0.014 (0.009)	-0.016 (0.009)	-0.004 (0.011)
Unemployed (non-JSA claimants)	0.0002 (0.007)	0.0003 (0.007)	0.004 (0.010)

*Note:* Robust standard errors in brackets. Covariates: gender; age; ethnicity; qualification level; disability; marital status; dependent children; socio-economic status and region of residence. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## A9.5 Placebo test

**Table A9.6:** two-quarter longitudinal LFS datasets used in the difference-in-differences analysis

	Quarter 1	Quarter 2
<u>Dataset 1</u>	July-September 2005	October-December 2005
<u>Dataset 2</u>	July-September 2006	October-December 2006

**Table A9.7:** summary of 'worsening' mental health for JSA treatment group and comparison groups

	'Pre-intervention'		'Post-intervention'	
	Maintained	Worsened	Maintained	Worsened
<i>Treatment group:</i>				
JSA claimants	549	7	615	8
<i>Control groups:</i>				
JSA claimants (NI)	43	0	33	0
WTC claimants	-	-	-	-
Self-employed WTC claimants	-	-	-	-
Unemployed (non-JSA claimants)	1,046	7	1,121	10

*Note:* table reports unweighted sample *N*s

**Table A9.8:** estimated intervention effect on the self-reported mental health of JSA claimants

Comparison Group	Model(s) A9.11: No covariates	Model(s) A9.12: Covariates	Model(s) A9.13: Matching
JSA claimants (NI)	0.003 (0.009)	-0.001 (0.009)	-0.005 (0.019)
WTC claimants	-	-	-
Self-employed WTC claimants	-	-	-
Unemployed (non-JSA claimants)	0.0006 (0.008)	-0.0001 (0.008)	-0.005 (0.008)

*Note:* Robust standard errors in brackets. Covariates: gender; age; ethnicity; qualification level; disability; marital status; dependent children; socio-economic status and region of residence. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$